A Fallibilist Social Methodology for Today's Institutional Problems

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INTRODUCTION

The rational thinking of individuals is a social activity. It follows rules. But these rules vary from context to context. Sometimes they are rather effective in furthering the pursuit of truth and/or the discovery of solutions to both practical and theoretical problems. But sometimes they are not. Central tasks of the social sciences are the identification of which rules are followed in which contexts, the appraisal of their good and poor qualities, and the development of new alternatives which do better. These problems go beyond the social sciences into political and moral problems on how to live well. The series of essays in this and a forthcoming book explain why these tasks are central to the social sciences and how attempts to solve them can improve to some degree both research in the social sciences in general and moral and political discussions in particular. They thereby offer specific suggestions for how important tasks may be more usefully pursued in various contexts.

The view of rationality as a social activity, carried out by individuals in social contexts, is explained. This perspective is an outgrowth of Popper's philosophy of science and the widespread critical discussions of it. Popper's philosophy developed from its beginnings, as he was still a member of Würzburg School in psychology; it continued until he passed away after his highly productive years as a professor at the London School of Economics. This development was at times quite good, as he moved from his limited description of the logic of empirical research to his broader studies of rationality. But at times it failed to move forward as, for example and central to the perspective of this book, when he rejected the application of his observations, that all rationality is both social and critical, to the rational thought processes of all individuals at all times. And at times his innovations moved backward as, for example, when he tried to reinvigorate the three-world theory he adopted as a student of Karl Bühler: he mistakenly took psychology out of the picture, just as psychology was needed for his three-world theory and he neglected the importance which metaphysical theories may have for research of varying kinds.

The emerging fallibilist perspective forcefully leads to significant modifications in virtually all the social sciences. These modifications are analyzed here in detail. In both economics and in sociology the widespread adherence to methodological individualism—Popper's perspective—or the rationality principle—often a justificationist version of Popper's view—has to be revised. If individual rationality is social, the social conditions and social practices of individuals have to be studied and used to form the basis of broad economic and sociological explanations. If rationality is social, then cognitive psychologists need to study how thinking individuals interact with each other to solve both their everyday and their theoretical problems. And psychology alone cannot provide a theory of how knowledge can be attained, as so many psychologists from Wilhelm Wundt to the present have sought to show without success.

In order to explain how attempts to solve today's problems can be improved, it is of crucial importance to demonstrate how mistakes are made today. But this problem has been handled rather poorly. A central reason for this result is that social scientists and philosophers have sought far too intensively on finding an ideal approach, when the majority of problems can be better handled and more significant progress made, when partial improvements to existing practices are sought.

This mistaken approach is largely a consequence of the mistaken perspective which permeates nearly all social scientific research, that is, the widespread influence of the positivistic philosophy of Rudolf Carnap and his students, collaborators and defenders. The theoretical mistakes of this philosophical movement and its widespread and damaging impact are here examined. A crucial aspect of the reform of the ways we try to solve problems lays in the rather detailed identification of the rules and techniques, which are products of Carnapian positivism, and which cause avoidable difficulties in our ability to solve problems.

PART 1

CARNAPIAN BLOCKS TO PROGRESS TODAY

When Popper put forth his early methodological theories of empirical research, one of his major competitors was Rudolph Carnap. Carnap had already built up a significant reputation with his *Aufbau der Welt*. He had raised hopes high that a new philosophy of science, based on the new developments in logic, would provide an explanation of how certain knowledge based on logic and sensations alone could be obtained. Speculation and errors could be avoided, if one only stuck to the two real aspects of knowledge, that is, to formal logic and combinations of sensations. It was a reincarnation of the hope raised by Francis Bacon; certain knowledge would be attained with indubitable methods. Young scholars such as Willard van Orman Quine came with great hope and excitement to Prague to absorb from Carnap the new revolution in the theory of scientific knowledge.

But Carnap's doctrine was not a finished product. It was at best a work in progress with significant gaps that were not easy to fill. But the hope of finding a certain and final theory of knowledge led many to pursue an allegedly known path to find the needed ways forward to epistemological certainty. If the mistakes in Carnap's approach could be found and overcome, the rewards would be considerable. But Carnap never came to some settled and generally recognized result. The work on finding the perfection of his program simply proceeded until today. And, as the weakness of Carnap's program became clearer, the alternative developed by Popper became stronger. The Carnapian School attracted far more members. It was, after all, an inspiring attempt to reach a goal which had been sought for centuries. But, after Popper's time in New Zealand and his return to Europe following the war, he attracted highly talented followers, who never let the Carnapian scholars forget, that Carnap's program suffered from central difficulties, which might appear somewhat differently at different times, but which were never overcome. The Carnapians knew the strength of Popperian alternatives and sought to block their influence, wherever and whenever they could.

One of the most influential of the attempts to save Carnap's program was made by his student Quine. Quine changed it significantly, but he maintained the Carnapian aim of reducing all knowledge as completely as he could to two aspects, that is, formal logic and the combinations of sensations. The first essay in this section tells the story of how he proceeded from one task arising out of Carnapian inadequacies to another, without ever reaching his Carnapian goal.

1a. Quine's Journey: Did He Find His New and Glittering Sights?

Abstract: The question is posed: Did Quine overcome the difficulties, which he found in Carnap's philosophy? In order to find an answer, the development of Quine's philosophy from his early Carnapian view to his mature philosophy is portrayed. The central difficulty to be overcome is the gap between empiricism and science. Quine's attempt to bridge the gap is deemed two-fold: he offers an empiricist theory of real languages and a so-called naturalized epistemology. In both aspects of his philosophy, he stays too close to Carnap to bridge the gap between empiricism and science. He puts closing the gap with better theories of language learning, perception, etc. on the agenda. But the Carnapian goal of unity vanishes like a mirage, just when one thinks one is approaching it.

1. Quine's Journey

Quine is widely reputed to be that philosopher who has traveled the farthest of all. He started his journey from a position within the confines of the logical positivist camp. But, according to his reputation, he developed his own criticisms of logical positivist views with such sophistication, breadth and depth that his own philosophy became one of the most original, most comprehensive and most challenging of all contemporary alternatives.[1] This widely held view of the development of Quine's

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[1] The list of essays and commentaries on Quine's philosophy is enormous. It is difficult to give a list of such publications which is anything like comprehensive and which would still be critical. They seem to me to fall primarily into three categories. On the one hand, there are detailed studies which seek to get Quine right with, perhaps, a correction here and there. There are also studies or discussions of Quine's philosophy which stem from his disagreements with Carnap or with modal logicians. These studies seek, in my opinion, an even more positivist view than Quine's. There are also a few studies of Quine's philosophy which are critical of his theory but not from a positivist point of view such as Ernest

philosophy has hardly been studied. It is important that a philosopher be accessible to outsiders—in this case to people not belonging to the camp of the logical positivists and their immediate descendants. But Quine is very hard to follow. This may be no blemish since the terrain is rough. And after a pioneer finds a trail to new destinations, those following may hurry along the newfound trail without examining it to arrive at new destinations quickly. But to understand where one is, it is good to know how one got there. A new study of Quine's philosophical journey might throw light on the puzzling difficulties in reading him. Are the difficulties Quine's philosophy faces systematic? Can one explain them by its origins in Carnap's philosophy? Notoriously Quine's philosophy has a paradoxical air: its conventionalist leanings are combined with realist doctrines; its skepticism and fallibilism are combined with conservative faith in up-to-date science. The air of paradox is often taken as a product of Quine's philosophical development. Can it be more basic and objectionable? Is the fascination of this philosophy produced by surreptitious, back and forth movement between old-fashioned Carnapian ideals and new views of science rather than by the substance of a unified view? An outsider's study of Quine's movement from the Carnapian positivism of his youth to his mature philosophy may still, then, be useful.

The question of whether Quine's philosophy is a replay of Carnap or something quite new has, of course, a trivial and uninteresting answer: It is partly new and partly old. But, if we concentrate on one problem, we may be able to say whether, regarding this problem, Quine's philosophy vindicates Carnap even while modifying his view, creates a new and unified philosophy and/or fails to overcome the difficulties Carnap faced

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Gellner's, "The Last Pragmatist," *Times Literary Supplement*, July 25, 1975, pp. 848ff. and Joseph Agassi's, "Ixman and the Gavagai," *Zeitschrift für allgemeine Wissenschaftstheorie*, Bd. XIX, 1988. My own point of view is close to these and I will use them here and there. Dirk Koppelberg in the introduction to his own historical study of Quine's philosophy (*Die Aufhebung der analytischen Philosophie*, Frankfurt: Suhrkamp, 1987, p. 12 and ft. 1) points out how few historical studies of Quine's philosophy there are. Koppelberg wants to fill that gap by discussing the development of Quine's philosophy as well as the place of Quine's system in a historical context. The former task seems to me, however, to be not so thoroughly carried out due to the fact that Koppelberg sees Quine's philosophy—from beginning to end—as a successful system. Nor can I accept the way he carries out the second task, where he views Quine's philosophy as a unification of empiricism and analytic philosophy using Neurath's views. See Agassi above for criticism.

by staying too close to him. This problem is: Can Quine bridge the gap between Carnap's empiricism and science?

To what extent does Quine adhere to Carnap's empiricism? May his view be seen as a continuation of Carnap's program? Quine first sought to employ fundamental ideas of Carnap's *Der logische Aufbau der Welt*. He saw that Carnap failed to bridge the gap between his empiricism and science. Quine then rejected Carnap's empiricism and developed his own. Yet it is not clear how different it is. The question is, does Quine's version of empiricism withstand Quine's critique of Carnap's empiricism? Can Quine's empiricism account for science? This problem is: Does Quine offer us a (re)constructed language powerful enough for science but restricted enough to conform to his own empiricist principles? Alternatively, does he offer some substitute for a language, an empiricist interpretation of scientific language that will perform the same task? Can Quine attain the Carnapian goal of the exclusion of (meaningless) metaphysics from science? If not, does he allow for metaphysics and does he demarcate it from both science and language? In short, are Quine's empiricism and science reconcilable?

The youthful Carnap had hoped to create a unified language for science based on given experience (or terms referring to given experiences) and logic alone, to the exclusion of all metaphysical frameworks. As Quine's numerous references to this project indicate, he was deeply impressed. Somewhere along the way, however, he lost his faith that Carnap could bridge the gap between his empiricist-logical point of departure and real science: the languages which Carnap sought to (re)construct could not carry one from his point of departure all the way to science. Quine went his own way.

As Quine drew his new travel plans, he did not fully abandon the hopes of reaching those beautiful sights of a completely scientific landscape which Carnap had so vividly pictured as he related his youthful dreams of far-off places. The new plans were designed to find new sights similar enough to those unreachable ones, which Carnap had dreamed of, to retain the empiricist attractions of the old but modern enough to be found along new and passable roads. Quine undertook to modify Carnap's travel plans just enough to find these new roads but to modify them not too much to avoid landing in some wholly foreign, thoroughly metaphysical land.

Was Quine's new journey successful? Did he find a passable road to new sights which enable him to find a language adequate for science, which can detect and avoid metaphysics, and which conform to revised but still sufficiently empiricist principles? Or does he fall prey in more subtle ways to the difficulties encountered by Carnap on his journey and discovered by Quine? Does he set out to find some apparently new destination which he cannot reach because he, just like Carnap, allows himself only empiricist provisions and vehicles and these are insufficient to travel along those roads to science, which are open? Or does he smuggle in quite other means of travel when the going gets rough?

Quine's journey was not, of course, the only one whose point of departure was the breakdown of positivism. The path from the positivists—especially Russell and Carnap—to Quine or the path from the positivists to Popper's later philosophy are in this respect two quite similar examples. The attempt by Leonard Nelson to find a quite different road by capitalizing on Hilbert's advances in logic in order to refurbish the philosophical method of Fries collapsed and fused with Popper's.[2] In each case the need to go beyond the limits of old fashioned positivism to new attitudes toward meaning and demarcation on the one hand and/or toward methodology on the other hand were crucial aspects in the development of new views as well as steps which lead to far more comprehensive views of science.

The journey undertaken by Popper was wild and adventurous with interesting side trips and many surprising discoveries. At first blush the journey undertaken by Quine seems in comparison to have been a rather dull trip, which not only stayed as close as possible to the directions which the tour guide—Carnap—had worked out in advance but did so even after it had become obvious that they led one far away from the desired destination of a modern theory of science. Many of the interesting sights that one might have found along the way were thereby ignored. Just why one should try to stay close to the views of early Carnap after they have failed may seem just as puzzling as if one were determined to visit Livorno because one found the road to Florence blocked. Quine's explanations of the rationale for his travel plans are rather sparse and not very satisfying. We might, however, be able to see the wisdom or folly of following Carnap's directions as closely as possible if we look at the journey which Quine undertook. Perhaps we metaphysically inclined foreigners have failed to appreciate the attractions of the austerity of the

[2] For discussion of the relation of Nelson's to Popper's attempt see John Wettersten, "The Road through Würzburg, Göttingen and Vienna," *The Philosophy of the Social Sciences*, Vol. 15, No. 4, Dec. 1985. pp. 487-506.

neo-Carnapian natives. But as an alternative we should keep Popper's journey in mind in order not to miss the best sights, should they fail to be on Quine's plans.

2. Quine's Youthful Home

The point of departure for Quine's philosophical journey is Carnap's research program of *Der logischer Aufbau der Welt*.[3] In this book Carnap tried to explain how knowledge could be unified: all scientific concepts should be reduced to logic, given experiences and constructions built with them alone, to avoid all metaphysical frameworks.[4] This austere research program sought to exclude from knowledge everything outside of science to base all that remained on given experiences alone. Carnap's reconstruction should show the boundary between meaningful science and meaningless metaphysics. Carnap's chief problems were to construct with his limited means a foundation which was broad enough and a superstructure which was strong and high enough to meet the demands of those towering edifices already built by scientists with other, richer and less tidy means.

This task could be accomplished Carnap hoped, by defining all (meaningful) concepts in terms of elementary experiences, perhaps even in terms of one (remembrance of similarity), with the tools of the new logic. A new formal language should thereby be developed in which all true sentences can be constructed with the use of these concepts alone. All meaningful synthetic sentences would be verifiable, since their meaning is rigorously constructed out of immediate experience, and all analytic sentences are trivially true.

This is the core of Carnap's program which so engaged Quine.[5] It is unclear whether Quine ever accepted Carnap's version of this program since it included the construction of a new formal language for science. Already in the thirties Quine proposed that the definitions which laid the basis for axioms or postulates had to evolve out of previous views rather

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[3] Rudolf Carnap, *Der logische Aufbau der Welt*, Hamburg: Felix Meiner, 1961, (first published, 1928).

[4] ibid., pp. 2-3.

[5] Quine suggests, following Carnap, that there are two aspects of a theory of science. These are a theory of concepts or of meaning and a theory of doctrine. We cannot say much of anything about doctrine since we cannot overcome the Humean predicament. (Epistemology Naturalized, p. 72) The theory of meaning, then, is central. Quine is concerned, then, with the conceptual problem of the *Aufbau*.

than be constructed as parts of artificial languages.[6] This task is part of science or mathematics or logic. It is a difficult but technical task. Since the nature of the methods is clear, there is little room for philosophy.

This view, as well as his later view, is nevertheless in accord with Carnap's philosophy insofar as it deems the primary problems of the philosophy of science to be problems of language and the problems of language to be those of explaining how the meanings of words are derived from observation. If we have a more or less pure empirical language, the appraisal of scientific theories should follow automatically.[7] There is little

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[6] Quine begins "Truth by Convention," (first published in 1935) in *The Ways of Paradox*, New York: Random House, 1966, p. 70 as follows: "The less a science has advanced, the more its terminology tends to rest on an uncritical assumption of mutual understanding. With increase of rigor this basis is replaced piecemeal by the introduction of definitions. The interrelationships recruited for these definitions gain the status of analytic principles; what was once regarded as a theory about the world becomes re-construed as a convention of language. Thus it is that some flow from the theoretical to the conventional is an adjunct of progress in the logical foundations of any science." The purpose of this essay is to question the sense of the distinction between the physical and the natural sciences according to which this progress may be completed in mathematics but not in physical sciences. Mathematics might be deemed conventional in the sense that it expresses through definitions logical truths but this does not mean that logic is purely conventional. To show how and to what degree mathematics is a conventional transcription of logic is an important and difficult technical task. But even what it might mean to say that mathematics is merely conventional requires further clarification. In "A Logistical Approach to an Ontological Approach," (first circulated as pre-print in 1939) in *The Ways of Paradox*, pp. 64ff. Quine presents his view that to be is to be the value of a variable. One problem which he wishes to solve in this essay, however, is how we may limit the entities whose existence we presume while extending the power of our language. His proposal is to extend the language by definitions which may function as if they were the names of entities but which may be removed. We may thereby introduce fictions or fictitious entities. There is still an ontological question, however, which is: How economical an ontology can we achieve and still have a language adequate for science? In each of these essays reduction is deemed an aim but in each case it is treated also as a task whose outcome we cannot yet be sure of. Rather, we need methods for carrying out reductions as far as we can. His problems, here, were to devise methods for carrying out these tasks.

[7] The solution to a conceptual problem, that of determining similarity of objects, is developed within a science and renders appraisal of sentences in science clear. The problem of induction thus dissolves into a problem for science. Quine, "Natural Kinds," in *Ontological Relativity and other essays*, New York: Columbia University Press, 1969, pp. 114ff. esp. pp. 121, 138.

more that needs be said about science. Quine's new stance is, thus, quite in accord with Carnap's early philosophy even if not identical to it. On Carnap's philosophy we aim to construct a formal language adequate for mathematics and science and based in a minimum of elementary experiences. On Quine's youthful view one seeks the same goal by modification or refinement of existing theories or better, by translating them into a more proper language.

In his youth, then, Quine was Carnap's fellow traveler. They shared the same goal and many of the same methods. They seemed not to have too much to quarrel about. Even if Quine had already in 1935 stopped following quite strictly the lead of his neighbor, who then brought out his first sequel to *Der Aufbau*, that is, *Logische Syntax*,[8] he set out to reach the destinations proposed by him with many, if not all, of the same methods.

Quine proposed, then, a modification to Carnap's program quite early on. He rejected the view, namely, that the conventional nature of logical truths—should their truth be conventional—laid the foundation for the possibility of the construction of varying languages for science. He proposed instead that movement toward the translation of synthetic or borderline statements into conventional truths had to follow by refinement of languages which one found at hand. The goal of the expression of all mathematics and science in a formal language remained. Reductionism was still deemed possible. But the path to its realization was different. The task at hand was not to show how to construct a language for the expression of all scientific and mathematical truths but to investigate the real possibilities for the translation of such truths into sentences demonstrably reducible to observations and logic. The goal, then, is to find an appropriate translation for all such truths.

3. Carnap's Destination is a Mirage

When Quine announced in 1951 that he deemed central dogmas he had endorsed in his youth to be false, he chose to ignore all those, outside of Carnap, who preceded him and who might have given the new traveler some useful tips about where the interesting sights were to be found if these dogmas offered poor directions.[9] He writes instead of two dogmas of empiricism, where "empiricism" may be deemed an alias given to the

[8] Rudolf Carnap, *Logische Syntax der Sprache*, Zweite, unveränderte Auflage, Wien, New York: Springer Verlag, 1968 (first published 1934).
[9] Willard van Orman Quine, "Two Dogmas of Empiricism," *From a Logical Point of View*, New York: Harper & Row, 1963, pp. 20ff.

logical positivism of his first tour guide Carnap. The alias should not mislead us into thinking that the discussion of alternative plans has a wide scope. Indeed, Quine relates his journey as if, at the time of his departure, he knew of no previous refutations of dogmas of "empiricism ". Only those two dogmas and only Quine's criticisms of them—criticisms which have been subsequently so much discussed while others have been ignored—are mentioned. Quine may like to travel but he mentions foreigners rather reluctantly and the Carnapian natives follow suit.

There are many reasons why Carnap's program of the reconstruction of a unified language for science or even Quine's program of the attainment of the same goal by refinement of languages at hand cannot be carried out. They were quickly apparent to some.[10] The (re)constructions of scientific sentences were by no means adequate, since, for example, there are dispositional terms in science (which thus had to be taken as meaningful) which could not be adequately defined in terms referring only to immediate experiences—even if these latter terms did exist. It was in any case already known by psychologists such as Popper that simple given experiences could not be identified.[11]

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[10] See, for example, Karl Popper, "The Demarcation Between Science and Metaphysics," in *Conjectures and Refutations*, London: Routledge and Kegan Paul, 1963. The development of Carnapian philosophy is hardly well studied due to the fact that the members of Carnap's school do not openly discuss the role of Popper in this development and the members of Popper's school are not interested in it. If we look at Quine's short historical portrayal," Five Milestones of Empiricism," in *Theories and Things*, Cambridge: Harvard University Press, 1981, pp. 67ff., we find hardly any description of the context in which the portrayed developments occurred. Such an essay is too short, of course, to expect much; but it does, in my opinion, reveal the tunnel vision of this school. Quine likes to travel and this was not, of course, the only journey he took. The most important other trip was in logic and mathematics. Hao Wang in "Quine's Logical Ideas in Historical Perspective," in *The Philosophy of W.V. Quine*, edited by Lewis Edwin Hahn and Paul Arthur Schilpp, La Salle: Open Court, pp. 623, has described this development though I do not think that this is the last word.

[11] Carnap in the *Aufbau* appealed to Gestalt psychology among others. (p. 92-93). This could, perhaps, allow him to take a relation—similarity or remembrance of similarity—as a given experience though this still sounds rather associationist. In fact he was not clear as to which psychology should be used and thus what should be taken as given. This had to be left to psychological research. (p. 148) The assumption that something is" given" is, however, already a psychological hypothesis. For Popper on the psychological base of science see *Logik der*

Although this was already known in the thirties, Quine apparently held to his modified reductionist program described above until the 1950's. Just when he rejected this program is, perhaps, not clear. The views presented in 1951 where, he says, developed earlier. But if Quine had been clear about these dogmas in 1947 he would hardly have written his famous essay with Goodman in which nominalism, that is, reductionism, is so clearly proposed.[12] During the fifteen or sixteen years between his first modification of Carnap's program in the middle thirties and the outbreak of a real struggle in the early fifties we find, apparently, a restlessness but no clear-cut outcome.

In "A Logistical Approach to the Ontological Problem" of 1939 his proposal that to exist is to be the value of a variable is presented along with a reductionist program. In "On What There Is" of 1949 this proposal is refined and more elegantly presented but with no reductionist program. In his essay with Goodman, he still seeks a reductionist way out. The program of "A Logistical Approach" splits; the two aspects of his program co-exist for a time and then one part, the strict reductionist part is dropped and a substitute for it sought while the view of ontology is maintained. The heroic efforts undertaken by Quine during this period in the foundations of mathematics were designed to further one aspect of his modified Carnapian program, that is, to carry out some of those technical tasks which were the necessary groundwork for the translation of mathematics into a more refined logical language.[13] How did this effort fit into the various aspects of Quine's long-term program? After the breakdown of reductionism this was no longer clear. A new alternative was needed.[14]

Forschung, Siebente Auflage, Tubingen: J.C.B. Mohr (Paul Siebeck), 1982, pp. 60ff.

[12] In *From a Logical Point of View*, p. 175, Quine forewarns the reader from trying to reconcile the opening sentence of "Steps toward a constructive nominalism," *Journal of Symbolic Logic*, Vol. 12, 1947, pp. 105-122 with views in that book. It should rather be deemed a hypothetical statement for conditions for the construction at hand. In his *Autobiography, The Philosophy of W.V. Quine*, ed. by Lewis Edwin Hahn and Paul Arthur Schilpp, LaSalle: Open Court, 1986, p. 26 he repeats that nominalism was the statement of a problem and not his view. It is not reconcilable with the position found in "On What There Is", which was published the following year. He would be a nominalist, he says, if he could make it go.

[13] Quine, in "Truth by Convention," p. 99, describes, I presume, his own undertaking.

[14] Quine's essay "On What There Is" seems to be the defense of a point of view but in fact is a statement of a program. This program starts with an analysis of how

The doctrine which Quine labels the second "dogma of empiricism," and which—he announces in 1951--must be false and which he now deems the most important,[15] is the following: each meaningful sentence may be reduced to some construction built solely on terms referring to immediate experiences with the tools of logic alone. The central tenet of Carnap's youthful philosophy—a tenet which was maintained even after Quine's early modification of Carnap's program—is false.[16] Before his Wanderlust got the better of him in 1951, Quine's hometown was, indeed, remote. How could he remain for so many years under the impression that hardly anyone had noticed the breakdown of Carnap's philosophy? (He mentions no one and he is, above all, a gentleman.) To be fair one must note that Quine does not merely criticize the more radical reductionist thesis just mentioned, which he announces at the beginning of his essay, but also the weaker thesis that to each synthetic statement there is associated a unique range of possible sensory events such that the occurrence of any of them would add to the likelihood of truth of the statement, and there is associated also another range of possible sensory events whose occurrence would detract from that likelihood.[17] (This is, as we shall see, very close if

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we may analyze what a language presumes to exist: to be presumed to exist in a language is to be a value of a bound variable. This is, according to Quine, a better methodological tool than to talk about names which can be in any case eliminated. But what will this tool bring us? Here the going gets rough. We want both a simple language and a language for science. We want both a phenomenalistic language, because it is epistemologically prior, and a physicalistic language, because we need it for science. We further need a language that is adequate for science. We cannot reduce the phenomenalistic language to a physicalistic one nor do we know how to find a simple physicalistic language which is also adequate for science. We have a host of problems and conditions for neo-Carnapian philosophy but scarcely any answers. Simplicity is a goal but has differing characteristics vis-à-vis differing languages. Reduction hangs in the air. It is seen as not possible but the reductionist tendencies remain strong as in, for example, the discussion of meanings. Mental entities or meanings are apparently rejected because they do not help us reduce meanings to behavior. Can all that be sorted out? We have to look at Quine's development to see how much he can clear the ground in his Carnapian jungle of roots which go this way and that and how far he may travel over seemingly impassable gorges. Finding his way out of his Carnapian jungle is, indeed, a serious matter, for, only when he does so can he explain that integrated view of the philosophy of language, of mathematics and of science upon which all his efforts should rest.

[15] Quine, "A Comment on Agassi's Remarks," *Zeitschrift für allgemeine Wissenschaftstheorie*, Bd. XIX, 1988, p. 118.

[16] Quine, "Two Dogmas," p. 20, pp. 37ff.

[17] ibid., pp. 40-41.

not identical to his own reductionist theory of observation sentences.)[18] Quine notices, then, that Carnap's various attempts to demarcate science from metaphysics on the basis of a demarcation of meaningful from meaningless sentences do not work.[19] They do not work because, as Popper had argued, reductionism fails. The reductionist goal Quine had shared with Carnap is, he now sees, unreachable. It is a mirage.

Although Quine saw that the goal which he and Carnap had been trying to reach was a mirage, he still deemed Carnap a good travel guide. He surprisingly maintains that his response to the breakdown of Carnap's various attempts to demarcate science from metaphysics or to the breakdown of reductionism--the response that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body—issues from Carnap's doctrine of the physical world in the *Aufbau*. This is curious. Carnap's own weakened forms of reductionism were rendered necessary by the breakdown of the *Aufbau* and, as Quine sees, even they will not do.

Quine's appeal to Carnap here, I think, can only be explained if we note an ambiguity between a mere physicalistic hypothesis, on the one hand, and an epistemological hypothesis, on the other hand. Or, to put the same point differently, between a metaphysical and epistemological theory. On the metaphysical conjecture there are only physical events or entities. All theories refer to these only. On the epistemological thesis all knowledge is reducible to statements referring to sensations alone. Having found something like the second in Carnap but having to abandon it, he falls back to the first which he also finds in Carnap. But this now appears to be metaphysics. And this Quine will not have. He needs a new view of it. He later calls this view a naturalistic epistemology, that is, it is not metaphysics but science. We will have to return to this view below to see if it is plausible.

The footnote, incidentally, to this statement concerning the source in Carnap of the idea of his response to the breakdown of Carnap refers to Duhem, who, Quine says, argued well for the (Carnapian?) doctrine he

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[18] See discussion of observation sentences below.

[19] In his "The Demarcation between Science and Metaphysics," Popper defends the same thesis. Popper's account is, however, far more detailed and historically oriented. Although this essay was published in 1964 (circulated since 1956), it relates sequentially those earlier developments which are ignored by Quine and thus gives a far more accurate and informative account of the difficulties of what Quine, in a somewhat provincial frame of mind, calls empiricism.

proposes.[20] But he first learned of Duhem, he also says, only after he wrote this essay. The footnote is added in its republication.[21] We will do better to look more closely at his travels to see how he reaches his own theory of science and to see just what his own destination is. Quine apparently moved somewhat unwittingly, but quite directly, to a Duhemian view of science. But this impression is, as we will see below, misleading: his adherence to Carnapian doctrines led to deep differences between his view and that of Duhem. Perhaps he did not then realize just where the road he was on would lead him and tried later, after he discovered where he was unwittingly headed, to find a different road which would lead him to destinations where he could feel more at home.

4. The Analytic-synthetic Distinction Breaks Down: Quine in Search of a New Destination and a Road to Get There

Quine began his portrayal of the breakdown of Carnap's view with a discussion of the breakdown of the analytic-synthetic distinction.[22] This distinction breaks down, he explains, because there are sentences which we intuitively recognize as analytic even though we have no theory which is adequate to account for them. There are two classes of apparently analytic sentences. One class is problematical and the other not. Non-problematical analytical sentences are logical truths. We may deem them analytic on the grounds that they remain true under any substitution of terms other than independently specified logical connectives such as "and," "or," etc. The problematical sentences are not logical truths as they stand but they can apparently be rendered logical truths if we substitute synonyms for certain parts of them.

Quine chooses his central example to reveal the difference between the two types of sentences. "No unmarried man is married." is clearly analytic since it is a logical truth: if we take the expressions "no" and "un" to be logical particles, then we can change all other components of the sentence by substitution, and it will remain true. The sentence "No bachelor is married.", on the other hand, would also seem to be analytic: it can be turned into a logical truth by the substitution of a synonym, i.e., "unmarried man," for "bachelor". As it stands it is not a logical truth. To show that it is nevertheless an analytic sentence we need clear cut means of substitution or of reinterpretation under which the preservation of its

[20] Quine, *From a Logical Point of View*, p. 41.
[21] Quine, "A Comment on Agassi's Remarks," p. 118.
[22] Quine, *From a Logical Point of View*, pp. 20ff.

truth would be guaranteed. Quine discusses alternatives and says they are not adequate to provide a theory of the analyticity of this second kind of sentence.

The alleged breakdown of this distinction is not, then, a full breakdown of the distinction between analytic and synthetic statements as traditionally conceived. In logic we may appeal to logical truth to identify so-called analytic statements. What bothers Quine is a breakdown of the ability to use the distinction between analytic and synthetic statements to identify any sentence found in (scientific) language as belonging without ambiguity to either the class of analytic sentences or to the class of synthetic sentences. Quine is bothered by the fact that sentences which we ordinarily deem analytic such as "all bachelors are unmarried," cannot be identified as analytic in a clear-cut way without, say, appeal to intuition concerning the meaning of words.

Why should Quine be bothered by that? We have seen that Quine ended his philosophic dogmatic slumber in the fifties. This awakening was due above all to the reverberating sounds of the collapse of reductionism which not even the intense concentration on making improvements in the foundations of mathematics could block out. In order to see why its breakdown was so important for him, we might ask, then, what is the role of the so-called breakdown of the analytic-synthetic distinction in his early view?

This question might seem superfluous. According to Quine's own statements the breakdown would not seem to have been crucial for his change of view. Quine maintains that he, along with Tarski, had long held that the distinction could not be upheld. He deemed this observation something merely negative however and thus saw no reason to publish. This explanation of his failure to publish is curious, however; and thus dubious. If he found a paradox in a Carnapian system would he not publish it? Is that positive? Is Rosser's discovery of a paradox in Quine's system merely negative and thus not worth publishing? We need not reject Quine's explanation entirely, however, if we want to find a fuller explanation for Quine's silence concerning the breakdown of the analytic-synthetic distinction in the years preceding his "Two Dogmas."

On Quine's early modified Carnapian view there could very well be any number of statements whose status was unclear. The task of scientifically minded philosophers or logicians such as himself was to reformulate or translate such sentences into more refined languages to make them clear.

The borders of ambiguity should be thereby pushed back and two clear realms of synthetic (empirical) statements and of analytic statements extended. The disagreement with Carnap over whether synthetic sentences and analytic sentences could be (now) clearly distinguished was a methodological problem: should one start with formal languages which presume such a distinction and seek to construct a language which rigorously holds to it or should one work towards such a language by extending the range, power and simplicity of existing formal languages such as that of *Principia Mathematica*?

This disagreement had already been aired in the thirties. The rejection of the adequacy of current views of the analyticity of such sentences as “all bachelors are unmarried” was, in the light of this early dissent, merely a clarification of his view that there exists a wide gray area of sentences which need to be clarified and analysed. One could even leave open the degree to which one could do that provided that one could approach the same goal. And this goal was still the reduction of all sentences to formal, analytic ones on the one hand and statements of fact, on the other hand. As long as the goal is clear and as long as we may get closer and closer to it, the possibility that it could not be fully reached could be deemed to pose no serious problem. We would learn these things more clearly as we got closer to the goal and discovered the limits of our progress in detail.

In the light of the rejection of reductionism, however, this all looks quite different. The rejection of reductionism means that the goal which had been sought can be sought no longer. One cannot get closer to it as one refines the language(s) of mathematics and science. The existence of the gray area of sentences which are neither clearly analytic nor synthetic is now deemed inherent in language. So far as I have noticed Quine does not discuss possibilities. But it seems to be his view or to follow from it that an increase in precision in one area might bring with it some disadvantages elsewhere as the uses of a language in varying contexts are brought together. The need not only to use sentences which are not clearly classifiable as either analytic or synthetic but also to use sentences which are embedded in particular languages in such a way as to defy further refinement without too great a cost poses not merely a technical problem, that is, a problem of finding ways of translating them into a language in which their status will be clear. Rather, we must come to terms with the fact that all of them are not even in principle capable of clarification. To judge analyses of individual sentences or languages, new criteria, new goals are needed.

We find here a shift away from technical problems of how to increase the scope, power and simplicity of formal languages to new, deeper problems of how to appraise the progress of analysis. Quine does not make this switch in concerns clear. One role of the theory of analytic statements on his earlier view, he fails to explain, was to provide us with a goal: as many sentences as possible should be identified as either synthetic or analytic. Technical success is progress towards this goal. The theory of this goal had been, following Carnap, reductionist. This is clear from Quine's criticism and even his suggestion that the breakdown of reductionism and the analytic-synthetic distinction are the same.

Quine's claim, then, that he had rejected the view that the analytic-synthetic distinction was adequate before he rejected reductionism is ambiguous. Before he rejected reductionism he had only rejected the theory that our demarcation of all sentences into synthetic (empirical) and analytic could be complete. He could still maintain, however, that we knew what our goal was. We knew what it meant for a sentence to be analytic. Indeed, if reductionism is possible, we must have a good theory of the nature of the difference: any successful reduction will make things clear.

In his later view he not only rejects the view that methods for demarcating analytic and synthetic statements can ever be complete but also that we do not even know how to analyze some sentences. We have no theory of analyticity. When he explains that he had rejected the distinction for some time before he published "Two Dogmas," he fudges the difference in levels, between being able to demarcate all sentences, on the one hand, and being able to theoretically explain the difference for a wide and indeterminate class of sentences, on the other hand. Before "Two Dogmas" he held that all statements were not now identifiable as analytic or synthetic, perhaps even that we could not place all statements in one class or the other. After "Two Dogmas" he held that the goal was not clear, that we had no methods for extending such classifications beyond logical truths.

We find here, then, the significance for Quine of the "breakdown" of the analytic synthetic distinction. This significance lies in the fact that, without the view that one can aim to put any sentence into one of the two classes, one can no longer use this distinction as a strict methodological guide. It no longer sets the goal for one's research.

5. Quine's Criticism of Analytic-Synthetic Distinction: The Importance of Quine's Positivist Home

Quine begins his criticism of the analytic-synthetic distinction by showing that Carnap's theory of state-descriptions does not explain how all meaningful statements can be demarcated into two classes of synthetic empirical and analytic. This we grant immediately. It was known long before 1951 that this view was untenable: it continues to identify synthetic sentences with empirical sentences. But Quine proceeds and says that we cannot explain the analyticity of his problematical sentence as due to the fact that "bachelor" is defined as "unmarried man." Since we are quite naive—among those who are easily soothed—that seems all right to us. Indeed, Quine's argument looks suspicious. For, Quine asks how we would know that the words are defined as the same. We see here no problem. They are so because we stipulate that they are or because we ordinarily presume that they are. Quine is not satisfied with that. He wishes to know how we can find out if the sentences really have the same meaning and suggests we have no way of doing that if we do not know in some other independent way which words are synonymous with which. Now we readily admit we can make mistakes. Our stipulations may, for example, turn out to be inconsistent or we may find that we use words we thought were synonyms with different meanings or shades of meanings. There are no guarantees. But this does not bother us either. We deem some sentences analytic on the basis of our stipulations and assumptions and, if we discover we are wrong—if differences crop up—and they are synthetic, we change our minds.[23]

But, Quine insists, we do not even know what we are talking about. Our words, he says, need clarification and presumably with reference to linguistic behavior.[24] We are puzzled. Why do they need clarification and why with reference to linguistic behavior? This seems to be an appeal to a reductionist theory of meaning which is rejected in the same essay. Why should standards for a theory of science or of language be higher than those for science itself? Quine hurries on without explaining. While underway we seek to understand. The only plausible interpretation we find

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[23] For a comparison of Quine with Popper on translation see John Wettersten, "The Place of Bunge," in *Scientific Philosophy Today*, Dordrecht, D. Reidel, 1982, pp. 465ff. See also Karl Popper, "Logic without Assumptions," *Proceedings of the Aristotelian Society*, 1947, pp. 251ff.
[24] Quine, *From a Logical Point of View*, p. 24.

is that our views of synonymy and definition on Quine's view will only be clear enough when we specify necessary and sufficient conditions for them.[25] This seems to us no problem. We borrow from and modify somewhat Quine's proposals which he rejects and suggests that it is necessary and sufficient for one word to be a synonym of another, if we can replace one word for the other in any sentence and the sentence remains unchanged in all other respects.

We do not expect Quine to be satisfied. He will complain that we do not know just what "all other respects" means and that there will be problematical cases where, for example, one sentence may remind us of some other context or have emotional content, etc. which we might deem a relevant change or not. We are not bothered by this. For, we can judge from case to case and context to context if these are relevant or not and whether, for the purposes at hand, we should deem the words synonyms or not. This we say, in answer to Quine, is good enough. We concede, of course, that the boundary between analytic and synthetic statements in ordinary and even scientific languages is not sharp and do not even preclude that a sentence may be deemed analytic in one context and synthetic in another due to shades of meaning. But we find it possible to separate our theory of analytic sentences—our idea of them—from the process of their identification. We may admit mistakes. Perhaps, as Quine suggests, we have admitted words into expressions which make analytic appearing sentences synthetic. We propose to inquire into that piecemeal and provisionally and believe the costs of that are far less than the costs Quine will have to pay for his all-or-nothing strategy: either we have necessary and sufficient conditions for the unambiguous identification of all analytic sentences or we have no clear distinction.

We see here that Quine's criticism of the analytic-synthetic distinction has two aspects which he deems inseparable simply because he remains so positivistic. On the one hand, there is the theory of what an analytic sentence is. On the other hand, there are the methods for correctly identifying analytic sentences. Without a sure-fire method Quine maintains we have no theory. This is to identify the truth of a statement with its proof—or provability.

[25] ibid., p. 25.

6. The value of the Breakdown for Quine's Homesickness

Quine's own conclusion concerning the breakdown of the analytic-synthetic distinction seems to deepen the problems for Carnapian positivism, which had been raised by the breakdown of reductionism. But in Quine's hands the rejection of this doctrine turns out to have another, positive purpose as well.[26] Before the publication of Quine's essay it was known by Popper and others that Carnap's reductionism could never be successful. Once having seen that, it would be quite natural to reject attempts at reduction, as Popper did, and pursue different methods. Quine's difficulties with these approaches appear to have been a consequence of his view that they moved too sharply away from Carnap's program: they abandoned the project of deriving all meaning from observation. Though Quine's rejection of the analytic-synthetic distinction appears as a criticism of Carnap, it is also a means of saving him by stopping the movement away from reductionism from getting out of hand. We do want to be carried away by Wanderlust!

If we reject reductionism and maintain the view that there exists a clear distinction between analytic and synthetic sentences—even while conceding that we cannot always say which are which and that some are ambiguous—we have at least three types of sentences in (scientific) language. We have clearly analytic sentences. (Even Quine admits these in the cases of logical truths and of shorthand expressions at least.)[27] We have empirical statements whose meaning is not reducible to observations but which are clearly synthetic. But we also have statements which seem to express some content—even content found in science—which are not empirical at all. These sentences would be, one would think, metaphysical and meaningful and especially so when they can express some of that content found in science.

When Popper abandoned reductionism along with the theory that all meaningful statements are proven, he took this move immediately. If we take, for example, a sentence like "each force can directly vary into any other force," we may deem it metaphysical and even part of a scientific

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[26] Quine says that the two doctrines are "at root identical." ibid., p. 41. But this is only true—whatever exactly "at root identical" means—if one continues to presume that all synthetic sentences are so only in so far as they are empirical. Only under this assumption does the failure to achieve demarcation of synthetic sentences by reduction indicate without further ado a failure to demarcate synthetic from analytic ones as well.
[27] Quine, *From a Logical Point of View*, p. 26.

research program.[28] This sentence is no definition and not empirical. It might be consistent or inconsistent with physical theory. Quine cannot condemn it outright as meaningless. It would seem that Quine has no other choice here than to follow Popper's move. Perhaps Agassi offers an even better example: Quine's own doctrine of the indeterminacy of physical theory.[29] It is neither analytic nor empirical but, we may presume, meaningful.

The hidden agenda of two dogmas is, it would appear, to devise a strategy for limiting and/or reinterpreting such sentences even while allowing them, out of necessity, in science (and in Quine's own philosophy). Quine's procedure, it would appear, is to explain the character of (metaphysical) sentences with his positivist principles, that is, with reference to analytic or synthetic (empirical) sentences. They may then be deemed analytic-like insofar as they concern the meanings of words or empirical-like insofar as they have content. Or, highly metaphysical sentences have less and less meaning, so we should and can limit ourselves to meaningful metaphysics.[30]

Quine's rejection of the analytic-synthetic distinction, then, helps him to avoid, as Carnap's program requires, the growth of metaphysics. He can avoid this growth by deeming those sentences which are not reducible to observations but which are also not analytical as occupying a gray zone between the two. This gray zone can be treated as a matter of language or of the relations between concepts, on the one hand, and as nearer to or farther from empirical statements, on the other hand. Unavoidable sentences traditionally deemed metaphysical may be placed there. The distinction between an analytic statement which says nothing about the world and a metaphysical statement, which says something about the world but is not testable, is thereby fudged: The more metaphysical a statement is the more it resembles an analytic statement, i.e., the less it says about the world. The rejection of the distinction between analytic and synthetic sentences is necessary to create this new and obscure status for

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[28] Joseph Agassi, *Faraday as a Natural Philosopher*, Chicago: The University of Chicago Press, 1971, p. 210ff.
[29] Agassi, "Ixmann and the Gavagai," p. 113.
[30] Quine, "A Comment on Agassi's Remarks," p. 117. "Repeatedly you have me banning metaphysics. Maybe you have in mind my rejection in "Epistemology Naturalized" of a "first philosophy." I see metaphysics, good and bad, as a continuation of science, good and bad, and grading off into meaningless." I am still puzzled. Is metaphysics a mixture of analytic and empirical of some sort? If not, how does it "grade off" into meaningless? If he does not wish to ban metaphysics—this is, unfortunately, impossible—does he not want to limit it wherever he can?

statements which one must accept as meaningful but which are neither reducible to observation nor clearly identifiable as analytic in some traditional sense. Since all analytic statements are now deemed conventional, apparently metaphysical statements or metaphysical aspects of scientific statements may be allegedly treated either in the same way as analytical statements, that is, as conventions concerning the use of words[31], or as statements with some empirical content or, more likely, as a mixture of the two.

Quine's new reconstruction of language is only possible if new structural supports are found. These should play that role in the new reconstruction, which analytic sentences played in earlier efforts. The presumed breakdown of the analytic-synthetic distinction is, however, helpful here. Without the breakdown nothing like analytic sentences could serve this purpose. The analytic sentences one could identify and/or count on would not be numerous enough or strong enough. Metaphysics would be needed. With the breakdown things look different. One may fudge a bit and this seems to positivists a good thing: sentences may be deemed to pass gradually from empirical ones to analytic ones and those in the middle of this transition may serve as the new supports. The new structure may even be deemed a dome-like construction with no pillars at all.[32] The pressure of the parts of the structure against each other is what holds it up. Such pressure depends, of course, on the maintenance of some definite arrangement of the parts and this function is filled by analytic-like sentences. In order to preserve this pressure, then, the system must only be changed slightly.

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[31] Quine, *From a Logical Point of View*, pp. 44. Quine here defends a strict instrumentalism or conventionalism. He denies belief in objects, and science is a mere tool for prediction of future events on the basis of our knowledge of past events.

[32] Quine uses this metaphor in *Word and Object*, Cambridge: Massachusetts Institute of Technology Press, 1960, p. 11. Paul Gochet, Quine zur Diskussion, Frankfurt: Ullstein Materialien, 1984, p. 53, sees here an advantage for Quine. His view of the empirical foundation of science—it is supported by all of the stones of the foundation simultaneously—is more radical than Popper's. Popper clings to the metaphor of pillars which are driven deeper into a swamp. It is, indeed, true that Quine sometimes seems to adopt more radical and skeptical views than Popper does. But Popper develops his view in accord with his principles. When Quine's empiricist principles lead him into trouble he retreats to naive views which he thinks he can defend by appeal to scientific common sense—as we shall see below—or even metaphor. Popper avoids such difficulties. But we, also, do not wish to make too much of metaphors: they help us to explain points of view but are, themselves, no arguments.

The new analytic-like but not quite analytic sentences have content as glue or as organization but not directly as descriptions of the world. They have to be maintained by maintaining convention.

For those of us, however, who have not grown up among Carnapian natives, it seems to be one of Quine's unexamined and somewhat wooly metaphysical dogmas that being naturalistic or pro-science or against wooly metaphysics is the same as being (neo-)Carnapian, that is, is the same as accepting either the doctrine that all evidence and therefore all meaning issues from the senses or some other weakened empiricist doctrine of meaning.[33] But even though we might find Quine's stance somewhat dogmatic and prefer an experimental to a dogmatic attitude toward neo-Carnapian views,[34] we can still examine this program to see what insights, if any, it has to offer, to see whether this journey might be more interesting than those others which seem to offer some excitement and adventure and to see whether it is inconsistent and/or subject to effective external criticism, say, from psychology. By looking at Quine's development we may be able to better understand his motivation and to

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[33] For a discussion of the relationship of views which are pro-science or anti-science with differing attitudes toward rationality see, John Wettersten, "Russell and Rationality Today," in *Methodology and Science*, Vol 18, No. 2, 1985, pp. 140-163.

[34] Quine emphasizes that he himself favors an experimental attitude. See "On What there Is," in *From a Logical Point of View*, p. 19. Yet on the same page he asserts that the phenomenalistic conceptual scheme has epistemological priority. I would think that would be a matter for science and metaphysics to decide if tolerance and pluralism were to be taken seriously. In "On Mental Entities," in *The Ways of Paradox and Other Essays*, New York: Random House, 1966, pp. 208ff., Quine discusses the postulation of mental entities but as the positivists did, i.e., primarily as sense data. He argues convincingly that perception depends not merely on input or stimulation but on, what some of those who postulate the existence of mental entities call, mental activity. He proposes a materialist account as a hypothesis. This is fine. But why should we not have two competing scientific hypotheses? The problems which Quine discusses seem, indeed, to call not for the rejection of mental entities but for new ideas about them as Oswald Külpe proposed. This has nothing to do with the alleged crucial insight of empiricism that any evidence for science has its end points in the senses (p. 212). So far as I can see this "crucial insight" is trivial unless it is taken, say, to be a doctrine of meaning according to which all meaning is derived from the senses as Quine sometimes seems to think. Otherwise, it has hardly anything to do with methodology or metaphysics. It is consistent with Popper's methodology and three world view. Yet Quine gives the impression he wishes to slide from this thesis to empiricism. If he does so, it would seem to be a slight of language.

examine whether the new adds to or complements or improves the old or whether it becomes confused and scholastic or whether it leads to a dead-end.

It would appear that Quine's strategy leads to Duhem's theory of science as Quine suggests. But the creation of the idea of this no man's land of sentences, which are neither quite analytic nor quite synthetic but some mixture of them, is by no means equivalent to having a map of it. According to his early view the task of analysis is to move from one to the other, from existing ambiguous sentences to a language which clearly identifies sentences as one or the other. The movement to Duhem is correct in that just as for Duhem, for Quine of 1951 and after, the direction is not linear. One can have adjustments of theory for other reasons than the clarification sought for by Quine. Clarification or simplification must, therefore, be relative to a (changing) system. Before we can see if it is Duhem's view which arises out of Quine's analysis, however, we must see more clearly what Quine's intended destination is and how he hopes to get there. Can Carnap be saved by giving us a map of scientific language which includes the no-man's land? Can metaphysics be banned enough, science included and empiricist principles adhered to with this new technique?[35]

[35] The analysis offered here differs from that offered by Agassi, who, if I understand him rightly, deems Quine's criticism of Carnap a cause for his more metaphysical realism. Agassi thus sees the apparent conventionalism of "Two Dogmas" as a mere negative move waiting for the more positive result to follow. I see this differently: Quine's negative result should retain empiricism as much as possible but not prohibit realism, which he later endorses. In order to render his own interpretation of Quine coherent, Agassi is forced to modify Quine's view so as to render his thesis of the indeterminacy of physical theories, of the indeterminacy of translation and his physicalism mere metaphysical hypotheses. Quine's philosophy then moves closer to those of Popper and of Agassi even though they reject these speculations. Agassi notices the tension and asks Quine to clarify matters. But Quine protests against Agassi's proposal. On my view Quine's critique leads to conventionalism and not to realism, but Quine seeks to avoid the consequences and is inconsistent. Agassi sees also an inconsistency and seeks to reinterpret the Carnapian remnants and in so doing renders Quine more modern than he cares to be. Quine could have moved in the direction of Popper's view quite easily, since both he and Quine may deal with problems of analyticity as piecemeal, technical problems. But this move renders each decision concerning analyticity one for a specific context alone. The general direction of analysis without reference to specific contexts or problems is thereby lost and with it the general presumptions of analytic philosophy which Quine clings to.

The description of the logic of the situation in this way does not, of course, correspond directly to Quine's. It is, I think, consistent with it. It is simply that Quine did not explain his motivation or his concern to block the movement away from Carnap from leading him too far from home. We might, of course, ask why he did not explain his own development. Why, for example, if he first saw that reductionism must go and only then that the rejection of the analytic-synthetic distinction might save empiricism from complete defeat did he not say so? I have no answer.

7. Travel Plans: Three New and Glittering Destinations Resembling Carnapian Ruins

If we can believe his travel reports, after his Carnapian travel directions broke down, Quine looked at few alternative roads. When Quine rejects positivist dogmas, he seeks to save positivist dogma. He set to work immediately to modify (Carnapian) logical positivism as much as needed in order to avoid Carnap's difficulties but as little as possible in order to salvage what one can of his so-called empiricism. If strict reduction is not possible, we can at least, Quine seems to think, base all meaning in observation or experience. And, if we cannot reconstruct an artificial language for science, we might still be able to explain how (our) language might be (re)constructed such that only the proper part—which is science or includes science—will be deemed to have content. Empty metaphysics may still be avoided, even if demarcation is a matter of degree.

Quine sought to follow new roads going in the same general direction as those he had used on his first journey to the old world. This was in the thirties when he had first come to know Carnap well. Of course, changes in old plans had to be made. In the thirties he had already modified Carnap's plans by substituting for the reconstruction of an artificial language for science, a reconstruction of the real language of science. A deeper change was required in the fifties: this reconstruction had to be built with other tools than the reduction of sentences to given experience and logic alone. How and where could one find them?[36]

[36] Although, so far as I know, no one has described Quine's development just as I propose to do, the basic themes of the theory of knowledge, the theory of language, the so-called naturalistic epistemology and the theory of science which should result are hardly new. It is also clear that this is not a mere sequence: In all phases, for example, Quine has had a theory of science, which in my opinion, changes as he proceeds.

There seems to have been two major destinations in Quine's new travel plans. The first destination was a theory of knowledge translated into a theory of natural or scientific language or, to put the same point differently, an empiricist account of (scientific) language.[37] Although Quine abandons Carnap's destination of the translation of epistemology into a theory of an artificial, reconstructed language, he still hopes to find solutions to two central (neo-)Carnapian problems. These problems are (1) how can we demarcate empirical sentences or systems of sentences (or the next best thing to them) from empty or meaningless ones (or empiricist interpretations of such sentences from empty, metaphysical interpretations)? And (2) how are those simple empirical sentences, which are fundamental for science, based in experience? Solutions to these problems should, like Carnap's, yield a theory of the language and of the borders of science. As a result, he should (3) demarcate science from meaningless metaphysics without, of course, excluding too much.

After Quine had set out from the *Aufbau* he sought to reach what I deem a second destination as well. He calls this destination "naturalized epistemology." It is created by translating epistemological problems into scientific ones. Quine deems this destination a mere elaboration on the first.[38] It is, however, unclear just when this second destination became a goal. Perhaps Quine reached his first destination and was not only rather disappointed with the sights to be found there but also had already found himself at a second destination without really having planned to be there. Once there, he liked the sights and decided to look around. (This is, admittedly, a conjecture, since reports of disappointments and accidents are not found in his travel reports even though most any traveler meets with them from time to time.) A theory of language such as was to be found at the first destination could not contain all that he had hoped for. The theory of science which should have been obtained by translating the problems of epistemology into those of a theory of natural or scientific language (or by providing an empiricist account of language) was not complete. Gaps had to be filled in to account for the content and structure of scientific language.

Quine traveled further, then, to a second destination which he christened "naturalized epistemology." Now naturalized epistemology is not epistemology at all. Rather, it is an attempt to explain and supplement the

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[37] I have here primarily in mind "Speaking of Objects" and *Word and Object*.
[38] I have here in mind such essays as "Epistemology Naturalized" and *Roots of Reference*.

structures he had looked for in vain at the first destination by translating them into problems of the sciences. He hopes thereby to fill gaps in his empiricism by explaining epistemological problems away without the need for metaphysics. At this second destination we should find an explanation of language in terms of physiology, perception theory and learning theory at least. Such an account of language will not merely be a substitute for a theory of language based on logic and observation alone but also the completion of that explanation of knowledge which one had hoped in vain to find at the first destination. An empiricist account of language in general should account for scientific language in particular by explaining how language is learned on the basis of stimulations alone.

It had become necessary to travel beyond the first destination. At the first destination the limitations of language which followed from empiricist doctrine were examined. And some attempt was made to explain how such a limited language could be adequate for science. But these explanations were incomplete. In some sense all meaning of synthetic concepts issued from senses. As consequences we find the indeterminacy of translation and the possibility of any number of theories compatible with observations. Is science and knowledge still possible? The trip to the first destination had not made that clear; the trip to the second destination should tell us. The map of the no-man's land created by fudging the difference between analytic and metaphysical statements should, we hope, be drawn in detail.

Perhaps, of course, Quine would say that the description of his travel plans given here is inaccurate. He might say, for example, that he always intended to go straight from *Der Aufbau* to the second destination as well even though he only explained that explicitly after he was well along on his journey.[39] We have to keep this in mind but it may still be useful to keep the first destination in mind in order to better understand the passage from the theory of knowledge to the theory of language to scientific

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[39] In *Theories and Things*, Cambridge: Harvard University Press, 1981, in his response to Schuldenfrei, p. 186, Quine says that Schuldenfrei is mistaken in thinking that the points of view found in *Word and Object* and in *Roots of Reference* differ. Rather, *Roots of Reference* is an expansion of the third chapter of *Word and Object* just as *Word and Object* was an enlargement of "The Scope and Language of Science" or the last section of "Two Dogmas of Empiricism." Perceptual similarity and stimulus and response play the same roles in both books. I find the story of the journey as I have outlined it nevertheless appropriate, since the growth of specific aspects change the nature of the destination, to say nothing of the development of theories of it.

explanations of language to a theory of science which began with Carnap's early work and ended with Quine's later work.

As he set out on his journey, Quine saw in his new travel plans not merely a necessity to broaden his perspective but also an opportunity to visit new and exciting places. If his trip was necessary to give a scientific account of language in order to preserve as far as possible good old-fashioned Carnapian empiricism, it was also an opportunity to give this empiricism a scientific foundation. A scientific foundation of empiricism would obviously be better than an appeal to (first) philosophy.[40] As a result of the development of this new, more scientific, program, then, we should find a scientific account of language which provides an account of knowledge as well—just as Carnap had dreamed of. Wooly metaphysics will be banned forever by right minded thinkers from the theory of science which, as is appropriate, may be left the scientists.

In his numerous travel reports Quine seeks to give a full portrayal of his two destinations such that any new traveler will find his way quite surely through the many narrow, winding and crossing streets which are just as characteristic of them as they are of Fez and Toledo. Unfortunately, however, the reports of his journeys to these two destinations seem jumbled. One follows a narrow street as carefully as one can in hopes of seeing a famous sight typical of epistemology-cum-theory of language and finds instead a quite unexpected type of structure which apparently belongs at the second destination of a scientific explanation of language or perception. One does not quite know if one is at the proper destination and when one tries to consult the map again—as many a traveler has done before—the difficulties are severe as many discussions among those travelers who have used this guide have shown.[41] Perhaps when Quine

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[40] Quine's rejection of so-called first philosophy is apparently a rejection of using philosophy as a foundation for science. I take it that he here has Carnap in mind. It is the breakdown of Carnap's radical reductionist program which makes the difference: Weaker forms such as he tried later do not eliminate everything but immediate experience and, if they do not, (partial) reduction is no longer important. ("Epistemology Naturalized," p. 78). This leads to a rejection of what he calls first philosophy which was, I take it, an attempt "to base science on something firmer and prior in the subject's experience," that is, Carnap's philosophy. ("Epistemology Naturalized," p. 87). As usual Quine deems all interesting philosophy to consist of *Der Aufbau*. He would seem to think that if that fails we can do away with philosophy.

[41] This is, perhaps, evident due to the many instances of discussions of Quine's philosophy, which take the form of a mere determination of what the theory is.

writes his travel books he has the trouble of a traveler who has, over the years, been to so many places that he cannot remember just where he saw what and when he saw it. Or perhaps the way is simply complex. Let us try to keep our minds open. We must note, however, that for many a traveler before us it became quite difficult to find a consistent or satisfying picture of either destination much less of both. We are forewarned.

We should also note that although Quine deemed his trips to these two destinations quite sufficient to yield a theory of science, we are by no means so sure and will, therefore, be forced to deem this a separate, third destination. It was never quite directly on Quine's travel plans, since he thought that he would find all that was worth seeing at the first two destinations. This is due to the fact that he identifies a theory of language with a theory of science or a language with a conceptual scheme. Yet all proper language is not scientific language. Since Quine is a holist, there are necessary and allowable degrees of metaphysics. Scientific method, which he says he has nothing to say about, is needed to identify proper scientific views. We may find this somewhat puzzling and will have to look further to see if the theory of science is reducible to a theory of language and if not what Quine's view is and why.

8. Destination One: Epistemology as an Empiricist Theory of Real Languages

Let us start, then, by retracing Quine's journey to his first destination. If we begin to get lost in back alleys, however, it may be better to seek our own way rather than to remain committed to following Quine's travel. As we head out on our way there are, we remember, two famous sights which we hope to visit: (1) the theory of the empiricist demarcation of those theoretical statements which are necessary but not reducible to observations, (2) the theory of observation sentences or those sentences which guarantee empiricism in some sense or other. Together these should yield an explanation and demarcation of scientific knowledge which thereby shows the adequacy of an empiricist language for science. Carnap's difficulties should be overcome with modifications which do not lead too far away from his dream.

One might look, for example, at the effort spent on exposition and correction in *The Philosophy of W.V. Quine*. Whether this is excessive or a sign of difficulties is, of course, a matter of perspective.

On the first day of our journey we hope to find our way to Quine's new empiricist demarcation of those theoretical statements which are necessary for science but not reducible to observations. Above all we hope to see the new explanation of the meaning of these sentences (or systems of sentences) as well as the new explanation of their adequacy for science. We wish to see if Carnap's problems have been overcome. It is difficult to find our way to this sight, however, and we begin to lose our way.

In *Word and Object* Quine develops a new interpretation of the meaning of sentences such as those we find in science concerning, for example objects, which, we would expect, explains these things. This interpretation is, in the first place, a new empiricist theory of the limits of proper language. But it also explains how these limits are compatible with science. This theory presumes, on the one hand and as we know from "Two Dogmas," that no reduction of all (meaningful) sentences to observations or experiences is possible. In scientific languages we must always find talk of objects even though there is no equivalence between sentences about objects and sentences about that which is empirically given, which Quine calls stimulations, where stimulations, taken strictly, are impingements on the organism through light, pressure, etc.[42] But, on the other hand, there can be no difference of meaning which has no empirical difference.[43] We must interpret the use of these sentences solely in terms of the given. The gap between empiricism and science which Quine seeks to bridge is, we see right off, a wide one.

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[42] Quine describes his movement from use of the term "experience" as the boundaries of science to "surface irritations" to "triggering of sensory receptors" in *Word and Object*. See, "On the Very Idea of a Third Dogma," *Theories and Things*, Cambridge: Harvard University Press, 1981, p. 40. He feels, nevertheless, that he can still talk of stimuli and responses and ostensive learning without explaining the gap between triggering and perception. Having once used a more sophisticated expression, he deems it allowable to return to the former naive one. We find this quite suspicious and will have to keep our eyes peeled to see if everything is what it appears to be.

[43] Quine, *Word and Object*, on p. 26-27 Quine makes clear that he seeks to avoid any talk of meanings independent of dispositions to verbal behavior. This may seem at first to be an assertion of a severe empiricism reminiscent of Carnap. Then again it may appear trivial since any possible difference in meaning could be built into some verbal response. I find the tension between the two readings unresolved. If it is radical, what does it exclude? If it is trivial and allows all theories, why bother with it?

Undaunted by difficulties Quine clings to some form or other of neo-Carnapian empiricism. His adherence to neo-Carnapian empiricism (or reductionism) in the face of these difficulties is expressed in his new hypothesis of the indeterminacy of translation.[44] This new hypothesis is his own negative theory of the (empirical) meaning of scientific sentences. According to this hypothesis there can be no determinate translation of sentences of one language into sentences of another language. This is so because sentences taken in isolation from other sentences do not have, in regard to differing speakers, fully definite links with non-verbal stimulation.[45] Since these links are the sole source of our ability to distinguish meaning, the looser the link, the more lee-way there is for differing and nevertheless correct translations. There is in fact nearly always leeway, though for some less than others, as we will see in his theory of basic sentences.[46]

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[44] Quine in *Word and Object* starts his discussion of translation and meaning—his "empiricist" themes—on p. 26. Before that we find a discussion of science including evidence and posits and truth. He here seeks to hold to an empiricist view of science, since all evidence is stimulation (or some more carefully worded expression therefore) organized above all by the criterion of simplicity. He moves away from his denial of belief in theoretical objects and says they are not mere make-believe and we cannot do better than "occupy the standpoint of some theory or other"—whatever that should mean. Quine has begun to move away from his strict conventionalism and has noticed that he needs more for science. But these are sketchy remarks indicating a conflict and not a resolution. We pass over them for now. See also "Posits and Reality," in *The Ways of Paradox*, pp. 233ff.

[45] Quine, *Word and Object*, pp. 27ff., p. 45.

[46] Quine, *Word and Object*, pp. 43-44. The problem of our knowledge of specific links would seem to be a separate one from the knowledge of their existence or non-existence. Quine denies their existence. (Quine, *Word and Object*, p. 73.) But he uses discussions of how we gain knowledge of specific links to explain his thesis of the indeterminacy of translation. (Quine, *Word and Object*, pp. 28ff.) This is misleading but throws light on the alleged difference between the indeterminacy of translation and the indeterminacy of physical theories. The first thesis asserts both the dependence of meaning on contexts (stimulations) and analytic-like sentences alone and the incapacity of these to determine in any case a single correct translation. The second asserts that any class of phenomena may be explained or organized in any number of ways such that any true physical theory is only one of these possibilities. Quine maintains that these are different theses. And we might concur since one thesis concerns the definitiveness of the meaning of any theory and the other the capacity to use various theories of whatever definitiveness of meaning to explain the same phenomena.

But for Quine the former thesis interpreted empirically—the only way he can interpret it—simply means that we can organize the phenomena of responses to stimulations in many ways consistent with these responses. The assertion that there is no other meaning besides the empirical content and the analytic-like statements is equivalent to this. Given Quine's empiricism, then, the first thesis becomes a mere special case of the second thesis. Quine nevertheless speaks realistically (metaphysically) when he explains the independence of the indeterminacy of translation, as if he were saying something more, and this leads quite naturally to Agassi's interpretation of this thesis as a metaphysical conjecture. But Quine himself must explain the meaning of this view empirically, that is, it only asserts that we cannot empirically distinguish between different translations. It is still metaphysical but in this formulation equivalent to the second thesis, the (other?) metaphysical hypothesis; hence, the confusion about the relations between the two hypotheses. Quine protests against the interpretation of the indeterminacy of translation as a special case of the indeterminacy of scientific theories in his response to Chomsky, *Words and Objections*, p. 303. He claims there that, if we should adopt his fully realistic attitude toward contemporary science, and from this realistic point of view, the totality of truths about nature, we should see that the indeterminacy of translation "withstands even all this truth." I must admit I find such metaphorical explanations rather vague and puzzling. Perhaps Quine means that the most complete realistic picture of the world which we could imagine does not block the indeterminacy of translation: the complete list of truths about the world can be translated in various ways. We might distinguish, then, the thesis of indeterminacy of any (given) scientific explanation of phenomena from the indeterminacy of the translation of any given truth. This realistic explanation—if I understand or translate Quine's metaphors correctly—shows a difference. But if we interpret the explanation empirically, the difference dissolves: the statement about the indeterminacy of translation is merely a statement about our ability to determine the phenomena of language with a theory of it in a determinate way.

I learn from Gochet (Quine zur Diskussion, p. 23) that Christopher Peacocke ("With Reference to Roots," *Inquiry*, Vol. 21, 1978) has noted a similar problem which is whether two theories which can be translated into one another have any difference in meaning at all. Or, if they are observationally equivalent and not translatable, we may, as Quine says, use both. Can one deem one such theory to be true and the other false? Then meaning is more than observation. If not, choice would seem irrational. (Peacocke, p. 111) I find Quine moving between his realistic and scientific view which gives competing explanations of the same phenomena different meanings and his empiricist view which explains these away. Gochet says these can be reconciled since the existence of different meanings is expressed by the impossibility of translating one theory into the other. (pp. 45-46) But this misses the point: do theories which can be translated in various ways have different meanings or is the difference merely verbal? And, if theories cannot be so translated and are empirically equivalent, does it make any sense to talk of truth? Since we may in principle translate the whole of science in many ways, all these possible translations have, I suppose, the same meaning. Or, if we cannot so translate them, then are they all true? Quine suggests that cultural relativism cannot be true because in order to assert it we must rise above it. ("On Empirically

The assertion of the unavoidability of an indeterminacy of translation between any two languages, then, is equivalent to the two assertions that (1) there can be no difference in meaning between two sentences distinct from differences in empirical stimulations prompting assent or dissent to them and that (2) science must include sentences which are assented to, or dissented from, by native speakers even though, taken in isolation, no definite class of stimulations calling forth these responses can be identified. This is Quine's negative result. On his view it is a consequence of his empiricism. He not only bites the bullet and accepts it, he is proud to do so. It provides, indeed, a foundation for his Carnapian procedure of talking only about languages as scientific or not, since science must be merely one of many possible linguistic constructions[47] and of emphasizing the dependence of meaning on stimulation alone. There is no Platonic world of meanings. Even if reductionism is false, we are not forced to reject empiricism.

Equivalent Systems of the World," *Erkenntnis*, Vol. 9, 1975, p. 327) The same must apply to scientific relativism as well. It would seem, then, that there is one true scientific theory—not, as Quine would say, immanently so. In order to assert the truth of that view which we have—or the right of others to assert the truth of that view they have—we must rise above our own view. All apparently differing theories, then, are either mere differing expressions for the true or they are false. But, then, Quine's theory of immanent truth, just as cultural relativism, must go.

[47] In his "Ixmann and Gavagai," pp. 111ff. Joseph Agassi discusses the difference between actual science and potential science which, he says, Wittgenstein introduced by default. Popper, Agassi continues, has two theories of demarcation. One is of potential science—all refutable theories—and the other of actual science—the current most highly falsifiable but not yet falsified theories. Neither Wittgenstein nor Popper are clear about the transition from science potential to science actual and Quine looks as if he has endorsed that view which would be an inconsistent reading of Popper, i.e. the view that science potential equals science actual, when he identifies language and conceptual schemes. Leaving Agassi's discussion, we may note that his identification of the two allows him to accept any language as science. But yet he too, as Gellner has pointed out, must distinguish the two. He needs some validation other than mere language and this he calls scientific method. See Gellner, "The Last Pragmatist," p. 850. See also Agassi "Ixmann and the Gavagai," p. 113ff. where Agassi explains the difficulty of understanding Quine has do to his conflation of language and conceptual scheme on the one hand and the separation of them as a consequence of his view on the other hand. Wittgensteinian commentators seek to save the first while explaining the second away. Agassi, as I see it, proposes the opposite move, notes difficulties and proposes removing some analytic remnants to achieve a coherent view. I see an unbridgeable gap and leave it at that.

But now we find a new problem which Quine needs to solve to provide us with the positive side of the empiricist picture of science. This problem is: Given the limitations which his severe empiricism places on the meaning attributable to sentences or to systems of sentences, how can he explain the sufficiency of such sentences or systems of them or languages made up of them for the conduct of science?

As we are seeking the answer to this question, thereby following his guidelines down this path and then the next, we notice that we seem to be at entirely the wrong destination. Instead of finding an empirical explanation of the use and adequacy of such sentences in science which we hoped to find at Quine's first destination, we find psychological explanations of how we might learn them, if empiricism were true.[48] Now that might also be interesting and the second destination, which is where we expected to find it, is also on our travel plan. But we seem to find this and other explanations in the wrong place. Perhaps we will have to read the map more closely. But at first sight it seems that the only reason we find them here is Quine's determination to use Carnap's travel guide as a

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[48] Quine, *Word and Object*, pp. 80ff. Much of Quine's effort in this book goes into the development of a system which explains the nature of the allowable meanings of sentences. Now this theory seems to me to be metaphysics: it offers explanations of the kinds of entities, which "meanings" are. These are said to be based in stimulations and observations and surface irritations alone. The theory itself is not empirical. It is, indeed, rather empty as Chomsky has noted concerning his view of reinforcement (Noam Chomsky, "Quine's Empirical Assumptions," in *Words and Objections*, edited by Donald Davidson and Jaakko Hintikka, Dordrecht: D. Reidel Publ. Co., 1969, p. 56) and, I expect, not testable. It is apparently based, on the one hand, on Quine's empiricism and, on the other hand, on Quine's commonsense interpretation of what it means to endorse science. But appeal to neither the one nor the other can reduce the metaphysical nature of Quine's psycho-genetic speculations. Commonsense differs between individuals and times—for a discussion see Joseph Agassi and John Wettersten, "The Philosophy of Common Sense," *Philosophia*, Vol. 17, No. 4, 1987--and metaphysical interpretations of science (or psychology and learning theory) do to. Popper presents his own view for what it is; Quine indulges in the pretense that all those who accept science share his stimulus-response metaphysics, etc. Richard Schuldenfrei ("Quine in Perspective," *The Journal of Philosophy*, Vol. LXIX, No. 1, Jan. 15, 1972, pp. 5ff.) discusses the refutability of Quine's view. He suggests that there can be no independent evidence which will refute his view, that a new view is needed but he is willing to deem that scientific. At the end of his essay he comes, with the help of Burton Dreben, to the conclusion that Quine's view is Hegelian. Quine's basic arguments are circular and include the theory of knowledge which is developing as a system—I presume to a higher level.

First Travel Guide: only minimal corrections allowed here! If there is no formal way of explaining how the sentences of science are built up out of given experiences, we can at least explain how they are learned on the basis of given experience alone. This is, in fact, a return to old methods used, for example, by Herschel and Mill in their unsuccessful attempt to refute Whewell.[49] But this does not help us see how he will bridge the gap between empiricism and science.

Quine quickly begins by describing a child, who, he insists, learns words describing mere phenomena, abstracted from any objects. The child learns "motherhood" and "water" as free-floating properties and not as objects and even the word "red" without reference to any object. We are puzzled. If Quine were to give an empiricist theory of language learning we would expect a serious, scientific theory of it. But we find mere remnants of an outdated phenomenalism translated into such a theory. How does Quine know that the baby does not perceive his mother as a distinct entity? And how can he possibly imagine that the third word some baby learns is "red"?

Anyone who has had children, much less any child psychologist, must know that the mastery of color words is quite difficult for children and comes late in their language learning rather than early.[50] They learn to talk

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[49] See John Wettersten, *The Roots of Critical Rationalism*, Amsterdam and Atlanta: Rodopi, 1992.

[50] Whereas Quine seems to think that a color word is learned simply by mere identification of a name with a sensations Elsa Jaffe Bartlett in "The Acquisition of the Meaning of Color Terms: A Study of Lexical Development," in *Recent Advances in the Psychology of Language*, ed. by Robin N. Campbell and Philip Smith, New York and London: Plenum Press, 1978, pp. 89ff. maintains on the basis of her empirical inquiries that children (her subjects were between 2 years 4 months and 4 years old) form systematic and stable hypotheses about which color words can be given the same name. Learning color words requires abstraction and already some sophisticated development. There are books which discuss how they may be taught color words such as *Farben, Formen, Anzahl*, ed. by Darmar Althous and Erna Duhn, Braunschweig: George Westerman, 1977. They claim that "Untersuchungen zeigten, daß Kindern im alter von 6 Monaten verschieden geformte Gegenstände, z.B. runde, drei eckige und quadratische Holzstücken, auseinanderhalten konnten. Die Unterscheidung der Grundformen rot, grün, gelb, blau ist vor dem 2. Lebensjahr noch ungenau." (Inquiries show, that children who are six months old can distinguish between various shaped objects, such as round three-cornered quadratic. The distinctions between such basic forms as red, green, yellow, or blue is not accurate before children two years old.) By the age of 4 or five they have learned nearly all colors. Is all this relevant? It would seem that

about objects first. We seem to be still in the first destination where phenomena or stimuli of some sort or other were deemed the foundation of all inquiry but that is not quite what Quine is doing either.[51]

Could we find here an answer to our question, a bridge between empiricism and science and an explanation of the adequacy of an empiricist language for science? It would seem possible if all language were shown to be learned according to empirical principles. But if that were true, then there should also be reductions of the meaning of these sentences to given experiences, which we agree do not exist. How can the former—an empiricist learning theory—bridge the gap without the latter—a full reduction? I see no answer but let us keep the question open until we visit the second destination on Quine's journey.

Before traveling farther to the second sight at Quine's first destination, we might also note how puzzling some aspects of this first sight are. We have learned that sentences are not strictly reducible to observations, sentences referring to objects, are needed. And that we grant immediately. But why must we accept the view that they are radically untranslatable? It could, it would seem to the inexperienced traveler, just as well be the case that the argument for the radical indeterminacy of translation is a reductio ad absurdum of the attempt to follow old travel guides when the landscape has been radically altered by new constructions.

We do not find any argument which removes our unease, however. Rather we find the mere transcendental argument that the consequences of

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children know about objects and use this knowledge to gain knowledge of sensations rather than the other way around and that the attainment of knowledge of sensations requires an active, hypothesizing mind. If this is so, how can Quine's presumptions about how a (real) child might learn language be (scientifically) defensible? If not, why should we deem it possible? Once more, In The Senses Considered as Perceptual Systems, Boston: Houghton Mifflin, 1966, James J. Gibson describes the perception of color as a problem "full of complexities." (p. 187). Quine takes it as an obvious and trivial foundation for his system and deems his view scientific.

[51] Noam Chomsky in "Quine's Empirical Assumptions" has discussed some of these questions such as how a child conceives of objects, say, in terms of a region in space or in terms of their function. Quine's response is curious: he denies that Chomsky has found anything in his theory to disagree with and he gives no suggestions as to where such disagreements might be found. Does Quine think that his theory of language is a pure first philosophy with no empirical consequences?

Quine's neo-Carnapian empiricism plus the facts must be true.[52] We do find brief statements of the nature of science, often using Neurath's famous metaphor of a boat which needs repair when it is under way. These statements appeal to Duhem's theory of science, according to which the purpose of science is to make predictions and then only by modification.[53] These references should apparently show that the view he comes to is independently supported by direct considerations from science.

Quine is, however, no Duhemian. For Duhem the theories of science are tools which we use to make predictions, but which we need not believe. Being an Aristotelian Catholic he entertained no such skepticism about meaning or radical translation such as that entertained by Quine.[54] And so we find here no reason why we should accept the doctrine of the radical indeterminacy of translation. Indeed, Quine believes, contrary to Duhem, that these non-reducible and somewhat ghostly sentences are true. I have to admit I do not understand the capacity of someone to believe a sentence which cannot be translated as he says they cannot: Quine has to make believe that the sentences of science refer to objects and then he has to make believe that one is not make believing in order to deem them true.[55] He advocates bridging the gap between an apparently relativist view of truth according to which truth itself depends on our local beliefs, on the one hand, and the demand for objectivity in science, on the other hand,

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[52] Quine, "Epistemology Naturalized" p. 81. Quine here considers this alternative and argues once more that all meaning must come from outside as if it were impossible to imagine some alternative theory. I find this argument a mere sign of lack of imagination. Popper offers one.
[53] Koppelberg argues that Quine's view is a synthesis of analytic philosophy with Neurath's empiricism. In his review of Koppelberg's book, "Ixmann and the Gavagai," Agassi points out that outside of the boat metaphor Neurath seems to play no role in Quine's philosophy, pp. 105ff. He gives the credit to Duhem but Quine, in "A Comment on Agassi's Remarks," p. 118, says he did not know of Duhem when he wrote "Two Dogmas."
[54] Jules Vuilleman has noted this sort of difference between Quine and Duhem. See below, ft. 78 and text thereto. Agassi in "Ixmann and the Gavagai," p.108, explains Duhem's appeal as due to his combination of fallibility and certitude. But he also notes that Quine needs a different resolution. But this does not work and he must revise Quine's view to render it coherent.
[55] Gellner in "The Last Pragmatist," p. 850, says that he teases us concerning his real view, whether truth is merely my truth for my context or objective and for all contexts. He may be read as objectivist—I have looked at my scheme from the outside and it is the best—or as relativist—I cannot leave my scheme; each man to his own; all truth is relative—or as objectivist—my conceptual scheme may be trusted as part of a general and beneficent evolution.

with mere make believe.[56] This may be fine for a subtle thinker at home in Carnap's neighborhood. For mere travelers to a foreign land, it is too much to ask. In any case all this does not give us an explanation as to why these ghostly sentences are adequate for a real-life science. We have merely a transcendental argument: science exists as a matter of fact; empiricism must be true; if empiricism is true, then besides strictly empirical sentences and clearly analytic ones, such ghostly ones are all that we have; hence, they are adequate for science.

There was, in any case, also a second famous sight at the first destination which we hoped to visit. So, let us abandon for now our attempt to get a good view of the first sight and look for the second. Maybe we will then know our way around a little bit better and will be able to find the first sight as well. This second sight, you may remember, was the new construction of a theory of basic sentences, which would show their foundation in (veridical?) experience alone. Now we know from other travels of our own that such a new construction was desperately needed. We know that Carnap in *Der Aufbau* had naively appealed to given experiences which, psychological research had even then already shown, do not exist. Popper criticized this view[57] and Carnap modified his view somewhat in Testability and Meaning, allegedly so as to take Popper's criticism into account.[58] But even there Popper's views were distorted.[59]

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[56] Already in *Word and Object*, p. 22 Quine says we should not look down at posits as make-believe, since that is the best we can do.

[57] Karl Popper, *Logik der Forschung*, 7th edition Tubingen: J.C.B.Mohr (Paul Siebeck), 1982, pp. 61ff. Popper points out that Carnap's theory is psychologistic, that basic statements need to be objectively testable and that a statement formulated in an objective mode of speech—say, the table is red—is just as certain as one formulated in a subjective mode—I see a red table.

[58] Carnap, "Testability and Meaning," *Philosophy of Science*, Vol. 3, 1936, pp. 419ff., Vol. 4, 1937, pp. 1ff. Carnap mentions Popper's argument for the impossibility of absolute verification, even of observation sentences (p. 426) and of the use of a frequency interpretation of probability to appraise them. In the second section of the essay, p. 13, he states his agreement with Popper that basic statements should not be psychological but objective and testable. See also pp. 26ff. for comments on Popper's view of falsifiability. The impact of Popper's critique on Carnap's view is, of course, still somewhat dubious since he tried mere modifications to save the day, which, as even Quine saw, were not enough.

[59] Agassi, "Whatever Happened to the Positivist Theory of Meaning," *Zeitschrift für allgemeine Wissenschaftstheorie*, Bd. XVIII, 1987, pp. 22ff.

Carnap's somewhat more sophisticated view was also inadequate. It did not move far enough. So Quine wishes to do better than Carnap. A condition for doing so is that he find a neo-Carnapian view which is (1) not subject to Popper's critique and (2) better than the alternative which Popper had offered. We look with some amazement at Quine's travel reports, however, when we notice that he fails to even mention Popper's endeavors. This is especially so when we note that the move from *Der Aufbau* to *Testability and Meaning* was so crucial for Quine: after the breakdown of *Der Aufbau* new attempts at reduction such as that in *Testability and Meaning* had lost their point. It seems unfair to conjecture that Quine did not even know that he was attempting to respond to criticisms of Popper, that the problems or reductionism and of basic statements or observation statements in the form he takes them up arose due to Popper's critique. But we also cannot imagine that he knew that and never even mentioned this important background of his own intellectual travels. We must wonder, then, how the journey to the new theory of basic statements can proceed well, when the path-finder seems to have learned so little from the reports of earlier travelers who on their journeys had found new and passable roads as well as dead ends.

We are no longer sure if what we are seeking to find at this first destination is just what we expected but we propose to look further anyway. We find that observation sentences or strictly empirical sentences are quite precisely defined. The meaning or stimulus meaning of any sentence—there is no meaning outside of stimulus meaning—is the paired class of two classes. These two classes consist of a class of stimuli each member of which confirms the sentence and a class of stimuli each member of which disconfirms the sentence.[60] These classes can vary from individual to individual and from context to context and according to the length of time we allow stimuli to count. So, Quine continues, there are occasion sentences which are sentences which speakers assent to or dissent from on the basis of current (non-verbal) stimulation alone. Observation sentence are occasion sentences which are unambiguously or definitively connected with stimulations by natives. The definitiveness of such connections, however, is always a matter of degree.[61] Now that is elegant, a fine sight and we are impressed.

We linger a bit, however, not merely to enjoy the sight, but also because we have another question. Quine's predecessors in this business of finding

[60] Quine, *Word and Object*, pp. 32-33.
[61] ibid., pp. 40ff.

an adequate theory of basic sentences sought to explain the role of these sentences in science. And Quine explains that observation sentences as he conceives them can play that role. They are the firmest sentences and those which scientists appeal to when defending their theories. But they are also fallible due to the fact that the definitiveness of their connections with (non-verbal) stimulation is a matter of degree.[62] Observation sentences meet, then, Popper's standards. But yet, when Agassi reads him as a fallibilist, he protests. He uses the old-fashioned view that the observation statement at the time of its utterance by the observer is as good as certain though later reports of it are not. Agassi had already shown, however, how empty this view is.[63] But we find very little, indeed, concerning the role of basic statements or their fleeting certitude in science. We will have to return to this problem below.

But this is not our only question. We still wonder whether such stimuli or classes of them exist, even if we cannot identify them. For, we know from psychology that in the same situation we may perceive varying phenomena. Oswald Külpe had shown, before any of these travels were undertaken that, if a subject is shown a group of letters with varying colors arranged in a shape he may notice the shape the letters are arranged in, or the colors of the letters or the individual letters. But, if the time he has is short, he will not notice the other aspects of the image before him.[64] He has presumably received in some sense the physiological stimuli which would enable him to do that; but in some sense he has not received the impressions of color or shape that he needs to in order to be able to assent or dissent from some various statements about them.

If Quine understands by stimuli something like sense impressions, which I take it he does not,[65] then his theory of basic statements is an old-fashioned view such as that which even the slow moving Carnap sought to

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[62] ibid., p. 44.

[63] Quine, "A Comment on Agassi's Remarks," p. 117.

[64] Oswald Külpe, "Versuche über Abstraktion," *Bericht über den I. Kongress für experimentelle Psychologie in Geissen* 1904, Leipzig, 1904, pp. 56-68.

[65] The talk of "stimulation" is a mere mode of expression which overlooks that fact that various aspects of some "episode" may be "salient." *Roots of Reference*, pp. 25-26. See also "Epistemology Naturalized," p. 85, where epistemological priority is measured by closeness to sensory receptors which is in turn causal proximity. This is, I think, mere vague speculation which might be deemed a metaphysic of perception or which might be rendered testable and empirical. Quine apparently chooses to deem such speculations "naturalistic" and thereby dodge such problems.

overcome. If he means perceived images or objects, then this varies with context somewhat independently of stimulation as Külpe's experiment shows. If he means something else such as, as he sometimes says, surface irritations—whatever that should mean—then he hardly has a theory of the nature of basic sentences. We have hardly any half-way decent theory about the relation between physiological stimuli and perception. Quine explains, of course, that he knows all that and has explained difficulties himself.[66] Yet Quine sees here no reason to doubt empiricism. The "crucial insight of empiricism", that "any evidence for science has its endpoint in the senses", is correct.[67] We need to stick to it and explain it, he maintains.

We find ourselves once more at the false destination. We are moving over to physiological explanations of perception. But vague talk of surface irritations is hardly of interest there. It is, in any case, a mere defensive maneuver which avoids objections from psychology while continuing to use old views. The talk of stimulation is a fudge word which seems to mean both surface irritations which are not perceived and perceptions even though these are quite different and, what is worse, we do not know how they are correlated.[68] It is a word which blocks the central questions from even arising: what is the role of mental activity in observing?

Our visit to the first two sights at the first destination on Quine's travel plan has been so dissatisfying that we may have had enough. There are

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[66] Quine, "On Mental Entities," in *The Ways of Paradox*, pp. 208ff. esp. p. 211.
[67] ibid., p. 212.
[68] In the *Web of Belief* Quine, with Ullian, notices that observation sentences might not seem to be directly and wholly connected to specific occasions because these sentences are formulated in terms of objects which are enduring. (p. 15) They concede that individuals do not necessarily or even often learn such sentences with ostensive methods alone but maintain they can. They claim this learning is crucial for learning scientific theory and it provides its vital continuing connection with sensory evidence. I find this assertion rather doubtful and a mere corollary of Quine's system. It presumes that there is a basis in observation which is freed from the theoretical components without indicating how this is possible. A sentence which includes reference to enduring objects which are understood as such must have a quite different meaning from one which is purged of such reference in order to refer to one occasion alone. The observation sentences we find in science are of the former type and not the later. I find rather fantastic the assumption that a real-life observation sentence such as we find in science could be learned merely ostensibly: surely the meaning of such sentences depends on the objects which they refer to and surely the assumptions about the nature of these objects cannot be separated without changing the meaning of the sentence.

significant aspects of his view such as his view of ontological commitment which we have not even mentioned. Ernest Gellner has discussed this aspect of Quine's philosophy in such a decisive way and shown both its possible dogmatic character and its emptiness that we need not say more of that here.[69] It was originally an important part of a reductionist program. Within the confines of this program it had considerable bite. Separated from this program it is no longer so clear what it accomplishes in Quine's philosophy. We had hoped to find a comprehensive theory of science as well, since on Quine's view it is hardly distinguishable from his comprehensive view of language. Before we seek this sight, however, it may be better to go on to the second destination. We seem in any case to have landed there a few times by mistake so we might try to take a better look at it. At this second destination we hope to find some interesting explanations of physiology as a basis for perception, of perception as a basis for science and of learning theory. We may also keep in mind that we may have misread Quine's reports and that there really is only one destination. We may need to travel to what I have called the second destination in order to find a theory of science.

9. Destination Two: Filling the Gaps between Empiricism and Science with "Scientific" Theories

We are anxious to find the road to the next destination on our agenda: Quine's explanation of language learning. This theory should bridge the gap between empiricism and scientific theory by bridging the gap between stimulation or something similar—say surface irritations—and the language of science. And it should do so with scientific explanations. We should find explanations of how basic impressions are built out of stimulations, of how simple sentences are built out of impressions with the aid of stimulations alone, and how still more complex sentences can be built out of these. This description is, perhaps, not quite right. I should say "learned on the basis of" instead of "built out of" to indicate that the problems of reducing (the meaning of) scientific theory to impressions or stimulations or something similar are to be translated into problems of how one learns on the basis of stimulations, etc. alone. The new problems should be amenable to solution where the old ones were not. The basic aim is the same however: empiricist principles should be vindicated.

This should surely be a fascinating sight which will set aside the dissatisfactions which we felt earlier due to the incompleteness and

[69] Gellner, "The Last Pragmatist".

misleading descriptions we encountered in our journey to Quine's first destination. Before traveling directly to this destination, however, we must consult Quine's travel plans once more. While underway he has made various amendments which we need to take into account. We do not wish to lose our way, finding ourselves once again at a wrong location and then be disappointed with the sights which are to be found along the way.

Quine's adjustment in his travel plans seems to have been rendered necessary by the failure to meet the very high expectations of fellow travelers—if not of himself as well—at the first destination. The sights he has described at the first destination are not what they might have been: they do not succeed in showing the way to the completed structures which Carnapians have been searching for. He wishes, then, to defend his further journey by explaining those modest but real virtues of the new sights he proposes we visit. In the future hopes should not be raised too high. But he still wishes to keep his job as chief (Carnapian) travel guide as well as the reputation of his travel bureau. He thus explains those virtues the sights he hopes to lead us to may have, even though many travelers might have had quite different expectations of a journey organized by a Carnapian guide.

The grand plan of Carnap's *Aufbau* could not be followed, we remember, because the path from given experiences to scientific theories is impassable if logical construction is the only means of moving forward. A new plan was then suggested according to which one would travel from given experiences—now modified and called stimulations—to a natural language. This language would be limited by the empiricist analysis of the theoretical possibilities of language and by empiricist decisions so as to include science and exclude metaphysics. But this road also proved to be largely impassable. After one had imposed the needed limitations on it, one could only find outlines of a road which came to an end long before one had traveled all the way to real science.

No empiricist theory of science had been exhibited. Rather, we found explanations of what a language must be like if empiricism were true and a seemingly gratuitous assumption that the real scientific language must be like that. At best there is a transcendental argument from the truth of empiricism and the existence of science to the adequacy of an empiricist language for science. Now Quine does not see any difficulty here: he believes. But he seems to be somewhat worried, nevertheless. He wishes to find a new path to show the possibility of bridging the gap between empiricism and science. This path is to be drawn in the sand by showing how scientific language might be learned on the basis of stimulation—or reception of physical impulses, etc.—alone.

In spite of the fact that empiricism is a trivial truth which he does not propose to doubt nor, we might add, to prove, he wishes to explain why it is possibly true, admitting implicitly at any rate that it is hardly commonsensical. In order to provide such an explanation, he will show with scientific theory that it is in principle possible to learn scientific theory, that is, the language necessary for and limited to science, on the basis of stimulations alone (with the aid of the triggering of sensory receptors alone). If we can show how we are capable of learning this language with the aid of stimulations alone, we may justify(?) the view that the meaning of language is obtained or even derived from stimulations alone. This may be so even if the meaning is not reducible to stimulations. Carnap receives thereby a vindication of sorts.

We remember that the gap which Quine wishes to bridge was that between sensation or given experience or stimulations, on the one hand, and scientific theories (or scientific language), on the other hand. Now, we were under the impression that this problem was a product of empiricist claims according to which all knowledge is derivable from, or reducible to, given experiences or stimulations, etc. The proof of this controversial epistemological principle would be the bridging of the gap. But Quine now treats the situation quite differently. The empiricist principle, he says, is necessary.[70] It is merely a part of science. And science, even though it asserts this principle, raises problems about how the gap can be bridged.[71] The task of bridging the gap, then, belongs to science and not to epistemology or philosophy. Quine suggests right off, we should note, that we cannot expect too much. We must use the tools of science to construct the road and roads constructed in this way are not necessarily so easy to construct or direct or easy to travel. They are, however, the best that we can expect to find.[72]

Quine's mere assertion, that all those who deny reductionism assert theories inconsistent with science, must come as a surprise.[73] We are

[70] Quine, "Epistemology Naturalized," p. 81. "The sort of meaning that is basic to translation, and to the learning of one's own language, is necessarily empirical meaning and nothing more".
[71] ibid., pp. 83ff. Quine, *Roots of Reference*, p. 2.
[72] Quine, *Roots of Reference*, p. 34.
[73] Quine asserts that his own epistemology is part of science in "Epistemology Naturalized," p. 83. Can contradictory epistemologies be also part of science? It

aware of philosophers and even scientists quite friendly to and knowledgeable about science such as William Whewell[74], Karl Popper and John Eccles who deny that.[75] If we look, however, for a reference to which science and which scientific theory Quine here bases his claim on, we will be disappointed. Nor does he say how non-reductionist theories are in conflict with science. Perhaps he means in conflict with the stimulus response psychology he favors or some other particular view. But that is not science per se. There are other scientific theories which conflict with these. We presume, then, that this claim for the scientific status of empiricism is sheer bluff. If you—well, not just anybody—cannot prove your epistemological theories true, you can, perhaps, declare them scientific and thus indisputable!

We are also, of course, puzzled, since the problems, which Quine says arise out of science, have in fact been problems for one sort of epistemological theory and became clear as philosophical critics of this theory pointed them out. It was Carnap, in the Aufbau who proposed the new, radical reductionism which fascinates Quine and it was above all Popper who showed how infeasible this program was. This view is one further development of a long philosophical tradition, from Bacon to Locke to Herschel to Mill to Russell, to mention a few which might be

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seems not, since the problem of explaining science as a product of stimulation seems to be, on Quine's view, unavoidable. If this is not trivialized, it is the claim those who deny reductionism are not scientific. See footnote 60.

[74] William Whewell, *The Philosophy of the Inductive Sciences*, reprint of 2nd. edition of 1847, London: Frank Cass & Co., 1967; see esp. Chapter II, "On the Fundamental Antithesis in Philosophy," pp. 16ff.

[75] Sir Karl Popper and Sir John Eccles, *The Self and Its Brain*, New York: Springer Verlag, 1977. Quine will say, I presume, that it comes as a surprise to him that those who deny reductionism assert theories inconsistent with science, since he does that himself. See for example his reply to Nozick in *The Philosophy of W.V. Quine*, p. 364. He continues by presenting his empiricism as a triviality, which no one denies, that is, that data regarding the external world reaches us only through sensory stimulation. This stance is, as I have suggested, quite misleading. Quine presumes that it has consequences for the meaning of sentences but, if it does, it is not trivial, since it then conflicts with the view that the mind can create ideas which it applies to the world. Data, on traditional views such as Whewell's and Popper's cannot reach us without sensory stimulation but they can also not reach us without mental activity. If Quine's theory is consistent with this view it is trivial and his succeeding speculations a near empty and misleading metaphysical system. (It is so because he talks of stimulations and behavior but in fact is willing to accept mentalistic philosophy.) If this principle is not consistent with the need for mental activity to receive data it is by no means trivial: It is false.

deemed predecessors. It has indeed been on the agenda for the philosophy of science, with defenders on both sides of the issue, for centuries.[76]

Now that Quine has made clear that he no longer believes that the journey to the reductionist sights we wished to follow him to can even be carried out, we might wish to change our booking right away. But Quine tries to convince us that there are still some interesting sights to be found on the back road we have found ourselves on. He proposes, indeed, that great progress has been made in bridging the gap between given experience or stimulations and scientific theory. This progress occurred when one discovered that it was not necessary to reduce the meaning of each concept found in physical theory to observation terms but that one could use a contextual definition, that is, the meaning of a whole sentence could be given by translating the sentence which referred to objects into one which referred to sensations.[77] Our hope rises since this might be a sight worth seeing, that is, the translation of each sentence in science into a sentence about sensations or given experiences or stimulations alone.

Alas, we do not find that either. Quine does not propose to translate individual sentences of science into sentences referring to sensations or stimulations. Indeed, he merely asserts that the whole system of science should be so translatable. But nobody could expect, of course, that he would do that. We feel annoyed once more. The fine promises that were made by the travel bureau—the fanciest, indeed, in the whole town—seem to have been mere come-ons. But perhaps Quine notices our disappointment. (He is so proud of what he has to show and of the elegance of his portrayals that it is sometimes hard to tell whether he notices our discomfort or not.) He seeks to hold us once more with a new idea about what we might find along this seemingly barren road. He suggests that we can conduct scientific research about the relations between the given, that is, stimulations or surface irritations, etc. and scientific theory, that is, scientific language. And this, he says, should be quite interesting. We are losing patience, but we have come this far so let us see what he has to offer.

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[76] For the best overview of the problem of reductionism and science that exists see Joseph Agassi, *Towards a Rational Philosophical Anthropology*, The Hague: Martinus Nijhoff, 1977. See also my review of this book "New Methods for the Study of Man", *Zeitschrift für allgemeine Wissenschaftstheorie*, Bd. XVI, Heft 1, 1985, pp. 167-176.
[77] Quine, "Epistemology Naturalized," pp. 72ff. For a later review of developments see Quine, "Five Milestones of Empiricism," in *Theories and Things*, pp. 67ff.

We find him busy right away explaining still further the new journey he wants us to undertake. We are not to suffer, he says, any (logical) timidity.[78] We should, we gather, be bold scientific, adventurers. We should realize that all knowledge comes from sensory stimulation. Our task is not to discuss whether that is true but to investigate how. On Quine's view we need not bother about objections from Gestalt psychology or the theory of perception, since it is clear that sensory stimulation is the source of our knowledge.[79] Perhaps we need to explain perception and Gestalt phenomena and that may be a challenge for us but no reason, so Quine, to change our plans or suspect that the reductionist road is blocked. The old epistemologists sought a base in perception or something firmer than science and that caused them trouble. But we do not need to go this route. We see that it was a false turn. On the road Quine hopes to find such phenomena as perception of wholes. They are merely an integral part of the journey from stimulations to scientific language. We are skeptical about Quine's plans, however. We have already seen at the first destination that Quine offers a mere sketch of a program which is hardly developed, and we find little that is new here. We need a theory of the possible intervention of mind or a theory which explains that away. Quine has neither but nevertheless confidently asserts that the latter will be someday forthcoming.

We turn quickly, then, to a discussion of dispositions and this might be interesting: it was just that problem which, Popper had pointed out, Carnap's reductionism could not solve.[80] Quine does not bother to mention such ancient history. He deems dispositions straight away to be either physical states or a provisional but scientific way of describing them.[81] Now this is justified on the basis of science and not on the basis of epistemology. So, we see something new. Empiricism and science are both true and, if in a pinch, one does not come further with empiricism then one can try science or vice versa. We no longer know where we are at or where we are headed.

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[78] Quine, *Roots of Reference*, p. 2.
[79] ibid., p. 3-4, Science, he says, has demonstrated the limitedness of the evidence for science and science must explain how it functions, that is, how Gestalt objections can be met. They are not then a challenge for empiricism but for science.
[80] Popper, *Logik der Forschung*, pp. 61, 65.
[81] Quine, *Roots of Reference*, pp. 13-14.

And, just when we think we have left Carnap for good in favor of "science", we find him reappearing. Quine proposes to develop his learning theory by using as the fundamental act of learning the perception of similarity.[82] This should, he says, remind of us Carnap's "Ähnlichkeitserinnerung." (Remembrance of similarity.) This perception of similarity is, Quine says, the basis of all learning. Even though we may perceive Gestalt patterns, if the basic patterns we perceive are similarities, learning is inductive.[83] We are puzzled still, since the old problems of the influence of theory on perception are not thereby solved. We have a mere declaration that they should be and can be solved. This declaration alone is, our tour guide reassures us, a pass to go to the next level of the learning process. It is clear that there are few if any philosophical policemen who are willing to question the validity of this dubious pass. Quine is well known in this region and merely has to flash his documents which nobody seriously questions. We travel further.

Learning proceeds, according to Quine's travel plans, with the ostensive learning of observation sentences. Here Quine does make some use of contextual definition. He defines the meaning of the sentence—not it parts—in terms of its truth conditions or, translated into learning, the conditions of assent or dissent. Thus he holds still to the verification theory of meaning.[84] We find this puzzling, since we have often found observation sentences to be complex affairs which can only be understood when we have mastered some theory. And Popper has shown how the simplest of sentences involve theoretical terms.

But, we find, this is not really what Quine has in mind at all. He avoids such difficulties which might arise if he considered observation sentences found in science by taking old, Carnapian phenomenalistic examples. We find the word "red" once more; this time it is a sentence. The question whether these sentences are just like observation sentences in science is not posed but anticipated and explained away: of course, we may learn sentences in later phases somewhat differently.[85] We are not, perhaps,

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[82] ibid., pp. 16ff.
[83] ibid., p. 19.
[84] ibid., p. 38, "The meaning of a sentence lies in the observations that would support it or refute it" is taken to be a commonplace. We cannot, he says, speak of the meaning of individual sentences. So, he limits the verification theory while accepting it. But we are still somewhat dissatisfied. Did he forget to write "lies solely in observations" in the above sentence? Does he fudge matters? If not solely in observations, then in what else?
[85] See note 68.

completely at sea here, since we see that Quine follows Popper and Popper talks of observation sentences instead of observation. But Popper talked about science and Quine does not.[86]. Here we should find the gaps bridged and we do not.

We are not sure how others feel but we have begun to lose interest in this journey. There are many small byways and curious structures which we have not visited. But we no longer see how they will bring us closer to the more interesting sights which we wished to see as we began our journey. How, for example, will all this lead to a decent theory of science? We are also terribly uneasy because we do not know how to judge whether we have lost our way or whether the road we are following is the right one. The appeal to perception of similarity, for example, as the basis of all learning seems to us to be a brand of naive associationism: we receive two distinct impressions and notice that they are similar. We have often found language chosen to span alternatives and to leave all options open even while appearing to take a strong empiricist stance. We doubt the psychological theory of the perception of similarity as a unique innate capacity on which other more complicated processes can be built—just as did psychologists around the turn of the century. And this should be a scientific theory, but we find only sparse reference to psychological theory and then nearly only those which might be sympathetic so that criticisms and difficulties are ignored.

We doubt whether observation sentences are learned ostensibly just as we doubt that the perception of similarity is fundamental for learning. But objections from psychology or the facts of science perhaps do not even count. They are mere challenges for science and no critique of empiricism. For, what we have here is speculations concerning how learning could take place if empiricism—or Quine's version of it—were true. It should, then, offer a proof or perhaps a plausibility argument that the theory is reconcilable with science, as Carnap's was not. It should show that the

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[86] Quine builds into his system Popperian ideas such as the fact that basic sentences should be intersubjectively testable and agreement should be (provisionally) obtainable and that they may be to this or that degree fallible. This is fine so far as it goes, but it does not offer a theory of the role of these sentences in science, as Popper does, which is worthy of the name. Rather Quine attempts to secure them further with a verificationist theory of their meaning. But this theory contradicts Popper's, since on Popper's view they are theory dependent. Quine claims that the gap can be bridged in principle because we could learn them without theory. But that is just what is in question and appeal to speculative child psychology does not settle the issue as Quine seems to think.

content of science could in principle be generated from mere stimulations by showing how we could learn this content. We are puzzled because Quine has informed us that his view meets the standards of scientific theory. What we find is an irrefutable metaphysical research program. We do not mind that, but we are annoyed by the continued false descriptions of these sights as the (uncontroversial?) results of science which we have found along the way.

We must, then, continually rewrite the tour guide to find a true description of the sights we visit. In each particular case we find that the crucial question is begged. This question is: does the meaning of any learned sentence or system of sentences chosen as you please consist simply of the combination of the stimulations which are the occasion of one's learning it or of something more? We are naive and think there is something more to it than the mere organization of stimulations and that this has consequences for empiricism and metaphysics of mind. Quine's examples of how individuals might learn sentences from mere sensations, sentences which appear to mean more than mere descriptions of sensations, do not lead us to change our mind. They do not do so because they give possible—to be generous—metaphysical interpretations of the occasions in which sentences are learned in terms of stimulations alone but do not explain the additional quality which these sentences seem to have when we use them. Quine dismisses our intuitions instead of explaining them.

We are reminded here of Wundt, who used a similar technique.[87] He based each level on the level before so that one could start with simple impressions and feelings and then move to descriptions and to judgments, etc. Each new level was built on the previous one. As a result the whole should consist of combinations of the lowest level alone. The difficulty with this approach, just as with Quine's, is that one is merely given occasions for learning and no psychological explanations of the content of what is learned, of, say, simple reports of impressions or of observation sentences. Külpe, Wundt's student, noticed this and offered a non-reductionist view which in the end led to Popper's philosophy of science and of mind. We should not, we think, forget this history and be misled into forgetting what is at issue. Reductionists have, over the centuries, regularly used the technique we find Quine using here. An outline of possibilities is given. Complete reductionism is asserted as true. Partial reductionism is offered as evidence and filling the gaps is deemed the task of science or further research.

[87] See John Wettersten, *The Roots of Critical Rationalism*.

10. A Traveler's Pause: Observations on Paths from Language to Science

We have followed Quine's travel reports as best we could but have nevertheless become lost in the back alleys of the two primary destinations. We have still not found the empiricist theory of science which was the most important sight on our agenda. We did find interpretations of language or of theory as mere tools for simplifying the ways we organize stimulations, a view which apparently leads to instrumentalism or, you may prefer, to relativism. This aspect of Quine's theory is represented by his reductionist views such as his theory of the indeterminacy of translation which seek to explain the limitations of language as seen by an empiricist.

We also found suggestions as to how these views of the limits of language could be reconciled with the realistic appearing language of science. One suggestion is that a scientific, psycho-genetic account of how scientific language is learned from stimulation alone can bridge the gap. This account should be adequate to explain the content and structure of scientific language. But this suggestion turned out to be a mere neo-Carnapian metaphysical research program. Quine does not scientifically investigate the truth of empiricism but simply presumes it. All of the enormous gaps between empiricism and science are declared problems for science. Even aspects of science which are relevant for the study of the question whether the gaps can be bridged are ignored. Alternative programs for explaining the phenomena of language learning are ignored and apparently deemed unscientific without further ado.

Still further we have found Quine interpreting scientific theories in a normal, commonsense realistic way. But he does this without reconciling the negative, empiricist interpretation of language with such use. In the end, conflicts between the empiricist theory of language and the scientific use of it are asserted not to exist. Yet, the prima facie conflicts are neither explained nor explained away.[88] This may leave dissatisfied anyone who is not a true believer in the adequacy of Carnapian empiricism. Perhaps a true believer such as Quine would not take our doubts any more seriously than a true Catholic takes the doubts of non-believers concerning the Trinity seriously. The Catholic would say right out he can no more explain how God can be three and one than Quine can explain how his empiricism

[88] See Joseph Agassi, *Towards a Rational Philosophical Anthropology* for discussion of explaining and explaining away, esp. pp. 46ff.

and his naturalism are to be reconciled. But both know that their doctrines are true and that reconciliation is possible. Unfortunately, we lack the proper faith.

Quine has developed his philosophy as an empiricist theory of language. This theory should give us a theory of science without further ado. It does not. We have found Quine assuming that there is a direct path from the theory of language to the theory of science. But we have lost our way as we have tried to follow Quine's directions concerning just where we should find it and how we should follow it. It might be well to take a pause on our journey to see if we might get our bearings. Where might we find a path from the theory of language to the theory of science? How should it appear and over which terrain must it wind its way?

The difference between viewing the study of language as continuous with the study of science, on the one hand, and the study of science as unique, on the other hand, has been recently deemed by Dirk Koppelberg the central point which divides Quine and the analytic tradition which he is deemed to carry forward—by defending it or by overcoming it is not clear.[89] Koppelberg thinks that Quine's method of deeming the study of language continuous with that of science gives Quine the advantage over Popper and enables him to render analytic philosophy and/or positivism defensible today. We look with hope, then, at Koppelberg's work. But we are quickly disappointed. Koppelberg claims, namely, that Popper has failed to distinguish between the problem of the demarcation of science, on the one hand, and the problem of the demarcation of meaningless sentences, on the other hand. Being interested in the first, he throws the second out.[90] What Popper in fact did was to reformulate the problem of demarcation which he shared with the positivists when he discovered that no theory of meaningless sentences could demarcate science.

Koppelberg falsely claims that the positivists wanted to draw their line of demarcation at a place different from Popper's: for the positivists the demarcation of meaningful from meaningless sentences should demarcate science. For them all and only all meaningful sentences would be empirical and all and only empirical sentences part of science. Popper's theory of the demarcation of science was, then, falsely read as a theory of demarcation of meaning.[91] The move from a theory of language to a

[89] Agassi, "Ixmann and the Gavagai," pp. 103ff.
[90] Koppelberg, *Die Aufhebung der analytischen Philosophie*, p. 93.
[91] Agassi, "Whatever Happened to the Positivist Theory of Meaning."

theory of science was, for the positivists, direct and clear cut. When it was discovered that no theory of the demarcation of meaningful sentences was available which could demarcate science, a new theory was needed. Since Quine moves from this early view, found in *Der Aufbau*, to a later view which is somewhat different, he needs, just as Popper did, a new theory of this relationship.

Both Agassi and Gellner have noticed and discussed the point that a theory of language cannot without further ado be a theory of science; a theory of language is much broader. Can a theory of science be part of a theory of language? How? Agassi has suggested a common reading of Popper and Quine which may shed light on this. On this reading each theorist offers a theory of science potential which is to be distinguished from his theory of science actual. On Popper's view science potential consists of all refutable sentences, whereas science actual consists of the most highly refutable but not refuted sentences. On Quine's view science potential consists of (meaningful) language whereas science actual consists of that which has been selected by scientific method.

This reading reveals quite glaringly a point which Gellner has already made in regard to Quine: his use of scientific method is a Deus ex Machina. Popper, on Agassi's reading, offers a theory of science actual whereas Quine merely appeals to allegedly clear and uncontroversial views concerning science. We have moved then from a theory in which the theory of language was a critical and radical tool for the appraisal of science to a view where it accepts all of established scientific method and science without a peep of dissent or criticism. The change of mood, noted by Gellner, could hardly be greater.

The various interpretations of Quine's view offered by Agassi, Gellner and Koppelberg seem in agreement on one point: he offers no theory of science proper. Koppelberg says the problem does not concern him but is different; Gellner says the problem is crucial for his view and he has nothing to say about it; Agassi says he has a view—a conjecture—which is Popper's and which renders views for which he is famous such as the indeterminacy of translation of mere implausible conjectures. Koppelberg, in an apologetic mood, seeks to bridge the gap by explaining problems of scientific method away. But as Gellner, in a similar vein but in a critical mood, has argued, they are merely dodged with a Deus ex Machina. Agassi sees the only possibility of remedying the situation in a Popperian reading of Quine. But this makes science empirical conjectures and

Quine's philosophy a metaphysical conjecture. And Quine will not have any of that.

Quine seems to have at least two views of the path from the theory of language to the theory of science. On the first view there is a direct path from one to the other. One needs merely to form a theory of language as based in stimulation and then turn to science to see how the selection among the statements of this language, that is, among empirical statements, is properly made. This selection poses no puzzle and those who concern themselves with explaining it would seem to have little to say. On Quine's second view of the path there is a smooth transition from an empiricist analysis of language to an explanation of the (empirical) nature of science. The explanation of how language is based in observation or stimulation provides an explanation as well of how science is based in observation. And the problem which is thereby solved is the central epistemological problem. This explanation is not methodological. Any methodological consequences it may happen to have should automatically conform to the practice of real science. If they seem not to do that, they are modified or reinterpreted so as to remove conflicts.

We find that although Quine's theory of language is supposed to provide us with a theory of science—insofar as such a theory is even needed—as a mere corollary, the path from the theory of language to theory of science is quite hard to find and follow. One reason for this is that the old-fashioned radicalism of Quine's hero Carnap has been abandoned in favor of a sedate conservatism. Carnap's empiricism was supposed to be a sharp-edged tool for cutting out the fat of science, for making sure that all that remained was lean and pure. Quine's view deems all science good enough and is merely concerned to explain why only science is worth taking seriously—even though we may have to extend the bounds of our serious interests in some cases due to the fact that the old-fashioned radical methods of demarcation do not work. One could continue to maintain, as Quine at first suggested, that his analysis of Carnap's research shows that Duhem's view, that is, conventionalism, is the true view of science. But once having maintained that this path is not the right one, we are left in the dark as to how we move from the theory of language to the theory of science.

11. Destination Three: A Search Here and There for Markers of a Theory of Science Scattered along the Way.

We seem to have arrived at the end of Quine's journey without having found a good view of science. But maybe we have overlooked something important along the way. If we recall some of what we have seen and take a few side trips, we might still find this view. We started with the observation that the roots of Quine's view are found in Carnap's. As others have noted, Carnap's view vacillates in its assertions about science. On the one hand science is merely a well-chosen language and there are many possibilities: formal languages can be constructed in various ways. Science consists, then, of conventions which enable us to predict and control the world. Since, however, all truths are either analytic or statements of fact, it also seem possible to deem science a true description of the world. When we abandon the strict view of scientific language according to which analytic statements provide the form and synthetic statements the (empirical) content of science, this problem may remain: are scientific statements true descriptions of relations between facts or mere conventions for their efficient organization. Quine rejected the strict view of scientific language and sought an alternative. Can his alternative resolve this difficulty concerning the nature of scientific statements?

When we look for resolutions to it we are puzzled. This puzzlement has its roots in the fact that Quine's theory of science is a theory of language. Now languages, we all agree, are highly conventional. And Quine endorses this view as well. But then, if science is not conventional, it must have some special features which go beyond those of proper language. Quine does not tell us what they are. He appeals, indeed, to scientific method to tell us what part of language science is. But it is just this method which his analysis of language should have elucidated or explained. But Quine deems science no problem. It is, then, no wonder that he has no theory of it. The Carnapian roots are so deep and strong that they oblige him to treat problems of scientific method as corollaries of a theory of language. But the theory of language, which these roots support, is so weak, it cannot be burdened with the weight of a clear-cut view of science.

Nevertheless, Quine does believe that science finds the truth. His theory of the truth of scientific theories is, as he says, immanent. His view seems to require that scientific truth changes as science changes. But not quite. He relies on the view that old theories are true over a range of events, which may then be specified as a special case of a new, more powerful theory. He knows that that is not quite enough, however, since it is not merely the range but also the theoretical presumptions which have to be changed. The task of doing that is the theology of science which he endorses.[92]

[92] Quine, *Word and Object*, pp. 250-251.

Presumably he thinks that whatever theologians say is true, is true. Perhaps his theory of truth is not merely "immanent", but also social or Hegelian: from the inside we can only relate statements within our system to each other but from the outside we declare the inside correct. On this interpretation we feel justified in doing this because the social system has survival value and overcomes contradictions by moving to a higher level through high criticism.[93] Perhaps he aspires to be an authoritative theologian.[94] I do not, however, find a clear answer to my question as to

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[93] Gellner in "The Last Pragmatist" p. 850 also notices the movement toward Hegel in *Quine's philosophy*. See also ft. 49.

[94] It seems plausible to regard Quine's work as an attempt to construct a unifying framework for all science which is such because it limits itself to observables. See, for example, Quine's response to Schuldenfrei (*Theories and Things*, pp. 184-186) where he claims to be using a scientific method analogous to someone replicating the work of Galileo by testing his theory under ideal conditions, i.e. in a vacuum, etc. Likewise, Quine sticks to what is observable in order to isolate, divide and conquer. He wishes to isolate an explicable component and to ignore that which does not fit, such as, in this case, other aspects of experience and belief not captured by his stimulus-response descriptions. He thus seeks to polish and refine those sure aspects of science—perhaps this is what he calls "high criticism" in comparison with theology—which can then serve to unify and/or serve as a basis for others. This theological activity seems to us naive: the appropriateness of the vocabulary of stimulus-response cannot be separated from fuller theories of experience and learning. That it is, allegedly, "observable" is no guarantee for either clarity or correctness: a theory based in so-called observables can be ad hoc and vague (see John Wettersten, "Methods in Psychology: A Critical Case Study of Pavlov," *Philosophy of the Social Sciences*, Vol. 4, No. 1, March 1974, pp. 17-34. Galileo's theory of falling bodies was not successful because it was based on the isolation of observable factors but due to its high explanatory power. Galileo was not in a position to isolate those factors which had to be isolated in order to conduct a good test, and Mersenne appropriately complained about the poor results obtained when Galileo's experiments were repeated. Quine knows all that of course and talks of someone repeating Galileo's experiments carefully. But if we are careful enough the results will in any case be wrong since Galileo's theory was corrected by Newton's. Whewell, for example, attempted to make such a test by comparing the motion of pendulums at the earth's surface and deep in mines. Quine's imaginary scientist, perfecting and confirming Galileo does not exist. If Quine thinks that that is what he is doing, he does not, qua scientist, exist either. Even a metaphysical theory not based in observables but presented as a conjecture of the reality behind them can be clear and useful for science. For a discussion of Faraday's metaphysical speculations and the relations of them to his physical research see Joseph Agassi, *Faraday as a Natural Philosopher*, Chicago: University of Chicago Press, 1971. Unfortunately, however, Quine denies that such

where he stands in regard to truth as an aim for science. Gellner says he teases us deliberately. The only aims he has are simplicity and better tools which he deems to be the truth.

We see that Quine endorses the views of one of the greatest defenders of the view that science does not seek the truth but true predictions alone, Pierre Duhem. But we realize just as quickly that Quine is no Duhemian. He says that scientific theories are tools, but then he also says that we believe them and we should believe them; Duhem rejects this later view. Quine endorses Duhem's holism but not merely for science; he applies it to all theories. Duhem's theory of science is, in short, based on a distinction between theories as tools and theories as descriptions of reality. We have both but science should be restricted to the former. For Quine there is no distinction and this is a direct product of his Carnapian roots: he refuses to separate the study of science from the study of language per se. This difference leads to a difference not merely in regard to his view of non-scientific views but also in regard to his view of scientific ones: the theory of how we understand and appraise scientific sentences or theories must be quite different from Duhem's in that it must not draw any sharp distinctions between it and our appraisal of non-scientific sentences. Duhem knows that in a pinch he can appeal to realism even if only as a compromise, since in metaphysics he is a realist. Quine cannot even do that. But in a pinch Quine feels no inhibitions due to his limited view of knowledge. He simply declares all claims of conflicts between his view and realism null and void.

We find Quine not only in agreement with Duhem, but also with Popper, in that he endorses the fallibility of science. We see quickly, however, that Quine disagrees with Popper as well. Quine endorses Duhem's conservativism even though he relativizes this conservatism with his view of simplicity: simplicity may take priority over conservatism. This move allows him, he thinks, to reconcile his demand for conservatism with an endorsement of revolutionary science such as that of Einstein. He denies, perhaps, that there is an asymmetry between confirmation and refutation and thus he deems empirical justification of a sort possible. Nevertheless he agrees with Popper that inductive inference cannot exist even if we demand mere probability. For Popper fallibilism requires a critical and adventurous methodological stance; for Quine it requires a conservative one.

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activity is useful. When he engages in it he confuses it with science and, as a result, has poor and confused standards for it.

These difficulties may raise doubts concerning Quine's view, since we may wonder if and how he can reconcile his agreements and disagreements with various aspects of the views of such thinkers as Duhem and Popper. It may also, of course, be an indication that he has found solutions to problems which blocked others. Let us look further. Does Quine reconcile conventionalist and realist elements on the one hand and skeptical and justificationist elements on the other hand within some theory of science? If he does do that, how does he do it?

We find in Quine's comments on science two apparent paradoxes, which turn up in various ways and at various places and which need to be explained away, if we are to find a coherent view of science built on Quine's empiricist principles.[95] In the case of the first paradox we find that Quine's empiricism leads him to deny realist views, which he then turns around and endorses anyway in the name of science. In the case of the second paradox Quine's empiricist views leads him to skeptical, fallibilist views, which he sets aside in favor of deeming up-to-date science justified.

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[95] In *The Philosophy of W.V. Quine* (1986) Quine maintains, p. 621, that he is in agreement with Popper that theories are separately falsifiable, but he denies here a ground for an asymmetry between confirmation and falsification, which is allegedly attributed to Popper by Vuillemin. (This attribution to Popper is not quite right, but let it pass.) These must be symmetrical since the falsification of any hypothesis is a confirmation of its negation. Quine wants to use his holism as a basis for his assertion of asymmetry, that is, the discovery of a refutation of a theory taken "conjunctively"—I presume the conjunction of all parts of the whole—cannot so easily be deemed a confirmation of the negation, because the negation is the confirmation of the negations of the component hypotheses taken in alternation and this is not how we habitually think. We might just as well, however, deem such a refutation, as Quine further explains, a confirmation of the denial of the conjunction and we are back to symmetry. Does Quine believe there is an asymmetry or not? He says he agrees that theories are separately falsifiable—he does not say that they are not confirmable as well—but has a different reason. He gives this (poor) reason for asymmetry between confirmation and falsification and then explains that it is not really a reason after all, because we may view matters differently and symmetry returns. This confusion is all unnecessary, since Popper has explained his answer to Quine's difficulties in understanding asymmetry quite clearly in his *Realism and the Aim of Science*, Totowa: Rowman and Littlefield, 1983, pp. 181ff. The asymmetry holds in spite of the fact that a denial of a statement is always confirmed when a theory is refuted because Popper only asserts the existence of an asymmetry between confirmation and falsification in regard to universal explanatory sentences of science. The negation of these sentences do not have this form so that sentences of the kind Popper is interested in are not confirmed even though some sentence is confirmed.

In the case of the first paradox Quine endorses instrumentalist views of science plus belief in scientific theories. This appearance of the paradox turns up in Quine's theory in various places and has been noted before. J.J.C. Smart has noted, for example, that Quine apparently moved from a conventionalist view in *From a Logical Point of View* to a more realist view in later work.[96]

Jules Vuillemin and Joseph Agassi have noted that sharp differences exist between the conventionalist views of Duhem and those of Quine.[97] Robert Nozick wonders whether there is anything for a theory to be true about.[98] And Ernest Gellner notes that Quine offers us various possibilities for a theory of truth without spelling out his real view. All of these commentaries refer to the same point: Quine limits the meaning of (scientific) theory to empirical consequences, thereby appearing to be an instrumentalist, but then he also insists that these theories are true and should be believed.

The paradox appears in Quine's view of method as well. He endorses conservative policies again and again on the basis of his view of science as mere tools for prediction or on the basis of his theory of the meaning of

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[96] Jules Vuillemin, "On Duhem's and Quine's Theses," in *The Philosophy of W.V. Quine*, pp. 595ff., esp. pp. 609ff. Quine replies and tries to reconcile instrumentalism and realism with naturalism. (p. 622) I see here no reconciliation but a mere assertion that realism must equal his form of instrumentalism or pragmatism, since nothing better exists and he accepts science. Such an argument does not bridge the real gap between the instrumentalism which his empiricism leads him to and the realism which his commonsense view of science leads him to. Quine explains the same point in his response to Lee (pp. 315ff.) in the same text. He asserts that truth can only be immanent. I do not find this to be much help either: instrumentalism is still instrumentalism even if is the best we can do and even if we call truth obtained in an instrumentalist science "immanent".

[97] Agassi, in "Ixmann and the Gavagai" p. 104 and Gellner in "The Last Pragmatist" p. 849-850 in addition to those listed below note the paradoxes in Quine's philosophy.

[98] Robert Nozick, "Experience, Theory and Language," in *The Philosophy of W.V. Quine*, pp. 339ff. see esp. 359. Quine replies quite calmly once more (p. 367) and suggests that Nozick is unduly worried about ontological relativity which is, he says, an adjunct of translation and at home translation is trivial as Tarski has shown. This is face value and he is satisfied. I am not. It reflects the positivist attitude that all that is to be known lies on the surface. This is, at any rate, a decision and not a consequence of science. Science, indeed, seeks deeper explanations; Quine seeks to stay on the surface in the name of security and conservatism. It seems clear that Quine really doesn't like to travel after all: it disturbs his view of the world.

sentences as embedded in a whole. But when a revolutionary scientific theory can achieve a simpler view of the world, he endorses that as well.[99] He once more deems the methodology of science in his empiricist, theoretical moments as an instrumentalist would but, when this stance conflicts with the commonsense realism of science, he drops it just as Hume dropped his skepticism as he played backgammon. Quine, then, endorses just those methods Popper finds the best. We might be inclined to think that Quine's recommendations allow so much that they become vacuous. And Gellner has pointed out just how little Quine says about scientific method even though he appeals to it as a cornerstone of his view. But perhaps we can be somewhat more generous and say he advocates conservatism first but allows adventure, if it is absolutely necessary, whereas Popper puts it just the other way around. This does not help us much, however, since Quine's empiricist principles do not give reasons for being adventurous though science does. (The only ground would be simplicity but, as Duhem has argued, we can increase simplicity without adventure.) The divide between empiricism and science is not thereby bridged. Nor does he want to give up either one, as his rejection of Agassi's modification of his view shows.

The second paradox in Quine's views arises due to his fallibilist views, on the one hand, which indicate that science may always be wrong and in need of improvement, combined with, on the other hand, a sort of empirical justification of our theories which he deems needed. The fallibilism of Quine, we should note, is different from that of Popper's. Quine's fallibilism is based on the indeterminacy of any theory *vis-á-vis* that part of the world it is designed to explain and on the indeterminacy of the translation of any theory into some other. Popper's fallibilism is based on the failure of any form of inductive inference or substitute method of justification. On Quine's view we can, indeed, properly induce one another to believe certain (scientific) views, even if we cannot justify them in any absolute sense nor even show them to be probable. They work and fit together and that is justification for (common) belief. Quine's fallibilism appears at first blush more radical than Popper's and it is in some respects. But then he consoles us by saying that we can and should believe not only in science anyway but even more: we should believe in up-to-date science. Popper on the other hand, explains how we can do without belief. I find Popper's view challenging, consistent and philosophical, whereas Quine's view seems inconsistent and, in denying that inconsistency, merely pious.

[99] See *The Web of Belief*, Chapter V for a list of virtues which theories should have.

Empiricism leads to skepticism but science leads to truth so they must be reconcilable.

How does Quine reconcile, then, his skeptical and limited view of knowledge, which follows from his empiricist principles, with the far more justificationist view, on the one hand, and his far more commonsense realistic view, on the other hand? I find no satisfactory explanations. Quine appeals to naturalism or to science, that is, he appeals to his faith in science and accepts the attitudes he finds there. This means that the gap between the picture of science which he builds on the basis of his empiricist philosophical principles and the picture of science which he finds compelled, and/or wishes, to accept on the basis of the normal views of scientists or of philosophers is bridged with a mere transcendental argument: Both are true so they must be reconcilable. I find this combined appeal to first empiricist philosophy and naive, uncritical philosophy of science together quite dissatisfying and hardly philosophical. Criticism is simply ruled out of court and real problems suppressed in favor of pious sounds.

We come back once more to Quine's ideal which he found in Carnap's *Aufbau*. The fundamental problem of this text was its inability to (re)construct a language which was powerful enough for science. If one started with science one could not reduce all of its statements in the desired way; if we start with the basic elements and seek to construct a powerful enough language, we hardly get off the ground. One of Quine's aims, then, was to form a new theory of knowledge as a theory of (scientific) language which would be strong enough to include science. The development of this theory as a theory of language would still be quite Carnapian and especially so if one could adhere to the modified empiricist principle that all meaning is derived from observation.

The development of this view of language, however, was inadequate to explain meaning. The old-fashioned methods of reduction were not available. But the sole source of meaning in observation should be maintained. What can we do, then, with that meaning which is not reducible to observation? We can deem it conventional or analytic but we cannot explain or identify these with logic. A substitute was needed. Quine deems psychological theory or more generally scientific theory to be that plausible substitute. We should be able to explain how (empirical) content is learned in the context of conventions of language. So, epistemology became "naturalized."

This naturalization process, however, was a curious one. It did not lead to empirical or scientific research but rather to the construction of a theory of how learning had to take place if empiricism should happen to be true. It often seems empty—I do not mean here that it is not empirical, but that it is couched in vague language interpreted in various ways—and it is clearly highly speculative and yet it should be somehow scientific or naturalistic. But, even after this resort to speculative "scientific" or "naturalistic" metaphysics, the gap between that entity described on the basis of the empiricist analysis of knowledge, i.e. language whose meaning is derived wholly from observation and convention, on the one hand, and real science, on the other hand, remains: the former is no more than tools which might be useful while the later describes the world truly; the former is a mere means of organizing the phenomena which allegedly has priority while the later shows that the phenomena are not given but that our thought in terms of objects guides our observation.

Quine's elaborate effort has not been able to overcome the basic problem of the *Aufbau*. He has not been willing to leave old doctrines according to which all content of (scientific) language comes from observation and all of (scientific) language needs to be constructed out of these with the help of mere analytic statements (or conventions?). He has thus been unable to construct a theory of science on the basis of empiricism: if we presume that all meaning comes from observation and all principles of language are mere conventions then we are forced to accept the thesis of the radical indeterminacy of translation. But if we do that, we cannot account for science: conventionalism—Quine's no-man's land of not quite analytical and not quite (empirical) sentences designed to exclude metaphysics—cannot provide the basis for realism and thus for science.

12. Quine Leads His Caravan through the Desert: Are They Pursuing Mirages?

When Quine embarked on his journey it seemed that he had set rather ambitious travel plans for himself. And we have seen that the journey was by no means easy. But after we have followed his tracks and arrived at the end of his travels, we have discovered how high the price has been for sticking too closely to the old travel guide. At times it seemed that the breakaways from it were quite adventurous indeed, but we find at the end of Quine's new journey the pale shadows of old Carnapian structures which fall prey to the same old problems. It is only because of their ghostly and vague outlines that that is sometimes so hard to see.

Quine embarked on his journey in order to find a new theory of science after he had noticed that that view, which had been his trusted Heimat for some time, had begun to show serious cracks and strains and signs of deterioration. This structure had been in broad outlines suggested by Russell, but it was Carnap who gave it the finishing touches which most appealed to Quine. These finishing touches appeared to make it even more radical and severe and to furnish the sure means of eliminating metaphysics in favor of pure science. Quine saw here not merely a home but a fortress from which one could conduct forays against any passing metaphysician. But, alas, this fortress turned out to be the ruins of an old-fashioned attempt to fight metaphysicians which was defenseless against modern ones. Carnap had failed to find either the foundation or the pillars for his proposed reconstruction which was heart of his fortress: the reduction of the meaning of all sentences to given experiences or logical constructs out of them failed, as Popper had quickly shown. Quine saw the defenselessness of his Carnapian fortress and sought to find a new one so as to continue forays against modern metaphysicians which Carnap's *Aufbau* should have made possible but didn't.

Quine's plans for travel required that he find a new map—the map of the way to first sight—which would show the way to a theory of meaning and demarcation which could not so easily be shown to be defective as Carnap's had been. The new reconstruction which he sought should have a foundation built out of the stones of observation or stimulation alone, but it should still be strong enough to carry the sky-scraping structure of science. The pillars of this structure, it would seem, could no longer be logic or analytic sentences. But they could be analytic-like sentences with dashes of empiricism mixed in to render them appropriate for this or that particular language.

Epistemology should be thereby translated into a theory of language as Carnap had proposed. All that seemed needed was a new, more sophisticated view of the empirical base, of reduction, of the non-reducible rest and/or of logic. At least so we thought. Although with the means of a theory of language alone it might have been possible to explain how to find one's way to an empiricist theory of meaning, it was not possible to explain, with the means found at this first sight alone, how language so conceived could be sufficient for science.

New epistemological or methodological means such as those proposed by Popper were not even considered as Quine sought to fill in the gaps. They would have further compromised his "empiricism". Instead, he set out for a new, second destination. He sought to fill the gaps with “scientific” or “naturalistic” explanations. He would explain (really, explain how one would explain) the empirical nature of basic statements or perceptions or stimuli with physiology and perception theory and he would explain (really, explain how one would explain) the empirical nature of scientific systems with learning theory.

An attempt to find this second sight shows that the road to it is by no means clear. We do not know which direction we should travel or how long the journey might be. The physiologically based “theory” of perception is a mere assertion that the empirical nature of basic statements and perceptions should be so explained without the actual explanation. Long known difficulties are not even mentioned. Quine starts the development of his learning theory, which should be empirical and scientific, with fantastic assumptions about how children learn which make sense if we believe in Carnap’s long outdated phenomenalism but in the light of empirical psychology are quite unacceptable. He proceeds with metaphysical speculations on how the learning of sentences might be explained under the presumption that a neo-Carnapian metaphysics (“empiricism”) is true.

We have a further problem as well in determining how the structures or planned structures which are to be found at the two sights are related or even whether there really should be two sights. We had hoped to find an explanation of demarcation and meaning, a meaningful substitute for the radical reductionism of Carnap. We found no such thing. We found a transcendental argument which begins with the assumption of empiricism and argues that if these are true, then the learning of language must be explained by the “inculcation” of meanings through stimulations. Experimental inquiries are hardly considered. For us this begs the question. It fails as well, for example, to account for clear (and interesting) metaphysical theories (Faraday) and vague (and tedious) empirical ones (Pavlov after modification sets in and before all empirical content is removed). We find no explanation for Quine’s attitude that the farther away from observations one moves the less meaning one finds.

One upshot, then, of our attempt to follow Quine’s journey is that much more serious studies of the relationship between the theory of knowledge and epistemology, on the one hand, and fields such as psychology or physiology, on the other hand, are needed and that we must be more willing than Quine has shown himself to be to abandon views such as those of Carnap which purport to be pro-science but turn out to be quite unrealistic. Quine has not done this historically and as a result he has apparently lost his bearings: he has clung too closely to Carnap's first philosophy and not noticed the other alternatives in methodology, psychology, epistemology and physiology which would enable him to travel much farther with much greater ease.

We found a number of problems with Quine's epistemology translated into a theory of language as well as epistemology translated into a so-called naturalized epistemology. We looked, then, at his theory of science and found a large gap between the view of knowledge he developed on the basis of empiricist (neo-Carnapian) principles and the common-sense views of science which he hoped to come to on the basis of these efforts. This gap was only bridged by transcendental arguments that the gap must be accepted or deemed bridged because the two poles are unquestionable. This seemed a mere cover-up of problems. And, indeed, we see in the movement from epistemology to a theory of language to naturalized epistemology to a naturalist view of science a continued effort to bridge the gap between the descriptions of knowledge, given neo-Carnapian principles—to science. But no effort was successful and the basic problem of the Aufbau which was the incentive to take this trip in the first place remains. This road winds into the desert before the real destination has been reached and we find no markers which tell us how to proceed further. We find Quine still undiscouraged setting out in the desert along new paths—even maintaining that the markings are good[100]--and urging his fellow travelers to go still farther. In the future, he self-confidently assures us, the road will become clear to all and science will show us the way to the further development of the view which we have sought. We are tired, however, and wish to return to those smoother, faster, longer and well-marked roads through lush landscapes with interesting sights we have come to know elsewhere.

Epilogue.

This essay was first written some years ago. At the time it seemed that Quine was a leading figure in the philosophy of science. His influence seemed widespread and strong; he was a man of the hour with a powerful point of view allegedly founded on a deep understanding of logic. This influence was no doubt an important reason why the essay was not then published. It was seen by reviewers, by readers as a portrayal of his

[100] See, for example, Quine, "Autobiography", p. 367.

philosophy which missed its significance. In one sense that is true: it does not portray Quine's philosophy as an inquiry worth either studying or following. The widespread high estimation of the alleged progress made by Quine in pushing forward the Carnapian dream of systematic and coherent philosophy of science to end all philosophies of science was the basis for the widespread assumption that any reading of Quine which did not explain this great success had to be missing the target; a better reading had to be possible; the philosophy of science profession employing his standards of logic could not get things so terribly wrong.

But it did get things so terribly wrong. The essay is relevant today because it clearly reviews just how things went wrong. This is an important commentary on the history of 20th century philosophy of science. Some things do go wrong a fair amount of the time. Two examples of such widespread mistakes from the 19th century are the philosophy of G. W. F. Hegel, whose success was a dramatic and lasting plague on philosophy, and the philosophy of science of William Whewell, whose rejection debilitated his discipline for a century. The understanding of what went wrong in the history of a discipline is of crucial importance for progress in that discipline. Just as Hegel's philosophy led to innumerable inquiries which sought to reveal the true contribution of Hegel's philosophy to the advance of knowledge, but failed again and again to reach some defensible conclusion, and just as the rejection of Whewell's philosophy lead to innumerable inquiries to develop philosophies of science which ruled out the important gains made by Whewell, but which again and again failed to make progress without these gains, the philosophies of Carnap and Quine have been followed by innumerable attempts to find new versions of these philosophies, which will somehow confirm that view that following their fundamental programs made whatever real progress was made in the philosophy of science. And this perspective leads a very wide percent of research today. It continues the search for a simple analysis of science made up of no more than logic and empirical sentences. But this Holy Grail is never found and the search continues. One more recent variant, for example, is the construction of new theories of "abduction". Defenders of these theories sometimes say that they are modern version of the philosophy of Charles Sanders Peirce, who invented the term. But they quickly make the same switch from a theory of science to theories of belief that William James made to Peirce's theory. Peirce quickly changed the name of his theory from pragmatism—a word that James stole to use for his own view—to pragmaticism: he did not want to abandon the hope for better theory of science, which would build on the advances made by Whewell, rather than fall into the trap of Jamesian relativism. But Quine's student Jaakko Hintikka shortly before his death pointed to “abduction” as the best path for future research. He learned well from his teacher Quine how to change the style to cover over failures, while appearing to remain true to the cause.

Interlude

A fine example of the widespread rejection of Popper's theory, due to the fear that it endangers science, is found in Bertrand Russell's reaction to it. He was far more explicit and open about his reservations than were many others, who shared his fear that Popper lowered the standards for scientific research too far.

1b. 'Russell and Rationality Today', *Methodology and Science*, 18, 140-63.

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Russell and Rationality Today

Introduction

Rationality is something most people at least occasionally worry about. In this survey, however, I do not wish to enter directly in to this concern. Rather I wish to take stock of some recent discussions of rationality by presenting part of the story of its development over the last fifty years or so. It is my hope that this survey may lead to an improved understanding of rationality, in particular to a better understanding of how we may develop rationality rationally I am mainly interested in the debate, which has centered around the philosophies of Karl Popper and Michael Polanyi. This discussion has often appeared to be a narrow dispute, not connected to the mainstream of philosophical discourse. It is by now apparent that this view of the matter is false. Yet both the broader contemporary scene and the discussion of rationality in this particular debate are misunderstood, because of the failure to set the particular debate in the broader context. I will, therefore, attempt to partially remedy this defect by surveying in a somewhat broader, though still not complete context, some discussions of rationality in contemporary philosophy, which may serve to better place the particular Popper-Polanyi discussion.

The result of the survey is a call for another survey. I explain how the problem situation has changed from a need to take stock of the problem of the rationality of the choice to be rational to the feasibility of the rational improvement of rationality or, from the attempt to form a coherent and comprehensive theory of rationality to the possibility of the piecemeal improvement of rationality.

1. Where do problems of rationality arise?

There is at least one respect in which rationality has traditionally been unproblematic. Rationality has been valued as the method of gaining truth. As long as this view was unproblematic there was no need to question either the aim or the value of rationality. This is the basis for the view that the discussion of rationality is only of importance for a particular school. The problem of rationality is only important for those who either question or find problematical the traditional goal. And this group is small—the Popperian critical rationalists and the Polanyite post-critical philosophers. This is, of course, already false. The influence of Popper and of Polanyi or Kuhn, if you prefer, is enormous. But the germ of truth in it is that there are a wide group of philosophers who are more traditional at least about rationality and therefore appear to dodge the problem.

The appearance is misleading, however. New problems of rationality occur in all contemporary schools of philosophy. The problem situation has been recognized to various degrees and with varying emphases, but there is a significant unity: The rationality of rationality is openly thought to be problematical by at least some representatives of almost any school you care to mention. Whether these philosophers think they know the answer or not, many of them feel required to mention the problem. Perhaps even more importantly, when we reconsider philosophical positions in the light of this problem they appear differently. Unarticulated and/or undeveloped views come to the fore and many of the problems emphasized by these philosophers begin to fade. To put the same point a different way: The problems of philosophical frameworks begin to take precedence over problems within those frameworks. This is a likely sign that we are hitting the more fundamental problems of twentieth century philosophy.

In this survey I will explain how viewing contemporary philosophy as attempts to respond to the problematic character of rationality provides an interesting and clear-cut challenge: How can we, on the one hand, manage to avoid unreasonable claims for rationality and yet, on the other hand, avoid relativism as a result of lowering these claims too much? It may be hoped that it is possible to develop and improve desiderata for theories of rationality which will enable us to pursue a successful course of inquiry in the rationality of rationality.

2. Bertrand Russell

Before actually beginning to survey the field let me introduce the problem once more with the central example of this essay. The foil I will use to evaluate other attempts to portray or avoid the problem is the philosophy of Bertrand Russell. This will enable me to illustrate though not, of course, prove the above-mentioned thesis that all contemporary philosophy recognizes the problem of the rationality of rationality with the most powerful of all the traditional theorists. Russell devoted almost all his efforts in the theory of knowledge to solving problems within a framework of frameworks near to logical positivism. But Russell clearly recognized the problematic character of the framework. He saw that justification of justification, i.e. justification of empiricism, had not been provided. Russell's appraisal of logical positivism or of an uncompromising empiricism is his appraisal of rationality: Russell believed that if rationality is possible, we must justify our theories and if any justification is possible, empirical justification is possible. A refutation of the theory that empirical justification is possible is thus a refutation of the possibility of rationality. Furthermore, he faced the consequences of the failure—his own as well as others – to solve this problem. Since rationality is the only possible means of creating and maintaining a humane and viable society and rationality cannot even defend itself, hopes of humanity are doubtful. He states:

> "The trouble (skepticism towards science) is an intellectual one, indeed its solution, if there is one, is to be sought in logic. For my part, I have no solution to offer; our age is one which increasingly substitutes power for older ideals, and this is happening in science as elsewhere. While science as the pursuit of power becomes increasingly triumphant, science as the pursuit of truth is being killed by a skepticism which the skill of the men of science has generated. That this is a misfortune is undeniable, but I cannot admit that the substitution of superstition for skepticism, advocated by many of our leading men of science, would be an improvement" (Russell, 1931, p. 100)

This pessimistic portrayal deserves to be considered if only because it poses a clear challenge to do better. So let me now quote Russell's appraisal of logical positivism, i.e. rationality:

"There is one matter of great philosophic importance in which a careful analysis of scientific inference and logical syntax leads—if I am not mistaken—to a conclusion which is unwelcome to me and (I believe) to almost all logical positivists. This conclusion is that uncompromising empiricism is untenable. From a finite number of observations no general proposition can be inferred to be even probable unless we postulate some general principle of inference which cannot be established empirically. So far there is agreement among logical positivists. But as to what is to be done in consequence, there is no agreement. Some hold that truth does not consist in conformity with fact, only in coherence with other already accepted propositions already accepted for some unspecified reasons. Others, like Reichenbach, favor a posit which is a mere act of will and is admitted not to be intellectually justified. Yet others make an attempt—to my mind futile—to dispense with general propositions. For my part I assume that science is broadly speaking true and arrive at the necessary postulates by analysis. But against the thorough going skeptic I can advance no argument except to say that I do not believe him to be sincere' (Russell 1956, pp. 381-2).

Russell's acute, honest and open realization of the weaknesses in the positivist program goes to the core of the problems in contemporary philosophy. Philosophers have alternative views about what is possible, what they should seek, and how they should seek. Yet they have little ability to arbitrate these disputes since they are, as often as not, about unarticulated attitudes, hopes and fears as about the success or failure of theories. For this reason few discussions of the problem of rationality have taken place in spite of the widespread recognition of its importance. The discussion of rationality is an attempt to make articulate and open these disagreements in order to subject them to rational debate. It is an attempt to push back the boundaries of rationality described by Russell.

lt is for good reason that most philosophers regard this effort as futile. Simply posing the problem plunges one into difficulty since in order to do so one must adopt a particular view of the problem situation—an overview of how to proceed, what to hope for and what attitudes toward rationality to adopt. Circularity and arbitrariness seem unavoidable. As a consequence many philosophers advance one form or another of relativism and/or dogmatism.

In this essay I hope to mitigate these problems to a degree by surveying alternatives available to us. I do not claim completeness for my survey. Rather, I hope in this way to move forward toward a view of the pursuit of rationality which will both avoid running aground on the Scylla of insoluble problems and avoid being swept under by the Charybdis of hopelessness about any improvement of rationality. I hope to explain part of the development of problems of rationality in order to improve our understanding of how we may go forward. Such an explanation must, of course, be not only biased but also partial; it can nevertheless serve to develop a point of view and understanding between competing points of view as well, thereby providing a foundation for progress.

3. The rise of contemporary problems of rationality.

A survey of problems of rationality needs to begin with the traditional aim of science: certain truth. Nobody nowadays takes this to be a reasonable goal—a clear indication that we all are attempting to negotiate between our hopes and limitations—yet all parties take certainty to be a goal that we must not miss by too wide a margin. If we stray too far we may as well abandon the whole project. And, since many have done this, those who wish to maintain rationality have all the more reason for being conservative. In order to start slowly in the hope that we may better understand the rationale for current positions let me discuss why certainty needs to be compromised at all. The belief that certainty needs in any way to be compromised is, after all, a recent development.

Nineteenth century philosophers, e.g. Kant, Whewell and Mill, almost universally believed that certainty in science could be achieved. Whewell and Mill thought that the history of science would sort out true theories from false ones. Kant thought that we simply had to be clearer about what it was that we were certain about. And even after Kant's view of synthetic a priori truths was widely doubted, if not rejected, the transcendental argument remained: Since science is true, we know how to attain the truth. Even those philosophers who repudiated science did so on the grounds that it was too narrow, perhaps that it could not understand man or history. Science remained secure in its own domain.

There were considerable signs of trouble however—at least we can see them with wisdom after the event. One sign of trouble was the development of non-Euclidean geometries. It was at least plausible that space was not Euclidean, as had been "established" by Newton. A second sign was the difficulties faced by associationist psychologies. Theories of the certainty of knowledge depended on a psychological theory of the reception and identification of not true but also indubitable or veridical perceptions. Kant's theory of perception, a theory which required an innate framework for knowledge to be possible, overthrew this view. Physics could be certain even if perception was not veridical. Yet Kant's view made knowledge highly problematic by severing knowledge from the description of "things in themselves". Wundt attempted to redo the job, taking Kant's view into account, but reestablishing perception as a source of knowledge. This led to further difficulties when experiments, especially in the hands of Külpe and his students, not only when against associationist views of perception, but further, with the rejection of innate frameworks, threw everything up for grabs. A third influence was the development of non-Newtonian physics—field theory—as a substitute for particles acting at a distance, which undermined the transcendental argument whose premise is that science is true.

These theoretical innovations probably would not, in themselves, have caused any great switch from the aim of certainty; they simply posed problems for this view. For example, how could an associationist theory of perception be plausibly developed in the face of experiments indicating of that prior conceptions or attitudes have effects on perception? Helmholtz had such a theory. Or, why could alternative mathematical frameworks apparently describe the world correctly, even though only one--Euclid – could actually describe the world correctly? As a matter of fact these problems were not taken as a serious threat to the certainty of science. Since almost everyone was Newtonian, a transcendental argument was used to prove they were soluble: Since we know the truth, we know that proper innovations are reconcilable with it even though we are not quite sure how.

The final undermining of the theory that certainty could be achieved was done by physicists—Einstein—and philosophers of science--mainly Popper. Prior to Einstein, there were theoretical reasons for believing that not only the details, but the theoretical frameworks of science could not be secure and, therefore, were subject to change. But changes were in fact thought to be minimal. After Einstein's relativity theory and after quantum theory, this view was no longer tenable. Theoretical frameworks in science did in fact change. They changed radically and unexpectedly. The problem situation changed drastically. Any theory of science as certainty could no longer presume that science was certain; on the contrary, it had not only to explain or explain away the failure of science to arrive at the truth, but also the failure of science for so long to even recognize its own mistakes.

As already pointed out, the difficulty for a theory of science was not the traditional one, i.e. to explain why error existed and how truth may be obtained. Even the best science we had was likely to be false and could not, therefore, be explained away as mere error and even the best methods we had seemed incapable of finding the truth. Popper studied the new problem: how can science provide us with knowledge, if revolutions occur and no scientific proof is possible? He argued that not only were revolutions to be expected, but that they were needed—in order to correct inevitable mistakes and avoid obscurantism, i.e. forcing our theories to fit the facts. With all these developments certainty as reasonable goal was demolished.

But not yet. The persistence of the goal of certainty in the face of these developments is puzzling and demands some explanation. Bertrand Russell—after Einstein and Popper—as quoted above, says he believes that what science says is true and if you do not believe it, you are not sincere. If Russell finds the skeptic insincere, I find it difficult to think Russell sincere—yet I have no doubt that he was.

There are two primary reasons, I believe, for the tenacity of the goal of certainty. The first is a strong belief in the importance of maintaining rationality. The second is a theoretical problem: How can one lower the standards of rationality without abandoning rationality altogether?

Let us take the second reason first. It has traditionally been thought that the only standard for rationality that we have is truth. The final test of rationality, on all traditional views, is whether the rational man will come to know the truth. This test has prevailed because no compromise appears viable that will save the original intent, i.e. to have a theory which enables us to identify theories which correctly describe the world. Indeed it is hard to even describe the traditional views without mentioning truth as their single aim.

Skeptics have, of course, argued that since we cannot know the truth we should settle for something less—either faith or peace of mind. This move, however, not only abandons the central aim of traditional views, it throws its own claims into doubt. If we cannot know the truth, how can we know how to have peace of mind? Thus, any move to settle for something other than truth is also unacceptable because without truth we will not know how to attain these other goals. Thus, it appears that, if we are to have knowledge or if we are to reach any goal at all, we must have the truth: A method which may or may not work appears useless, since after its use we still would not know what the truth was. It appears that if we abandon certainty, we will abandon rationality altogether. If truth is problematical, any view may do, since they are all equal, i.e. uncertain. Thus, in order to avoid relativism, we must achieve certainty.

This leads to the other reason for maintaining certainty as a goal. Rationalists, whatever their version of rationality, have thought--indeed, many still do—that civilization depends on the maintenance of rationality. This, l suppose, sounds rather pompous, but nevertheless needs to be said. Rationality is what is thought to distinguish civilized man from barbarians. It provides humane ways of settling disputes and the ability to discover what is best for humans to pursue; it has been thought to provide a foundation for ethics and politics. And even though these prospects may not be thought to be entirely successful, the project is thought to be important. If all these views hinge on rationality, and rationality hinges on certainty, then the abandonment of certainty is a drastic plunge in the dark.

Russell is probably the best example of the tension this raises, since he believes that civilized life requires rationality, rationality requires certainty and certainty is impossible. On Russell's view, a free man's worship depends on rationality; to abandon the quest for certainty is thus to abandon his most fundamental hopes and aims. Russell continued to live with this tension. He continued to advocate rationality. It may be that he did this only because he thought it was cowardly to give way was to despair when hope was possible. Yet he did not think such a stance would be possible for mankind. Ernest Gellner, also a skeptic, may be the closest to Russell of all contemporary philosophers. He also sees the tension between the need for the absolute truth and the inability to attain it as irresolvable. But he is milder than Russell. He believes the situation can be maintained. We do not overcome it intellectually but institutionalize it.

4. Attempts to avoid problems of rationality.

The maintenance of the goal or standard of certainty in order to maintain rationality raises an interesting problem: How rational is the maintenance of traditional rationality? In order to further explain this question, let me discuss two ways in which traditional rationality is maintained. One is to maintain the goal of certainty and its problems. The other is to explain the problems away by open or surreptitious appeals to authority.

I will use Russell as illustrative example of the first type. Russell is a good candidate, because he does not accept bad solutions to problems: If no good solution is available he readily admits the fact and lets the problem remain open. Russell's position involves the greatest of tensions. He believes, on the one hand, that one ought to accept only justified beliefs. Yet, on the other hand, he believes that no adequate theory of justified belief or knowledge exists, including no justification for his belief that one ought to accept justified beliefs. Yet he attempts to live in accordance with this theory in the most rigorous fashion possible. In his ethics—in "A Free Man's Worship" he attempts to provide a theory of what to live for which is based on a rigorous intellectual honesty. In politics he stood against war, appalled by the hysteria and jingoistic attitudes of those who favored it—attitudes which blocked reasonable consideration of the policies and led to the worst kind of human action. In his essay on marriage and morals, he attempts to distinguish prejudices, superstitions and folly from rational sexual theories and attitudes. In one respect he does not believe that his results meet his standards. He believes that they all hinge on assumptions which he passionately believes, but which he also believes are not rational, i.e. have no justification. Yet he defends these views in the name of his appeal to rationality.

But let us slow down: What are the open tensions in Russell's philosophy? First, there is the problem which has already been discussed: The foundation of logical positivism cannot be justified, yet the aim is to provide a theory which will lead us to accept only justified theories. Secondly, even within logical positivism we cannot provide an adequate theory of science, i.e. a theory which will allow us to justify some theories in accord with some principle—regardless of how we find or justify such a principle—such that justified theories will be always or for the most part true. Thirdly, if the aim of logical positivism is to be comprehensively pursued, it must be pursued in ethics and politics as well as science. Yet, in these other areas there is even less hope than in science of providing a viable theory of justification. Russell seems to end up with a Kantian utilitarian theory—we ought to seek human happiness. But he confesses that he can provide no argument, i.e. justification to show that his position is preferable to its denial. Nevertheless, he regards its denial as clearly more than a reasonable disagreement, perhaps an inhuman one. His view thus wavers between a Kantian moral imperative and a hedonistic or Spinozistic naturalism. Each side fails since neither naturalism nor an apriorism provides the needed justification.

Russell's philosophy may be read in part as a series of attempts to formulate and solve the above three problems. The attempts succeed admirably in posing the problems but fail to solve them. Thus, in much of Russell's writings, especially in his ethics and politics, Russell ignores them and proceeds to develop his view in accord with Kantian principles. In science we know that physical theory and the presuppositions of science are true; in ethics and politics we know each individual is a valued end in itself whose happiness should be promoted. He believes these views but does not believe them to be justified. But Russell's reluctant use of Kantian philosophy cannot be rationally maintained.

This brings us to a second position: the degenerate defense of justificationist rationality. Many philosophers accept the basic outline of Russell's philosophy, but unlike Russell are unwilling to leave it at that, and like Russell, believe that the problems Russell raises cannot be solved. The stopgap measure left is to explain away these problems. The Oxford and Cambridge schools of language philosophy represented by Ryle, Austin and Wittgenstein and so definitely repudiated by Russell, on the one hand, and the phenomenological and hermeneutic philosophy on the European continent such as that represented by Gadamer, on the other hand—two movements which not quite plausibly seek common ground—are major twentieth century examples of such attempts.

The characteristic feature of these schools is the appeal to some authority without discussing this authority. The language philosophers appeal to language—yet are reluctant to be drawn into a discussion of the exact use of language as an authority or, if so used, whether such use is legitimate. These philosophers repeatedly appeal to intuition to justify the appeal to authority. Analysis as a method of providing justification requires the appeal to intuition as judge. In a similar fashion the phenomenological and hermeneutic studies of continental European philosophers must implicitly appeal to the authority of whatever their subject matter exactly is and the intuition of analysts to judge whether this subject matter has been correctly interpreted. Yet the problems of the unreliability of intuition and of the theories or views to be interpreted remain unsolved. This type of position is, unfortunately, of vast appeal. I label it degenerate, because it adopts a method of solving a problem, which presumes that the very problem raised is already solved in some other areas with no explanation of this optimism.

This brings me to the irrationalist defense of justification. Some philosophers, accepting the view that rationality requires justification and that justification cannot be justified, conclude that one can and should openly adopt authorities in an arbitrary fashion. These authorities can then be used to justify other views or actions.

Theories which endorse the adherence to views which do not meet any standard of rationality—views which are simply chosen—are, I think, more popular than those which use subterfuge. These views not only endorse irrationality as a necessity of existence but deny that this poses any special problems of rationality. It is held that only problems of choice within a framework remain. There are two varieties of this alternative; one is anti-science; the other is pro-science. The anti-science philosophers include Sartre, Heidegger and other so-called existentialists and related figures such as Adorno and Habermas at least. They tend to hold that scientific approaches and attitudes are often misleading and damaging.

The second group of irrationalists—those that are pro-science—is represented and led by such thinkers as Polanyi and Kuhn. These philosophers endorse the view that choices of what we believe are beyond reason. But they nevertheless develop a theory of how science may be rational to a degree. Their theory is that in science we may arbitrarily choose a framework and within this framework we may be empiricists or scientists. Science is the activity of a group of people working within an arbitrarily specified or at least comprehended framework. Those who do not know the framework are not scientists. This theory, while endorsing science repudiates individualism, which is a cardinal feature of traditional rationalist views. Ironically anti-science irrationalists sometimes maintain this tenet, e.g. Camus. It is thus sometimes hard to say whether the pro-science or anti-science irrationalits are closer to traditional rationalists.

Pro-science irrationalists, such as Duhem and Polanyi, for example, use their irrationalism not only to develop theories of science but to justify irrationalism in other areas as well—both adopted Catholicism and used the theory of the limits of rationality to justify their religious beliefs.

There is also, as a response to the difficulty of the rationality of rationality, a pro-science skepticism though not irrationalism. This is represented by Poincare, who held that the proper aim of science must be exclusively to make true predictions, and even though metaphysical questions may be meaningless, we may at least rationally discuss the path of science; the degree to which scientific theories are true and the consequences of scientific theories being true. I have some difficulty placing this view—it is difficult to comprehend since, in a fashion, it abandons rationality in science—it adopts the view, that science can only obtain true predictions, while maintaining a degree of rationality outside of science. This is paradoxical but is maintained—I suppose but do not know—because it is supposed that in science we need a clear theory and guidelines to proceed. We need to maintain precise standards to maintain the unity and progress in science, while outside of science, since we have lower, i.e. vaguer expectations, we may not worry so much about our failure to meet the highest standards of rationality but may be satisfied with lower expectations and standards. In spite of the curious double standard for standards of rationality--high standards for science block rationality in science while low standards of rationality allow some rationality outside of science, the view retains some popularity. Marxists, for example, tend to hold that Marxian theory provides the truth while (natural) science is only a tool. More recently, Stegmüller flirts with such a view, but fails to overcome the problems it presents.

Before turning to Popper who makes the whole debate much more acute, I wish to discuss Carnap. I do so not because of the significance of his position, which I regard as inferior to Russell's, but because the views of Carnap and his descendants, on the hand, and Polanyi and Kuhn, on the other hand, are by some philosophers gradually becoming conflated. This conflation is significant as it brings together two distinct responses to the problem situation. It seems prima facie incoherent, since one group openly advocates commitment while the other seeks justification. Yet, on second glance, the marriage is reasonable. Russell has pointed out that positivism has no foundation. So, if the view of Polanyi and Kuhn is accepted, the technical problems of positivism, i.e. the problems of confirmation, are retained; they are simply reinterpreted. Let me explain.

Carnap's method of approach is based on an attitude toward science: We need to have agreement and peace. We need to find the methods or techniques by which agreement is attained. The method for doing this is to analyze our concepts or set all our concepts in the context of logical systems. The solutions to the technical problems of constructing formal languages are part of an attempt to succeed in realizing the goal of agreement and peace in science by calculation. In this way, we may "construct" instead of "infer" concepts. Such construction will further enable us to specify meaning in clear indubitable ways. Once this type of project is completed, we will simply need to choose appropriate clear languages. Our disagreements will be limited to disagreements about the relative usefulness of various well-constructed, i.e. formal, languages. It is, I think, Leibniz's dream, that when faced with a disagreement, the two parties may sit down and say, let us calculate. There is a further aspect to Carnap's approach which should be mentioned: He wishes to go slow. Rather than tackling problems which immediately generate controversy and difficulty, he deals with the narrow range of problems, which can be solved which are preparatory to more grand accomplishments. He starts with narrow languages and problems of these languages and then seeks to build on them to make them more powerful and comprehensive. In effect, the strength of his program may be explained by its patience: Any difficulty may be postponed as tomorrow's part of the program as long as small problems are being solved today.

By now the similarity between Carnap and Polanyi and Kuhn may be apparent. Carnap employs a seldom articulated program to a variety of small puzzles. Polanyi's theory can describe the Carnapian program, methods and movements. Prior to the forced comparison of this program with others—forced by Popper and others—this similarity may not have been apparent, but now it is. As a result a modified Carnapian-Polanyite program is emerging: In this program the methods of the Carnapian framework are used in the puzzles to be solved within the framework, while the use of a framework is justified by a Polanyite philosophy. The problems of rationality may thereby be put aside and the details pursued.

I should stress, however, that this is one reading of Carnap; there is also a second. The second reading is that methods of confirmation are themselves open to empirical evaluation. They need not be dependent on some definitive framework. In *Conjectures and Refutations* Popper has pointed out that Carnap has at times apparently adopted such a view. I conjecture he was ambivalent since the empirical evaluation of the method could not be allowed to interfere with the broad program of agreement and peace. Furthermore, the proposal faces insurmountable difficulties, as Popper points out. Carnap was aware of such difficulties.

Let me summarize thus far. We have first a tradition of rationality represented by Russell, which remains incoherent by demanding that standards, unattainable standards, be met. Secondly, there are attempts to explain away this difficulty by surreptitious appeal to authority. Thirdly, there is anti-science irrationalism, which deems all views equal. Fourthly, there is pro-science irrationalism, which deems rationality to some degree possible within science. Fifthly, there is a view that the rationality within science is limited to the ability to make true predictions, but some more extended possibilities exist outside of science. Finally, the problems of traditional rationality themselves are interpreted as lying entirely within some framework, which is itself arational but not irrational. This is the last resort of those seeking to maintain traditional programs and problems of rationalists. This last alternative indicates the unity of those philosophers who retain justificationist views; they all have turned either openly or surreptitiously to authoritarian positions. There are hardly any like Russell, who maintain both justificationism and the failure of justificationism; the only way to make justificationism work is by appeal to authority.

5. Popper and Bartley.

An alternative is clearly needed. The major efforts at providing an alternative have come from the Popper school. But Popper himself did not, at the beginning of his efforts, have this problem in mind. He was interested in how science grew. But in forming his theory of science he broke the rules of the traditional view of rationality thus opening up an alternative path.

Popper developed his theory of science first; the problems of rationality developed later, when it became apparent that his theory of science upset far deeper theories, attitudes, hopes and fears, than was initially supposed. The theory of science through adventurous and innovative ideas starts rather innocuously enough; Popper endorses Hume. But he claims to have a response to Hume. And this is where the difficulties arise: What counts as an adequate response to Hume's skepticism?

Popper's response to Hume was to form a theory of how science grew by showing theories false. This view differed from all previous theories in rejecting the theory that science progressed by showing that theories were true or likely to be true. Popper claims that he has provided a solution to Hume's problem, because he shows, contrary to Hume's contention, that knowledge is possible. He does this by providing a theory of how we can get closer to the truth.

This theory is that science progress by making bold conjectures, finding refutations, and solving problems thereby formed with new conjectures. We are closer to the truth, because each new theory predicts more events, i.e. it has a wider range than the previous one. Thus simplicity, depth and predictive power—all the same on Popper's view—are all simultaneously increased by a few, simple and bold conjectures. The process is unending, never proves any theories true, but continually gets closer to the truth.

There are three types of attacks on Popper. One is that he has not accomplished what he claims, since he mixes justificationist and fallibilist moves; the second is that the fallibilist methods may be improved upon. These are at times interesting and bear on the issue at hand. But the third type of criticism—Russell's—the criticism that even if he succeeds in how own claims, his philosophy is one of despair is of more relevance here.

He states: "Should we, perhaps adopt the somewhat despairing theory of Professor Popper" (Russell, 1961, p. vii).

Russell's criticism is far simpler and grants Popper much more, yet it is also much deeper. It grants Popper more because it acknowledges that his theory does not surreptitiously use some justification: It is deeper because it requires a theory of rationality to respond to it. A theory of rationality is needed to appraise the rationality of a theory of science without justification. The problem becomes acute for Popper, since previous theories assume that rationality is equivalent to justification. Popper's reply to the criticism Russell voiced is that he had minimized irrationality.

Popper believes, like Russell, not only that there is no rational basis for rationality, but that there can be none. Though we begin with faith in rationality, this is all we need. We do not need commitment to some particular view of rationality. Furthermore, we can avoid, as Russell cannot, some of the tensions resulting from endorsing unreachable demands. We can do this, because we can replace the demand for justification by a demand for criticism. We can be empiricists—we can form and improve scientific, i.e. empirical theories, and we can distinguish knowledge from sham and hypocrisy. The moralistic tones are strong and no accident. Popper, like Russell, believes that humanity and morality are at stake. Perhaps, since he is proposing a lowering of standards, he moralizes as much as anybody by pointing to the superior virtue of his own standards and the necessity of maintaining them. Popper's attitude towards his critics reflects the same moral concern that Russell had. He also had a serious problem however: If all theories of rationality view his theory of science as endorsing an irrationalist view, how can he defend it? Popper made deep and courageous advances, which Russell did not. This left Popper with a curious problem: He needed to defend his own new standards. Yet it was not clear how to defend his lowering of standard in such a way that would allow his lowering but block others, which might lead to a betrayal of rationality. He did say that his own view described science when properly done and that it could provide an adequate theory of progress.

This analysis is curious: The justificationist, Russell, is resting without justification, the non-justificationist seeks to settle the issue, i.e., to find a convincing defense for a particular variety of non-justificationism. This discussion reflects the confusion about how we can reasonably lower standards. If the lowering meets old standards it is unnecessary; if it does not, it is irrational.

This brings me to Bartley. Bartley is enormously important, because he was the first to attempt to take stock of the above situation and to provide a theory of rationality, which would provide what Popper lacked, i.e. a way of arguing that his standards of rationality were rationally superior to other standards. It is the crux of the problem: How can we argue rationally about rationality, when such a debate must presume some standard of rationality?

In order to explain the pathos that this problem raises, let me mention Russell once more. In discussing science, Russell explains that science is no longer believed, due to a rise of skepticism, and he fears the rise of superstition as a consequence of the breakdown of the credibility of science. Russell believed in science; he did not believe science had established its credentials and he saw no way to do so. As a result he feared irrationalism. There could be no third way; there could be no rational way of arguing about the standards of rationality.

Bartley's theory is most significant for what it attempts to do, i.e. argue about rationality or, to make the choice of rationality rational. He wishes to do more; he wishes to show that rationality is superior to irrationality and to develop and defend a particular version of rationality. In order to explain this, we need to consider the problem situation.

First, as I have explained, traditional versions of rationality were incoherent. Secondly, irrationalist views were developed which used this incoherence to justify irrationalism. Since the choice of rationality may apparently be deemed just as irrational as any other alternative, e.g. as commitment to some religion, this irrationalism may be defended as rational. Thirdly, Popper had to a degree removed the incoherency of rationality by removing the demand for justification. But he maintained the view that the choice of rationality was an act of faith. Thus, Popper in the first edition of *The Open Society and Its Enemies* endorsed the same view of the choice of rationality as that endorsed by the irrationalists, i.e. the theory that the choice of rationality was not rational.

Bartley sought to provide an explanation of why the choice of rationality was itself rational. Bartley's theory is easy to explain: Understanding what he has accomplished however is not so easy. First, Bartley proposed that if we regard rationality as criticism or as holding theories open to criticism we may show that rationality is coherent, i.e. that rationality is itself rational. We may do so, because the theory that a necessary condition of rationality is to hold theories open to criticism may be met and rationality may therefore be rational. According to Bartley the virtue of this proof is that it defeats the irrationalists. They can no longer hold that irrationality is as rational as rationality since - rationality meets at least this necessary condition for rationality and the irrationalist does not.

Thus far it is easy. But neither Russellians nor the irrationalists are likely, I think, to be convinced. The reason why I presume their skepticism— to my knowledge no representative of either group has taken any public notice of Bartley—is that they would say, yes, rationality of the sort you mention indeed meets its own standards, but such rationality is not good enough: rationality on your view is only rational, if it is not rationality.

This is my portrayal of a traditionalist's view. Let us turn to a fallibilist's appraisal.

Suppose one accepts Bartley's theory of rationality for whatever reason. One need not worry about the type of problem that traditional theorists had, i.e. one need not worry about the problem of the rationality of the choice of one's particular brand of rationality given that brand of rationality. But further problems remain. Since we have opened the Pandora's Box, we cannot close it. Since we have questioned, our standards of rationality, we can only retain skepticism about our current standards. So, questions of rationality remain open. We still have on the agenda questions of the sufficiency of our standards of rationality for specific goals or improvement of rationality, as we understand and practice it. For example, on Bartley's view a more developed theory of criticism may be needed. Such a theory may, of course, lead to a rejection of Bartley's view.

This leads to a remarkable result. The problems of the traditional rationalist and the Bartleyan rationalist are of a similar kind; each needs to appraise various proposals for the solution of piecemeal problems to determine if they can improve rationality. And each also needs to determine whether the aims and goals of rationality posited by his theory are consistent with the results of the piecemeal discussion. Justificationists have piecemeal problems of, e.g. confirmation. Fallibilists have piecemeal problems of criticism. Each needs to consider whether these piecemeal problems block the achievement or pursuit of rationality as they envision it or whether solutions can he found to develop their views. To say this is to say nothing more than that no comprehensive theory of rationality exists: Each theory leaves open problems and must do so, since no theory can specify sufficient conditions and remain consistent. Though Bartley's theory cannot be deemed to have settled the problem of rationality, since there are ever new problems at hand, it may, nevertheless, be viewed as an enormous success. It leads to new piecemeal problems and provides an alternative which is not, like others, capable of immediate rejection on the grounds that it does not meet its own standards.

But now we must go slowly. Bartley has provided an alternative. The alternative simply requires that we take stock again: It does not eliminate a problem situation, but, as Agassi has shown, transforms it. It transforms it by making the appraisal of alternative theories of rationality part of the development of rationality. But an appraisal of some theory of rationality must always use to some degree standards external to the theory;

otherwise various theories, each meeting their own standards, would have to be judged at equal value. This indicates that rationality cannot be the adoption of any particular theory of rationality because the rational discussion of rationality, which cannot he avoided, requires that we go beyond all the available theories. Rationality must continually be used to develop and improve theories of rationality, so rationality is not dependent on any such theory. This is the beginning of Agassi's recent analysis.

6. New Problems.

From this point in the survey it gets quite difficult to proceed, because the traditional reading of the situation shared by traditionalists and by Bartley is overthrown. This reading is that rationality may be described by a theory and rationalists are those who believe and live by that theory, which is true. On the view of the problems just suggested; we cannot identify rationalists by the particular theory they hold nor could we hope to do this even if we had a true theory: There is no single theory, which may be said to describe rationality. This raises a new problem. It could appear that if rationality is not to evaporate, we need to have a demarcation between rationality and irrationality. The situation just described, however, seems to confirm the worst fears, i.e. that the rational discussion of rationality will destroy it and lead to superstition. This is too fast. Indeed the argument depends on the old reading. Rationality will only he destroyed if it depends on a particular theory of rationality. But this need not be the case. Rationality may be a human activity, which can be improved upon, developed and reappraised from time to time. We may evaluate rationality in terms of degrees and improvement rather than all-or-nothing rationality or irrationality. Such evaluations will involve evaluations of both theory and attempts to improve them.

This raises a further problem: We seem to need a theory of the development of rationality. Such a theory of the development of rationality would seem to be the complete theory of rationality just rejected. There is at least one such theory, which shows that this need not be the case. Agassi proposes that rationality may develop in a bootstrap fashion: We use our current theories to pose problems, which lead to new theories, which can then allow us to reject our old theories; the idea is to use the same skeptical-critical-optimism in discussion of our theories of how to proceed, as we use in our theories of science or metaphysics or any individual discipline. On this theory, we need to study rationality in a piecemeal fashion. The theory itself is considered a piecemeal theory, since it only solves one problem of rationality, i.e., How can we study rationality?

We appear to have the following alternatives. First, we may maintain traditional standards of rationality; but this seems unprogressive even in Russell's hands. Secondly, we may make traditional rationality a peculiar religion by making the problems of rationality exegetical; we can surreptitiously appeal to authority to explain problems away. This was the first trend Russell, to his regret, saw occurring. Thirdly, we may give it all up as a bad joke and resort to the second trend Russell saw –the open acceptance of authority or the substitution of power for ideals and of superstition for knowledge; or finally, we may seek to be as rational as it is possibly to humanly be—we may seek improvement, however possible and whether within or without of science.

Before concluding let us begin to take stock of this new problem situation: What problems can we expect to face, if we expect a continuing debate between alternative theories of rationality? One immediate consequence, already touched on, is that all theories of rationality, regardless of how comprehensive they are, are partial. No theory can encompass rationality. This partial aspect of any theory of rationality is the key factor which we need to take stock of.

Any statement about rationality can at most, lead to progress; it cannot "capture" the situation. As a consequence, belief, or belief plus action in accordance with any theory, is not capable of characterizing rationality or a rationalist. Indeed, it may be better to drop entirely the question of rationalist vs. irrationalist and simply speak of degrees of rationality in discussion or action. People then are rational to the degree they engage life rationally. This can be measured partially in accord with different measures.

This partial theory poses a problem: How can we have piecemeal theories that are both critical and progressive? I may explain this problem by discussing a piecemeal alternative which is neither critical nor progressive. This device will enable me to further develop my alternative and to explain the problem.

The language philosophers and John Austin in particular had a piecemeal theory of how to do philosophy though they never, to my knowledge explicitly discussed the problems of rationality Nevertheless, the piecemeal theory proposed by Austin may be properly viewed as a partial piecemeal theory of rationality. It is so, because it describes how to solve problems (rationally) in one type of situation (philosophy). The description of the method is problematical, since Austin and his collaborators were themselves reluctant to describe their method, since such a statement would probably violate the method advocated. It would be general and not piecemeal. Austin did, however, discuss his method once, though he apparently regretted even this attempt. The single discussion is presented as a methodological commentary on the study of excuses in “A Plea for Excuses”. In this essay, Austin explains that, if we treat problems in detail—piecemeal—conflicts will be removed; at least we can postpone such discussions; after the piecemeal tasks are done they may not be needed. The idea, which I must put into Austin’s mouth, is that detailed analysis can enable us to explain away contradictions and puzzles. If we have a puzzle, it is because we are too general in our analysis. On this view all problems must either be dissolved or accepted as challenges for the piecemeal approach. Such dissolution of problems—to return to our reason for discussing Austin--is uncritical because it aims to preserve all views by the methodological device of eliminating contradictions by distinguishing the context of application of these various views. Since the views are used in different ways in different situations, they are not contradictory. In this way a piecemeal view can endorse all competing theories; it can defend all existing practices.

The device for blocking this result is clear; we must not allow piecemeal studies to be too piecemeal. But now we have new problems. How general should we seek to be? How can we use such general appraisals to provide the condition for effective criticism? And how can we use criticisms as a spur to better views?

This is, of course, the beginning of another story. But I will end here. The beginning I have just described throws everything up for grabs since rationality needs to be evaluated on several criteria. Russell, for example, is, in his openness and candor, exceedingly rational; yet, by another standard he is unprogressive and this limits rationality needlessly. Furthermore we need to view criteria as competitors and to develop criteria for criteria. To some this appears to be a breakdown and to others it appears a viable program. The stock-taking of this new problem situation is only beginning; it will no doubt change the problem situation as previous appraisals have already done.

Summary.

The debate concerning rationality between the Popper and Polanyi schools has appeared to be a narrow dispute, not connected with the mainstream of philosophical discussion. This dispute is here placed in a broader context. Russell is used as a touchstone since he was a traditionalist who saw that traditional views had broken down and analyzed the alternatives. Both Russell's clinging to past ideals in spite of difficulties and attempts to avoid the problem of rationality, so sharply criticized by him, are deemed inadequate. Thus, the new attempts by Popper and Bartley are needed quite generally. They are deemed to lead, in the work of Joseph Agassi, to a new problem situation calling for a piecemeal approach to rationality.

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Interlude

The sharp negative reaction to Popper's philosophy of science has regularly claimed in dramatic terms that Popper's fallibilism is a danger to science: philosophers or scientists cannot accept his fallibilism and still properly defend science. The following essay shows why such claims are quite mistaken.

1c. 'Does Fallibilism Underestimate and Endanger Science?' *Ratio*, June 2007, pp. 219-235.

Does Fallibilism Underestimate and Endanger Science?

Abstract: All fallibilist theories may appear to be defective because they allegedly underestimate the security of at least some scientific knowledge and thereby leave science less defensible than it otherwise might be. When they call all scientific knowledge conjectural, they may seem at first blush to underestimate the superiority of science vis a vis pseudo-science. Fallibilists apparently fail to account for the fact that science turns theory into facts, because even "facts" are held only provisionally. This impression is false: the relatively secure establishment of facts can be accounted for with a fallibilist view. After theories have been honed through sharp criticism, there is often no reason to doubt some aspects of them. These aspects are what regard to be factual knowledge, even though these facts are also provisionally accepted as such. We then explain the newly won factual knowledge with deeper theories, which often correct our factual knowledge in spite of its apparent security. Theories of justification add nothing useful to the fallibilists' observation that science finds the best theories, because it has the highest standards of criticism. Fallibilist theories today give the best account and defence of science. We may abandon the quest for some kind of assurance that goes beyond the determination that some theory has can answer all known objections to it and take up more interesting problems, such as how we can find new objections and how criticism may be improved and made institutionally secure.

I

Many thinkers are impressed by Popper's criticisms of induction: induction seems of no use as a method of discovery and offers no plausible theory of how to justify theories. But, even among these thinkers, there are those who find that Popper's conclusion, that is, that all science is conjectural, so implausible, that there must be a mistake in his approach. Discoveries of the orbits of the planets or the circulation of the blood are surely more than mere conjectures. Thinkers sceptical of Popper's central thesis may not be able to say exactly where the mistake in Popper's theory lies, but, quite intuitively, there seems to be one. The search for something better, something that shows just how truths which no one doubts are established, thus goes on.

A central reason for this intuition that a better explanation of how truths are established must be possible is that much of science is obviously not so insecure as fallibilism sometimes allows it to appear. To say that all of science is (mere) conjectures, even conjectures that are held open to criticism, makes it look as if we are seriously underestimating the ability of science not only to find knowledge but also to render it secure. It cannot be the case, for example, that scientific conjectures are no better than (refutable) astrological ones or that the description of the circulation of the blood in animals is a mere conjecture which has not been established. No scientist can take seriously some astrological theory merely because it has been put in a refutable form, and no one expects that Harvey's theory will be overthrown.

The task of finding an explanation of the security of at least some of scientific knowledge is not merely a matter of solving some philosophical problem which has no practical consequence. It is also a matter of showing that and how scientific knowledge is vastly better than the views of charlatans of this or that sort. No doubt should be raised that science contains established truths which serious and responsible persons do not call into question. Doing so may open the door to claims that non-scientific approaches are just as good.

As he mounted his attack on induction Popper was aware that he had to offer an alternative to the inductivist view of the superiority of scientific knowledge over non-scientific opinion. He stressed, therefore, the importance of the problem of demarcation and offered a new solution to it. He sought to meet the charge that, if no solution to the problem of induction as it has been traditionally formulated is found, no defence of scientific knowledge is possible, as Bertrand Russell thought. When science is done right, Popper said, new conjectures are refutable. Every new effort thus lays the groundwork for still further progress which ensues by refutations of the newest suggestions. But many, following Russell, find this solution not good enough, even when it is acknowledged that it does distinguish much of science from much of that which is not science. The fact that science is critical might explain how it has discovered exciting new theories, but it does not explain how it has rendered some knowledge secure. How has science been successful not only in finding mistakes in theory after theory and in discovering wonderful theories with very high levels of explanatory power, but also in rendering some remarkable conjectures secure? Harvey's theory of the circulation of the blood is only one example: his theory was a bold conjecture when he put it forth but today no reasonable person calls it into question.

Popper's explanation of how science has gained new knowledge, that is, by making daring but refutable conjectures, seems to fail to demarcate sharply and powerfully enough between the success science has had and the emptiness of non-scientific competitors, be they "creation science", astrology, or whatever. The two intuitions that (1) fallibilism does not adequately explain how science succeeds in establishing its theories and that (2) fallibilism does not offer a powerful enough defence of science are closely related, since it would seem that a better answer to the objection that Popper underestimates the security of scientific knowledge would lead to a better and more powerful defence of science.

Let me mention an example to explain the first criticism of fallibilism according to which there is a fact, that is, the security of some scientific knowledge, which neither Popper nor his followers have adequately explained. (I return to the second complaint according to which fallibilists cannot demarcate dramatically enough between scientific knowledge and the fantasies of charlatans, below). When William Harvey studied the flow of blood in the 17th century, the conjecture that the same blood which was pumped out of the heart was re-circulated in the body and returned to the heart was a wild one. It was a wild conjecture because it had been refuted by the standards of the day. Scientists had sought to find the connecting links between those vessels which carried blood away from the heart and those which brought blood to the heart. They could find no such connections. The theory that such connections were too small to be identified with the means then at hand seemed ad hoc and preposterous. Nevertheless, this hypothesis turned out to be true. What was then a bold conjecture is now quite properly regarded as a simple fact. It is hardly "conjectural" in any meaningful sense of the word. The same story may be told about DNA and innumerable other episodes in the history of science from its beginning until today.

Can Popper's criticism of induction and his view that no theories can be justified, that all science is conjectural, on the one hand, be reconciled with, on the other hand, the fact that we find in the history of science innumerable examples of the growth of knowledge which fit this model? Even if we take, say, the examples of Kepler and Galileo, which are also examples of theories being refuted, we come up against such intuitions. Kepler said that the orbits of the planets were perfect ellipses. They are not, as we now know after Newton's theory enabled physicists to calculate them more exactly: they are rather egg-shaped. But even though Kepler's Pythagorean metaphysic which led him to the hypothesis that they were perfect ellipses is false, and even though he did not describe the orbits of the planets quite correctly, he came so close that it seems wrong to say that his theories are merely refutable conjectures. If we take Galileo's law of falling bodies, we may point out that this statement is false, because it does not take into account the fact that the attraction of the earth also plays a role in determining the speed of a falling body or that the attraction of one body to another varies with the distance—a defect which left open the objection, which was quickly raised, that Galileo could not explain what would happen when a body was half-way between the moon and the earth. But one can also point out that one merely needs to add a neglected factor to Galileo's equation in order to make it right. The fact, that with this correction we leave the rest of the equation intact, shows how stable the growth of knowledge in so many cases have in fact been.

We can partially explain such success with the conjecture that we have been successful in explaining one kind of event, say the orbits of the planets, even if true laws of nature which explain this kind of event as well as other kinds of events as instances of the same underlying processes, should turn out to be wildly different from the ones we now have, say as Newton's theory turned out to be wildly different from Kepler's and Galileo's while explaining and correcting both. We have already seen a number of such revolutionary changes. But this observation merely points to the possibility that, when the success of a relatively narrow explanation is explained in some deeper, broader way, the factual descriptions may not be radically changed. Kepler explained why the planets moved in perfect ellipses with the theory that God constructed the world in accord with ideal mathematical figures and Galileo succeeded in providing a good mathematical description of how bodies fall. Newton explained the nearly elliptical orbit of the planets and the speed of falling bodies in a radically different way with his theory of the attraction of bodies, but he changed only slightly both Kepler's and Galileo's descriptions of the facts. Fallibilism emphasizes that today's scientific explanations may be overthrown by theoretical revolutions. Yet it seems that the possibility that newer and deeper theories, even when they are revolutionary, will dramatically change narrower, established theories, cannot be taken seriously for wide swaths of scientific descriptions of the world.

II

We can make this same point, that is, that Popper's theory does not account for the security of factual knowledge which science produces, because it fails to distinguish between the security of the latest adventurous theoretical conjecture and a theory such as Harvey's, in a slightly different way by turning to William Whewell. As Whewell studied the growth of scientific knowledge, it was the fact that conjectures were turned into fact which so impressed him and which he hoped to explain without appealing to induction, which he, like Popper, thought could neither explain how knowledge was discovered nor how it was justified. Contrary to Popper he believed that scientific theories are in fact justified, but not in the way the dominant Baconian tradition said they are. Whewell proposed that scientists make conjectures and then improve them by empirical research. Some are discarded, but the true ones are steadily refined until they describe all the relevant facts clearly and simply. The educated mind—the person who has understood the tests which a theory has been subjected to, the theory's explanatory power, and how criticisms of it have been met—sees that no other hypothesis is possible. The theory is thereby proven to be necessary and, when seen by the educated mind to be necessary, is, of course, justified.

The theory that the educated intuition of scientists can see that theories are necessary was rejected immediately by all of Whewell's contemporaries and cannot be refurbished today: scientists have often claimed that some theory is true and have then—admirably—changed their minds under the pressure of new tests, new discoveries, and/or new theoretical criticisms. Criticisms of even the most successful theory—Newton's—have been taken seriously. Leibniz's complaint that Newton's theory of gravity appealed to an occult cause was not taken as a cause to reject Newton's view, because Newton's theory had so much more predictive and explanatory power than any competitor. But it was not simply dismissed either. It later played an important role in the work of Boscovich and of Faraday. And the work of Faraday led, of course, to Einstein's revolution.

Whewell described the development of science as the turning of theory into fact, or, alternatively as the idealization of the world. These two statements, he emphasized, make the same point. For, when ideas become facts, the world becomes ideas. But Whewell also said, we cannot do this completely, because then we would not have knowledge of an external world. So, there is always a remainder which is based on observation, and which is not quite secure. He did not overcome the problem which then arises: how can theories be proven by the fact that there is no conceivable alternative, when we always presume that no complete reduction of observation to theory is possible?

III

Now both the common-sense observation that it is absurd to doubt that some scientific knowledge is quite secure and Whewell's accurate description of how theories are changed into fact in the history of science run counter to Popper's thesis that all scientific knowledge is conjectural. If Popper's claim, on the one hand, and this observation and this description, on the other hand, cannot be reconciled, there must be a problem either with Popper's criticism of induction, on the one hand, and/or with the common-sense observation and Whewell's description of the growth of scientific knowledge, on the other.

The fact that we have discovered secure knowledge does not contradict the claim that we have not justified that which we apparently know. This has been clear since Hume's successful criticism of induction and his endorsement not only of science, but of the continued use of induction to gather knowledge. Yet the fact that there is no straight contradiction does not remove the unease, because there seems to be a large remaining gap between the security of theories such as Harvey's and the fallibilist's claim that all science is (merely?) conjectural. Popper can describe how knowledge is obtained when we move from one false theory to the next. But this theory does not explain the production of secure knowledge. It also is not a very good historical description of scientific progress. Although Popper's is better than Whewell's because he explains continued theoretical revolutions, Whewell's description shows a defect in Popper's, because Whewell starts with conjectures but explains how secure results emerge from them.

We can improve the fallibilist's historical description of how science produces knowledge if we incorporate some elements of Whewell's theory into a fallibilist view. But the incorporation of Whewell's idea, that factual knowledge is the result of the refining of theories, into a fallibilist view cannot be accomplished by merely taking over Whewell's theory, which itself faces two thorny problems in explaining the history of science. Whewell noticed the first problem: ideas which become facts are subsequently changed, as in the case of the theories of Kepler and Galileo which, on Whewell's view, became facts, even though they were changed by Newton. Whewell said that truth changed. But this will not do, as his contemporaries were quick to point out. The second problem is that Whewell's theory does not account for theoretical change. On his view all true scientific theories need to become facts. But we do not want to exclude Newton's theory of space from science, although it is not now deemed factual knowledge. We need then to accommodate two apparently conflicting tendencies in the history of science. Science does often turn theories into facts, as Whewell described. And it does proceed when one scientist explains the facts which his predecessors have discovered. But it also regularly refines and even changes these "facts". Still further, science often replaces one theory with a better one, without turning all significant aspects in the former into facts.

Fortunately, Joseph Agassi has modified Popper's theory so as to take into account both the fact that science produces new facts, as Whewell had done before, and the fact that these new "facts" are modified and/or changed when theories change, a problem which Whewell faced, but could not adequately solve because it conflicted with his justificationist assumptions about science. Agassi suggests that we do not simply try to explain facts which we observe independently of any theory, as sensationalist and even modified sensationalist theories would have it. (Scientific) theories, he agrees, determines how we see facts.[101] But they also seek to explain the facts as science sees them and, when necessary, explain why science has been led to false descriptions of the facts. His prime examples come from chemistry. He takes examples of attempts to determine the molecular weights of elements. Using the erroneous theoretical assumptions of the day—he presumed that water contained hydrogen and oxygen in equal proportions—and the techniques he could devise, John Dalton found that on average the atomic weight of oxygen was 6.5. Humphrey Davy devised better methods which yielded 7.5. Agassi proposes that Dalton's result was respectable in spite of being false. Some scientific progress results from the critical study of the best factual knowledge of the day, which may be merely explained, but which may also be corrected.

Agassi explains how science can advance by producing new factual knowledge even when scientists take at the beginning of their research

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[101] This fact has led some to the view that theories are incommensurable because they portray the world so differently that no common basis in factual descriptions can be found. But this pessimistic conclusion is not needed, since a critical view of the competition between theories which describe the world differently remains possible, even when theories and facts are far more deeply intertwined than they are thought to be on sensationalist and even on many modified sensationalist views. Priestley and Lavoisier thought differently about the objects they were discussing and thus used different terms to describe them, but they nevertheless had a productive debate, as Agassi has shown in his *Towards an Historiography of Science*.

descriptions of the world, which they try to explain and which in the course of their work turn out to be false—a problem Whewell stumbled on but could not solve. Science progresses, Agassi says, not only by explaining factual descriptions of the world which previous theories have brought to light, but also by explaining how these descriptions have been right or wrong. This theory agrees with Whewell's insofar as it asserts that the latest scientific theory explains the world as seen by previous theories, but it also solves a problem which Whewell noticed but merely explained away. (Agassi did not discuss Whewell in this context). Agassi explains how scientific explanations may also correct views of world which scientists have as they seek to solve new problems.

Agassi's portrayal of the growth of science comes much closer to Whewell's portrayal of the growth of scientific knowledge than Popper's does, although it still differs from Whewell's. It comes closer to Whewell's conception because, on Whewell's view, new science does not explain the facts directly but explains the world as described by the best theories of the day, as for example, Newton explained and corrected to a degree the theories of both Kepler and Galileo in one fell swoop. In a process Whewell calls the consilience of inductions new theories incorporate old theories and unify them by explaining them. Newton was not called upon merely to explain observations of the movements of the planets. Newton sought to explain why the orbits were elliptical. When seeking to explain how bodies fall, he could explain why bodies fell in accordance with Galileo's law.

Agassi's view also differs from Whewell's theory in viewing science as a continuing dialectic. We do not turn all theories into facts. Rather we have competing theories which explain facts and we use these theories to improve our factual knowledge. Newton was able to improve the factual knowledge offered by Kepler and Galileo because he offered a new theory to explain it. This theory was turned into a fact but in the continuing dialectic of science it was replaced by a still better theory. Factual knowledge in a fallibilist theory of science, as contrasted with Whewell's view, is a residue which builds up as scientists use theories to refine factual knowledge, but it is not the whole of science, as it is on Whewell's theory.

In contrast to Agassi's approach Popper's theory still shows remnants of the positivist climate in which he developed his theory. He holds to the view that facts which are used to test scientific theories should be determined independently of them. He artificially separates, then, the testing of so-called basic statements from the testing of theories. This lends to his theory of determining which theories to trust when trust is needed a "whiff of inductivism", as he said. For, he could only say that we should trust the theory which has best withstood tests, rather than the more Whewellian version which says we trust theories when we cannot find any reason to reject them. The later version removes the whiff of inductivism while providing for provisional trust, which is all we can achieve.

We here bring Whewell's powerful, though false, description of the advance of science and a modern fallibilist theory closer together. We take over from Whewell the observation that science regularly turns theories into facts which are then explained by some deeper theory. But we also solve the problem which Whewell faced when he noticed that the "facts" which were explained, facts such as Kepler's theory of the motion of the planets and Galileo's theory of falling bodies, are not merely explained but are also corrected, and the discrepancies are explained. But it also points out that we use theories, even false theories, to improve our factual knowledge and that not all theories are turned into facts.

The misapprehension that fallibilism requires one not to regard any scientific knowledge as secure can be overcome by this combination of Agassi's modified version of Popper's theory, that is, that the theories of today should explain the successes and the failures of yesterday's, to Whewell's description of how theories are refined and become established through a tradition of criticism, described above. Popper was right to point out that even among established theories we cannot say for sure which ones will endure and which will not. But it is reasonable to presume that theories which have been subjected to a critical process and for which no one has any suggestion as to how they might be refuted are true. Under this assumption we may seek to advance knowledge by subjecting them to further criticism, if we can think of some way of doing that, and we may use them to solve practical problems.

Before we use them in any given case, we may very well subject them to tests relevant to the particular application. The assumption of truth always comes with a caveat: the assumption may be wrong. And this is important when we apply theories because it calls for the exercise of care. It is advisable to anticipate failures as best we can and design tests to see if they might occur before we trust the application of theory. We thus presume that Harvey's theory of the circulation of the blood is true, and we have no conceivable criticism of it. We also presume that the theory of DNA is true, but we may want to be careful in applying it. In the latter case at least, we may use this fact in further research or seek to explain aspects of it.

IV

Now many thinkers who note that science produces secure knowledge go one step further. They ask, "How or by what criterion can we distinguish theories which are deserving of confidence and should be established from theories which we have given some misplaced trust, and which deserve to be handled with scepticism or even rejected?" Here Popper and his followers say, "Stop! This question can have no answer which goes beyond what we have just said. When our attempts to criticize and refute some theory have failed, we let things stand. We do this because we have no idea about how to do better. But we never close the book. We never say, O.K., we have now subjected a theory to such severe tests that no new tests or criticism should ever be made". This is the step which Whewell took, and which is even problematic on his view, given his observation that no compete reduction of fact to theory is possible without explaining away knowledge. As long as we have knowledge of the external world, we still have some uncertainty, even on his view. Fallibilists may, of course, provisionally close the book. We may see no possibility of refuting various theories and work elsewhere. (Fallibilists do provisionally close the book on any attempt to solve the traditional problem of induction.) Here we may accept Whewell's observation that we often come to this position after studying the matter carefully. But this does not mean we cannot reopen the discussion, if someone comes up with some good and surprising criticism of some seemingly indubitable theory.

Fallibilists also often offer justifications for particular moves. But these justifications are rather different from the traditional ideal. They are not absolute. They are not offered as proof of the truth of theories. Rather, they are relative to contexts. If one presumes a specific framework, one can then say that within this framework some theory must be true. A change of framework or context may reopen the issue.

V

Whewell's observation that science turns theory into fact, and the handling of this phenomenon I have suggested here may evoke two quite different responses. The first is: Is that all Popper's theory comes down to? Then his theory is not so new after all. The second is: That is too little for the defence of science. We need to explain why science can find these facts and show in some way that goes beyond the normal critical appraisal of theories that science offers, that they are, indeed, facts. Only then, it is thought, can we explain why science is better than its competitors. Let us look at these two responses in turn.

According to the first reaction the acknowledgement that science turns conjectures into facts, even though all science is conjectural, is taking back in the fine print what was heralded as a revolutionary discovery in the headlines. We can hardly say, on the one hand, that it is a great discovery that all science is conjectural but nevertheless that science produces knowledge not only by improving daring conjectures but also by turning daring theories into facts. Although accepting the observation that science turns some theories into fact as a fact about the history of science is, indeed, a concession to some of the more startling claims about Popper's philosophy, the core of his philosophy remains: science progresses by making bold conjectures, refuting them, and replacing them with better ones. Although some of them will be replaced by quite different and new conjectures, others will not be replaced at all and some of them will be replaced with quite similar views. But just what will remain fact always retains a degree of uncertainty, and some of our most impressive theories will be replaced by theories with quite different theoretical assumptions, just as Newton's was. They are not all turned into facts, as Whewell erroneously maintained all good theories were.

Facts are not established by any method of justification or identified by any criterion of truth. They are a residue of attempts to remove error. We can never be sure that we have removed all error or that what we take as facts are, indeed, facts. We simply do our best to find error and, to borrow a phrase from Agassi, that which remains is a consolation prize for our failure to find mistakes and thus to make further progress.

VI

This brings us to the second reaction to the claim that, even though science turns some theories into facts, all science is conjectural. Many do not think that this claim is strong enough to defend science. Let us take the theory of evolution as an example. Virtually all scientists see no serious alternative to the theory of evolution as an explanation of how life has come to be and why it has taken the form it now has. Yet among the general and even educated population there are many, especially in America, who, on the basis of their religious beliefs, reject this view and defend a creationist theory of the origins and development of life. They demand that "creationist science" be taught in the school as science on a par with evolutionary theory, because the latter is "only a theory, not a fact".

It would, perhaps, be nice to have some universally and easily applied standard to show that this is nonsense. But we do not have such a thing. We can only defend science by exhibiting its explanatory power and its ability to withstand criticism, while also pointing to the lack of any alternative which can come anywhere near to meeting these standards. These are the same standards we use within science. Such arguments will not convince many creationists, who see their faith threatened by the theory of evolution. But it is a very good defence which is quite compatible with Popper's theory of science. It has the virtue of being honest. For, nothing plays more into the hands of the enemies of science and rationality, than false and/or exaggerated claims being made for science in order to show its superiority. Sceptics quite properly ask: if science is so good, why can't you defend it with open, honest, and straightforward arguments?

When we try to do better, we regularly do worse. For, we are then caught out by clever irrationalists and dogmatists, who will be quick to point out that evolutionary theory has gaps in it, that it cannot solve all problems, that some developments are unexplained, etc. Such arguments will have little impact on an honest defence which relies on a forthright portrayal of the strengths and difficulties of the alternative theories in the current stage of debate.

There can be no guarantee that science will not be rejected. There is no way of meeting the determined and clever irrationalist in fair debate: in order to lose a debate, one has to freely accept the standards of debate. And this is just what clever irrationalists do not do. They play with a "head's I win, tails you lose" policy, and think that they are smart to do so. They find the confirmation of their cleverness in the fact that they never lose. But this is no cause for panic. Science has made wonderful achievements which have always inspired the imagination of great numbers. Its success in reshaping the world, in curing diseases, etc. has been so powerful, that, as Ernest Gellner has pointed out, no culture which has had the opportunity to adopt it or reject it, rejects it.

The value of Popper's philosophy in this discussion is that it makes clear that science is not a culture of justification and dogmatism but of criticism and growth. It requires continual learning and open minds. He makes the connection between an open society and a scientific society in a superior way than it has ever been made before. His philosophy thus offers a better defence of a scientific culture, because it has a better theory of what this culture requires. It is, indeed, the only way to honestly defend such a society. For, even if one hankers after a theory of justification in science which somehow goes beyond a culture of openness, high critical standards, and active criticism, or even if one seeks guarantees for that which we already know—that some scientific results are secure—one can surely find no such theory in political and social theory. Here we surely need to abandon the hope for justification and defend an open society of criticism. Only then can we give an honest defence of a science-based culture.

VII

Popper developed his theory of science in direct competition with the positivists of his day. He began by assuming, as they did, that facts could be determined independently of any theory; that they were veridical. By the time he wrote *Logik der Forschung* he already knew better. But even as he dramatically rejected all theories of induction, he moved slowly and cautiously away from his view that basic statements were independent of theory and relatively certain. He retained this aspect of his theory as much as he could in order to have a good means for refuting scientific theories. His theory of the growth of knowledge thus retained a view of basic statements which separated them from theory, even though he saw that they were theory dependent. We accept them, he said, provisionally. But their conformity to this or that theory is irrelevant to any test of them. He remained a modified sensationalist. In order to test them one merely had to find out if they could be repeated. The progress of science involved improving them, but this was not anywhere near as important as explaining them with more powerful theories. And all theories remained conjectures.

This picture of the development of science ignores a feature of science already noted and described by Whewell. This feature is the ability of science to turn at least some theories into facts. Harvey's theory of the circulation of the blood was turned into fact, but, contrary to Whewell's view, Newton's theory was not. An adequate theory of science needs to take account of both phenomena. This theory can be formed by modifying Popper's claim that science is conjectural. We need merely note that some of the results of science are quite reasonably now deemed to be facts. They have come be deemed facts not only because they have survived severe criticisms, but also because we have run out of criticisms. We need not have a precise theory of which theories have become facts. Indeed, such a theory can do no good, because any presumed fact should be open to debate, if someone has a good criticism of the claim that it is a fact. We do not need standards to identify facts, but standards for good criticism. No standard for a fact can preclude a change of opinion as a consequence of good criticism. Good standards for criticism allow us to reopen debates which we have provisionally closed but prevent useless debates about matters which there is no good reason to question.

This strategy in regard to the results of science is also the best one for the defence of science. One can best defend science by showing that it adheres to high standards of criticism. When difficulties arise, it does not hide them. And, when weaknesses are found in any admired theory, it puts them on the agenda as tasks which science needs to solve. The strength of science lies in its intellectual integrity. Any weakening of this integrity poses a far greater danger than any admission that scientific knowledge is conjectural, when in fact it is. This is the core of what Popper argued for, especially in his criticism of conventionalism in which he rightly saw a weakening of the standards for criticism of scientific theories. A modified version of his theory adequately describes scientific progress. It does not underestimate it. And it provides for a very strong defence of science, provided science maintains its intellectual integrity. The greatest threat to the defence of science is the overestimation of what it can do, and the greatest threat to the integrity of science is the failure to maintain high standards of criticisms applied with openness and honesty. Fallibilist views are the only ones which both clearly identify this need and give it the central place it deserves.

All that said, there is a problem which traditional theories of science have tried to solve which remains open. This is the problem of explaining why we are successful. The question of how science has been so successful in establishing knowledge has not been sharply separated from the question why science has been so successful, largely because the two problems are identical on justificationist but not on fallibilist views. Even if we explain how science has succeeded in establishing knowledge, we still have not rendered the miracle of scientific success intelligible. Popper thought no theory of knowledge should answer this question and that the fact that we can understand the world is improbable. (*Objective Knowledge*, 23) And I will not try to answer it here. Traditional inductivist theories have a program for answering it, that is, we can explain why science has been successful by showing how science deduces theories from facts. But this program fails. Others try evolutionary epistemology. I do not think this works either, but no will not go into this issue here. Rather, following Popper, I have separated the question of how science has produced secure knowledge from the question of why this has been possible in order to answer the first question independently of any answer to the second.

We have described how we are successful, but not why this procedure works. There is no necessity that ideas of men correspond to the world. There is no theological explanation of some preordained correspondence between the thoughts of men and the world which scientists, qua scientists, can accept. (Whewell could have accepted such an explanation, but he did not think he needed it, because he had proven, by his own lights, that scientific ideas were true.) And even though we may learn from evolutionary theory how to explain some of our current success, since men could not have developed as they have if their views of the world had nothing to do with the way the world is, we still have no explanation of the success of science. Science is not necessary for survival; perhaps it is a danger to it. Perhaps there is no good explanation for our success. Perhaps someone will offer one soon. Some find hope in evolutionary epistemology. At any rate neither fallibilist views nor any other view today can explain this success in any way which will stand up to serious criticism. But this is not peculiar to fallibilism.

But this lack of an explanation of why we are successful does not mean that Popper's theory of the growth of knowledge is defective, that it, say, has to be supplemented by a theory of justification of scientific theories. A theory of the justification of scientific theories would, indeed, solve this problem. It would not only explain how science makes progress but would also explain why this is progress. So, a theory of justification can still be thought of as filling a gap in Popper's account of science, just as Whewell's critics thought that his description of the history of science had to be supplemented with a new theory of justification, since Whewell's was not good enough: one could not rely on educated intuition as a source of proof, as Whewell claimed. But a return to this old program hardly looks progressive. Unless one can find a good reason to doubt the criticism of induction, there seems little reason to continue down a path which has been taken for hundreds of years without any success. Before Einstein there was a transcendental argument to justify such a search: we do not know how we know the truth, but we do know that we have found it, because we have virtual certainty that Newton's theory is true. And we know that, if Newton's theory is justified, it is empirically justified. Hence, we must be capable of using induction to find the truth.

Today we may take it as a fact that induction does not work. This does not prohibit anyone from trying to show that there is something wrong with the best critical appraisals of induction today. But this requires that those critical appraisals of induction today which lead to the supposition that it is a fact that induction does not work be shown to be in error. This work seems quixotic, but it will no doubt continue, because there is a gap in our best account of science today which it would fill: we have no good explanation of why science is successful. But this continued work on inductive philosophy is no reason not to deem a fallibilist account of science the best we have today, even for those working on it. And there is good reason to accept the facts that induction does not work and that related, watered down means of justification cannot work. Today we may look elsewhere for progress in the philosophy of science. We may seek to improve our methods of criticism and leave aside any quest for improved methods of justification or the establishment of secure knowledge.

Interlude

Conservative philosophy of science has mistakenly led to the sharp separation of philosophical and historical studies of science. The so-called philosophical studies are reduced to studies of the logic of science. But there should also be methodological studies of how science is conducted. A fallibilist alternative can overcome this debilitating separation.

1d. 'The Philosophy of Science and the History of Science; Separate Aspects vs. Separate Domains', *Philosophical Forum*, XIV, 1 (l982): 59-79.

The Philosophy of Science and the History of Science; Separate Aspects vs. Separate Domains

Introduction

This essay does not concern itself to make empirical studies of research in science. Rather it proposes to analyze problems which arise in the standard research program. Two significant difficulties arise when, according to the standard view, the philosophy of science and the history of science are treated as separate domains. These difficulties arise due to contemporary background knowledge that knowledge is uncertain and methodological theories adopted by scientists change. It further suggests that a new view of rationality—a view of rationality as itself improvable can resolve these problems. It cannot show that a sharp separation of philosophy and history of science could never be successful. It only shows that, given current back-ground knowledge, seemingly irresolvable problems arise and that an alternative is more hopeful, albeit untested.

Philosophers of science have traditionally attempted to keep their philosophical problems separate from historical ones and historians have traditionally attempted to keep their historical problems separate from philosophical ones. Such separation now seems inadvisable as we seem to have no good methods to resolve expected conflicts between separate prescriptive and descriptive studies. Yet on the traditional view no such conflicts are allowable. Such separation further now sacrifices either the theory that science is rationally conducted or the theory that science has a unified method. These two problems may be avoided if we seek the rational improvement of rationality with combined descriptive-prescriptive studies. This alternative has been proposed and discussed by Joseph Agassi,[102] to whom the analysis provided here is deeply indebted.

The common aim of philosophers and historians of science

One of the traditional rationales for studying the method of science is to understand how people are and ought to be rational. Since science

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[102] Joseph Agassi, "Rationality and the Tu Quogue Argument," *Inquiry*; *Towards A Rational Philosophical Anthropology* (The Hague: Martinus Nijhoff, 1977).

presumably exhibits the successful use of reason, the problem of understanding how truth can be achieved has been deemed simply a question of under-standing how science worked. The common aim of philosophers and historians was to be pursued separately. Philosophers study prescriptions though their prescriptions should also describe; historians study descriptions though their descriptions should also prescribe. Results on either side should have as corollaries the results of the other. For example, Whewell's description of the history of science is designed to show that his methodology is exemplified by the history of science.[103] Duhem does the same.[104] Historians such as I. B. Cohen, E. A. Burtt, or Koyré presume that their historical studies provide a framework for the philosophy of science.[105]

The common aim and expectation has created tension in the work of virtually all twentieth century philosophies and histories of science. Such tension occurs when the historical description fails to provide an adequate methodological theory of growth or when methodological theories of growth fail to provide an adequate historical account of science. Joseph Agassi has provided numerous examples of the former.[106] Duhem's methodology failed to account for Einstein, as did most others. Popper's failed to account for Kuhn's "Normal Science"[107] and Kuhn's failed to account for the use of criticism.[108] Tensions can be resolved by contrasting what happened with what should have happened; but that is a loss of the initial rationale.

On the traditional view, when conflict occurs each side needs to reconcile its results with the results of the other side in order to maintain the

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[103] William Whewell, *History of the Inductive Sciences from the Earliest to the Present Time*, 3rd ed. (New York: D. Appleton & Company, 1958-59).
[104] Pierre Duhem, *Le System du monde; de Platon et Copernic* (Paris: A. Hermann, 1954-59). *The Aim and Structure of Physical Theory* (Princeton: Princeton University Press, 1954).
[105] I.B. Cohen, *The Nature and Growth of the Physical Sciences* (New York: Wiley, 1954). E.A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, rev. ed. (London: Routledge and Paul, 1932). Alexander Koyre, *From the Closed World to the Infinite Universe* (New York: Harper & Row, 1959),
[106] Joseph Agassi, *Towards an Historiography of Science*, Beiheft 2 (The Hague: Mouton, 1963); facsimile reprint (Middletown, CT: Wesleyan University Press, 1967).
[107] *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970).
[108] Ibid.

integrity of its own Position. This is necessary since a failure to achieve reconciliation means that independent pursuit of description and prescription has broken down. Furthermore each side needs to insist on the precedence of its own results; if the other side is not blamed, one's own research must be im-properly conducted. The approach of each side uses the same rationale: its discipline can be conducted independently yet is compatible with the other since either side studies the single proper use of reason. If either side could achieve reconciliation only by changing its results in accord with the other side, it would be abandoning this rationale by admitting that separate studies could not find true descriptions of how reason was (properly) used. An objection to this analysis comes from the fact that many philosophers do history and historians such as Koyré seem to be philosophers. This is prima facie evidence that no such split of the type I describe exists. This is misleading, however. The cases where the division seems blurred tend to combine results of the two disciplines but not the disciplines. When a historian makes a philosophical innovation or vice versa, there is no method of resolution except to put the suggestion on one side of the divide or the other.

On the failure to reconcile descriptions and prescriptions

We may divide the Problems contemporary philosophers of science have attempted to solve into two types. On the one hand are Problems of how knowledge may be obtained or justified. On the other hand are problems of how to properly describe actual procedures used by scientists. As I have already said, it has been generally believed that a successful solution to the problem of how knowledge may be properly obtained must be in conformity with a description of the methods actually used in science. Therefore, even though the standards for a successful history differ from those for a successful philosophy of science, both endeavors should result in the same explanation of the growth of knowledge.

This was not important in discussions of the methodology of science until rather recently. The overwhelming majority of philosophers of science accepted the theory that the history of science was the history of the accumulation of true theories. We knew what these theories were, and we knew that they were justified, i.e., shown to be true. The only remaining problem was to understand more clearly how such discovery and justification were achieved. The growth of knowledge was not problematic: accumulation of justified theories is evidently growth. This theory has collapsed. The cause of the collapse was Einstein. If the theory that science progressed by the accumulation of true theories was true, either

Einstein was not doing science or this theory of method was refuted. Einstein's victory over Newton meant that Newton's theory was presumed by scientists to be refuted. This is true because Newton's theory contradicts Einstein's on various points, e.g., on the existence of a greatest speed or on the perihelion of Mercury. If Einstein's theory is true, Newton's must be false. Any refutation of Newton's theory was also a refutation of the accepted theory of justification, namely, the inductivist theory of the history of science, since it showed that the best scientific theories were not properly justified.

This result means that philosophers of science must redo the job of explaining how science works—they must, that is, if they wish to maintain that there is a single method used in the history of science. How can this be done? One test is now the test of the history of science. This is so because events in the history of science refuted the prevailing view. A new and better view must account for these events in a better way. Thus philosophers now have to show that their theories of method correctly describe the methods used throughout the history of science. The presumption that competent methodological studies would necessarily provide correct descriptions became a test of competence of methodological theories. In recent discussion this has led to a new emphasis on the use of the history of science as source material for testing the adequacy of theories of science or theories of method. Such philosophers as Duhem,[109] Meyerson,[110] Popper,[111] Polanyi,[112] Kuhn,[113] Agassi,[114] and Lakatos[115] have appealed to the history of science in their attempts to show either that their theories were acceptable or at least that others were not. (These philosophers use the test as a test of truth and not competence, but as long as the presumption of viability of separate domains is present it must have competence as a corollary. The meta-theory must still be true and justified, even though scientific theories change. It is no accident that conflicts between methodology and history generate so much heat.

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[109] Pierre Duhem, *Le System*.
[110] Emile Meyerson, *Identität et Realität'*, 4th ed. (Paris: F. Alean, 1932).
[111] Karl Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959).
[112] Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (Chicago: University of Chicago Press, 1958).
[113] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1958).
[114] Joseph Agassi, Towards An Historiography.
[115] Imre Lakatos, "Proofs and Refutations," *The British Journal for the Philosophy of Science*, 14 (1963-64), 1-25, 120-39, 221-43, 296-342.

Attempts to test theories of method, though at times successful in showing inadequacies in various theories of science, have proven largely ineffective in providing the clear-cut tests or even proofs that have been sought. The ineffectiveness has led in turn to a failure to achieve the means of adjudicating between alternative histories of science. The prima fade evidence for this is that, in spite of tests, theories of method which are at wide variance with each other seem to survive.

The ineffectiveness of tests can be partially explained as follows. On the one hand some historical event we use to test a methodological theory must be capable of refuting at least some plausible methodological theories in order to be interesting. Yet it must also not be so easily applied as to refute any methodology, even the true one, by providing a counterexample from bad science or activities peripheral to science. We must be capable on the one hand of determining whether or not a methodology can explain events, i.e., research procedures and their success, or all events, in the history of science. On the other hand, we must set up our tests in a fair way so that methodologies will not be forced to conform with arbitrarily chosen events in the history of thought.

We can now see that the prima fade ineffectiveness of historical tests of methodological theories can be partially explained by the need to specify what constitutes science and the lack of any independent means to do so. Since we specify what constitutes science with our theory of science, the test of this theory against the practice it selects would easily vindicate the theory tested. When this is done, not only is the presumption of complementarity of methodology and history untestable, but any particular claim for success is untestable as well. The particular claim is untestable because the demand that a theory correctly explain the history of science is a limited one: a theory must only correctly describe the method or methods which have led to progress. Difficult cases may therefore be easily explained away. Since the test is implicitly a test of the competency of the methodologist, such methods are of course used. This completes the explanation of ineffective-ness. The maintenance of the standard view of separate domains thus has a high cost since it makes each side impervious to criticism from the other; errors cannot be uncovered and progress is blocked.

Let me discuss Popper's response[116] to this Problem in order to explain difficulties. Popper's proposal for how we can provide theories of science

[116] Karl Popper, *The Logic of Scientific Discovery*, pp. 49ff.

which do not define what they purport to explain and thus enable us to make historical tests effective is the following: instead of seeking a true description of the scientific method we should propose aims and methodological rules for pursuing these aims. The aim that Popper proposes is the continued growth of knowledge. The rules are designed to attain this end by giving us our best chance of rapidly getting closer to the truth. We can evaluate proposed rules by evaluating whether or not they are useful, fruitful, or needed to produce the growth of knowledge. Popper's proposal thus far appears to provide a technique for evaluating prescriptions. But it is already confused. The term "growth of knowledge" is used ambiguously to mean both growth of knowledge and history of science. The procedure is thus a technique for simultaneously explaining what should be (the growth of knowledge) and what actually happened (the history of science). But things get worse. Popper not only wants his methodology to be a true account of the methods actually used by scientists, but he also wants to explain how such an account is generated. So he develops a further explanation of what he is doing. He does this by adding an analytic feature to his research program. His view of the philosophy of science includes the idea that he is observing a game being played. His job is to understand the rules which cannot be told to him. He sees (intuitively) that the most characteristic part of the game is the way changes are made. So his primary job is to understand the rules which regulate change. lt is not clear but perhaps he may also suggest some new and more explicit conventions for how the game is to be played, but if so they should modify and refine the game and not change it.

In attempting to save a rather standard view of the philosophy of science, Popper wishes to maintain the distinction between prescription and description, as well as the independence of each from the other. In order to do so he needs independent criteria for history and philosophy. The use of these criteria must produce the same theory of the proper use of reason stated descriptively as history and prescriptively as philosophy. Nevertheless, Popper's theory faces a great difficulty brought on by Kuhn's claim that in fact most science is Normal Science. Kuhn's simple point hits Popper hard because it throws doubt not only on his theory but his procedure of doing philosophy of science.

Popper and Kuhn start with contradictory theories. On the one hand, Popper claims that science proceeds by critical discussion and by refutation of theories. He recommends critical attack on all theories so that they may be refuted and replaced by better ones. On the other hand, Kuhn claims that science proceeds by commitment to a paradigm which is modified in small ways to increase its ability to make true predictions; science only rarely changes its commitments. He thus recommends commitment to a paradigm combined with modifications.

Each philosopher believes that his theory can account for the growth of knowledge (Popper by the replacement of old refuted theories by new highly falsifiable ones; Kuhn by the increase of the true predictive power of paradigms through modification). Each philosopher points to examples in the history of science of the use of the procedures which he claims constitute the history of science. Popper points to critical discussions and revolutions, e.g., Lavoisier and Einstein. Kuhn points to long periods of modification which increase our ability to make true predictions, e.g., eighteenth century Newtonian physics and much of contemporary physics. So each view is prima facie a successful theory of science; each explains the progress and development of science by methods actually used in the history of science.[117]

But each philosopher has a problem: What should he do about the discovery of methods used throughout the history of science which are, according to their own theories, not characteristic of science and the growth of knowledge? Popper and Kuhn each maintain the thesis that science is characterized in the way they originally maintained but they modify their theories to include some uncharacteristic or less important activities. "On the one hand, Popper admits that he has taken too little account of what Kuhn calls "Normal Science." He admits that it occurs but contends that it is of less importance than Kuhn claims. And he continues to argue that Kuhn, even in dealing with these "normal" periods, has underestimated the importance of argument. On the other hand, Kuhn claims that he has not rejected the importance of arguments and critical discussion but that Popper fails to recognize the valuable role of "Normal Science" in the growth of knowledge.

Yet each theorist claims to be offering a significantly different theory not only of progress—which they no doubt do—but also of scientific practice. One claims that we can properly describe the history of science as a sequence of commitments to paradigms. The other claims that we can describe the history of science as a long and continuous critical discussion. Two descriptions of the same human activity, as widely different as these, would prima facie seem to require some conflict, at least some crucial tests. But when the discussion is completed, no clear difference in their

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[117] Imre Lakatos and Alan Musgrave, *Criticism and the Growth of Knowledge.*

descriptions of scientific practice remains. When crucial tests are found, one or the other of the Parties uses one of the commonly employed techniques to explain away the alleged counterexamples. So we lose the ability to apply reasonable standards of historical explanation to theories which purport to provide such explanation.

Popper or Kuhn might reply to this criticism on the grounds that I have only shown that standards for a reasonable historical explanation of science appear to be abandoned only because I have set these standards unreason-ably and improperly high in the first place. They might contend that theories of science which purport to describe the history of science are indeed testable but not in the way indicated, i.e., by finding single refuting cases. On this view one may admit uncharacteristic events in the history of science but still require of a theory that the characteristic events are sufficient to explain the growth of scientific knowledge. If a theory of science could be refuted by showing that success could no longer be explained by the characteristic events, it would still be testable.

If we attempt to make this sort of test strong enough to be interesting, we need to propose some criterion for the sufficiency of the corroborating events to produce the growth of knowledge. If we attempt to decide if the number of events successfully interpreted by some theory is sufficient for the growth of knowledge, we would need to know whether events in the abstracted history could produce the (same?) results produced by the real history of science. There could only be thought experiments of such a proposition, and the success or failure would depend for the most part on methodological arguments. Thus any methodology which could select its own version of the history of science would be nearly invulnerable to such a test. I conclude that the attempt to test theories of scientific method in the history of science either leads to quick refutations or methods which cannot identify refutations.

The rational method and the rational conduct of science

The presumption of separate domains of the history and philosophy of science has an even more serious consequence. In order to maintain a theory of the rational method of science one is forced to abandon theories of the rational conduct of science; since no single theory could have guided the research of even most scientists, and different theories lead to different methods, scientists could not have acted in accord with their theories, i.e., their conduct could not be rational, if they have all used the same method. Paradoxically, the rationale for constructing a theory of the rational method of science is that science has been, in fact, rationally conducted. Let me explain. If, on the one hand, we explain the history of science by the rational conduct of individual scientists, we would most likely be interested in the beliefs, the methodological theories, or the arguments which scientists thought to be important. If, on the other hand, we want to show how the use of a single method leads to the growth of knowledge, we would be interested in finding in the work of scientists the methods which we assert are useful or needed for the growth of knowledge. But the methods which we cite as progressive cannot in fact be the methods cited by even the majority of the scientists themselves, since scientists disagree. Of course, it may be the case that some of the methods which are cited by methodologists as producing the growth of knowledge are the same as those methods which a scientist may have thought were important. But the existence of conflicting interpretations of science by scientists indicates that no theory of a single method can also concur with the views that even most scientists employed when they pursued their research. The well-known existence of conflicting methodological theories held by scientists, e.g., Galileo,[118] Newton,[119] Lavoisier,[120] Faraday,[121] Heisenberg,[122] and Einstein,[123] indicates the impossibility of finding a theory of method which concurs with a theory of how science has been (rationally) conducted if rational conduct is in some sense to act in accord with theories. This fact is further illustrated by the continued disagreement among scientists concerning the proper methods of science. Individual examples have been worked out. For example, Burtt shows how Kepler's beliefs in God and in perfect figures were important methodological principles.[124] Yet such religious or mystical views would

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[118] Galileo Galilei, *Dialogue Concerning the Two Chief World Systems*, 2nd ed. (Berkeley: University of California Press, 1967). Stillman Drake, *Galileo Studies: Personality, Tradition, and Revolution* (Ann Arbor: University of Michigan Press, 1970). Alexandre Koyre, *Etudes Galileennes* (Paris: Hermann, 1966).
[119] Sir Isaac Newton, *The Mathematical Principles of Natura! Philosophy* (London: Dawson, 1968). *Opticks* (New York: Dover Publications, 1952). I.B. Cohen, Franklin and Newton (Philadelphia: American Philosophical Society, 1956).
[120] Antoine Lavoisier, *Elements and Chemistry* (New York: Dover Publications, 1965).
[121] Michael Faraday, *Scientific Papers* (New York: Collier, 1910). Joseph Agassi, *Faraday as a Natural Philosopher* (Chicago: University of Chicago Press, 1971).
[122] Werner Heisenberg, Physics and Philosophy: The Revolution in Modern Science (New York: Harper and Row, 1958).
[123] Albert Einstein, *The World as I See lt*, abridged ed. (New York: Philosophical Library, 1949).
[124] E.A. Burtt, *The Metaphysical Foundations*.

not be considered to be Part of any current theory of scientific method, even though in a broader sense metaphysics would be. Agassi discusses the curiosity of Popper's claim that Newton (unknowingly) followed Popper's methodology.[125]

The existence of this variety of theories of science held by scientists would not be significant if we could successfully argue: first, that all these theories have something in common which accounts for the rational conduct of science; second, that scientists failed to use their articulated theories of science when doing science but used some other theory they may have in common; or third, a combination of these two moves.

The first alternative, i.e., scientists have used differing theories in the conduct of their research, but these theories all have something in common, leads to the Problems already discussed in the first section of this paper. The methodological problems of finding a common element among theories of science would be the same as those which occur when we try to find a common element among methods actually used in the history of science. Any theory of a common element which was reasonably clear-cut would be easily refuted; if we modify our theory to take care of difficulties the claim will lose empirical content; there will be no means of deciding empirically between widely different alternatives. The loss of empirical content would be a loss of ability to explain rational conduct, since without such tests explanations are simply a priori. This argument is of course quite a priori. Perhaps it can be refuted. Yet the pursuit of such a refutation is more than merely problematical.

The second alternative is to forget theories of science altogether and simply look at procedures on the grounds that one cannot learn what scientists do by listening to the descriptions of what they do. This approach has some plausibility, since it is clear that in any activity the people conducting it may not be adept at explaining what they do. But it also has a grave weakness. First, it seems implausible to completely disregard a scientist's view of what he is doing; it is likely to have some effect on the direction of his research. Secondly, if we attempt to specify the common approach, we can expect empirical tests to collapse for reasons already given. When empirical tests collapse, the theories of the rational conduct of science seem needlessly weak since such theories are

[125] Joseph Agassi, "Scientists as Sleepwalkers," in Y. Elkana, ed., *The lnteraction Between Science and Philosophy*, Proceedings of the Sambrusky Symposium, the Val Leer Jerusalem Foundation (New York: Humanities, 1975), pp. 391-405.

historical, anthropological, or sociological, and we have empirical theories of this type.

The inadequacy of inarticulate rationality

Polanyi has a way of avoiding the Problem of the inadequacy of the methodological theories of scientists to account for the (unified) practice of science. On Polanyi's view, the rational conduct of science need not depend on a theory of method; some methods are known tacitly, i.e., they are known in practice but are not articulated.[126] Methodological theories cannot adequately describe the method of science. The question of the ability of methodological theories to adequately account for science is thus avoided. The theory of the unity of method can never be overturned by conflicting descriptions of method or by alleged assertions of the application of conflicting theories since these methods are never fully described. The descriptions of method are epiphenomena of the method of rationality of science. This is so because the methods of science are transmitted by example and are known only in practice. They are known personally and inarticulately by practicing scientists.

This alternative avoids the need for an internal critical evaluation of science or of our theories of how to do science. The only serious Problems are the relationship of science and society. lt is reassuring in that it allows one to believe that we are progressing in science without any serious Problems for the direction of research, the evaluation of theories, or the possibility of future progress—as long as we do not interfere. lt is conservative in outlook by viewing rationality as limited by our commitments yet endorsing our mental habits by the claim that they cannot adequately be grasped and hence must simply be practiced.

The reason this view is unacceptable is that there is no reason why, even if our knowledge of methods is only partial, this knowledge should not be important for, or even characterize, the rational conduct of science. lt may be that knowing how to do science requires practice and acquaintance with experts. lt may further be the case that the best description of scientific methods is always incomplete and distorting. But it does not follow from this that the process of describing and engaging in critical discussion cannot be used to understand and improve methods. lt does not follow from the fact that communication often depends on tacit understanding that the border of articulate views cannot be pushed back and improved.

[126] Michael Polanyi, *Personal Knowledge.*

The inability to achieve complete understanding through articulation cannot be reasonably used as an argument against pursuing increased understanding through articulation or as an argument against the view that the use of partial theories characterizes the rational conduct of science. On Polanyi's view scientists are uncritical about the methods of science; yet even so, rational conduct depends in large measure on articulated theories and discussion of them. Polanyi's view thus fails to avoid the problem of reconciling theories of method and rational conduct since it requires but cannot sustain the theory that articulated views and criticism of them is entirely irrelevant to rational conduct.

The need for improvement of rationality

Recent discussions of rationality such as those by Polanyi, Popper, and Bartley have centered on the question: is it better (more rational or more moral) to be rationalist or irrationalist? This question has, both historically and recently, only made sense if rationality is limited. If rationality can pro-vide truth, peace, and physical well-being, there is hardly any question of its value. The question is thus an outgrowth of recent attempts to rethink traditional philosophy of science. The question presumes that we know the two choices open to us. We may choose some limited rationality or we may choose irrationality.

This is a modern version of the traditional debate between skepticism and reason. Whereas the traditional debates asked whether rationality was even possible, the new question asked whether a limited version is better than none. In the traditional debate, however, all Parties agreed on what rationality was: it meant the discovery of truth. In the contemporary discussion we are not so clear; the limits of reason must be described before we can be clear about what the choice amounts to.

The choice is further complicated because no choice of complete irrationality is possible. In the first instance, our problem appears to be between partial rationality and irrationality. But this dichotomy vanishes once we observe that all individuals are to some degree rational and no individual is completely rational. Our problem thus is not a problem of choice between partial rationality or irrationality, but a problem of the investigation of the limits of reason and policies for implementing rational policies to one degree or another. Furthermore, we cannot pose a problem of choice between competing theories of partial rationality as a final choice because we have no common method of rationality to use in the dispute; each theorist would have his own methodology and complementary history. The choice would then be an all or nothing affair. The interpretation of the problem situation as a choice between competing views of rationality thus leads to relativism. The choice to be rational is interpreted as a choice of a particular brand of rationality, or, there is only one variety and all other contenders are irrational. So, competing rationalities do not exist. Paradoxically, either we allow for alternative rational views of rationality or we lapse into relativism. Denial of alternative rational views of rationality leads to relativism because each person's view of rationality must be the sole and complete judge in its own case.

The only viable alternative that we have is to view the maintenance of rationality as requiring the improvement of rationality. This is so since the uncritical maintenance of any view of rationality must be irrational; rational integrity requires that rationality always be taken as a Problem.

Let me go over this material again. W. W. Bartley III provides a view of the problems of rationality which both goes beyond the traditional views and captures the problem situation nicely.[127]

On Bartley's interpretation, the traditional view of the problem of rationality—the most widely held ideal from Plato to the present—is that to be rational is to accept all justified statements and only justified statements. This has not been thought to be problematical. What was problematical was whether it was possible to behave in such a way, and if so, how. So the major problems of philosophy were how to refute skepticism, i.e., how to show that rationality was possible, and how to justify theories. All theories of science said either that science did justify theories or that science did not and did not give us knowledge (as distinct from true predictions). Bartley saw that this view of the problem needed to be revised if we adopted Popper's theory of science. Popper offered a theory of science which held that science did in fact provide us with knowledge but did not justify theories. This theory of knowledge was incompatible with the standard views of rationality, all of which held that if one were rational one accepted all justified theories and only justified theories. In order to develop an alternative, Bartley proposed that there are three central problems of philosophy: the problem of knowledge (how do we gain knowledge?), the problem of rationality (how do we think rationally?), and the problem of reconciling knowledge and rationality, or

[127] William Warren Bartley, *The Retreat to Commitment* (New York: Knopf, 1962).

as I would put it, of reconciling our theories of how we gain knowledge with our broader theories of how we can think rationally.

Popper's program, which I presume Bartley endorses, is as follows. There is a unified scientific method which needs to be described and developed by a theory of knowledge. The theory of unified method needs to be both descriptive and prescriptive. lt needs to be a correct description of a proper method which should be used to guide future research. lt should be used to guide future research because the appropriate scientific method is already in use. The problem of discovering this method is to describe it and explain how it leads to success in producing knowledge. This is the Popperian program criticized in Sections I and II.

If this program is accepted, we are led to certain views about the second problem—the problem of rationality. The problem of rationality which flows from this view is a Problem of how we can think properly and/or progressively regardless of the area of inquiry. This is so since rationality is broader than scientific method, but like scientific method it must be unified. Yet it must also incorporate scientific method as an instance of rationality. So the problem of rationality will be conceived as a problem of determining necessary desiderata for acceptable or correct thinking. The third problem of Bartley's view is the problem of reconciling rationality and knowledge or, as I would put it, reconciling theories of rationality and theories of knowledge. Since the theory of rationality is broader than a theory of science but must incorporate such a theory, it is a requirement of a theory of rationality that it concurs with a theory of knowledge. We need a unified view of rationality and knowledge. Bartley adopts the view that such a unified theory may be obtained by forming a theory of rationality which provides a framework for the (correct) theory of scientific method. Bartley argued that Popper had solved the problem of knowledge and that he (Bartley) could solve the remaining two. He could provide a new theory of rationality which was not only reconcilable with Popper's theory of scientific method but which would be superior to the traditional view of rationality as well; the traditional view could not meet its own standards, but Bartley's could. The traditional view held that all rational theories were justified, but it could not justify itself. Bartley held that all rational theories were held open to criticism, and Bartley's theory was itself held open to criticism. Bartley could therefore show that it at least met its own standards. I have already provided criticism of the foundation of Bartley's approach in the first two sections of this essay. This is so because the subject of criticism in each section is a cornerstone of Bartley's program. In the first section I have criticized attempts to form a theory of the unified method of science. I have argued not only that proposed theories are misguided but that the program is misguided. We have reasonable proof that any attempt to form a theory of unified method will lead to essentialism; this will lead to arbitrary standards. On Bartley's view the unified method of science provides a desideratum for theories of rationality. If the criticism given is effective, no such desiderata can be presumed. In the second section, I have argued that if we presume the unity of method, we are led to the conclusion that science was not rationally conducted, as well as to further problems of essentialism, including arbitrary standards. Such a result also forces changes in Bartley's program since he presumes that a study of the rational conduct of scientists may lead to a correct theory of rationality without need for improvement, for rationality is in fact practiced.

The criticism given in the first two sections of this paper has raised a problem with our traditional programs for understanding rational inquiry, rational conduct, and the relationship between them. This problem is the following: the traditional aim of a unified method of inquiry is no longer satisfactory because, first, the theory of a unified method leads to essentialism and, second, the traditional view that all rational conduct and all rational inquiry must be the same cannot be maintained. So our study of the problem of rationality as a unified endeavor goes by the board with the unity of scientific method.

Thus on the view proposed here, Bartley's third problem becomes the crucial one. It is crucial because only partial solutions are available to the first two problems. Furthermore, reconciliation can never be presumed to have been effected if we wish to maintain rationality. New developments in rationality and method need to be sought and new conflicts generated. The problem of reconciliation thus becomes not a problem of completing an effort but a source of criticism for further growth. All theories of rationality must be viewed as partial and unprovable. Our central problem then is the improvement of these theories through criticism of one level through the use of another.

Let me provide a broad overview of this position. Traditional views of rationality were optimistic in the highest degree. This optimism was based on an exciting dream, a dream that the use of reason would reveal the truth; when we knew the truth we could solve our problems. The truth would enable us to cure diseases, provide for physical well-being, and enable us to conduct society in a rational manner. lt is the dream portrayed in *The New Atlantis* by Francis Bacon.[128] The ground for the traditional optimism was a theory of the existence of a method and/or innate ability to recognize the truth. The use of the method would free our innate ability to recognize the truth. There was of course some disagreement about the method. Both Descartes and Bacon held that such a method existed, but they differed to some extent on the nature of it. This has led to several centuries of attempts to find the method.

In the twentieth century, this problem has become far deeper and more serious than it was in the past. Previously it was widely and reasonably thought that we knew the method for gaining the truth; as a matter of fact we employed it in the actual practice of science. The problem that we faced was to describe how in fact we discovered and/or justified true theories. Such a problem situation kept optimism alive, since the value of achieving the truth was already being obtained. We only had to improve, speed up, and preserve the process.

The new twentieth century problem is a result of the Einsteinian revolution. This revolution convinced almost everybody that there are in fact revolutions in science other than an initial one. With the benefit of hindsight, we see that this could have been seen all along. But revolutions were not so serious if sometimes revolutions achieved the truth and thus stopped further revolutions—as, for example, Lavoisier's chemical revolution is commonly interpreted. After Einstein, however, no revolution to stop all revolutions could be believed in. This simple fact meant that the traditional program for the philosophy of science collapsed. It collapsed because we could no longer hold the scientists already knew a method for achieving the truth which philosophers and historians needed only to describe. It was clear that even scientists had not overcome the fallibility of our knowledge.

The fact that scientists have not overcome human fallibility poses a serious Problem for the optimistic view of a society based on science; the reigning view from Bacon to Einstein has been firmly shaken. We have to face the possibility of error and disaster and of continued ignorance in spite of science. In order to deal with this situation, scientists have argued that science should still be interpreted as providing the best and proper example of the use of reason, even though science does not produce the truth; in some other sense it does produce the growth of knowledge. We

[128] Francis Bacon, *Essay, Advancement of Learning, New Atlantis and Other Pieces*, ed. Richard Foster Jones (New York: Odyssey, 1937).

should seek to describe what scientists do and explain why what they do leads to the growth of knowledge.

This view is regarded as optimistic because it claims, in the face of Einstein's revolution, that science can continue to progress. Speaking of Einstein's universally acknowledged success as a harbinger of disaster is ironic, but the reaction to Einstein's success is quite reasonably and seriously ambivalent. The aforementioned new view of science holds that science can still reveal some of nature's secrets and make life better, but it incorporates a radically different attitude than the programs of the past. The new attitude is that we now need to defend rationality and science as the best that is possible rather than use reason to remove irrationalism. The new attitude is a defensive posture. Prior to Einstein science could claim to deliver all the goods. Now it can only claim to deliver some. And it is pressed to argue that what it can deliver is better or more valuable than what any charlatan might serve up.

The effort to carry out this defense has been carried through with traditional methods. Philosophers of science have presumed that we must describe the actual behavior of scientists. We cannot change or criticize more than small aspects of this behavior because to do so would be to overthrow our final touchstone of rationality. If we do not defend the actual practice of science, the final bastion will be lost and anything will go. So the defense of rationality has led to a fixation that rationality cannot be improved.

But it is this assumption that the limits of rationality are fixed by science which becomes the source of pessimism and irrationalism—even in the rationalists. It does so because it becomes uncritical and authoritarian. When problems in science are uncovered, they do not become challenges for improvement, but a fate to which we must submit. (This point of view has been most clearly articulated by Ernest Gellner.)[129] So, the more we presume that science is the best we can do, the more we fall back into pessimism and irrationalism. This problem could not occur where there are no limits to reason which would block us from knowing the truth. But since we now believe that there are such limits, we need a policy to deal with them.

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[129] Ernest Gellner, *Legitimation of Belief* (London: Cambridge University Press, 1974). John R. Wettersten, "Ernest Gellner: A Wittgensteinian Rationalist," *Philosophia*, 8, No. 4 (October 1979), 741-69.

This problem is curious because it is clear that the problem posed by the limits of rationality requires a theory of rationality for its solution. This raises new and interesting problems. I do not wish to discuss the details here. But I do wish to point out that upon uncovering these Problems we must decide to pursue them or not before we can see whether we can make progress. This reveals that the pessimism discussed above is not a corollary of a true theory of rationality but is the result of a decision about whether or not to pursue solutions to certain problems. lt seems to me that there are no proofs, though there are arguments, that rationality cannot be rationally improved. Yet, there is also ground to suppose the problems are soluble. If this is true, the decision to seek greater or lesser rationality is not based so much on an appraisal of our chances for success but in our decision to attempt or not to attempt to solve problems. We may decide to attempt to meet the arguments and difficulties in rationally improving rationality or accept the constraints of current theory.

Rationality, optimism, and criticism are inextricably linked. The attempt to defend rationality by limiting the aims of such defense can never succeed unless these limits are themselves taken to be a problem. The approach taken by Popper has the paradoxical result that by seeking to find a firm and strong defense of rationality, Popper falls into a somewhat pessimistic, somewhat authoritarian, and somewhat irrationalist position. This is needless.

Recent discussions of rationality have centered on the problem of the choice of rationality or irrationality as the best way to live. The above discussion shows why this is the case. lt shows that rationality is taken to be a definitively known way to live with relative strengths and weaknesses. We have only two choices; we must decide which choice is the best. We must also decide how to decide. The above discussion also enables us to show why such a view of the situation leads to relativism. The choice to be rational is interpreted as a choice of a particular brand of rationality, or, there is only one variety and all other contenders are irrational. So, competing rationalities do not exist. Any formulation of the problem as a com-petition between alternative (rational) views of rationality is blocked. We cannot do this because we have no common method of rationality to use in the dispute. It is an all or nothing affair. If we seek a common method, i.e., if we treat the rational evaluation of rationality as a problem, we no longer have a problem of choice but one of improvement. Paradoxically, we either allow for alternative views of rationality and rational improvement of rationality, or we lapse into relativism. Again the logic of seeking the rational approach forces all disagreement into disagreement between rationalist and irrationalist. This, of course, may lead to relativism since we commonly disagree. lt leads to relativism because each person's view of rationality must be sole and complete judge in its own case.

I suggest that pessimism and irrationalism can only be overcome by attempting to rationally improve rationality. Rationality does not offer stability without adventure or truth without mistakes. Stability and truth can only be attained to the degree that we are adventurous and critical. The "stability" and "truth" attained through rising standards of rationality must always be chimerical since it must always ignore problems as a matter of policy. Rationally improving rationality may or may not lead to improvement. There is good reason to believe that, whatever we do, disaster will come in the end. (For example, nuclear war seems likely in the long run.) But we can preserve integrity, courage, and rational attempts to solve our problems. Any other course presumes without warrant that we cannot solve our problems.

Philosophers have traditionally and correctly been careful to separate prescription from description. They have done this by attempting to make the study of description one domain and the study of prescription another. For example, ethics is separated from psychology; theories of how science ought to proceed are separated from historical discussions of how it has; and theories of how societies should be run are separated from theories of how they have been run. The separation thus instituted, however, leads to new problems that rest on the need for reconciling the results of the separate inquiries. The need for such reconciliation is rooted in the fact that the Separation has been undertaken with the intent of achieving secure results securely placed on one or the other side of the divide, plus the discovery of incongruities between the sides. Indeed, the discovery of serious conflicts between the two sides should call into question the whole approach that commends separation.

Recent theories which reject certainty as a goal—mainly developed by Popper and his followers—naturally lead to the rejection of the theory that prescription and description should be studied as separate domains, since they reject out of hand the possibility of secure results on either side, let alone the security of the separation itself. Nevertheless, needed changes in the theory of rationality have blocked such a change. Here an analysis of the new problems and a new view of rationality capable of solving these Problems has been proposed. The distinction between description and pre-scription is still quite important and may be retained if prescription and description are viewed as separate aspects of the same domain. So, for example, we may study contemporary sociology as a single domain, with separate aspects of how it is and ought to be conducted. The distinction between prescription and description as separate aspects of the same domain rather than two separate domains enables us to use theories of aspects as a source of interaction and criticism between the prescriptive and descriptive attempts not available and even not permitted on traditional methods of secure separation of secure theories. lt is not available on traditional methods because separation presumes that the results of some inquiry—either prescriptive or descriptive but not both—are established in-dependently of the other. When it becomes clear that independently achieved results of one side are inconsistent with independently achieved results of the other but separation of domain is maintained, only ad hoc procedures can be used. This is so since any non-ad hoc method would re-quire breaking the barriers between domains in such a way as to make any result a product of a discussion with both descriptive and prescriptive aspects: it would require construction of unified views of how the descriptive and prescriptive aspects of one domain, e.g., nineteenth century physics, fit together or alternatively interact.

Not only will our methods be more powerful if we treat descriptive and prescriptive elements as different aspects rather than different domains; we may also be clearer in our use of the distinction. Since confusion becomes common when thinkers unsuccessfully attempt to stay on one side of the divide, a clear statement of how the divide is straddled is a better method of maintaining the distinction. lt allows for the discovery of error in either side due to contradiction by results on the other side as well as a critical and open discussion about how the two sides are related.

In order to develop and explain this broad view of how to distinguish and discuss prescriptive and descriptive aspects, I have presented a critique of the separation of prescription and description with one example: the philosophy of science as a prescriptive domain and the history of science as a descriptive domain. I have illustrated how separation of prescriptive and descriptive domains leads to ad hoc methods of reconciliation and (illicit) sacrifice of the rationale for the separation of each domain. I have done this by showing how ad hoc methods are seemingly needed to protect methodologies from historical criticism and by showing how theories of the rational conduct of science conflict with methodologies. No resolution of this conflict is seemingly possible without sacrifices of the theory that science is a paradigm of rational conduct. So the program collapses. Since the maintenance of the thesis that rationality is understood leads to the breakdown of studies of rationality, we need alternative theories of rationality and its investigation. If we seek the rational improvement of rationality, we may avoid the bad consequences just mentioned and improve rationality while more clearly maintaining a distinction between prescription and description. All this is only one instance of many attempts to separate prescription and description by domains; different analyses would be needed to show how description and prescription may be studied as aspects rather than domains in other cases. Yet the broad Problems are similar and a general argument for the need of such analyses is given here: all prescriptive theories need to be tested against descriptions since no prescriptive theory can be stated without some philosophical anthropology, i.e., a theory of man and how his actions can be explained. This is so since any prescription—in ethics, law, politics, etc.—presumes a great deal about the logic of the situation since prescriptions must have a factual context. Key factors are the rationality of the actor and the degree to which rationality is explanatory. Many descriptive theories, such as histories of science, need to be tested against prescriptive theories since prescriptive theories, like other types of theories, offer points of view which provide differing interpretations of events. For example, they interpret them as progressive or not. Such a description has a clear prescriptive aspect. Such testing can be attempted wholesale or piecemeal. If it is done wholesale, i.e., by comparing entire domains, we are led to fuzzy standards and means of progression from one theory to another. Ad hoc moves seem needed and sacrifice the rationale for separation. If done piecemeal, clear standards and clear means of Progression are possible.

In the former case, the means of progression are fuzzy because we do not know when and to what degree whole prescriptive or descriptive theories should be tested or rethought in the light of their counterparts. There is no time in the discussion when it is reasonable to break the barrier; any decision to do so is an ad hoc move. Since this move is already ad hoc, it is natural to protect each side with ad hoc moves. Failure to do so would require starting from scratch rather than rethinking in the light of criticism, because the method requires that each side be built up independently of the other side. Furthermore, the rationale for each side depends on its separability; accommodating them to each other can destroy this rationale. In the latter case, the process of improvement is continual. Any problem may be easily posed as a Problem of the encounter of a theory with some particular criticism. Since there is no requirement that any problem be in one domain or the other but not both together, it is possible to formulate problems, and debate about problems in such a way as to let this debate determine the seriousness of any problem of reconciling prescription and description and the depth of adjustment required.

PART 2

SOME INNOVATIONS FOUND IN FALLIBILIST METHODOLOGICAL THEORY

In order to move beyond Popper's innovation in the philosophy of science, it has been exceedingly useful to move from a theory of rationality as criticism to a theory of rationality as embodied by social rules of criticism. One aspect of this development of the theory of rationality has been to move from being a theory of one ideal procedure to being theories of various alternatives.

2a. 'Styles of Rationality', *Philosophy of the Social Sciences*, 25, 1 (1995): 69-98.

This essay is a revised version of a lecture given in Tel Aviv in the fall of 1994 as part of the Bar-Hillel Colloquium for History, Philosophy, and Sociology of Science. I. C. Jarvie and Joseph Agassi read the last drafts and made many detailed suggestions for improvement.

Styles of Rationality

This article discusses the following: (i) The acceptability of diverse styles of rationality suggests replacing concern for uniqueness with that for coordination, (ii) Popper 's lowering of the standard of rationality increases its scope insufficiently, (iii) Bartley's making the standard comprehensive increases its scope excessively, (iv) the pluralist view of rationality as partial (i.e., of Jarvie and Agassi) is better but its ranking of all rationality eliminates choice of styles, (v) styles diversify the standards of rationality, (vi) rationality is not merely a matter of style, (vii) diversity raises new, interesting problems, (viii) allowing diversity permits reconciling differences better than does the absent unique standard, and (ix) cultural heritage and rationality are complementary.

Introduction: The Acceptability of Diverse Styles of Rationality Suggests Replacing Concern for Uniqueness with That for Coordination

Numerous personal, cultural, and philosophical styles interact closely in today's world. They enrich our lives, although these call for new ways of coordination: it invites a search for unity, it seems. Possibly, some established umbrella of a standard is needed to resolve peacefully conflicts between the many practiced ones. A unity of standards seems desirable, especially when all else fails. It is hoped that at least one rational common ground may be found that should be independent of cultural backgrounds and personal styles because if various cultural or personal styles are allowed to interfere with this rationality, it is feared that this interference will dose the access to unity. We may or may not find unity in interests, styles, feelings, traditions, cultures, and so on, but as rationality should be common to all, it may offer the hope for this unity. Leibniz and Descartes had hoped that the unity of rationality would bring peace; their hope still dominates our efforts, even though it also seems quixotic in today's world.

These days it is often doubted whether rationality can provide the required unity of intellectual and of social standards. So, it is not dear what the standard of rationality should provide, and this has deeply divided the philosophical community. But although the traditional view of rationality as the basis of unity is problematic, some sort of unity is still upheld as a minimum condition for rationality. Traditional justificationism takes rationality to be universal, relativism takes rationality to be confined to within frameworks, and fallibilism takes rationality to be corrective. They all share the hope that, insofar as we are rational, we may find unity, that is, sufficient agreement to resolve differences. Our rationality should not be influenced by our different cultures, traditions, or personal styles and tastes. One central task of a theory of rationality, then, is to overcome these differences. It should show how all humans, insofar as they are rational, may be unified. To that end, methods should be found that should help pursue the truths that we would all agree to share. As a consequence, it is hoped, sufficient unity may be found in social standards, in morality, and particularly in knowledge claims.

The traditional proposal, then, is that regardless of culture and of personal style, all rational individuals should follow the same rational procedures. lt is the task of a theory of rationality, then, to say what these procedures are. There are various approaches to this task. The overwhelming majority of contemporary philosophers adhere to the view that equates the rational pursuit of truth with the quest for justification. They seek to realize the goal of a description of the proper methods of rationality by pursuing the goal of a description of a method of justification—first in science and then in ethics, politics, social theory, and more. These efforts are frustrated: There still is no generally accepted standard of justification.[130] Others abandon all attempts at unifying us all through the unity of rationality. They limit rationality to that which is found within some framework or culture or social situation. So, instead of having rationality serve as the condition for unity, they make unity a condition for rationality.[131] Still others follow R. G. Collingwood and deem the traditional project of finding unity in rational procedures unrealizable but, so as not to abandon completely the project of seeking unity in rationality, they seek to develop two-tiered theories of rationality. At one level, they recommend some established framework and limit discussion to alternatives that fall within it. At another level, they recognize the diversity of extant frameworks.[132] Unity within frameworks will not do, however. It will lead to barbarism, Popper has argued so eloquently, as the absence of first principles other than those found within any framework renders hardly attainable any unification of frameworks or even the mere reconciliation between them.[133]

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[130] See W. W. Bartley III, *The Retreat to Commitment* (New York: Alfred A. Knopf, 1962), and his "Rationality vs. the Theory of Rationality," in *The Critical Approach to Science and Philosophy*, edited by Mario Bunge (New York: Free Press, 1964). Bartley was the first to characterize theories of rationality to the dominant justificationism and the critical nonjustificationist He thereby explained the common offhand rejection of Popper's view. When it was not totally rejected, it was deemed severely incomplete and in need of some justification.

[131] For a discussion of some recent versions of relativism, see I. C. Jarvie, "Relativism Yet Again," *Philosophy of the Social Sciences* 23 (1993): 537-47. For a discussion of relativism in the sociology of science, see my "The Sociology of Scientific Establishments Today," *British Journal of Sociology* 44 (1993): 69-102.

[132] See Yehuda Elkana, "Anthropologie der Erkenntnis: Ein progammatischer Versuch," in his *Anthropologie der Erkenntnis* (Frankfurt am Main: Suhrkamp, 1986), 11-124. R. G. Collingwood still is the best and most significant defender of this view. For a discussion of Collingwood, see my "The Place of Bunge," in *Scientific Philosophy Today*, edited by Joseph Agassi and R. S. Cohen (Dordrecht, Netherlands: Reidel, 1982), 465-86. lt should be added here that his approach can hardly avoid some sort of Hegelian consequences—that is, some principles that are fully determined by history and not by rational inquiry.

[133] This is the central thesis of the classical work of Karl Popper, *The Open Society and Its Enemies* (London: Routledge & Kegan Paul, 1945), which is a plea for rationality in politics. Because the theory of rationality expressed in his earlier book was inadequate for that, he sought one adequate for that end and thus opened

The most novel and most interesting contemporary attempts to find a new path to the unity of rationality abandons the quest for justification and characterizes rationality as the exercise of criticism or as problem solving in the pursuit of goals. The use of rational methods may not lead to agreement about which theories are true but, it has been hoped, we can agree about which methods should be employed to pursue our aims and, consequently, about which theories solve which problems or about which theories are false. The commonly held standards for these should provide the desired unity in the appraisal of theories.

Difficulties plague even this attempt to achieve unity by finding a universal method or procedure or style. There may be merit, then, to exploring the possibility that *we will do better if we admit not one but various permissible styles of rationality*.[134] The search for unity through rationality may have better prospects without postulating the unity of all rational styles or practices and by trying to explain instead how users of the diverse styles of rationality in the pursuit of truth may cooperate even as they disagree about which methods best serve this purpose.

How should rationality provide unity? This problem is quite traditional, yet the history of philosophy tells us little about the desiderata for its solution except that all rational practice should abide by the same rules. I propose to reject this desideratum. I should say immediately that my view is not relativist and is not as radical or astounding as it may sound at first. I do not wish to argue that rationality is merely a matter of style, nor do I wish to argue that there is no core of rational procedures that all rational individuals share, nor do I wish to argue that we should not try to find this core. Rather, I suggest that restricting ourselves to this core will make rationality too narrow to guide us effectively in our choices and that

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a new phase, adding the tacit desideratum that rationality should be adequate for the task of preventing “the return to the beasts.”

[134] The view of rationality as unique is widely held. It is held, for example, by all of the scholars mentioned in this essay, with the possible exceptions of Jarvie and Agassi. The latter two talk as if they, too, endorse this view; at least they do not reject it, and they do not discuss the consequences of its rejection or of its acceptance. They may thus slide between the two views all too easily; they are realists seeking the true theory of rationality, the correct way of pursuing truth, yet they are also pluralists who recognize differing styles of rationality. They assume the existence of levels of rationality, and they recommend as superior the workshop mentality because it is flexible and thus permits variations. See their diverse essays in Joseph Agassi and I. C. Jarvie, eds., *Rationality: The Critical View* (Dordrecht, Netherlands: Kluwer, 1987).

extending this core in one particular way will make it a minimal condition of rationality, not a universal one—unless we will dogmatically declare it universal. Let us explore, then, the option that there are differing styles of rationality and that some problems of rationality concern the ways the various styles of rationality might be developed within acceptable bounds, as well as the ways these styles might be socially coordinated. Might we not be better able to find sufficient unity in our differences if we admit differing styles of rationality and seek coordination between them?

My proposal, then, is to abandon the search for unity in rationality and to try to coordinate the different styles of rationality. This proposal may seem at first blush to be an oxymoron or vacuous or trivial or far from new. Allow me, then, a few prefatory remarks to present my proposal as distinct. Rationalists of most colors, it might be said, do not try to explain away style. Rather, they identify the rational with the universal.

What, then, does it signify that there are styles of rationality other than the trivial observation that, in addition to their rationality, people may employ different styles? My answer to this question makes my view distinct. Styles signify for the theory of rationality because the different theories of rationality, or at least elements of such theories, may be appraised and used quite differently in the context of differing styles, say, in the context of conservative or adventurous styles—the central example of this essay. Further, I suggest that we cannot separate the elements of rationality from the styles in which they are integrated so as to evaluate them piecemeal.[135] Consider the question, how should we be critical, or how should we respond to criticism? The answer may differ not only in contexts or when different aims are pursued but also when the same aims in the same contexts are pursued within different styles. Within certain bounds (whatever these bounds happen to be), there is no need to seek to determine which approach is really rational or even which has the highest degree of rationality. Rather, we can seek in each context to appraise various styles with the idea of understanding them, of improving them, and of finding out how they fit together. Individuals will then develop their own styles. How can that be done? What are the bounds of rational disagreement about styles of rationality? How can social arrangements be made that take account of such differences? If the suggestion offered here

[135] This is similar to the view of Jarvie and Agassi that we cannot separate the science from the magic of the magically minded. See their "The Rationality of Magic," *British Journal of Sociology* 18 (1967): 55-74, reprinted in their *Rationality: The Critical View*.

is fruitful, then problems such as these be-come new and pressing problems of the theory of rationality.

My proposal may appear trivial. It may appear to amount to more than the observation that there is a variety of styles that rational people employ when thinking rationally or a variety of different competing theories of rationality. It may, finally, be taken to be a mere description of the current scene—whatever one may think of the various alternatives.

Not so. My proposal differs maximally from traditional views. It allows for a variety of styles of rationality, none of which need be declared the only correct one; the traditional view insists that all styles of rationality outside of a single permissible one must be false, as there is only one permissible style of rationality. I may put this difference differently in terms of the problems of a theory of rationality. In the traditional view, various theories of rationality compete for the position of the correct description. In my proposal, we should seek to develop various acceptable styles of rationality and the means of coordinating those styles recognized as lying within the bounds of rationality.

Finally, my proposal may also appear as a mere repetition of ideas already presented by others. For example, the idea that there are various styles of scientific research has also been presented by A. C. Crombie.[136] It is not that he wants to deny the objectivity of science, although he does seek to take account of the background of science as an influence on the way science is conducted. So far as I know, he has not developed this view as a theory of rationality, nor did he explain how or why it need not lead to relativism. He has not discussed the problems to which a theory of rationality might lead, much less how to solve them. Does he think that rationality is constant, as he sometimes seems to indicate, or does he say that it changes or progresses? Does he consider the background to research better or worse, true or false, or does he consider it a mere matter of style? I am not clear about these questions. In the work that Crombie has hitherto published, he has not undertaken an attempt to answer these questions. His view, then, remains a mere suggestive title for his description of various approaches in science. lan Hacking has taken up Crombie's suggestion and

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[136] A. C. Crombie, *Science, Optics and Music in Medieval and Early Modern Thought* (London: Hambledon Press, 1990). He has sought to describe various styles of scientific research that he founds in various historical periods. He will presumably describe still more in his long-awaited but not yet published *Styles of Scientific Thinking in the European Tradition*.

uses it to plead for tolerance.[137] He has not developed Crombie's view of scientific style; he merely suggests that the use of the idea of styles as a useful tool for historical analysis will depend on Crombie's development of his historical examples. He nevertheless finds it an interesting idea and especially so if it can be reconciled with the rejection of relativism, which is, then, an aim that he and I share. He seeks to explain why it may lead to relativism and how relativism may nonetheless be avoided.

My concern is clearer than Crombie's and differs from Hacking's. Unlike Crombie, I explain the variety of acceptable styles of rationality rather than merely describing their rise in the history of science; I reconcile this variety with objectivity, present some advantages to which this reconciliation leads, and solve some of the problems to which it gives rise within the theory of rationality. Hacking interprets Crombie's display of the variety of styles of research in a neopositivist manner; he argues that these styles might determine which sentences have and which sentences lack truth value. (That is, he argues that styles determine whether we may or may not deem some sentences true or false, so that we may or may not discuss the question of whether they are true.) Because various styles could do this in different ways, he suggests, he sees here a problem for the demand for objectivity and for unity. This interpretation is not based on Crombie's own descriptions, and their views are in conflict in that Crombie assumes and Hacking denies the common ground of all scientific discourse. Hacking's essay attempts (somewhat obscurely) to explain how this comes about and the problems to which it leads. He concludes that even though relativism is not imposed by the admission of different styles of rationality, it is imposed by the admission, that the ability or inability of some sentences to possess truth values depends on style of research. He thus faces the problem; how can various styles of research be justified? He sees no solution, as we cannot justify the style we choose by the use of some other style. He nevertheless suggests some considerations that we might use so as to make rational decisions as to the choice of styles. I do not find it useful to discuss them. Rather, I offer my own solution, not to the question of choice between styles but to the question of how we can allow for them all and coordinate among them fruitfully.

[137] Ian Hacking, "Language Truth and Reason," in *Rationality and Relativism*, edited by Martin Hoffis and Steven Lukes (Oxford, England: Blackwell, 1982) 48-66.

Popper's Lowering of the Standard of Rationality Increases Its Scope Insufficiently

Let me trace the development from the nonjustificationist view of science to the fallibilist view of rationality as a sketch of the desirability of the change to which Popper's philosophy has led. Let me discuss the various alternatives and the progress to which the discussion of them has led as a sketch of the way to new and better problems.

I here use the bootstrap method proposed for the study of rationality by Joseph Agassi.[138] We may set our standards, develop a view that seeks to meet these standards, and use what we have learned through this effort to revise our standards and then begin all over again. The goal is a theory of rationality that correctly describes those procedures that all rational individuals employ; with each step forward, this goal seems to move still farther away. At each stage of the development to be sketched here, I sketch the failure of the proposed unifying factor to serve its purpose and the need to lower standards still further so as to pursue unity further. I offer, then, a preliminary view of the consequences of the failure of our efforts to find unity by specifying universal rational procedures. What problems and what hopes may be entertained if rationality should be considered not so unified as to force all who qualify as rational into the same mold but not so diverse as to prevent the achievement of all common goals and all cooperation? Young Popper was not concerned with the problems of rationality. He stumbled into them as the consequences of his attempt to follow the lead of the positivists became clear.[139] Under their influence, he sought to use the new developments in logic to find a new view of science with the minimalist approach of describing its logic. He agreed with them that the problems of the demarcation of science and of induction should be soluble with the tools of the new logic alone. He proposed, however, an immediate lowering of standards. The theory that scientific method equals empirical proof cannot account for science, he noted. So, we need to broaden our view, he concluded. To broaden our view, however, we need to lower our standards.

[138] See Joseph Agassi, "Testing as a Bootstrap Operation" *Zeitschrift für allgemeine Wissenschaftstheorie* 4 (1973): 1-24, reprinted in his *Science in Flux* (Dordrecht, Nether-lands: Reidel, 1975), 155-88.
[139] For a detailed description of Popper's development, see my *The Roots of Critical Rationalism* (Amsterdam: Editions Rodopi, 1993).

Before proceeding with the story of this development, a remark about the lowering of standards is in order here as the idea of lowering of standards seems to some to be quite obscure. Following Bertrand Russell, let us view the highest standard for rationality as the one that demands the highest goals and presume that the highest goal for a theory of rationality is proof. The further we move away from proof, then, the lower our standards become. So, refutation is a lower standard for a theory of rationality than is proof. The rejection of the aim of demarcating rationality by demarcating science lowers standards for rationality as the demand for, say, refutation is higher than the demand for mere criticism because, obviously, not all criticism is refutation but all refutation is criticism. The rejection of the demand, that the methods of rationality are common to all rational individuals, lowers standards once more, because it is removes the requirement that we specify one true view but allows us to be satisfied with the specification of a range of views.

A comment on this should be considered: perhaps Popper's rejection of induction could also be seen as his raising of standards because he bans inductive inferences as illegitimate. This comment is not true because Popper does not ban inductive inferences; rather, he dis-counts the claim that their fruits have some special status. This comment nevertheless catches nicely the spirit of Popper's move and of this essay: the lowering of goals raises the level of practice and removes the demand for goals (such as proof) that cannot be met and whose pursuit may steer away from more profitable activities. The questions here are, then, how did the presumption of various standards for theories of rationality lead to difficulties within these theories and how does the lowering of our standards for a successful theory of rationality lead to more interesting theoretical problems of rationality as well as to an increase in the degree of rationality in practice?

Popper's view is quite simple: the logic of research that characterizes science is the retransmission of the falsity of a conclusion of an argument to one of the argument's premises. This simple but very powerful logical analysis led to problems before Popper had even begun to write his *Logik der Forschung*. The description of the logic of science alone, Popper quickly discovered, could not adequately characterize science so as to distinguish it from all nonscience. lt could not explain the status of basic statements as veridical—which Popper initially presumed—nor could it preclude the use of ad hoc modifications of a theory to protect it from rejection, as Neurath stressed and as Reichenbach quickly pointed out when Popper first published his idea in a short note.[140]

Popper's attempts to solve these two problems appear in chapter 4 of *Die beiden Grundprobleme der Erkenntnistheorie* as well as in his *Logik der Forschung*.[141] At each stage in the long development that followed, new and interesting problems arose. In *Logik der Forschung*, for example, Popper developed as an answer to Reichenbach's criticism the view that science is characterized by methodological rules that block ad hoc modifications. He saw further that basic statements were not veridical and that they must be accepted only provisionally, as a matter of convention. (This was a consequence of the psychology of the Würzburg school that Popper had adopted before he wrote on philosophy, but possibly he developed it in response to Neurath's view, that the truth of statements depends on the whole of science. I suppose this is so but do not know.) In these moves, Popper abandoned the attempt to characterize science by means of logic alone, and so he added a set of conventions to the logical characteristics that he ascribed to science. Because conventions are problematic, Popper sought to describe minimal conventions, ones that would nevertheless suffice to describe science. He saw no other alternative, as Agassi has pointed out, than that between truth and convention.[142] If logic alone were not sufficient, then it had to be supplemented by convention.

With these moves, Popper opened up the problems of rationality; they begin to show some contours already in his early *Logik der Forschung*, in which he skirts around some problems of the interaction between science and metaphysics, for example.[143] They first became clear and pressing, however, only in his *The Open Society and Its Enemies* in 1945 and in his studies of the interaction between science and metaphysics of the 1950s. Although his central problems of rationality had two separate sources, one in the philosophy of the social sciences and one in the theory of metaphysics,

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[140] Popper's first publication of his view is in the short note, "Ein Kriterium des empirischen Charakters theoretischer Systeme," in *Erkenntnis*, Bd. 3, 1932-3, S. 426-7. Reichenbach added his criticism in a short commentary.
[141] See my *Roots of Critical Rationalism*, 137ff.
[142] See Joseph Agassi, *Towards a Rational Philosophical Anthropology* (The Hague, Netherlands: Nijhoff, 1977); see also his "Modified Conventionalism" in his *Science in Flux*, 365-403.
[143] See, for example, K. R. Popper, *Logik der Forschung*, vol. 7 (Auflage, Tübingen: Mohr, 1982), S. 93, where Popper notes the value of Kepler's metaphysical faith in harmony (his "Vollkommenheitsglaube") for his scientific research.

the core of these problems was the same. This core concerned the rationality of nonscientific or nonempirical theories. As he sought to apply the results he had obtained in the study of the natural sciences to the social sciences, he discovered that the view of the rationality of science that he had developed in his *Logik der Forschung* left scarcely any place for the rationality of nonscience. (He had, to be sure, avoided the extreme and untenable view of his more positivist colleagues, according to which metaphysical or nonempirical views were meaningless, but he left quite unclear the question of whether or how metaphysical theories could be rational.)

Popper noted that his *The Open Society and Its Enemies* broadened his view on rationality, but he scarcely explained this move. The broadening was the move from the view of the desirability of refutations to the desirability of criticism, which is a broader category. On the view expressed in his early *Logik der Forschung* but not on that expressed in his *The Open Society and Its Enemies*, all the nonempirical views, such as those concerning values, are based on private decisions alone. The reason is rather obvious: in his concern to combat fascism with rationality, he found his early view to be rather catastrophic; so in his The Open Society and Its Enemies, he presented a new view of rationality.[144] During the 1950s and 1960s, he tried to extend his view of the scope of rational inquiry still further to include some metaphysical discussions. He explained how rational methods could be employed beyond the limits of science by proposing that philosophy could have real problems, although ones which arise outside of philosophy.

This was progress but was not yet a fundamental discussion of the limits of rationality, not to mention the rationality of rationality, which he spoke of only in passing in his *The Open Society and Its Enemies*. This matter was taken up by Popper's students because it required a considerable change of his philosophy of science: his theory of demarcation of science was intimately connected with the theory (which is implicit in his first book) that all that was outside of science was not rational. Without this identification of rationality with science, the problem of the demarcation of science was altered into, and perhaps even replaced by, the problem of

[144] This dashes with Popper's claim (in his autobiography) that, in his early work, he was concerned solely with physics and that he then applied his results to social affairs; this is true only on the understanding that his attempt to broaden his scope made him broaden his view. See my *Roots of Critical Rationalism*, 183ff.

rationality. This is how Bartley saw the situation.[145] Popper himself never made such a change. Perhaps he thought the outright abandonment of his theory of the demarcation of science to be too high a price to pay for the extension of the domain of rationality. At any rate, he did not want to lower standards of rationality by abandoning his criterion of demarcation of science. For further developments, we need to look elsewhere.

Bartley's Making the Standard Comprehensive Increases Its Scope Excessively

Popper's student, W. W. Bartley III, sought to solve the problem of the rationality of nonempirical views without justification. A key to the difficulty faced by both Popper and his justificationist opponents seemed to be that the decision to be rational was not itself rational. Bartley sought to overcome this difficulty by explaining how rationality could be upheld rationally. He hoped to present thereby a comprehensive view of rationality that would fully replace the traditional justificationist view. If rationality itself could be upheld rationally, he suggested, then, rationality could thereby be extended unreserved.

Bartley's idea was simple. If rationality consists in being critical, then we may be rational in all of our affairs. We need not accept rationality on faith or on the basis of a decision, as Popper had thought. We may adopt rationality or any other view or proposal provisionally and hold it open to criticism. Thereby, said Bartley, we are rational. His idea is a nice generalization of the falsificationism of Popper's theory of scientific method to a fallibilism or nonjustificationism in the theory of rationality, which Popper himself had not carried out thoroughly enough. He had only indicated how some nonscientific theories might be rationally discussed and how philosophy could avoid irrationalism by finding its problems outside of philosophy. Some of the difficulties his theory faced could be removed by this generalization of Bartley's.

The simplicity and power of Bartley's idea led quickly to its popular rejection. It was rejected out of hand with no discussion by the (justificationist) majority because it lowered standards of rationality still further as the overwhelming majority simply continued to seek methods of proof. (No one used this phrase, but the attitude has been clear: it is not

[145] See W. W. Bartley, "Theories of Demarcation between Science and Metaphysics," in *Problems of the Philosophy of Science*, edited by Imre Lakatos and Man Musgrave (Amsterdam: North Holland, 1968), 40-63; and W. W. Bartley, "Reply," in *Problems of the Philosophy of Science*, 102-19.

serious because it rejects the quest for methods of proof as a theory of rationality.) There seems no rational way of deciding how to change our standards of rationality. If rationality demands proof, then if we suggest a view of rationality without proof, we are, by previous standards, quite irrational. We cannot show to the satisfaction of those clinging to traditional views that this move is rational because we cannot prove that it is correct.[146] Any lowering of established standards, wherever we may happen to set them, seems to be in effect an abandonment of rationality.[147] So, the majority choose to err on the side of conservatism. They do not want to risk a slide (not to mention a quick fall) into irrationalism. Bartley's view was also rejected because it did not go far enough; it rejected relativism, which has been seen as the only way to allow for tolerance and openness while holding to universal and binding standards of rationality as well as a method for reconciling rationality with some religious beliefs.

There were other critical responses as well, ones that were internal and more fruitful. One criticism, developed by Watkins, was that if Bartley's theory included both necessary and sufficient conditions for rationality, then it cannot be refuted.[148] The very readiness to accept criticism, then, the act of holding one's views open to criticism, reaffirms it. So, if it is refuted, it is reaffirmed by meeting its own standards, which is absurd. Bartley responded quite correctly and simply by pointing out that a theory both reaffirmed and criticized should be rejected, but Watkins had failed to show that this holds for Bartley's idea.[149] This answer to Watkins's criticism makes clear, how-ever, that it is a theory of necessary but not of sufficient conditions for rationality.[150] But then it has lost its claim for comprehensiveness. At the very least, it needs explication. All views held

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[146] This is true not only for the thoughtless, and so Popper could not show to Russell that his falsificationism is rationalist.

[147] This is the view given a sophisticated expression by Russell and a vulgar one by Stove. Its popularity can be seen from the prevalence of the complaint of reviewers of Agassi's *History and Theory*, vol. 2 (Gravenhage: Mouton, 1963); after he illustrates so vividly the low standards of studies in the history of science, at the end of that work he surprisingly pleads for the lowering of standards.

[148] John W. Watkins, "CCR: A Refutation," *Philosophy* 46 (1971): 56-61.

[149] W. W. Bartley III, "On Watkins, Evolutionary Epistemology," in *Evolutionary Epistemology, Theory of Rationality, and the Sociology of Knowledge*, edited by Gerard Radnitzky and W. W. Bartley UI (LaSalle, IL: Open Court, 1987), 337-41. For further discussion, see *Evolutionary Epistemology*, 314, note 3.

[150] See Tom Settle, Joseph Agassi, and I. C. Jarvie, "Towards a Theory of Openness to Criticism," *Philosophy of the Social Sciences* 4 (1974): 83-90.

open to criticism meet one condition of rationality, or one condition for a certain level of rationality, but not all conditions. Some may still have a low degree of rationality.

This point of view is not merely abstract. Agassi points to the Talmudic and scholastic traditions of criticism as examples of traditions that, although highly critical, display a low degree of rationality. We need, then, to explain how we may be critical and whether this or that approach is better than another. We have then no single view of rationality and its methods as Bartley had hoped but various competing views with differing methods.

One might think that Bartley could reply to this criticism with his theory of the ecology of criticism, of the setting of criticism,[151] for he did explain something about the methods of criticism that he considered universal. He mentioned the test of experience, the test of other theories (especially scientific ones), the test of problems, and the test of logic. This extension is valuable, but I see no way that his claim for comprehensiveness can be saved. His extensions still leave open various possibilities. We might find, for example, that some views will be better in one respect and other views in another. One view may comply with higher standards, whereas another may comply with standards that are more broadly applicable. One view may comply with standards better designed to encourage and evaluate new ideas, whereas another may comply with standards that better encourage the development of particular points of view. One view may better encourage the pursuit of depth, whereas another may better encourage the discovery of solutions to practical problems. Just how far these problems move us away from Bartley's comprehensive critical rationalism is perhaps not entirely clear because they all follow Popper's and his lead in taking rationality to be critical. There is a significant difference, however: here the search is no longer for comprehensively specified universal methods of rationality but rather for various possibilities, pursued in a piecemeal fashion.[152]

Here, then, we have a new question, for we are no longer speaking of rationality but of degrees of rationality and, perhaps, of its

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151 See W. W. Bartley BI, "The Philosophy of Karl Popper, part III: Rationality, Criticism, and Logic," *Philosophia* 11 (1982): 121-221; see especially 161ff.
152 See also John Wettersten and Joseph Agassi, "The Choice of Problems and the Limits of Reason," in *Rationality: The Critical View*, 281-96.

improvement.[153] This follows even from the Bartleyan exposition of the need for a critical environment for theories. Agassi has thus argued that the problem Bartley proposed to solve--that is, the problem of explaining how rationality could itself be rational—was not central.[154] Rather, Agassi finds that we are all rational to one degree or another. The problems that we face, then, are how to appraise our degrees of rationality and how to increase them. William Berkson has suggested that Bartley so concentrated on the logic of the problem of rationality posed by Popper's view that he falsely presumed that the purpose this view was designed to serve could be fulfilled if the logical problem could be solved.[155]

We find, then, various partial views of rationality, and we do so even if we all see the core of rationality in criticism. Are these various views each acceptable even though they make recommendations that contradict each other? Should we presume that we will overcome our differences and, in this way, find closer approximations to the true theory of rationality? Here we must turn to the theory of the progress of rationality. What can or should we hope for?

The Pluralist View of Rationality as Partial (i.e., of Jarvie and Agassi) Is an Improvement, but Their Ranking of All Rationality Eliminates Choice of Styles

Jarvie and Agassi have each developed new views of rationality. These have differing backgrounds and emphases, as Agassi's work grew out of his studies of the history of physics and Jarvie's out of his studies of anthropology and its methods. They have sufficiently similar views, however, that they found common ground in a series of joint essays. They hold that rationality must be evaluated in its context, that it is goal directed, that it is partial, and that it is progressive. They have applied this view to the study of various degrees of rationality, of the rationality of magic, of dogmatism, and even of irrationality.[156] According to their theory of rationality, our rational practices have improved as we have learned more about rationality. The rationality of any individual or

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[153] This view was announced in Jarvie and Agassi, "The Rationality of Magic."
[154] See Joseph Agassi, "Rationality and the Tu Quoque Argument," *Inquiry* 16(1973): 395-406, reprinted in *Science and Society* (Dordrecht, Netherlands: Reidel, 1981), 465-76.
[155] William Berkson, "In Defense of Good Reasons," *Philosophy of the Social Sciences* 20 (1990): 84-91.
[156] See the various essays by Jarvie and Agassi in their *Rationality: The Critical View*.

institution is a matter of degree. Instead, then, of seeking to be rational by following the universal procedures of rationality, we seek to improve the degree of our rationality. On this view, one might even be rational to a degree without being critical. Primitive peoples, they claim, possess this minimum degree of rationality. The problems that anthropologists face, however, are quite different, and they should be more critical than the peoples they study. It is not even sufficient to be critical, for methods of criticism may be good, bad, or indifferent. We must ask which goals we want to pursue and whether this or that method may lead to improvement in our degree of rationality. The core of their view is their idea of the practice of rationality on the model of a workshop. Various methods and procedures may be used to solve specific problems, and various individuals work together to solve problems. We need, then, a view of the improvement of rationality.

Following Popper, they gave up the doctrine of rational belief and replaced it with a theory of rational conduct; they could thus integrate the theory of rational thought and rational action. They admit different kinds of rationality, though, beginning with purposeful conduct in the light of received knowledge and ending with scientific, critical thinking. Yet they see here a line of increase of rationality. Their theory is that rationality increases as our theories of rationality do and that the approximation of our theories of rationality to the true theory of rationality parallels Popper's view of the development of science as increasing the approximation of our best theories to the true theory of the world. Now, if progress is toward an ideal, then all different degrees of rationality approach this same ideal as they progress. At the moment, I am not confident that this is correct. Some of his writings lead in this direction and others less so due to their high praise of pluralism.

It is not clear to me that Jarvie and Agassi are consistently able to integrate their progressivism with their pluralism. Jarvie's view of the matter is closely linked with the workshop view of rationality, which allows for pluralism and even demands it. Agassi's contribution to the view I am discussing here may also be seen as a development of his rejection of the dichotomy between nature and convention.[157] Rationality is then neither pure nature, and therefore not necessarily one, nor pure convention, and thus subject to some bounds. Still, the view suggested by Jarvie and Agassi that there is a current best view seems to indicate that there is one true rational approach that we seek to find.)

[157] See Agassi, *Towards a Rational Philosophical Anthropology*.

The view of the progress of rationality as exhibiting the increase in the approximation of our views of rationality to the true theory of rationality is a natural view, a reasonable way to approach the problems of rationality as it intertwines with the view that theories of rationality may be improved. lt thereby offers an intriguing view of the unity of rationality: rationalists exhibit disunity in that they characterize rationality in diverse ways; their characterizations can now be unified with the aid of this view.

Progress toward the true theory of rationality is achieved, on this view, in a bootstrap fashion. We have at some level of inquiry a theory. We could have, say, a theory of rationality that we criticize and find defective, and this may lead us to change our standards for theories of rationality as explained previously. We may then seek new standards and a new theory of rationality, or we may seek to apply a theory of rationality, to use it to interpret science or some aspect of it; or we could seek to develop a theory of rationality to account for science as we see it; or we could seek to find conflicts between theories at various levels and to reconcile them. In general, we use one level to pose problems for another. We progress in a bootstrap fashion because nothing is deemed established. We make a conjecture at one level. This leads to developments at another. And this very development leads us to return to the point at which we began, to criticize with the help of newly won knowledge, and to improve on it. This process may continue indefinitely, but it should be progressive. lt should bring us closer to the truth about rationality. We maintain the traditional ideal of a true theory of rationality to which all rational individuals adhere even if we do not deem ourselves to be in possession of it.

On this view, we find unity among rationalists, I presume, insofar as they follow the same up-to-date practice. The degree of rationality that this unity exemplifies will depend on how closely the best current view is implemented and how close this view approximates the true view. We may also find unity, I presume, in the development of rationality in a way similar to that we find in science: Just as scientists agree more or less about the progress of science, so may we agree about the progress of rationality up to a point. And beyond this point, where disagreements still exist, we seek to decide between them.

This view is certainly the most liberal we have, but it still maintains that there is both a best procedure, which is most advisable for all rational individuals to follow, and a best procedure now, which is now the best approximation to that best procedure. Yet this view is still too narrow. Should we allow not merely for various procedures in cases where our disagreements have not yet been overcome but also for various procedures incorporating comparable degrees of rationality? Should we not do this, with the aim of avoiding the difficulties faced by predecessors who had to but could not fully specify the proper rational approach? Are the presumptions too strong that there is only one path for the improvement of rationality and only one destination? Should we not lower our standards again and set aside the suggestion that all individuals should follow the best current view while seeking the true view and propose instead that all individuals should seek to develop and improve their own, somewhat differing, styles of rationality?

Styles Diversify the Standards of Rationality

We all agree that we do not agree about which theory of rationality is the best. We find both theories of science and, when extended, theories of rationality that reflect differing attitudes toward the pursuit of truth. Can we overcome these differences in the manner proposed by the view that our theories of rationality progress by approaching the true theory of rationality? Can we meet the desideratum for a theory of rationality that there be only one correct way, at least insofar as we eliminate all but one way? Because our views of rationality may rest on attitudes beyond reason and because we cannot predict the growth of knowledge, this goal may be unreachable.

Our differences concerning rationality are based in part on attitudes that are, in and of themselves, rational or irrational or neither. We find, for example, conservative thinkers who predict we will do better if we make few changes, and we find proposals from other thinkers who claim that we will do better if we are adventurous. These policies seem at least prima facie to be extensions of more fundamental attitudes toward life. Now, it may be the case that one kind of attitude, when it is reflected in a theory of rationality, leads to a lower degree of rationality than does another. But these attitudes themselves can hardly be judged today as rational or not, and it would seem arbitrary to judge them, to say one should or should not entertain one attitude or the other. The burden of proof, then, rests on those who wish to argue for the irrationality of one procedure or another.

Can we, then, make the case that those who do not accept the proposals for adventure, or alternatively those for conservative policies, in the pursuit of truth should be deemed to have a lower degree of rationality than do those who take the opposite course?[158] There are key arguments for each side. On the conservative side is the argument that good theories need to be developed and tested slowly. Excessive adventure—indeed, adventurism—will not lead to improvement of theories but to a confusion of alternatives, each one being worse than the other. We need to cooperate so as to develop. Those in favor of adventure argue that only new ideas can bring new knowledge and that the sooner we do this, the better it will be. Conservative policies serve to block advances.

Now, we cannot predict which methods—say, conservative or adventurous ones—will deliver the most goods in the long run. As Popper has argued so eloquently, we cannot predict the growth of knowledge. It may be, for example, that adventurous attempts will break down and not lead us forward simply because we fail to find good new ideas. It may also be the case that conservative approaches will block new ideas. Either approach has its opportunities and dangers. We might also observe that these dangers are most severe when one or the other side is excluded. If we allow both, however, we may thereby reduce them and so the strength of those arguments in favor of one side to the exclusion of the other. We might still find various styles that differ but remain within the bounds of rationality by not violating any necessary requirement for rationality.

If this is so, then it is difficult to argue that disagreement is merely a matter of confusion found on one side or the other, that we are on our way toward better theories; at least for now, we do not have the means to settle some of these issues. Pluralism by this argument is, then, not merely a methodological technique that we employ to move closer to the truth about rationality. It is, rather, a fact about rationality that there are various ways of being rational. It seems desirable, then, to lower our standards once more and say that there are various styles of rationality that lie well within the bounds of rationality.

Rationality Is Not Merely a Matter of Style

If we lower our hopes for finding unity in rationality in the way described previously, it may seem that rationality will be merely a matter of style. If this were true, then it would seem that the lowering of standards was, indeed, as Russell would have it, a council of despair. I think, however,

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[158] See my “On Two Non-justificationist Moves,” *Synthese* 49 (1981): 419-21, reprinted as “On Two Non-justificationist Theories,” in Rationality: *The Critical View*, 339-41.

that this need not be the case. We need not be blocked, as David Miller apparently contends, from evaluating various styles even if we do admit that various styles are within the bounds of rationality.[159] We still have two tasks. One is to set the bounds within which we find all styles acceptable as they meet some set of minimum standards, and the other is to improve some of them.

Let me illustrate how differing styles may be rational by elaborating on my example of conservative and adventurous styles. I need, of course, to modify this discussion because the parties to this dispute have sought to determine the true universal methods of rationality whereas I will discuss the alternatives as differing styles. On the view of Michael Polanyi, the rationality of science depends on its leaders. Science will be pursued best when their influence is felt widely. We need a single view to which more or less all adhere. Scientific research will be, with few exceptions, on the conservative side. Presuming that research should use conservative methods, we can ask, then, how rationality can be improved. Now, any conservative approach such as Polanyi's could be used or misused, he observed. It can be misused, for example, to defend an oppressive centralization with onerous characteristics such as ideological restrictions on publication. Conservatives as well as adventurers seeking truth will, I presume, see that conservatism should not become reactionary.

Conservatives might, then, seek a method that remains conservative even while (carefully) increasing the democratic nature of science. They might seek, for example, to lessen the distance between the leaders and the rank-and-file scientists. This was, indeed, the view that Ludwick Fleck, who has

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[159] See David Miller, "A Critique of Good Reasons," in *Rationality: The Critical View*, 343-58. He contends that there can be no (good) reason for doing anything or for deeming any theory true. He still maintains, however, that there is a difference between those who classify statements as true or false while seeking truth and those who do so frivolously, irresponsibly, or with no interest in truth. I do not understand what, in his view, is this difference. We can, he says, refer to disadvantages in classifying theories one way or another. But can we study varying approaches to such classifications? Miller 's view seems to me to be insufficient; we learn by making plans and following them, just as we learn by making conjectures. And we have methods for choosing such plans. Whether this constitutes giving (good) reasons in Miller's view is not clear to me. See Berkson, "In Defense of Good Reasons," David Miller, "Rejoinder to Berkson," *Philosophy of the Social Sciences* 20 (1990): 92-4; and William Berkson, "Methodology Is Pragmatic: A Response to Miller," *Philosophy of the Social Sciences* 20 (1990): 95-8.

been seen as the forerunner of Thomas Kuhn, presented in a rather undeveloped manner.[160] Those impressed by Fleck's view, of which today there seem to be many, might seek to devise methods to check the influence of the leaders in case they might become too dogmatic. The methodological theory might be modified by specifying that regular and open exchanges should take place, for example, about agendas among the acknowledged elite and those who are candidates to replace them. The difference between the science of the textbook and that of the elite might be lessened, in accord with the view of Fleck. This might lead to piecemeal and genuine improvements that would be seen to be so regardless of whether we prefer conservative or adventurous research. It would also remove the central argument against this view as irrationalist.

We might look at this the other way around. The adventurous may seek better ways of developing alternatives to increase the degree of pluralism. They might, for example, as Agassi recommends, encourage the development of competing metaphysical research programs.[161] Kuhn will certainly object. From the point of view of Polanyi, too, or even from that of Fleck, such an approach might be judged undesirable but might nevertheless be an improvement of an adventurous style because it might better provide research programs that could be developed and appraised within science even as they view it. This might prevent frivolous proposals or mere adventurism that would encourage the proliferation of views that appear astounding but have no substance. This would remove the charge of potential irrationalism from the side of the conservatives.

We may, then, proceed piecemeal with varying styles and seek to improve them. We presume that each style provides a way of pursuing the truth but that no single style is the correct one. Some styles may also, of course, be so deficient as to not be serious contenders. And some ways of proceeding may be dead wrong; that is, they may be so constructed as to hinder the pursuit of truth and have no redeeming features.

What is a matter of style and what is a matter of principle? How sharply can we separate such matters? Here I have no general answer, only a

[160] For a discussion of Fleck, see my "The Fleck Affair: Fashions vs. Heritage," *Inquiry* 34 (1991): 475-98.
[161] See Joseph Agassi, "The Nature of Scientific Problems and Their Roots in Metaphysics," in *The Critical Approach: Essays in Honor of Karl Popper* (New York: Free Press, 1964), 189-211, reprinted in his Science in Flux, 208-32. See also his *Faraday as a Natural Philosopher* (Chicago: Chicago University Press, 1971).

beginning of an attempt to try things out; I make no claims about how successful such a project might tum out to be. If there are different styles of rationality, however, a new task will emerge: to sort out which differences concern merely differing styles, each of which represents an interesting way of pursuing truth, and which differences concern assumptions about rationality that are true or false, such as the assumption that theories may be empirically justified or that the leading scientists or thinkers possess an intuition that guides research best. The task of developing various interesting styles of rationality will include that of determining which proposed styles incorporate a degree of rationality too low to be taken seriously. The problem of finding a core and thus the bounds of rationality is not eliminated even on the presupposition that differing legitimate styles of rationality exist. We have only lessened our hopes for what the search for this core and of these bounds should accomplish. It may even be the case that our knowledge of these bounds themselves grow. We need not abandon the idea proposed by Jarvie and Agassi that rationality improves in a bootstrap manner, although we do need to conceive of this progress in a slightly different way. The increase in knowledge of rationality may lead to an increase in knowledge of various advantageous ways of being rational rather than to some best approach.

Diversity Raises New, Interesting Problems

The explanation of why it may be desirable to presume the existence of various styles of rationality presented thus far might be criticized as follows. If we presume that there is only one true approach to rationality, then our standards for a theory of rationality and, as a consequence, for rational practice (should we be successful in our pursuit) will be higher. Because we cannot know that there is no such view, it is desirable to presume that there is one; we will not miss the truth by default, by not even seeking that which we might find. The criticism might go further: the presumption that there is one true theory of rationality will lead to debates about those issues that divide us and without which we will never achieve unity. We may pursue unity in this way even when the causes of our disagreements about rationality—such as, say, the disagreements between Russell and Popper or between Polanyi and Popper—hinge on the background knowledge that is used to formulate our problems of rationality. When agreement about rational procedures is not at hand and this disagreement is due to the existence of varying background assumptions, then these assumptions themselves may become the objects of inquiry.

This observation is correct. But it is not sufficient to show that the disagreements we find about rationality pose a mere practical difficulty raised by philosophical disagreements that we may, in the long run, resolve. Let me once more take Russell as my example. Russell gave no detailed criticism of Popper's view of science. Rather, he simply said it was not good enough. On his view, no theory of rationality could be deemed acceptable that does not justify the justification of scientific theories. We can argue relatively easily about whether this or that method of justification is successful; but when we are faced with the problem of the necessity or desirability of justification, we have a different type of question. We can, of course, discuss advantages and disadvantages here as well, but there also seems to be some questions of value or, I would say, of style about which we will have to disagree. They may be embedded in things such as the wish for security, for example. I am often unable to judge whether one person's wish for security is right or that another's wish for adventure is right. I cannot judge the one or the other as more rational, but our views of rationality or our views of what we should seek to obtain rationally may very well be highly influenced by such attitudes.

If we seek to be conservative and rational, we will seek to employ differing methods to improve our conservatism to make it more rational than we will if we seek to be adventurous and rational. If we are conservative, for example, we might make a rule such as that proposed by Polanyi or by Kuhn: always make a small change when faced with a difficulty. Or we might modify this rule in the way Mario Bunge has done: always make the smallest change possible first and admit or seek a larger change only when no small change is successful.[162] If we are adventurous, we may follow Popper instead. He has proposed that the theory with the highest degree of testability is always the best, that we should always seek a large change when difficulties arise. But then we need differing methods for the quick proposal and dismissal and/or for the integration of large changes. The methodological lines we choose to follow thus depend on which attitudes, goals, or hopes we entertain. We cannot, it seems to me, contend that someone's conservative or adventurous attitude is per se irrational. If it is possible within the framework or context of such an attitude to develop one style of rationality, or one way of pursuing truth, then we have found a limit to the theory of rationality that we should

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[162] For a brief summary of Bunge's view, see Joseph Agassi, "Bunge on Background Knowledge," in his *The Gentle Art of Philosophical Polemics* (LaSalle, IL: Open Court, 1988), especially 442ff.

respect, on a pain of dogmatism and intolerance—that is, on a pain of a fall into irrationality.

On the view proposed here, we do make progress in our understanding of rationality and of its limits, but we do not make progress the simple way. We cannot find a simple progression toward better theories of rationality that will provide unity for all those keeping up with the progress being made. People going in differing directions should be deemed rational, and there is no common scale to judge their degree of rationality, say, their distance from the best theory. This answer to the objection, that we should presume that there is one theory so as to keep our standards high, is still not complete, however, because it might be good to presume that there is one theory to keep them as high as possible even if there is no such theory. This approach has disadvantages, however; it precludes from consideration those problems that arise if we presume there are differing styles.

Allowing Diversity Permits Reconciling Differences Better Than Does the Absent Unique Standard

The supposition that there are various styles of rationality represents one more step toward the lowering of standards for a theory of rationality. lt says that it is good enough to develop a good way of pursuing the truth; we need not seek the only correct way of pursuing the truth. But it opens up new, interesting, and potentially important problems as well. It might therefore serve to raise standards of rational practice (at least in that it will enable us to encourage more diversity and perhaps also more adventure). The discussion of these problems may be more fruitful than the attempt to determine the only correct rational procedure.

To explain what some of these new problems are, it is useful to mention the differences between the new framework for the study of problems of rationality and the traditional one. The philosophy of science as well as of rationality traditionally has identified the problems of the rationality of our institutions with the problems of the rationality of individuals. A solution to one problem has been deemed a solution to the other. We may describe how an individual who is rational proceeds and deem only those rational who behave just in that way. Almost all views of scientific method—views that more or less stem from Bacon and Descartes—reflect the problem of rationality in this way. Social norms, customs, and traditions may have to be honored, but they are not themselves rational or not. We may, on the contrary, follow Polanyi and view as rational only those who follow exactly the rules of the scientific society.

On the traditional framework, problems of rationality are treated as problems of either individuals or institutions, but it is also presumed that an answer on one level will yield an answer on the other level as an immediate corollary. On one view, we study rationality by studying how individuals pursue the truth and then, if we choose, we may generalize this to society, as Bacon and Descartes did. Bacon's *New Atlantis* is a social theory developed out of the theory of how individuals should pursue the truth. On the other view, we may study society first. We may seek the social ties to which individuals who contribute to the rationality of the whole should conform. On this view, individuals do not possess any rationality independently of institutions but are merely assigned social roles that contribute to the rationality of the whole. Holistic social theories follow this procedure. Hegel's philosophy is a grand attempt to explain rationality in this way. Mannheim's view of a free-floating intellectual is so problematic because he endorsed the view of rationality as social in describing societies but had to take his observer out of society, somewhat artificially, to explain scientific rationality; the traditional framework gave him no way of integrating the rationality of the autonomous individual with that of the collective.[163] Once we know how a rational society functions, we merely assign roles to individuals and/or appraise societies on the basis of whether these roles are played properly. Modern relativist sociology of science seeks to use such a procedure.[164]

The view of rationality discussed here poses both the problem of the rationality of institutions and that of individuals. The problems are quite different, but their solutions need to be integrated. A theory of the rationality of institutions will set bounds for the actions of individuals. The theory will have to take into account the various ways in which individuals can proceed rationally. A theory of the rationality of individuals or of some rational style will have to take account of the social context in which such a practice takes place but will also require procedures not defined by this context. Both the rationality of institutions and that of individuals must thus be deemed partial. We need, then, continuing examination of how individuals may be rational and how their rational methods place new demands on institutions. In addition, we need analyses of how new institutions place new demands on the rationality of individuals and

[163] For a discussion of the reduction of the psychological to the social or the social to the psychological, whereby one or the other side is explained away, see Agassi, *Towards a Rational Philosophical Anthropology*.
[164] See my "The Sociology of Scientific Establishments Today."

analyses of how individuals may pursue the truth in particular contexts so as to improve the growth of knowledge.

If we presume that there are various good styles of rationality and that all rational individuals, whichever style they happen to adopt, are seeking to come closer to the truth, then we will want to know how they can cooperate effectively. This poses new problems, because we seek not merely the description of the proper methods that individuals use in line with the Baconian or Cartesian traditions, nor merely the description of social procedures in line with the recent social studies of rationality. We can no longer define institutions that should serve as the context for rational discussion—not even science—by the particular rules that all members of some social group follow. Rather, we will need to explain the personal styles and the institutions that enable individuals following different styles to contribute to the growth of knowledge in the most effective way.

The rules and procedures for the pursuit of the truth cannot be transferred to the society. The use of the rules found in any specific style of rationality as social rules will be too restrictive for some. Nor will the use of institutional rules and procedures, limited to the core of rationality common to all, suffice for the guidance of rational practice; they will need to be supplemented by the techniques de-signed within a given style.

We need, then, a society that not only allows for various styles of problem solving but also provides for some integration of them. Various styles are not merely differing methods used to solve differing problems, because conservative individuals will want to solve many of the same problems as adventurous ones and individuals with metaphysical interests will share some problems with those who have in mind practical ones. A society that allows each to act so freely seems desirable. But solutions will also clash. More importantly, research programs will clash. We need, then, to pose the problem: How can we make such conflicts productive?

We will also need to find institutional procedures or rules with which to limit irrationality, for various styles could still further or hinder rationality. An adventurous style may, for example, be shown to be quite superficial, to lead to the appearance of great innovation when none or little is at hand. (I have argued elsewhere that Emil du Bois-Reymond's physiology fits this description.) The advocacy of conservative styles might lead to the imposition of excessively narrow social standards. (Agassi has argued that this was the case with most of 19th-century physics.) So, in the context of various styles, we will still need to discuss how various procedures effect the advancement of knowledge and how they affect society and its ability to find new solutions to new problems. We will need to see that no single style will become dominant. But, at the same time, we will need to identify those styles that detract from rather than contribute to knowledge--for example, reactionary institutions, sheer adventurism, and so on.[165]

Traditionally, those who have emphasized tradition have been placed in the category of the irrationalists or those too near to them. This is so even when they were quite knowledgeable of, and friendly to, science, as the case of William Whewell shows.[166] The paradigm of this type of irrationality is, of course, taken to be Hegel. Rationalists, on the other hand, were traditionally seen as those who have sought to overcome tradition. This radical split between the irrationalists such as Hegel and the rationalists who seek to overcome the influence of tradition by purging it of all of its nonrational elements seems to be a severe block to the exploration of various possibilities. This block may be overcome to some degree when problems are formulated in the way suggested here.

Perhaps not. One may claim that tradition does not identify the rationality of institutions and that of individuals; one may rest this claim on the true observation that, for centuries, standards for proof for civil courts differed from those for criminal courts and neither has served as standards for individuals. One may also mention as evidence the wide body of literature on corporate/committee decision making (including work on how juries ought to arrive at their decisions).

These alleged counterexamples do not refute my view, however, of the traditional framework in which discussions of rationality have taken place. They lie outside the discussion of rationality per se. They are discussions whose relevance to the discussion of rationality was hardly ever noticed. Consequently, there is hardly any coordination between these studies and

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[165]For a study of adventurous and conservative styles of scientific research from this perspective, see my "On Conservative and Adventurous Styles of Scientific Research," *Minerva* 23 (1985): 443-63.

[166] For discussions of Whewell, see John Wettersten and Joseph Agassi, "Whewell's Problematical Heritage," in *William Whewell: A Composite Portrait*, edited by Menachem Fisch and Simon Schaffer (Oxford, England: Clarendon, 1991), 345-69; see also my "Rethinking Whewell," *Philosophy of the Social Sciences* 23 (1993): 481-515, and my "William Whewell: Problems of Induction vs. Problems of Rationality," *British Journal for the Philosophy of Science* 45 (1994): 716-42.

those of rationality per se. This nice criticism may, then, help to show just how useful the proposal to broaden the scope of the discussion of rationality may be, for the traditional discussions of rationality in philosophy, especially in the philosophy of science, do identify individual and social rationality.[167]

This shows a gap between the social and philosophical theories relating to rationality, whereas the traditional philosophical framework is inadequate for the conduct of the social inquiries. This gap may be closed by opening the way for better normative-cum-descriptive studies of how to be rational in many areas that offer possibilities of coordination, integration, and/or criticism of one field on the basis of results in another. That such studies might be productive was pro-posed some time ago by Agassi, but he has not succeeded because his proposal is wanting; the integration of the empirical studies of the rationality of institutions with the philosophical studies of rationality has been blocked not only by the assumption that individual and institutional rationality are the same but also by the assumption, which he shares, that rationality is one.[168]

Cultural Heritage and Rationality Are Complementary

Does the postulation of various styles of rationality in the narrow sense discussed here have anything to do with the differences and interactions of cultures and styles with which I began this essay? In the face of such differences, can it help us achieve unity better than does the quest for one description of proper methods that is binding for all rational individuals? In any case, if we admit there are various acceptable styles of rationality, then the logic of the discussion will be somewhat different. Instead of setting independent standards of rationality and applying them to whatever views we happen to have, we may seek to develop these views with some style of rationality. We may seek to explain how one can proceed rationally in the context of some culture, some set of aims, some style. This admission should excitingly allow for an easier integration of

[167] This explains the absence of comments on Agassi's claim that as the legal standards of corroboration differ and as corroboration is legally required in technology where standards of testing are often prescribed by law and by regulations, no current philosophical theory of corroboration is adequate although Popper's comes closest. See Agassi’s “The Confusion between Science and Technology in Standard Philosophies of Science” in his *Science in Flux*; also see his *Technology: Philosophical and Social Aspects* (Dordrecht, Netherlands, and Boston: Kluwer, 1985).
[168] See note 38.

rationality and culture, as it may allow for new problems of appraisal that may set aims more realistic and more progressive, thereby raising the level of our rationality.

A first goal will be the development of a style of rationality within some broader context. We may seek to change the context to improve our style of rationality. We may then use some style of rationality to appraise the very context in which it was developed. The broader, and perhaps improved, context may then be used to appraise those theories of rationality developed within it or its predecessor. We may use here the bootstrap method proposed by Agassi, albeit without the assumption that there is a single line of development for all rational individuals. Any broader viewpoint should produce some rational style. This allows, of course, for improvement in various directions but not necessarily for the common setting of standards.

The only difference in styles of rationality I have discussed here is that between being adventurous or conservative. Other examples come to mind easily and might be developed out of various attitudes toward the purpose of rational inquiry, such as the top down and the bottom up. We may start from a deep metaphysical interest such as those of Spinoza, Maimon, or Einstein; or we may be moved by a primarily practical interest such as those of Peirce, James, or Quine; or we may have both interests but wish to separate the two, as was the case of Duhem. Differing traditions bring forth different styles of rationality, including differing ways of pursuing the truth. Insofar as all these ways share the aim of pursuing the truth in a legitimate context, we may be able to deem them different styles. We may not be able to convince one another to change, but we can understand and criticize and improve, nevertheless. One could not convince Russell that his demands on rationality were too high, nor could one convince Einstein that his metaphysical interests were illegitimate, nor perhaps could one convince Quine that he restricts rationality too much to practical concerns. It is not exactly obvious which assumptions concern matters of style and which concern facts. But it may be useful to sort some of this out in various debates about rationality. We find in these traditions a spectrum of (or shades of?) rationality and of its problems. We may see that the variety cannot be integrated by revealing which approach employs the correct view of rationality. We may thereby avoid two extreme reactions, universalism and parochialism. (The parochialist accepts one heritage as given and tries to work within it. The defender of universalism rejects all tradition entirely and tries to establish a comprehensive rationality that stands above and beyond all else.) We may also avoid the combination of the two. The separation and acceptance of science as comprehensively rational and of religion as completely beyond the bounds of reason is here, of course, the prime example but not the only one. Modern versions of relativism do this as well, merely substituting relativism for religion.[169]

These strategies seem hardly acceptable in the modern world. The mere acceptance of heritage is too little. We have to integrate, to a reasonable degree, societies with varying, and often competing, heritages. And the development of a radical view of rationality, that excludes all but the universal aspects of rationality, is now, and I think forever, quite out of reach. The more radically we seek to purge our views of rationality from any element that is not universal so as to render them complete and/or comprehensive, the more we limit rationality. This expurgation of nonuniversal elements aims at the extension of rationality to all traditions by a futile search for the elements of rationality shared by all of them. If we seek to make rationality more powerful by allowing various extensions of it in various ways but then isolate these various ways from each other, as relativists (and Hacking) suggest, then we sacrifice that unifying element we have hoped to find in rationality, which has traditionally been so important and which is so pressing today. Tolerance is absolutely necessary but is by no means enough. We also do need integration.

How, then, can we find a more modest approach, an approach that will produce broader and more effective results? We can seek to improve our heritages without seeking to banish them or to fall prey to them. To do that, however, we must also choose them. We can, to be sure, choose a heritage only partially. In any case, we have no effective means of radically changing the heritage of our birth, as even radical conversions are partial. But we can choose to change aspects of any heritage. We may absorb aspects we find in traditions or cultures other than our own and that seem to improve those in which we find ourselves. One way to improve them is to improve the style of rationality that fits within some background.

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[169] See note 3 above.

Interlude

Beginning shortly after a conversation with Alfred Tarski, in which Popper learned that his description of the logic of (empirical) research was not, as he had previously claimed, Modus Tollens, he worked for ten years to develop his own logic. One central aim of this project was to show that logic did not require both a meta-language and an object language, as Tarski maintained. The specification of an object language in a logic meant that one did not have a (universal) logic, but instead one had a "logic of". This had the consequence that there could not be a single logic of research; there could only be various logics of research and this result undermined his own "logic of research". He wanted to avoid it but failed in his concerted effort to do so. His failure showed that the Carnapian attempt could also not avoid the same result. This meant that no reduction of the meaning of all sentences about the world could be reduced to sensations and their combinations. The story of the development of Popper's logic, the purpose of Popper's logic, its failure and Popper's attempts to move around the problem this failure posed for his "logic of research" is explained in the following essay.

2b. The importance for Popper of his failed quest to show that there is a universally applicable logic; the philosophical changes that followed.

Abstract: After Popper's publication of *Logik der Forschung* in 1934 there is a significant gap in Popper's published research in the philosophy of science until his six essays on logic without assumptions appeared from 1946 to 1948. In these essays he wanted to show that Tarski's claim, that the construction of rules of valid inferences required the use of a meta-language which includes an object language, was false. A single undefined logical term of deduction should be enough to provide the definitions needed for logic. Popper quickly conceded defeat in the face of serious criticisms of his essays by leading logicians. In reaction to his defeat he sought to show that the use of a meta-language which includes an object language did not create any serious problems for the use of logic in the pursuit of truth and to develop a new theory of verisimilitude using Tarskian logic. He also reinvigorated his research in the philosophy of science, but with an entirely new theme: Whereas he previously sought to show how non-empirical statements could be removed from science, he then sought to explain how one can rationally deal with non-empirical statements. After my portrayal of this development I will discuss Joseph Agassi's attempt to move forward in logic within the framework developed by Popper (and Tarski) after his failed attempt to provide a logic without assumptions, that is, with "logics of".

The background of Popper's study of logic without assumptions: his philosophy of science 1934 to 1946.

Popper first began his attempt to develop a new logic of science in *Die beiden Grundprobleme der Erkenntnistheorie*. He had a simple idea: the logic of research was, he said, modus tollens. But modus tollens was not what he intended. What he did intend was the retransmission of the falsity of the conclusion of a valid argument to at least one of the premises of that argument, as he later presented his view. He learned of his mistake during a conversation with Alfred Tarski shortly after the publication of *Logik der Forschung*. The reason that modus tollens was not what he wanted to say is that in modus tollens one deduces that p is false from "if p then q" and "not q". But the statement "if p then q" does not say that q follows from p. It only says that, if p is true, then q is also true. This can be the case when q follows from p, but it may also be the case when q does not follow from p.

At first Popper simply did not notice that his theory did not merely presume that negative statements were proven true, as they are by modus tollens. He did however presume that one central purpose of the logic of science was to show that some statements were false. He thereby made the retransmission of the falsity of the conclusion of a valid argument to at least one of the premises of that argument a part of logic for the first time. This amounted to a change in the assumption of what logic should enable one to do, as Agassi has pointed out (Agassi 1978, p.7, 10-11). And this change turned out to be more complex than it might have at first blush appeared.[170]

Tarski's problems: How secure is any logic? How extensive is any logic?

Even after the publication of *Logik der Forschung* Popper had a dilemma regarding the truth of non-proven sentences. He had no theory of their truth, which explained what one said, when one said that a non-proven statement was true. Tarski had posed the question of what one asserted, when one said that a statement was true. Up until then, it was generally assumed that one said that the statement referred to was proven. One could also say that one asserted that the statement corresponded to the facts. But there was no theory as to just what that meant. Tarski provided a theory of the truth of statements, which were not proven: One states in a meta-language that a sentence in an object language is true. The sentence in the object language is true, he said, if and only if it describes the world as it is. His theory does not explain how one can determine if that is the case; it only explains how we can under certain limited conditions clearly assert without any contradiction that a statement is true, even when it is not proven.

On Tarski's approach, the development of sentences in a meta-language asserting the truth of other sentences in an object language was only possible when the object language was completely specified and consistent. This was the case, because only then did the statement, which

[170] Agassi is the only scholar to have noticed Popper's important change in the subject matter of logic from one of the proof of statements alone to one including a rule of the refutation of statements. Popper gives no indication that he noticed this change in logic when he first introduced it. He later presented his reform of logic in his lectures in London. These were never published, though detailed notes of their content carried out by Czeslaw Lejewski and Agassi are available in a few copies. I have found there no discussion of the retransmission of the falsity of a conclusion to at least one of its premises.

was specified to be true, have a surely unambiguous content in the statement asserting it to be true. But nevertheless the principle, that a statement asserting the truth of another statement could be correctly formulated, opened the door for Popper: He could explicitly presume, as he up until then could not, that this was also true in science. He could even do this without a particular system of logic to provide the basis for such assertions in science.

Tarski's theory of true statements was a valuable, albeit partial, opening for Popper. It allowed him to integrate into his theory of the logic of science the results of (Tarski's) logic. But Tarski's results in logic also created a new problematic situation for Popper. From the beginning of his research on the methodology of science he wanted to explain how science progressed without the use of any nonscientific background knowledge. In accord with the positivist momentum of the period, the description of science should be limited to logic and empirical theories. Popper had, of course, already moved beyond this severe limitation on the philosophy of science with his theory of the necessity of following specific methodological rules in order to do science properly. But he had worked somewhat around this philosophically unwanted extension by emphasizing that they were mere rules, perhaps, say conventions. His position in regard to the status of methodological rules was, however, not all that clear. Tarski's theory of true statements limited the severity of this problem, by opening the possibility that they, just like scientific statements, could be true or false. But it did not solve the problem of whether non-empirical statements could be true or false statements about the world. Tarski's logical theory of true statements made Popper's quest for a theory of science without empirical statements quite difficult. True statements on Tarski's view are expressed in object languages. Could non-empirical statements be expressed in an object language? I presume that Popper felt that this was a problem, though I have not seen any reference to it by him or anyone else.

Popper needed Tarski's demonstration that non-proven, even non-provable, sentences could be said to be true without any theoretical difficulty. At the core of his (Einsteinian) theory was the assertion that scientists sought the truth. But Tarski's theory had limitations, which Popper wanted to avoid in his theory of science. Tarski argued that logic and the theory of truth could only be constructed in the context of a meta-language. The meta-language could be proven to be consistent. But this proof required that one use some further meta-meta-language to set the context in which the proof would be given. A never-ending regress would begin, if one tried to prove or prove the consistency of all languages to be used in the development of logic. This means that any particular statement that some statement is true, which is not made in the context of Tarski's theory, lacked the certainty of unambiguousness provided within this theory by the formalized object language.

According to Tarski's logical theories, no logical theory was universal. Each logic was a logic of some systematically developed and specified object language. One could not presume that any Tarskian logic could be applied to all languages about the world. But for Popper the logic of science was supposed to be universal. He did not want a theory of the logics of science, rather than a theory of the logic of science. He wanted then to have a logic applicable to all sufficiently developed languages.

Popper's attempt to overcome limits of Tarski's results in logic.

According to Popper's own report of his research in logic, he first began a serious attempt to provide an alternative to Tarski's logic around 1936. His research began shortly after his meeting with Tarski and led to six essays. One of his aims in doing this research was to solve a central problem arising for Popper in his possible use of Tarski's logic as a basis for his own logic of science. This problem was, how could one make a universal logic rather than merely a "logic of"? The view he wanted to replace was Tarski's theory of the construction of rules of logical inference (Tarski 1936) Tarski argued in his article about how such rules were constructed, that, in order to do so, one needed a meta-language which incorporated an object language, exactly as he had argued in his theory of the truth of statements which were not proven. This has the consequence that the result is not a universal logic but rather a logic of some particular object language.

Popper never tried to explain how one could explain the meaning of a statement p, that some other statement q was true, without putting that statement p in some meta-language, which included the object language to which statement q belonged. The first purpose of his theory of logic without assumptions was to replace Tarski's theory of logical inference. Popper wanted to integrate some Tarskian ideas into his philosophy of science, in order to solve some fundamental difficulties it faced. In his logic he did not want to use a meta-language which included an object language, as Tarski's approach did. He wanted to devise a logical system which would be completely independent of any specific object language. Only then would it be universal.

Popper himself never clearly explained the philosophical background for why he set about the task of developing a logic without assumptions. He simply said he wanted to improve logic. My conjecture about why he was silent on this philosophical point is that he did not want to reveal how close he was to trying to meet a standard set by the positivists, that is, the standard that there should be only logical and empirical statements. They were, after all, his main competitors and he emphasized that he was breaking with their logical positivist approach to the philosophy of science by both rejecting induction and, more to the point here, the reduction of the meaning of all sentences about the world to observation sentences. Of course this is not quite right: His methodological rules were meaningful, but they were neither part of logic nor empirical. My second conjecture as to why Popper did not spell out his philosophical purpose for developing a logic without assumptions was that he may very well have been uncertain whether he could succeed. And he may have not wanted to make the possibility of defending his philosophy of science dependent on his success in developing a logic without assumptions.[171]

Popper's essays on a logic without assumptions were reviewed by at least three experts in the field; two of them found that he had failed. H.B. Curry claims that what Popper describes as definitions are "technical subterfuges", that there is a counterexample of an inference Popper uses to develop his logic, and that Gentzen's "Hauptsatz" goes further than one aspect of Popper's proposal. J.C.C. McKenney argues that in defining a valid inference Popper does not say that the conclusion of a valid inference must also be a premise of that inference, but without this assumption Popper cannot achieve his goal. E.W. Beth gives a short description of Popper's results with no evaluation.

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[171] Popper's steady, but in some respects gradual, separation between himself and the logical positivists has been described by Victor Kraft (Kraft 1974). In some respects his description is detailed and accurate. But he makes two serious mistakes, one of which in his reply to Frankel Popper pointed out. Kraft says that Popper's anti-inductivist approach was due to Kant. He thereby ignores the importance of Karl Bühler and Bühler's teacher Oswald Külpe, as I have in detail elsewhere analyzed (Wettersten 1987; 1988; 1992; 2005). And he describes how Popper's theory of scientific theories as testable rather than confirmable influenced important members of the Vienna Circle. But, as Popper points out, he fails to note that at least two deep seated differences between Popper and these members of the Circle remained: They clung to their aim of developing a theory of meaning of sentences which Popper decisively rejected, and they insisted that inductive methods could justify scientific theories.

In Popper's only mention of Gentzen (ft. 7, On the theory of deduction, Part I, 1948,) Popper maintains that it is not quite clear whether Gentzen offers a metalinguistic predicate asserting some kind of inference as he—Popper—does. One of his reviewers—S.C. Kleene disagrees. The criticisms of his four reviewers—E.W. Beth; S.C. Kleene; H.B. Curry and J.C.C. McKinsey—are short but quite precise. They offer explanations why Popper's results are not as good as Gentzen's. Since there has been no response to these criticisms, I take them for correct and will not discuss them any further.

Much later than these early criticisms Czeslaw Lejewski, a former student of Popper's lectures in logic, argued that Popper's logic was incomplete but, when completed, it allegedly had an equivalence to the logic of Alfred Tarski, whose theory Popper was trying to replace (Lejewski 1974). This essay played, of course, no role in Popper's development being discussed here. Popper never tried to show that Lejewski had made any mistakes. He did point out, that Lejewski's essay ignores the central theme of his essays on logic, that is, logic without assumptions (Popper 1974). He notes that the logical term 'identical with' cannot be defined without the inclusion of an object language, which is just that combination of meta-language and object language which he had hoped to avoid.

Although some of Popper's followers have attempted to refurbish his logic without assumptions, no one has succeeded; his attempt has been widely viewed as a failure. And Popper has accepted this judgment; he never returned to the task he had set himself of explaining logic without assumptions.

The significance for Popper of his failed attempt; the aftermath: Tarski's theory of true statements generalized and a new Tarskian theory of verisimilitude.

After Popper gave up his attempt to develop a logic without assumptions, his only return to logic was 1) his use of Tarski's approach to develop a new definition of verisimilitude (Popper 1963c), 2) his explanation of how logic was applicable to reality (Popper 1963d) 3) his discussion in *Objective Knowledge* (Popper 1972a) 4) his philosophical comments on Tarski's theory of truth (Popper 1972b), and 5) his essay on dialectics (Popper 1963b). His comments on logic came down to the claim that the application of Tarski's theory of truth did not depend on the specification of formalized object languages and the creation of a Tarskian theory of verisimilitude. Each theme is discussed first in *Conjectures and Refutations* and then in *Objective Knowledge*.

In *Conjectures and Refutations* Popper argues that, although the application of Tarski's theory of true statements requires the use of a meta-language and an object-language, it does not require as is often said, the use of a formalized object language. One may have artificial languages which are not formalized (Popper 1963e, 398-99). This perspective opens the door for a more wide-spread application of Tarski's theory of truth, than its restriction to merely formalized languages such as that which Tarski himself used would do. It would also largely remove the need for the elimination of the alleged need for the use of both a meta-language and an object language in some logical contexts. I have, of course, claimed that one of Popper's central aims in his six essays on the removal of assumptions in logic was to remove the need for this dual approach. After the failure of these essays to have achieved their goal he may have changed his point of view. And his essays sought to remove the need for a meta-language and object language in the construction of the rules of valid inference, but not in the theory of true statements. So, I find no coherent and defensible reading of his view at the time he wrote his essays on logic without assumptions: Did he think that Tarski's approach of using both a meta-language and an object language needed to be overcome or not?

In *Objective Knowledge* Popper picks up his view that Tarski's theory of true statements can be used without formalized languages. He presents there Tarski's theory while using, on the one hand, German as meta-language and English as object language and then English as meta-language and German as object language. Either way can be used, he says. He merely adds that when we do so use them we can avoid paradoxes (p. 316).

His presentation of a new theory of verisimilitude is far more extensive in *Conjectures and Refutations* than his discussion of the theory of true statements. He picks up this discussion again in *Objective Knowledge*. In each case his desire is to explain the verisimilitude of a statement as a product of the relation between its truth content—the class of its true statements—and its false content, which he measures in accord with a theory of probability in Tarski's system developed by S. Mazurkiewicz (Popper 1963c, 392).

His definition of verisimilitude only applies to specific linguistic contexts, as Tarski's theory of truth also did, and he points out that there is no possibility of determining the verisimilitude of any statement, just as there is also no possibility of determining the truth of any statement. But he still viewed this result as providing him with an explanation of why it was reasonable to talk about the verisimilitude of statements. He thereby wanted to refute Quine's claim that it was not. This new theory came to replace the theory of verisimilitude he had previously developed and which was shown to be incapable of determining the verisimilitude of any universal statement: It was always possible to change the result in regard to verisimilitude with small and plausible changes in the descriptive terms of the theory whose verisimilitude was to be determined (Miller 1974; 1975).

The relation between Popper's research in logic and his methodological theory: Popper and logical positivism.

In contrast with the many commentaries and criticisms and praise of all other aspects of his wide-ranging research, Popper's research in logic has never been placed in the context of his methodological innovations. He proposed that logic could be developed in a more fundamental way than it had been, for a number of years before, thought possible: It would not need any linguistic contextual limitation, that is, any meta-language which contained an object language. But why this was of philosophical importance for him has never been explained. Perhaps no one thought it needed a philosophical explanation: It was by itself an interesting project which grew out of the advances and challenges that above all Tarski had made in logical theory.

But this perspective leaves aside interesting questions of the relations between his ambitious project in logic and his desire to improve his philosophy of science between 1934 and 1948. In his philosophy of science, which he developed in *Logik der Forschung* and which he maintained while he was working on his logic without assumptions, one important problem was how to keep out of science any and all philosophical and/or metaphysical assumptions. Though he thought that at least some non-empirical statements were meaningful statements about the world, his theory of demarcation should limit scientific research to the examination of statements with empirical consequences. Non-empirical theories should, he then thought, play no role in scientific research. This aim placed his theory of science rather close to the research of his logical positivist colleagues, especially to that of Rudolph Carnap. But he was determined to make clear and to emphasize the important differences between his theoretical aims in the philosophy of science and those of Carnap.

Carnap, as well as Popper, wanted to preclude all non-empirical statements from science. But Carnap wanted to do it with a theory of meaning; Popper rejected such an approach as not feasible. Carnap held that only empirical statements have meaning and all empirical statements should be part of science. His theory of meaning was simultaneously a theory of the demarcation of all knowledge, that is, of science. For Popper all non-empirical statements should be excluded from science, but no theory of the meaning of statements could achieve this goal. But just why he thought that all non-empirical statements should be excluded from science was not clear at the time. In order to defend his theory of science Popper then needed an alternative, which would stop the invasion of non-empirical assumptions into science, without any theory of meaning.

One way non-empirical assumptions could work their way into science is through logic. Popper had not noticed this before he learned Tarski's theory of truth. And Tarski's theory became of crucial importance for the development of his theory of science. Without it he had no theory of the truth of non-proven statements. And, only with the assumption that non-proven statements could be true could he defend his claim that the aim of science is to pursue the truth by critically examining theories, by presuming that putative truths were to be shown to be false.

But just as Tarski's theory enabled Popper to set aside his awkward question regarding whether scientific statements could be true when not proven and not given any clear description of how they corresponded to reality, it posed a new and serious problem: The development of any logical system in which statements could be expressed and be true required the use of a meta-language. But such a meta-language introduced into scientific discussions non-empirical assumptions about the world. They could not be treated as testable scientific hypotheses. They also could not have any independent demonstration that they were consistent.

It might seem at first blush that a logic without assumptions, that is, without any meta-language which included some object language, would have enabled Popper to have his cake and eat it too: He could use a Tarskian theory of true statements without the use of any meta-language containing non-empirical assumptions about the world. But this is not the case: The Tarskian theory of non-proven true statements would still introduce a meta-language which was also an object language.

Popper never explained that some avoidance of the need for any meta-language which included an object language was a significant motivating force for his research in logic, nor did he ever offer an alternative theory of true statements. Afterword he said that the theory could be applied to scientific theories as it stood. There are at least two possible reasons for his not saying that avoiding the use of non-empirical statements about the world was an important aim of his research, even when it was. One of these reasons is that this program put him quite close to Carnap and the logical positivists—a point made by Carnap, and one which Popper was reluctant to discuss. The second reason is that Popper was never quite sure that his program would be successful. It may very well have been that for this reason he did not want to say that his philosophy of science depended on its being successful.

Some consequences of Popper's failed program in logic for his philosophy of science.

Popper's philosophy of science from around 1937 to around 1948 was characterized by his assumption that the growth of knowledge was obtained without any non-empirical background assumptions. He avoided any treatment of any problem posed by the use of non-empirical background assumptions, first in any research which he identified with scientific research and then in scientific research, which he distinguished from other non-empirical research. Perhaps he at first assumed that no such problem existed. Only after logic without assumptions failed die he admit that such problems could not be avoided. One reason for the neglect of any analysis of Popper's shifting perspective from one which excluded the use of non-empirical background assumptions in scientific research to one that included their use, is that from 1934 to 1948 Popper never insisted either that non-logical and/or non-empirical sentences could have no meaning or that they could have meaning. If they had no meaning, they could not be used, but if they had meaning they could, and perhaps their use could not be avoided.

In this respect the difference between his theory of science and those of Carnap—though not only of Carnap—was then not so clear as it later became. Carnap insisted that only empirical sentences had meaning as statements about the world, and therefore only empirical sentences could be used in science; Popper merely left open the possibility that they could have meaning, but he excluded them from science. Neither of them precluded that non-empirical sentences could serve some other, say, emotional, purpose.

Soon after his concerted attempt to provide a logic without assumptions as a possible basis for a treatment of the problem of how all non-empirical background knowledge could be dispensed with, Popper began step by step to revise his philosophy of science: He moved one small step after another away from the quite ambitious attempts of his earlier research to remain as close as possible to the strict empiricism of Carnap and the logical positivists, while at the same time maintaining a sharp contrast between his view and theirs.

After Popper later developed his theory of rationality to include the rational discussion of non-empirical theories, Quine became the substitute leader for the competition with Popper engaged in by reactionary right-wing logical positivists. But Quine never really became the same strict competitor that Carnap had been. Quine abandoned both the attempt to reduce the meaning of all statements to empirical assertions and inductive logic. But, far more intensively, as Popper had done before, he tried to remain as close as he could to Carnap's philosophy, the thrust of which he had never stopped admiring, since he was a student of Carnap in Prague in the 1930s. In contrast to Popper, Quine based his attempt to explain knowledge on a theory of the central, albeit not complete, meaning of sentences as empirical.

For a long period of time Quine deemed Carnap to be the leading philosopher of science; he adhered as closely as he thought possible to Carnap's attempt to show that all knowledge consisted of statements, whose meaning was their empirical consequences alone. But Quine built into his theory the norm of historically established languages, which was just that which Popper, before the breakdown of logic without foundations, wanted to avoid. Afterword he could not accept them as a norm but did not want to exclude them. Quine's theory of knowledge kept too much of the Carnapian empiricist theory of meaning for Popper to take his view as a serious alternative to his own. And Popper rejected too much of Carnap's theory for Quine to view it as something other than a movement too far away from what he thought were the demands of logic, which he could not or would not distinguish from the demands of positivism. And, still worse, Quine could only carry out his project of remaining as close as possible to Carnap's broken theory of meaning, by severely limiting criticism, debate and growth by change by demanding close adherence to established doctrine. In order to secure meaning one had to prevent changes as far as one could. Quine simply ignored the adventurous in the practice of science and its history.

Quine's contribution to the collection of essays about Popper in Edward Schilpp's *Library of Living Philosophers* could have been the only direct confrontation they might have had. But it produced nothing of the kind. Quine chose as his theme an alleged paradox concerning confirmation raised by Carl Hempel. Hempel's paradox is that, if any black raven is partial evidence for the statement "All ravens are black", then any non-black non-raven should be partial evidence for the statement "All unblack things are non-ravens". But this is logically equivalent to "All ravens are black". This, Hempel and Quine say, seems odd. Quine says the alleged paradox can be avoided by distinguishing between the "projectability" of the predicates "raven" and "black" and the fact that "non-raven" and "unblack" are not "projectible". He presents this briefly as a minor comment, which he does not want to extrapolate on. Popper rejects this hypothesis: It is merely an attempt to provide some theory of what evidence is positive and what is not. This is a project which he finds worthless.

Popper gives a somewhat long reply about he handles this situation, which includes an explanation of how the popular fantasy that positive instances provide positive evidence could have arisen. But this no longer has anything to do with the difference between Quine and Popper.

The underlying difference between them is that Popper denies that there is any inductive logic, while Quine still seeks some positive view of positive evidence as some kind of substitute for logical inductive inference. Quine does not even say Popper's denial of inductive logic is false. And, indeed he never defended inductive proof. In fact Quine had no third position which would provide a theory of confirmation as evidence for the truth of some theory without any claim that there was inductive proof. He wanted confirming evidence without inductive logic but could never explain why he wanted that or how it could be possible.

In order to show the peculiarity of Popper's theory Quine could have used an example which Agassi discusses (Agassi 1978, pp. 6-7). Popper says that the statement "there is a unicorn in the zoo" is a scientific statement. But the statement "there is a unicorn" even though it follows from "there is a unicorn in the zoo" is not a scientific statement. On traditional views this would not have been the case. But Quine's example may have been preferable for him since many philosophers deem "inductive logic" a part of logic although Popper does not. He could thereby restrict his commentary to logic, including of course inductive logic, even if there is no inductive logic.

Popper took a significantly different path from that taken by Quine as he tried to show how rational discussion, which was not reducible to empirical research, could be conducted as part of the search for truth. The intellectual proximity of Carnap, Quine, and Popper was over as Carnap (and his disciple Quine) tried their best to limit rational discussion as a consequence of the continued adherence to the positivist view—or one close to it—that the only meaning sentences about the world had was their empirical consequences. Popper correctly saw the pressing need to explain how research could be rationally conducted. A theory of how research could be rationally conducted could not exclude the meanings of statements which are not reducible or not nearly reducible to their empirical consequences. Such research, which included the use of non-empirical statements, applied logic and could be part of science, and could be rational when it was not.

Popper's skeptical attitude toward non-empirical research had not ended with his failure to construct logic without assumptions. But he was nevertheless open to new possibilities of endorsing some kinds of non-empirical research. Popper's philosophy of science in the 1950s had its roots in his remaining sympathy for some aspects of logical positivism, even while he some time earlier rejected crucial aspects of this view, which were still strongly defended in differing ways by Carnap and Quine for the durations of their careers. This historical development of Popper's perspective led to crucial variations in attitudes toward non-scientific rationality between him and his excellent students in London in the 1950's, such as J.O. Wisdom, John Watkins, Paul Feyerabend, Joseph Agassi, Ian Jarvie, William Warren Bartley III and Imre Lakatos; but his students did not share Popper's skepticism toward it.

The failure of Popper's logic without assumptions: The extension of Popper's theory of research to include non-empirical statements.

The failure of Popper's logic without assumptions had at least two intellectually interesting consequences for Popper. These were the study of new relationships between logic and science and the extension of his theory of rational research to include the study of non-empirical statements. I have discussed above the first consequence and turn here to the second. Popper realized that the explanation of the growth of knowledge could not do without an explicit theory of how non-empirical background assumptions played roles in the development of science. But from the beginning of his research in the philosophy of science Popper had sought to limit the role of any non-empirical statements in scientific research. First he simply wanted to describe the use of logic in the study of empirical statements. Then, in the face of problems with this approach, he added methodological rules, whose status he failed to make clear. He still thought his theory of science needed no further assumptions. But when faced with the fact that no logic could be developed without the use of non-logical assumptions—the meta-language which needed to be used to study logic and the object language which set the context for the logic to be developed—he had to add the additional use in scientific methodology of non-empirical assumptions. This problem became a central aspect of his methodological research after the breakdown of his attempt to find logic without assumptions.

Popper also might have learned that, if the development of any set of valid logical inferences required the specification of some object language, there could be no universal logic. There had to be a unique logic for each linguistic context. Various logics had to be developed so that there would be one for each linguistic context. This makes logic a quite different field, than it had been before the work of Tarski and others. It also raises a meta-problem: Are there various ways to develop various logics? A central aspect of Popper's attempt to develop logic without assumptions was his claim that all previous approaches to develop logic relied on a specific method, which used background assumptions about logic as their foundation. But his attempt to develop an alternative method for the development of logic, which avoided this assumption, also broke down. Does that mean that the traditional method described by Popper is the only method? Or can methods vary? Popper never posed these questions. An apparent consequence of Popper's failure to develop logic without assumptions was that a logic, or various logics, may be needed. Each logic would be designed for its use in one scientific context. Popper also never posed these questions. Agassi did; I discuss his contribution below.

I turn here then to Popper's new research, which began with the role of non-empirical statements in science, but which quickly became the problem of the roles which non-empirical statements could play in the pursuit of truth. This was the one consequence which played an important role in research done by Popper in London shortly after the breakdown of his attempt in logic beginning the 1950's. His research became closely integrated with that of important students. The two most important figures in the beginnings of the new development of Popperian philosophy of science were Joseph Agassi and William Warren Bartley III. In his studies of Faraday's development of physical theory Agassi introduced metaphysics as the source of possibly excellent research programs for scientific research. This was a significant break with Popper's exclusion of any systematic influence of presumed non-empirical background assumptions in scientific research. The significance of this change was never made clear at the time. Popper insisted he was not against background assumptions and he could even concede that at times they might be useful. But, he added, whether they were useful had nothing to do with any systematic approach to scientific research. Agassi showed that Popper's theory was false: The use of non-empirical background assumptions did indeed have a systematic role to play in at least some significant scientific research. Agassi had offered more than a simple observation of the research of a single physicist, Michael Faraday, as it seemed at the time.

Agassi and Bartley also offered new and more general theories of rationality. These theories radically changed the Popperian philosophical approach. The new theories tried above all to explain how non-refutable hypotheses could be dealt with in a rational way, when there was no proof of any theory. Popper admitted that his best students were dealing with problems, which arose quite directly out of the progress he had already made, but he did not note that their substance was significantly new and differed from his. During this time he translated *Logik der Forschung* into English. And this translation illustrates a prime task he had set himself: He wanted to show that the view had defended in *Logik der Forschung* had been vindicated, as it was formulated in *Logik der Forschung*. This task could not be carried out, as has been explained here. Popper had improved his view and some of his improvements contradicted his earlier view. Popper was well aware of this fact but was determined to conceal it. This was a terrible intellectual, moral and political mistake. He could have said that he had learned from his mistakes, just as any good scientist does, according to the view he presented in *Logik der Forschung*. Instead he incorporated some changes and advances in his translation, which he insisted were not improvements of his former theory but were simply due to his quest for a good translation. One need look no further than the title of the English "translation" to see that this claim is false. The correct translation of Logik der Forschung is logic of research. But Popper translated *Logik der Forschung* as *The Logic of Scientific Discovery*. This was not the mistake of a translator, who did not quite know what the proper translation would be. When Popper wrote *Logik der Forschung* he presumed that he was writing about all research. When he translated the book into English he knew that he was writing about only a portion of research and that it would be good to call this specified research scientific discovery. This separated his view more clearly from those theorists—nearly all his competitors—who wanted to explain how correct theories were selected. So, he replaced “Forschung” or “research” with “scientific discovery” in order to make the English title conform more closely to the view he held during the translation, and not the view he had held when he wrote the book.

Further philosophical progress which grew out of Popper’s early philosophy of science was made in mathematics. J.O. Wisdom was the first to do this in a study of the historical role which Berkeley’s criticism of calculus had played in the history of mathematics (Wisdom 1939), though it is unclear whether at this early date his view had been influenced by Popper’s philosophy of science. This progress was followed by Lakatos’s theory of the refutation of mathematical hypotheses (Lakatos 1976) Wisdom’s contribution was, unfortunately largely ignored. Popper said that Wisdom’s sympathetic discussion of Freud went too far astray and he thereby ignored Wisdom’s excellent discussion of Berkeley’s place in the history of mathematics. Lakatos’s theory of refutations in mathematics, which built on Wisdom’s earlier research, was highly praised. But Lakatos made no mention of Wisdom.[172]

In regard to the fallibilist contributions of Agassi and Bartley, Popper claimed to have by and large said all that before. But his claims for priority were exaggerated to a high degree: Popper never wanted to concede that he had learned from his mistakes and especially not those which had their roots in logical positivism. For a short period of time Paul Feyerabend seemed to be a star follower of Popper. But Feyerabend’s break with Popper came swiftly as he defended his anything goes attitude. The sharp break with Lakatos came a bit later but very bitterly, as Lakatos surprised him with his quite poor but quite influential theory of research programs. Popper’s angry reaction is found in a letter he wrote to Lakatos about his break with Popper. Lakatos responded with a post card. Before this break Popper, Lakatos and Bartley had talked about working together on the philosophy of mathematics. Perhaps if this nasty break had not occurred Popper might have returned to research in logic and mathematics. But that never happened.

[172] Teun Koestier has made a nice study of the degree to which Lakatos’s theory can provide an accurate history of mathematics (Koestier 1991). He found that refutations do indeed occur in the growth of mathematical knowledge, but they are used to develop various aspects of mathematics, which then remain by and large unchanged as parts of mathematical knowledge.

Agassi on Popper's logic.

Outside of the of the critical reviews which appeared shortly after Popper published his series of essays on logic without assumptions, there is no significant discussion of Popper's treatment of logic that I know of, assuming that one does not view Lejewski's as such. As far as I know the only discussions that come into question as responses to Popper's research on logic are two essays by Agassi (Agassi 1978; 1982). Agassi presents a view of logic, which, he argues, is Popperian but which takes the discussion further. One aim of his discussions is to explain how Popper's advances in the theory of rationality and his own fallibilist theory of rationality, which builds on Popper's, are the best background framework for the development of "logics of". He hopes thereby to give an additional dimension to these theories of rationality and a contribution to the theory of how to do logic today. From this perspective, he argues, on the one hand, that not only are presuppositions for logic needed, the fallibilist theory of rationality now offers the best ones available, and that, when viewing the tasks of developing logics for various situations, one may usefully employ the fallibilist version of methodological individualism developed by Popper. This discussion goes beyond Popper's; Popper said nothing about this problematical but interesting development. I will turn to Agassi's discussion of "logics of" in the following section. In this section I explain Agassi's more general appraisal of Popper on logic.

Although Agassi views Popper as an important logician because he argued that there could be no inductive logic and introduced the discovery of error through the deduction of the falsity of some statements as a crucial part of logic, he does not provide a more comprehensive overview of the importance of Popper's contribution to logic. And, whatever Popper's importance for logic may be, he did not make any contribution to the field in ways other contributors, such as Bertrand Russell, Gottlob Frege, David Hilbert, Tarski, Lowehein-Skolem, and Gentzen, did. These logicians developed logical principles, had theories of how to avoid contradictions, analyzed what could be proven and what could not and much more. Popper provided no significant formal contributions to logic, as he might have done had his logic without assumptions been successful. But Agassi does not claim that Popper's importance for logic lies in such a contribution. Rather, his important lies in showing that and how logic is a study of dialectic, of how we learn by discovering errors, rather than a method for proving the truth of some statements from other statements.

Agassi's view ignores how Popper's six essays on logic without assumptions fit into his own philosophical development. Insofar as he speaks about Popper his analysis it is quite ahistorical. He does not describe Popper's perspective on logic only as it appeared after the failure of his attempt to present a new logic without assumptions, but rather how it appears to him and how it might have appeared to Popper. He does not, however, say that that is the case.

In another aspect Agassi's essays are historical. He elucidates many of the problems which have emerged in the development of logic, above all in the last part of the 20th century. He treats this development as a background for his fallibilist presuppositions for logic; they lay the groundwork for them. Agassi also presents Popper's view of logic as part of his ongoing critique of positivism. This critique began with Popper's first forays into the philosophy of science and lasted until the end of his career. But he ignores the historical fact, pointed out here, that Popper's view of logic without assumptions fits nicely with positivist views, because it separates logic from any problematical meta-language and any limiting object language. This result was crucial for logical positivists, who wanted to divide all meaningful statements into logic and empirical statements alone and to demarcate these two kinds of statements.

On the face of it Agassi's aim of explaining the importance of fallibilist theories of rationality for logic seems quite interesting and worth pursuing. But vis-á-vis Popper it has the weakness of ignoring his changing points of view and the fact that there is simply no coherent, comprehensive statement of Popper's view which was published before or after Agassi's essays were published. This lack has a great deal to do with Popper's changing perspective of logic. If one wants to be accurate, when discussing aspects of Popper's views it is scarcely possible to avoid mentioning in regard to each aspect of the discussion just what point in Popper's development of his views on logic one is referring to.

Agassi deals extensively with the need for background assumptions in the developments of logics of, that is, of logics which refer to some limited area, say, some language or mathematics or parts of mathematics. His discussion of many of the details in the recent history of logic which led to this perspective is excellent. But there are at least three aspects, which conflict with my portrayal of Popper's development.

As I have already pointed out, the first of these conflicts is historical: How did Popper's view emerge? One historical conflict between my view and that offered by Agassi is that Agassi presents Popper's philosophy as significant for logic because of Popper's assumption that rational inquiry needs to be investigated and that this inquiry must also treat the problem of how non-empirical statements should be handled in rational discussions. Agassi is, of course, right that this view of rationality was important for Popper's philosophy during the vast majority of his time in London. And it may serve as a contribution to the presuppositions of logic. But this view of rationality was not his view when he was developing his own logic without assumptions. Agassi stresses quite accurately that positivism is the theory that neither logic nor science makes any assumptions outside of their own domains. But this means that one cannot start with an analysis of Popper's treatment of logic without noting, that he invested enormous effort in his attempt to show how logic without assumptions was possible, that is, how two important aspects of the view which Agassi accurately calls positivist were possible.

Long before his attempt to construct logic without assumptions Popper had rejected positivist views of science for at least two reasons, that is, the positivists claim that inductive proof of some kind is possible and that the meaning of all proper statements is entirely empirical. But, as explained above, for a long time he sought to remain close to the positivists in regard to logic; when he discovered through the breakdown of his somewhat positivist view of logic without assumptions that he had to move farther away from them, he did so with fear and trepidation.

Agassi makes a significant claim about Popper's importance for logic. Popper, he says, did not view logic as simply the study of valid inferences which proved statements, as virtually everyone before him had. He claimed that the logic of science amounted to the retransmission of the falsity of a statement deduced from other statements to at least one of the statements from which it was deduced. This observation amounted as well to the introduction of the view that logic was not merely the study of valid inferences which proved statements, but also the study of valid inferences which showed that some statements were false.

The second conflict between my portrayal of Popper's treatment of logic and Agassi's arises, because Agassi treats the use of Popperian rationality as presuppositions for logic as an example of the use of methodological individualism, the theory of how individuals act rationally. This view of explanation in the social sciences was defended by Popper and has been further endorsed by virtually all his followers. But it is different from his theory of rationality as a dialectical process. Agassi uses this latter theory to explain his alleged movement from logic to logic of—something that never happened. He then treats the problem of providing a logic of rationality as the problem of developing a logic of the rationality of individual action. Agassi's two treatments of Popper's two theories of rationality, one being the theory of dialectics and the other of the rationality of individual action, create two views which cannot be integrated. I might add that I have long argued that Popper's theory of the rationality of individual action is false. Popper pointed out that the methodology of science was social. But he failed to apply his view of rationality as social to individual thought. This was an opportunity lost, which I have taken up (Wettersten 2006; 2007; 2010a; 2010b; 2011; 2012; 2013; 2014).

When one views rationality as one process in which various rules are followed in differing social contexts to solve problems, the view of how to do "logic of" may be improved. The problems posed are not those of individuals seeking to reach goals in specific social situations. They are theoretical problems in which one seeks useful rules which can be effectively followed in differing theoretical contexts. These rules should be critically evaluated to find the strengths and weaknesses of following them in those contexts for which they have been devised. Studies to this effect are an extension of the fallibilist theory of rationality advocated as a background for the inquiry into logics of, as Agassi has recommended. But they are not examples of individuals pursuing the solutions of specific problems to arrive at specific personal ends as Agassi's application of methodological individualism indicates they are.

What remains valuable in Agassi's discussion is his exposition of how both positivism—the theory that the meanings of all meaningful non-analytic statements are exclusively empirical—and logical positivism—the theory that neither logic nor science makes non-empirical assumptions—failed, how the developments of logic showed that there can be no universal logic, and what new problems have arisen from this development.

Agassi on logic today or logic of.

What are the problems of logic and what is its place in rational discussion today? Do we have some central problem of how a unity of the approaches to "logics of" may be achieved? It would seem Agassi thinks so, but it seems to me to be a potpourri. The central task seems to be to find interesting problems of logic of. At the moment we have no understanding of what, if anything, may be found which is valuable.

In regard to the existence of differing logics, Agassi notes that this situation is not the same as that concerning differing geometries. Non-Euclidean geometries were created not merely by dispensing with the parallel axiom of Euclidean geometry, but by also by substituting alternative axioms of parallel lines. But some changes in logic which may have seemed at first to create alternative logics such as a logic without the law of the middle—either p or not-p—or by failing to limit all statements to the alternatives of true or false but by adding, say, some third alternative, have been shown, according to Agassi, by Gödel to be subsystems of the logic they may seem to replace (Agassi 1978, 10). This fact may limit the significance of alternative logics of. They do not add new possibilities but present more limited ones.

There is one further theme in Agassi's discussion, which I have not yet mentioned. He emphasizes that the history of logic has been dominated by two theories of the nature of logic, that is, it is either deemed to be a matter of nature, a somewhat Platonic entity, or deemed to be a set of mere conventions. This latter alternative, he says, hinges on the sharp distinction between formal language and object language. And this distinction cannot be upheld when the construction of formal definitions need assumptions about object languages. Agassi points to Abraham Robinson's theory of infinite numbers and infinitesimals as a theory which can be neither natural nor conventional, but must have some other character (Agassi 1982, 476-477). He does not go further to explain how we might view such cases.

The alternatives which Agassi considers, that is, logic is either natural or conventional, are not the only ones available. On the view suggested here, according to which the study of logics of is the study of rules of procedure in specific contexts, we do not have to pose the question of whether they are natural or conventional. They are specific rules invented by scholars, which serve certain purposes better or worse. I have not examined Robinson's mathematical theory but perhaps it can also be so interpreted.

Agassi attempts to give a coherent overview of Popper's contribution to or, at times, perspective on, logic. But, due to the fact that Popper's view developed and changed from his early work on *Die beiden Grundprobleme* until his time in London, no such coherent or constant view can be found. Agassi takes no notice of the difficulties this poses for his portrayal. When we do take Popper's development into account, we can see a never ending attempt to find a proper place for logic in his philosophy of science and then more broadly in his critical rationalism.

I find it somewhat curious that Agassi reveals more insight into the problems of the aims of logic and the knowledge we can obtain from it than any other commentator on Popper's (evolving) perspectives. He places Popper's view well in the context of the development of 20th century logic; he poses well the problem of the nature of logical knowledge; he shows the connection between positivism and logical theory even though he entirely ignores the influence of positivism on Popper's own views. And his central theme, that is, the problem posed by the needed move in our perspective on logic from "logic" to "logic of" is made quite powerfully. My three criticisms are, one, this is Agassi's perspective; though it builds on Popper's research in rationality and logic, it is not Popper'; secondly, it erroneously applies methodological individualism to the study of logic, whereas a view of rationality as social is superior, and, thirdly, his ahistorical portrayal of Popper ignores Popper's own historical development and the importance of taking this into account when seeking to understand his philosophical results.

Conclusion: The Varied History of Popper's Relationship to Logic.

Popper began his career with his research in psychology and pedagogy. As a student he began to learn mathematics as well, and an essay on non-Euclidean geometry bolstered his knowledge of mathematics and of physics, especially of Einstein. But when he then turned to the philosophy of science in research, which appeared many years later as *Die beiden Grundprobleme der Erkenntnistheorie*, he still had limited knowledge of logic, even though his central thesis on the philosophy of science was the logic of science. After the publication of *Logik der Forschung* he learned more about logic, above all from Tarski who corrected the mistake he had then made as he called the logic of science modus tollens, when it should have been the retransmission of the falsity of a conclusion of a valid argument to at least one of the premises of the argument. He thereby, as Agassi has pointed out, changed logic; I would say he did so somewhat unwittingly. But for some time he said hardly anything about this change the central problems of logic.

Popper's relative silence about logic continued until he made his intensive effort to create a formal logic without assumptions. In a matter of a few years after its publication this attempt was shown to have failed; he once again said very little about logic. During his years in London he did offer lectures in logic which gave an historical overview of the change in the study of logic from deductive and inductive logic to deductive logic alone.

And he also pointed to the study of the logic of dialectics, of critical inquiry.

Agassi is the only scholar who has tried to bring a Popperian logic to life with his studies of "logic" and "logic of". This research does not offer a new logic, but it does offer a new perspective on logic. It has not, however, been taken up again.

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Interlude

After Popper discovered that problems posed by Tarski's conclusion that any logic has to include a meta-language and an object language could not be avoided, his treatment of logic was not adequate. He simply explained the problems away: they did not matter after all. Joseph Agassi tried to do better with a theory of 'logics of'. But he made the mistake of interpreting them as cases of rational action as described by Popper's false methodological individualism. But it is possible to apply Tarskian semantics in a fallibilist methodology.

2c. The applications of Tarskian "logics of" in science revisited.

Abstract. Popper's discussions of logic pose interesting problems. He offered a new fallibilist approach to the applications of logic in the methodology of science. But the failure of his initial attempt to replace Tarski's approach to logic with an alternative left the problem of how logic can be applied in science open. Agassi tried to fill the gap. But Agassi's failure to note changes in Popper's view of logic and his application of methodological individualism to interpret the applications of 'logics of' led to a dead-end. When the rational actions of individuals are viewed as social and Tarski's logical theory of semantics are viewed from a fallibilist perspective, the productive use of Tarskian semantics in scientific methodology can be explained.

The background to the problems discussed in this essay.

The main problem of this essay is, how can Tarski's logic be applied in the quest for truth? This problem is raised by the fact that only under limited conditions can it be applied. These limited conditions are above all set by the object languages or models built into the definitions of logical terms. This view of logic flies in the face of the thesis that logic must be universally applicable to all languages in all situations. This traditional aim for modern logic was above all set for 20th century philosophy of science by Rudolph Carnap.

Before turning to a direct discussion of problems and theses defended in this essay, two previous discussions of these problems will be briefly presented. One of these is Popper's attempt to develop a new and unified comprehensive logic as an alternative to Tarski's. Popper hoped to remove the need for the specification of object languages in logical definitions. A second is John Etchemendy's much later attempt to show that the universal logic endorsed by Carnap and sought by Popper for a while must be achieved if the proper aims of logic today are to be achieved.

Shortly after Popper learned Tarski's logic he saw that this logic was the best he had encountered, perhaps above all due to Tarski's theory of true statements. But he also saw that its application posed problems for his own theory of science. This theory of science, Popper then thought, required a universal logic which could be applied in all contexts without reservation, just as Etchemendy, following Carnap, later claimed. This was then a standard perspective; Etchemendy's essay is an indication that it still is.

But Tarski's logic showed how this assumption that the aim of logic must be to produce a universally applicable logic was false. Tarski showed that any formal logic had to include both a meta-language and an object language in order to precisely define fundamental concepts such as logical inference. But an object language cannot be universal. It thereby sets the limits for that logic, which presumes it as part of its basis. Each and every one of the varying logics on this view must have its own basis.

Popper set out then to show that he could construct an alternative logic without setting any object language, which would define, and thereby limit, the range of its application. In his seven essays on logic without foundations Popper tried to do this. But he failed, as three leading logicians quickly pointed out. Popper then changed his stance in regard to the problem of applying Tarski's logic in the pursuit of truth. He proposed that the differences in 'logics of', that is, logics with object languages in the development of their definitions and theorems, could be applied quite widely, perhaps universally, because the ranges of application specified by their differing object languages were not in any significant way different.

By far the most widely adopted reaction to the alleged limitations on the range of applications found in Tarskian 'logics of' has been to suggest that Tarski had the aim of providing a universally applicable logic, such as that sought by Carnap, but that he had failed to achieve his goal. One important task then facing logicians adopting Tarskian methodology in logic, which they virtually all do, is to show how logics, or some specific logic, can be developed in such a way as to render its application universal. In order to better explain why this dominant alternative cannot be maintained, I will discuss the failed attempt by Etchemendy to turn Tarski's theory of logical inference into a theory of how all logical inferences of all statements about the world regardless of the language used to form these statements can be properly defined.

Etchemendy maintains that his central problem of finding a (universal) theory of logical inference is partly due to the alleged fact that Tarski's theory of logical inference is nearly universally accepted as a correct definition of logical inference, but in fact, Etchemendy says, it cannot be so interpreted. The reason, that it cannot now be interpreted in this way, is that it cannot now be applied in such a way that all statements about the world through its application can be logically inferred from some other statements about the world. The reason this goal cannot be achieved is that some substitutions of one word by another word which should be acceptable, say, the substitution of the name of one object for another apparent name of the same object, can lead to a change in the validity of the inferences in which the two sentences are found. This difficulty should, he claims, be avoided.

The first question which jumps out of this assertion is: What would Tarski say about this claim, that his theory is inadequate for this reason? Tarski would say that his theory was not intended to solve this problem, that this problem is not a problem within the domain of logic, which is above all mathematics. But Etchemendy's problem is a problem of the logic of science, whereas Tarski observed that there can be no logic of science, even though perhaps some statements of some interest may be made about how scientific knowledge is obtained. This view is explicitly stated in his *Introduction to Logic and to the Methodology of Deductive Sciences*. (By 'deductive sciences' he means mathematics.) But this view and this book are not mentioned in Etchemendy's essay.

Etchemendy does not explicitly say that Tarski's view of the problem he is discussing is false, but implicitly he does. He does this implicitly by claiming that this problem is a problem of the 'founders of modern logic'. He implicitly presumes that any problem these alleged founders claimed should be solved by modern logic, needs to be solved by modern logic. These alleged founders made, he presumes, no mistakes in this regard. His founding fathers consist of two individuals: Bernard Bolsano and Rudolf Carnap. He concedes that Tarski was unaware of Bolsano's research when he wrote his 'The Concept of Logical Inference'. He simply presumes that Tarski was trying to solve at least some of the same problems as Bolsano and these common problems included the problem he claims Tarski failed to solve.

The case of Carnap is somewhat different. Tarski did note in his article on logical inference that Carnap had attempted to develop a theory of logical inference before he had done so. But he quickly argues that Carnap's proposal needs improvement and that any attempt to do so would be quite complex. Tarski does not go into details, but lack of simplicity is an indication of inadequacy for Tarski. And Tarski nowhere claims he is trying to solve that problem, which Carnap wanted to solve and which Etchemendy says must be solved, that is, the problem of explaining how logical inference may be universally applied.

One of the fundamental reasons that Etchemendy gives for writing his book is that logicians nearly unanimously not only endorse Tarski's theory of logical inference, but they also view it as a success: It resolves, they allegedly think, the problem of precisely stating what logical inference is. But, Etchemendy says further, this is not true, because Tarski failed to solve the problem of how all sentences describing the world can be parts of singularly defined logical inferences, either as premises or as conclusions. This claim about logicians presumes, either that these numerous logicians have no knowledge of Tarski's theory of logic or they think that Tarski's argument concerning this problem comes to a false conclusion. He claims, then, that they are either ignorant of Tarski's view or muddled about what Tarski did. I find this unbelievable; but Etchemendy gives no examples much less evidence for it; I will not seek to show that his claim is false.

Questions of the relevance of Popper's logic and of his comments on logic.

Popper's philosophy of science is categorically rejected by most philosophers and scientists; perhaps the most common reason for this widespread rejection is that stated by Bertrand Russell: It is not deemed good enough. (Wettersten 1985) But this same philosophy of science is deeply admired, defended and developed by a relatively large minority of scholars. They see it as an opening to new paths forward. In sharp contrast to this positive reaction to his philosophy of science Popper's logic is defended by virtually no one. The critical comments on it by first-class logicians left Popper with seemingly no place to go (McKinsey 1948; Curry 1948; Kleene 1948; 1949-50). Popper had made a bold attempt to revolutionize formal logic. But he failed in his attempt to offer an alternative to Tarski's approach: Popper could not show how logic could be formally developed without the use of a meta-language which included an object language, as he had intended to do (Popper 1947; 1948a; 1947a; 1947b; 1948b; 1948c); Tarski claimed this combination was unavoidable (Tarski 1936; 1956a; 1956b). Popper thereby failed to show how formal logical systems could be universal. After the criticisms of his attempt were published, he never came back to it. He simply sought to explain away the problems he had hoped to avoid with his logic without assumptions. These problems were, he argued, not so significant that they limited the applications of 'logics of' in any situation.

It is worth noting in passing that Czeslaw Lejewski redid part of Popper's logic in order to present a modified version of it, which would avoid some criticisms (Lejewski 1974); but he did not try to achieve those aims Popper had set himself, as, in his response to Lejewski, Popper pointed out (Popper 1974b). Lejewski merely suggested it needed some additions and modifications in order to meet more of the recognized Tarskian standards for correct research in logic.

Only one philosopher, Joseph Agassi, has posed the problems of which consequences of Popper's failed attempt might be significant for (1) the development of alternative formal logics, for (2) the identification of which consequences of it ought to be taken into account, when we seek to understand the problems of logic today, and for (3) an explanation of how Popper's fallibilist theory of rationality can improve our understanding of logic (Agassi 1978; 1982). Though there are a few essays which deal with some aspects of Popper's logic, such as that by Nimrod Bar-Am (Bar-Am 2009), Agassi is the only commentator who seeks to draw a broad picture of the significance of Popper's research in logic.

I pose here the questions of to what degree Agassi made his case, to what degree is the significance of his claims limited, and to what degree did he make any mistakes. Does viewing logic today in view of Popper's statements about logic, in view of his failed attempt to reshape formal logic, and in view of his fallibilist theory of rationality present us with any new and valuable framework for research in logic and its applications?

The reason for posing these questions is that Popper's approach to logic was, as Agassi points out, quite revolutionary in that he eliminated so-called inductive logic from the field and recast this field as the study of dialectics (Popper 1963b). The aim of logic should lie, Popper said, not in the formation of proofs of statements about the world but in the discovery of the existence of mistakes in our assumptions about the world. Agassi points to one change in problems concerning the applications of logic, which is of some significance for the appraisal of Popper's contribution. This change is the adoption of the observation made by Tarski, which was endorsed by Popper only after his failure to develop his own logic, that any logic must be built in a meta-language which contains an object language, that is, a language used to describe the world. This theory of logic has the consequence that no logic is universal; each logic is a 'logic of' some specified context. Can we usefully design logics for interesting and/or useful applications in various contexts? Does Popper's reformulation of the purpose of logic, from the creation of methods of proof of statements about the world to the discovery of methods of finding mistakes in our assumptions about the world, set a new framework for the productive pursuit of 'logics of'? Popper never posed these questions; Agassi does.

Agassi on Popper's logic.

The only discussions that come into question are, then, two essays by Agassi (Agassi 1978; 1982). Agassi presents a view of logic, which, he argues, is Popperian in both its view of the purpose of logic and of tasks, which logic can promisingly formulate today for its advance. In his attempt to take the discussion of Popper's relevance for logic further, he resets the framework for studying logic by noting the challenges researchers face today in, above all, the application of logic in various contexts and/or in new developments of formal logic.

One aim of his discussions is to explain how Popper's advances in the theory of rationality, carried further by his own fallibilist theory of rationality and by William Warren Bartley II (Bartley 1962; 1964), offers the best background framework for the development of Tarskian 'logics of'. How to make progress by developing 'logics of' is one central theme of his discussions. In sharp contrast to Popper's formal essays on logic without assumptions he argues that (1) presuppositions are needed for each variant of formal logic, that (2) the fallibilist theory of rationality now offers the best ones available for this purpose, and (3) that when describing the tasks of developing logics for various situations, one may usefully employ the fallibilist version of methodological individualism, as it was developed by Popper and further defended by his followers. This approach should give a progressive theory of the nature of logic today by relating each 'logic of' to specific aims of individuals.

Agassi views Popper as an important logician because he argued that there could be no inductive logic and introduced, as a crucial part of logic, the discovery of error through the deduction of the falsity of some statement or statements, thereby showing that some other statement or statements are false. He does not, however, provide a more comprehensive overview of the importance of Popper's contribution to logic itself. He does suggest that the significance of Popper's contribution has not been appropriately recognized, because Popper changed the understanding of the roles which logic should play in the pursuit of truth (Agassi 1978, 7). In any case, whatever Popper's importance for logic may be, he did not make any contribution to the field in ways many other contributors, such as Bertrand Russell, Gottlob Frege, David Hilbert, Alfred Tarski, Lowehein-Skolem, and Gerhard Gentzen, did. These logicians developed logical principles, had theories of how to avoid contradictions, analyzed what could be proven and what could not, and much more. Popper provided no significant formal contributions to logic, as he might have done had his logic without assumptions been successful. But Agassi does not claim that Popper's importance for logic lies in such a contribution. Rather, according to Agassi his importance lies in showing that and how logic is a study of dialectic, of how we learn by discovering errors, rather than a method for proving the truth of some statements while assuming other statements are true. He opens the question of whether Popper's formal logic might lead to new knowledge but does not indicate how this might be possible.

A significant weakness of Agassi's view is that he ignores how Popper's six essays on formal logic without assumptions fit into his own philosophical development. Insofar as he speaks about Popper, his analysis is quite ahistorical. He does not describe Popper's perspective on logic as it appeared after the failure of his attempt to present a new logic without assumptions. Rather, he describes how it appears to him and how it might have appeared to Popper throughout his career. He does not, however, say that that is the case. He also does not say that Popper's views on this point ever changed. But they did. This failure to describe the changes in Popper's views of logic leads quite directly to problems in his analysis of the possible importance of Popper's theoretical contributions to logic: They lead to an attempt to unify competing and contradictory views. This prevents him from arriving at his sought-for coherent perspective.

From a somewhat different point of view Agassi's essays are indeed historical. He elucidates how many of the problems within logic have emerged in the last part of the 20th century. He treats this development as a background for Popper's fallibilist presuppositions for logic; these new problems, he implicitly suggests, lay the groundwork for Popper's problems and perspective. Agassi also presents Popper's view of logic as part of his ongoing critique of positivism: When logic is properly understood one cannot reduce all knowledge to empirical statements and logic alone. This critique began with Popper's first forays into the philosophy of science and lasted until the end of his career. But Agassi ignores the historical fact that Popper's view of logic without assumptions would have fit nicely with some aspects of positivist views, because it sought to separate logic from any problematical meta-language and any limiting object language (Kraft 1974). This separation was crucial for logical positivists, who wanted to divide all meaningful statements into logic and empirical statements alone. Popper, of course, wanted no such thing. From the beginning of his research in the philosophy of science to the end, he maintained that only empirical statements should be part of science, but non-empirical statements can be meaningful.

On the face of it, Agassi's aim of explaining the importance of fallibilist theories of rationality for research in logic seems worth pursuing. Fallibilist theories can help in the formulation of useful problems today. The justificationist alternatives, which seek a universal and proven logic, are quite simply false. Agassi deals extensively with the need for background assumptions in the development of 'logics of', that is, of logics which refer to some limited area, say, some object language which could be some parts of mathematics. His discussion of many of the details in the recent history of logic which have led to this perspective is excellent. But there are at least two aspects of his discussion, which do not adequately portray Popper's logic and its relation to his philosophy of science.

As I have already pointed out, the first of these conflicts is historical: How did Popper's view emerge? Agassi presents Popper's philosophy as significant for logic, because of Popper's assumption that rational inquiry needs to be investigated and that this inquiry must also treat the problem of how non-empirical statements should be handled in rational discussions. Agassi is, of course, right that this view of rationality was important for Popper's philosophy during the vast majority of his time in London. And it may serve as a contribution to the identification of significant presuppositions of logic. But this view of rationality was not his view when he was developing his own logic without assumptions. Agassi stresses quite accurately that positivism is the theory that neither logic nor science makes any assumptions outside of their own domains. But this means that one cannot start with an analysis of Popper's treatment of logic without noting that he invested enormous effort in his attempt to show how logic without assumptions was possible, that is, how two important aspects of the view which Agassi accurately calls positivist could be possible.

Long before his attempt to construct a formal logic without assumptions Popper had rejected positivist views of science for at least three reasons, that is, the positivists claim that inductive proof of some kind is possible, that the meanings of all statements describing the world is entirely empirical, and that knowledge can be explained by analyzing how words have meaning. But, as explained above, for a long time he sought to remain close to the positivists in regard to logic: Logic should be independent and universal. But the breakdown of his view of logic without assumptions required that he move even farther away from the positivists. He did so with some fear and trepidation: He did not want to call into question his theory of the demarcation of science as refutability and, therefore, did not want to find a place for non-empirical sentences in science. In his studies of research programs Agassi did find such a place. But his agreement or conflict with Popper was hardly ever spelled out.

A second conflict between Popper's treatment of logic and Agassi's arises, because Agassi treats the use of presuppositions for logic as an example of the use of methodological individualism, that is, the theory of how individuals act rationally. This view of explanation in the social sciences was defended by Popper at the time he developed his logic, and it has been further endorsed by virtually all of his followers. But it is different from his theory of rationality as a dialectical process, which takes preeminence in his discussions of logic. Agassi uses this latter theory to explain Popper's alleged movement from logic to 'logic of'. But this movement merely amounted to Popper's concession that he could not avoid 'logics of' combined with a claim that this view of logic posed no serious problem for the applications of 'logics of'. Agassi then explains why a 'logic of' should be developed as a logic of the rationality of individual action. Agassi's two treatments of Popper's two theories of rationality, one being the theory of dialectics and the other of the rationality of individual action, create two views which cannot be integrated. Popper pointed out that the methodology of science was social. But he failed to apply his view of rationality as social to individual thought. This was an opportunity lost, which has been taken up by Wettersten (Wettersten, 2006; 2007; 2010a; 2010b; 2011; 2012; 2013; 2014).

When one views rationality as consisting of various processes in which various rules are followed in differing intellectual contexts to solve problems, the view of how to do 'logic of' may be improved. The problems posed are not those of individuals seeking to reach well-defined goals in specific social situations. They are theoretical problems in which one seeks useful rules, which can be effectively followed in differing theoretical contexts. These rules should be critically evaluated to find the strengths and weaknesses of following them in those contexts in which they possibly could be applied. Studies to this effect are an extension of the fallibilist theory of rationality advocated as a background for the inquiry into 'logics of', as Agassi has recommended. But they are not examples of individuals pursuing the solutions of well-defined problems to arrive at specific personal ends, as Agassi's application of methodological individualism indicates they are.

Agassi emphasizes that the history of logic has been dominated by two theories of the nature of logic, that is, it is either deemed to be a matter of nature, a somewhat Platonic entity, or deemed to be a set of mere conventions. This latter alternative, he says, hinges on the sharp distinction between formal language and object language. And this distinction cannot be upheld when the construction of formal definitions need assumptions about object languages. Agassi points to Abraham Robinson's theory of infinite numbers and infinitesimals as a theory which can be neither natural nor conventional, but must have some other character (Agassi 1982, 476-477). He does not go further to explain how we might view such cases.

But the alternatives which Agassi considers, that is, logic is either natural or conventional, are not the only ones available. Agassi fails to find the third alternative he seeks, because he is trying to integrate Popper's formal studies of logic without assumptions and his own view of the presuppositions of logic. On the view suggested here, according to which the study of 'logics of' is the study of rules of procedure in specific contexts, we do not have to pose the question of whether they are natural or conventional. We may simply view them as specific rules which serve certain intellectual purposes for better or worse.

Three aspects in Agassi's discussion which remain valuable are (1) his exposition of how both positivism—the theory that the meanings of all meaningful non-analytic statements are exclusively empirical—and logical positivism—the theory that neither logic nor science makes non-empirical assumptions—failed, (2) his portrayal of how above all Tarski's recent developments of logic show that there can be no universal logic, and (3) his observation that new problems have arisen out of this development.

Agassi on logic today or, on "logic of".

Agassi's discussion returns closely to Popper's original problem, that is, the problem of finding a methodology for the development of alternative theorems in logic. As Agassi points out, there is a need for the use of a meta-language in any development of theorems in logic and the consistency of this meta-language cannot be proven; any attempt to do so will face never-ending regress. He also points out that all fundamental terms used in logic cannot be defined. Negation is an example. We can specify how statements which include negation can be properly translated into other statements with the same meaning, but this does not give us a determination of the meaning of '—'.

At least one central and unifying task is to find various interesting problems of ‘logics of’. But at the moment we have no sufficient understanding of how interesting alternatives, if any, may be found. In regard to the existence of differing ‘logics of’, Agassi notes that this situation does not offer the same opportunities as the discovery that Euclid’s parallel axiom could not be proven given his other axioms. Non-Euclidean geometries were created not merely by dispensing with the parallel axiom of Euclidean geometry, but by also substituting alternative axioms of parallel lines. This created non-Euclidean geometries. But some changes in logic, which may have seemed at first blush to create alternative ‘logics of’ such as a logic without the law of the excluded middle, have been shown, according to Agassi, by Gödel to be subsystems of the logic they may seem to replace (Agassi 1978, 10). This fact limits to some uncertain degree the potential significance of alternative ‘logics of’. At least some of the time ‘logics of’ do not add new possibilities, but merely present more limited ones.

Difficulties such as these three—the impossibility of proving the consistency of needed meta-languages, the impossibility of defining the meaning of some terms such as negation, and the fact that some new logics of are merely limited cases of known logics—mean that all knowledge obtained by developing theorems in logic is conjectural. As Agassi has explained, such knowledge has no unquestionable basis. In this situation one may render the search for limitations in ‘logics of’, resulting from false assumptions, found in specified object languages, a possibly significant part of the study of the application of logic. This perspective might open up new and/or interesting problems. Various ‘logics of’, each of which might have some value, seem at least in principle possible, even though we do not have any good ideas of how to proceed from where we are now. If there cannot be a single foundation for all of logic, then there may be various foundations which may or may not be systematically unified in some unspecified way.

How applications of Tarskian semantics in scientific methodology work.

A possibility of dealing with “logics of” is that we may find problems in applications of specific ‘logics of’, which are consequences of mistakes about the nature of the world which occur in object languages. This is an extension of an approach to learning from argument, which was proposed a number of years ago. It has been suggested that in practice learning from argument is normally fundamentally different from the two leading theories of how we learn from argument. According to one leading theory we learn from argument by proving statements. This view is false because we cannot prove any statement. According to the second leading view we learn from argument by proving that some statements are false: The falsity of some statement shows that at least one of the statements in some purported proof of it is false. This view may also assume that the logical inferences in such cases cannot be called into question. And this is false, as the analysis of the lack of a demonstrated foundation for 'logics of' shows. We may, then, also learn from 'logics of' when we seek to find limits to their applications, as we have traditionally done in regard to applications of logic.

We have learned from Tarski that all logics are 'logics of', that is, their development requires their formulation in a meta-language which contains some object language. The application of any logical theorem may, in principle, lead to arguments which appear valid, but which may be mistaken or merely plausible, when the consequences of its use in the context of some object language are better understood. We do not then necessarily find some problem in the theory of logical inference. Rather, we can presume that the theory of logical inference is proper when formulated in the context of some particular object language, but that the assumptions we make about the formulation of the theory of logical inference with some other object language are false. They may be false because we erroneously presume that contradictory statements in our presumed object language are consistent or because statements about the world found in our object language turn out to be false. The discovery of such errors may be made by simple observation but their existence might also be rendered clear by the discovery of errors in logical inferences. The applications of 'logics of' then are not merely the quest for proofs. They are also the quest for the discovery of mistakes.

Contrary to Tarski's claim, that logic cannot be usefully applied to the theory of knowledge, this quest means that it can be. Tarski did not consider this a possibility, because he limited the purpose of logic to the discovery of true theorems. He could still develop logic as the logic of deductive systems, that is, of mathematics. This limitation enabled him to limit the specification of object languages to precise formulations; it would not lead to interpretations of rules in the metalanguage of inference which would not lead to true results, that is, which would not be proofs. By rejecting this sharp limitation placed on the specification of object languages, we open up the application of 'logics of' to the pursuit of true non-mathematical statements about the world. We do not do this merely by seeking proofs of statements about the world, but even more importantly by the seeking to discover false assumptions about the way the world is.

The proposal posed here for using the discovery of invalid inferences, which are examples of the use of a rule of inference, presumes a relationship between the metalanguages of logics and the object languages associated with them. Tarski argued that each object language should be simply, clearly and precisely identified. He carried out this view successfully. But it leads to the limitation of logics to 'logics of' mathematics: Only there is such a specification possible. On the view proposed here the metalanguages of logic are associated with specific object languages, but they are not specified so simply and precisely as on Tarski's approach. This decrease in precision is needed in order to expand the kinds of objects languages which may be associated with metalanguages containing logical inferences. But some specification is nevertheless needed.

Tarski himself has claimed that there cannot be any semantical theories of scientific theories. His central reason for this claim is that the construction of semantical theories requires that the relevant object theory requires a clear, simple and precise formulation. It is possible to construct mathematical object theories which meet these high standards, but it is not possible to do so for scientific theories. Among the reasons why this is so are first, that scientific theories are always partial. They are being developed to increase their descriptive capacity. Their relevant descriptive terms cannot then be simply, clearly and precisely identified. A second reason is that the application of Tarskian semantics to science is not possible, because it would not be consistent with the unity of science, which, he thought, must be preserved if science is to achieve its aim of finding the truth.

But neither of these reasons must prevent the application of Tarskian semantics to scientific theories. Neither the insistence that Tarskian semantics would only then be applicable in scientific research if each object language is simply, clearly and precisely formalized nor the insistence that Tarskian semantics cannot be applied in science if it does not unify science in one formalized object language is correct. Tarski held to these very high standards in his development of the logic of mathematics. But that does not mean that only if they are upheld, as they cannot be in science, that Tarskian semantics cannot be successfully applied in science.

The purpose of Tarskian semantics is to express connections between expressions in a language and the objects and states of affairs referred to by these expressions. Examples of such connections are denotation, satisfaction, definition and truth. The question, then, of whether Tarskian semantics can be usefully applied in scientific research is whether connections between expressions in a scientific language and the objects and states of affairs referred to by these expressions can be described in ways which may bring scientific research forward.

The argument that this application is possible and useful begins by rejecting the high claims for results which Tarski claims must be fulfilled to apply his semantical approach in scientific research. Instead of insisting on complete and near perfect results one must see value in piecemeal improvements of scientific theories. These piecemeal improvements may render somewhat more precise the objects or states of affairs denoted by expressions, may single out the erroneous assumption that some terms can satisfy some conditions, and or find and remove ambiguities in the definitions given certain terms or expressions. Such piecemeal improvements would not render any theory or any discussion perfect; but they could improve the explanatory power of some scientific theories. And that would suffice to render the applications of Tarskian semantics in scientific research useful.

The never-ending pluralistic realism of science: Examples.

The application of Tarskian methodological semantics in scientific research presumes that proper scientific research aims at finding realistic explanations of the nature of the world. It also presumes that there is no achievable final description of the world. Rather, there are always competing explanations and all such alternatives can benefit from improvement. Helping in the quest for such improvements is the aim of applying Tarskian methodological semantics in scientific research.

Let us look at a few examples from the history of science to improve our picture of how this may play out in science. A constantly repeating problem in the history of science has been the existence of competing explanatory theories. Three very large and often referred to examples of such a problem are (1) the divisions which occurred between Lavoisier and the defenders of the phlogiston theory such as Joseph Priestly, (2) the division between the atomistic theoretical framework of Newton and the wave theoretical framework of Michael Faraday and (3) the conflicts between wave theory and particle theory in quantum mechanics.

These conflicts have virtually always been treated as examples of differing and competing theories of the (metaphysical) nature of the world. This perspective is true. But these competing theories also embody differing and competing languages. The question posed here, then, is whether Tarskian semantical studies of such alternative languages can be useful in improving theories facing such competitive languages and/or whether they can help indicate which of the competing languages provides the best basis for that theory with the better explanatory power.

Before proceeding it is useful to say a few words about the object languages to which the Tarskian methodological semantical approach should be applied. These languages are to varying degrees partial. And they cannot be so developed that they can meet the high standards Tarskian meta-mathematical models meet. The relevant languages are, however, identifiable by some central words and assumptions which they make, as the following examples indicate. The purpose of the application of Tarskian methodological semantics is to render the use of these terms somewhat clearer and somewhat more precise. The objects which some words denote and the states of affairs which they describe should thereby be better identified. And this should improve their critical appraisal. Are they, at some point in time, the best available descriptions of some aspects of the world?

Let us proceed with a short analysis of the dispute between Lavoisier and Priestly. Each of these thinkers held a conservative methodological theory, that is, they believed that inductive inferences proved the correct empirical theories to be true. And each agreed that Lavoisier's new innovations in regard to the nature of chemicals contradicted the traditional phlogiston theory. Lavoisier argued that phlogiston did not exist and, that many other chemicals besides the five entities postulated by the then dominant phlogiston theory—earth, air, fire, water and phlogiston—as the only entities out of which the world was made, did exist. Lavoisier and Priestly each thought that the assumptions made about the world by the other had to be false. Scientific methodology, they thought, demanded that.

But each theory was designed to explain the same general states of affairs that the other explained. From a Tarskian methodological semantic point of view[173] one could state the rules in a metalanguage which stated how

[173] I call Tarski's semantic approach 'methodological' in order to make clear, that the application of his semantic research in science requires the removal of his

particular statements in each language were connected to specific states of affairs. One can further presume that the stated connections identified by these rules could be examined for their clarity, simplicity, and precision. A result, which could show that rules for one language better meet these semantic standards than some alternative, would be a strong argument for preferring that language. A further task which these metalinguistic rules can be used to carry out is to determine which objects particular words in some particular language denote. In this case the word 'phlogiston' denotes one of five elements out of which the world is allegedly made. Lavoisier denied that such an object existed. In the case of Lavoisier's theory there are words denoting at least two kinds of gasses, one of which burns (burnable gas) and another of which does not (fixed gas). As a defender of the phlogiston theory Priestly denied that such objects existed.

A task, then, for Tarskian methodological semantics would be to determine whether there would be clear, simple and precise metalinguistic rules, which determine the relations between the words in each language with objects these words are intended to denote. As a matter of fact it became clear to chemists, that connections between Lavoisier's words and particular states of affairs and particular objects were clearer, simpler and more precise than the connections between Priestly's words and particular states of affairs and objects.

The explicit use of Tarskian semantics would overlap with some methods already widely used in the history of science. An interesting question, then, is whether the explicit use of Tarskian methodological semantics could refine and improve these methods. Could it give scientists some better tools which they could use to make theories clearer, simpler and more precise? And, if that is possible, would scientists have better tools for conducting their discussions about which theories are better than their competitors or which versions of which theories are superior?

Let us turn, then, to the second example mentioned above, that is, the enormous difficulties Faraday faced as he increasingly expressed his view about electricity in a wave language, which sharply contradicted the established and widely deemed proven Newtonian atomistic language. Agassi has excellently described the difficulties Faraday faced in describing electrical processes in a partially but significantly newly developed language. His contemporaries had serious problems understanding just

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assumptions about the near perfect results which he thought his approach to semantics should bring about.

what it was that Faraday was claiming (Agassi 1971). But these problems did not arise simply because Faraday made innovations in the language. He did this explicitly as when, for example, he sought suggestions from Whewell for new words, which would improve the clarity of expressions in his theory. This was, indeed, a rather primitive semantical approach. But on Agassi's portrayal of Faraday's research, Faraday's problems also arose due to his own uncertainty about just how he could effectively improve the language used to describe electrical states of affairs and to do so in such a way that his colleagues, critics and competitors could better understand his own theoretical innovations.

In his own language Faraday tried at times to remain as close as possible to the dominating atomistic theory. On the one hand he seemed to think that doing that would help him to be understood. On the other hand he believed that scientific theories were inductively proven, as a private note of his mentioned by Agassi, in which he highly praised the inductivist methodology of John Herschel, shows. In short he needed and lacked a Tarskian semantical methodological approach, which might have enabled him to make the changes in the connections between words and objected and/or states of affairs far more explicit and far easier to understand than the ad hoc changes he employed; these were intended to be new but were in part made arbitrarily in order to conform with established usage. This led to confusions about just what was being said.

A third example from science, which offers an indication about the possible usefulness of Tarskian methodological semantics, is quantum theory. This example indicates that it is not merely the case that Tarskian methodological semantics could have been of some use in the history of science, but also that fundamental problems which can be tackled to some degree by its use remain: Fundamental problems concerning the nature of the world continue to be and will always be important.

In order to make my case I will discuss Manfred Stöckler's analysis of Popper's attempt to defend realism in quantum theory (Stöckler 2019). Stöckler's discussion is relevant for my case that scientific methodology always involves the formulation of new theories expressed in new languages, because he seeks to explain away—as far as he can—the view that quantum theory offers an example of such a development. He expresses as strongly as he can the view that agreement among scientists is far more extensive than it has often been portrayed.

Stöckler portrays Popper's efforts with some sympathy, but he offers as well various observations as to why they did not have any wide ranging influence on quantum theory. He does take Popper's propensity theory of probability as the best contribution Popper made to the discussion. My purpose here, however, is not to appraise Stöckler's sympathetic and critical discussion of Popper's attempts to contribute to the discussion of difficulties posed by quantum theory. Rather, Stöckler portrays his own view of the central problems facing physicists seeking to improve quantum theory today. I also do not here say anything about the degree to which his alternative offers an interesting or valuable program for the way forward. I merely want to point out that his presentation of his program makes assumptions which place limits on a sharp distinction between Bohr's Copenhagen interpretation and Einstein's desire to find innovations, which would give it a clearer realistic substance. And, his assumptions move in the direction of a Carnapian approach to science, though he himself makes no such explicit claim.

We may start with Stöckler's reading of the history of the methodological discussion of quantum theory. He maintains that, not only was the division between Bohr's Copenhagen reading of the theory and Einstein's not so strong as sometimes maintained, but also that the Copenhagen interpretation offered a solid basis for all physicists from the beginning. This is so, he says, because the results of the commonly accepted theory, if not its further development, were explicit and clear. There was never any cause to call them into question; they offered a solid and lasting empirical base for all research into quantum theory, whatever methodological point of view one had and whatever direction such research might take.

My problem with this claim is that it presumes not merely that a series of results had to be taken into account—that is true—but that they would not be changed by theories which did that. One cannot say without some erroneous positivist assumptions that explanation of given descriptions of states of affairs will not change assumptions about the nature of the substances used to describe them. They may or may not do that. But progress in science will not always be served by clinging to current assumptions about the world.

Stöckler proceeds to explain that Popper's contributions to the discussion of quantum theory were quite limited, because he failed to adequately take into account four competing interpretations of the theory as well as the mathematical structure it now has. To what degree this mistake was simply due to the fact that Popper took only early developments made by physicists into account or due to a mistaken reading of the problems is not quite clear. But Stöckler builds on his reading of earlier developments by maintaining that current interpretations of quantum theory and its current mathematical structure have to be unified and rendered coherent. To what degree they can be changed is not clear. But, once again, there is no methodological reason for supposing that changes cannot be deep and that these changes cannot involve new ideas of the nature of the objects whose physical processes are being studied.

Stöckler provides a very nice quotation from Einstein in which he states that his (Einstein's) efforts of over fifty years to find a new view of a quantum of light had come to naught. But Einstein had tried to do something which Stöckler would seem to push aside. Stöckler does not explicitly do that. Perhaps he merely seeks to portray a unity among physicists. But there is no need for such unity; the history of science shows how disunity has many times led quite impressively to the growth of knowledge. This observation is, however, no comment of the value of Stöckler's research program. My point is merely that it is valuable to leave options open and open options means that a Tarskian methodological semantics may be useful in their refinement.

Conclusion.

Popper saw a great difficulty with his theory of the logic of science, which was pointed out to him by Tarski: The logic of science described by his theory of science was not modus tollens, as he had claimed. Around the same time Popper was enormously impressed by Tarski's theory of truth and by his approach to logic. But a second difficulty arose quite soon: Tarski's approach to logic requires that each study of logical theorems requires that there by both a metalanguage and an object language. Under this assumption no logic was universal, as Popper badly wanted it to be. Popper tried to overcome this problem by developing his own logic, which was supposed to avoid the use of a metalanguage and an object language, thereby making logic universal. This project failed. Popper then simply said the problem he had intensively worked on was not so important; logics with a metalanguage and an object language could be rather universally applied.

This somewhat unhappy result was present for years without any effort to improve it. Agassi noted this state of affairs and went to work on revitalizing Popper's contribution to logic. Two aspects of Popper's possible contribution to logic considered by Agassi were (1) his elimination of induction and his proposal that the purpose of logic was to uncover mistakes and (2) his development of some significant observations in his formal logical theory. In regard to the second point Agassi has nothing clear to say. In regard to the first, some points he makes are on the mark, but he cannot connect them to some further results. He tries to do this by viewing the use of Tarskian 'logics of' as examples of individuals trying to solve problems, as Popper's methodological individualism says they do. This attempt goes awry: Individual rationality is social. Furthermore, alternative interpretations of logic are not limited to Platonic and conventionalist views, as Agassi claims they are, as he seeks some third view. After Agassi contribution is finished, we are nearly back to where we were when he started.

By taking Tarski's approach to logic and combining it with a fallibilist view of the growth of knowledge progress can be made. Tarski claimed his approach to logic could not bring some meaningful contribution to the theory of how empirical knowledge could be obtained. But he based this observation on his assumption that results obtained by applications of his approach to logic could not be universal nor could they lead to proofs of sentences describing the world. These assumptions are true, but the conclusion Tarski comes to is false. It can be shown how applications of Tarski's semantical theory can be used to improve the critical discussion of various scientific languages: They can enable the critical appraisal of how words are used and the development of new more precise, clearer, and more comprehensive versions of how some words are used. Logical theorems cam be applied as rules for solving problems in particular contexts. Such contributions aid the pursuit of knowledge.

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Increasing Knowledge by Analyzing False Results

Introduction

Popper's fallibilist philosophy of science touches rather directly a difficult problem: When we presume, that all theories are false, how can we simultaneously maintain, that we get closer to the truth through the application of scientific methods? Popper invested rather a lot of work trying to solve this problem. But his proposals are by no means satisfying. His first attempt to develop a theory of verisimilitude failed, because small changes in an empirical theory lead rather easily to a change in the judgment of verisimilitude. It is then not possible to determine, if one theory is closer to the truth than another one. His second attempt, which was based on Alfred Tarski's logic, offers a definition of verisimilitude which is, however, not applicable to any theory and as a consequence cannot serve to compare the verisimilitude of differing theories.

When we look at the life-long work of Hans Albert, we can observe, that some of his rather problematical theories contributed to progress. Which standards, if any, can we apply to defend the hypothesis that some of his problematical results have brought us closer to the truth?

We can only answer this question when we have developed a new theory of verisimilitude. Up until now all suggestions, from William Whewell until today, have presumed that verisimilitude refers to the relative distance between specific theories and the true description of the world. All of these attempts have failed, because no one has succeeded in offering a clear portrayal of such a relationship. But there is another alternative, which has not been considered. On this theory we do not get closer to the truth, when our new theories have increasingly less distance from a true description of the world, but then, when our new perspectives achieve a deeper understanding of new explanatory problems, even then, when they are false.

In this essay it will first be explained which characteristics false theories should have, in order to offer a new perspective of our understanding of explanatory problems, which will also deepen our understanding of them. Secondly, a rather short portrayal of how various theories in the history of science have contributed to our knowledge in this way will be presented. Thirdly it will be explained how this theory of verisimilitude can be applied to a theory of Albert.

Which characteristics do false theories, which deepen our understanding of explanatory problems, have?

It is rather clear, that alleged proofs of certain conjectures cannot be certain and normally give us no deepening of our understanding of the world. But it is also rather clear, that some refutations can provide us in significant ways with such deeper knowledge. As scientists provided refutations of the best scientific theories, they have significantly improved our understanding of the world. I mention a few examples. Copernicus's refutation of the theory, that the world is the center of the universe; Galileo's refutation of the theory that entities in space are made of different materials than the earth is made of; Kepler's refutation of the theory that the orbits of planets are circular; Faraday's refutation of the theory that the world is made of atoms; Einstein's refutation of the theory that space is Euclidean. Each of these scientists developed better theories than their predecessors had, but which nevertheless were also refuted.

An important character of these theoretical changes is that each of these refutations led to the formulation of deeper problems. If the earth is not the center of the universe, how can the motion of the planets be explained? When objects in space are made of the same materials as the earth, how can their actions be explained? When the orbits of the planets are not circles, what are they? If the earth is not made of atoms, what is it made of? When space is not Euclidean, then how can space be described?

These examples are so simple and clear, that one needs no standards to demonstrate their importance. That is partially due to the fact that we know them so well that we easily understand their meaning. If we want to generalize this point of view, we need universally applicable standards in order to judge whether new refutations also deepen our understanding of the world. I do not try here to give a complete list of such standards. At first it is sufficient to provide a few examples of clearly identifiable and clearly applicable standards.

The applications of these standards show, that the proposed standards of refuted theories identify theories which are progressive. When a refuted theory should count as progressive it should bring about a change which is clearly comparable with changes brought about by the refutations of other theories. The characteristics of the refutations of progressive theories should, on the one hand, lead to interesting problems and, on the other hand, should enable scientists to formulate clear and specific standards, which should be sufficient to identify solutions to the new problems.

One can presume that the refutations of many theories do not lead to any new and interesting situations. The refutations of theories with limited explanatory power or with implausible assumptions will normally not lead to any interesting results. The refutations of theories with widely accepted assumptions, on the contrary, will normally do just that. They show that a new framework for the formulations of new theories is needed. In addition they have an important characteristic: They give indications what kind of theoretical changes are needed. In the examples offered above they show,

That a new theory of the center of universe is needed,
That a new description of the planetary orbits is needed,
That new theories about what the world is made of are needed,
That a new explanation of space is needed.

The indications of how the theoretical assumptions must be changed are of enormous significance. They would be, however, of less significance, if they failed to provide specific conditions for the progressive formulations of new problems. But they do spell out such conditions. The refutation of the assumption, that the earth is the center of the universe did not only show the need for a new theory of the structure of the universe, but more specifically that a new description of the relations between the orbits of the planets and the sun had to be supplied. The refutation that the orbits of the planets were circles indicated that an improved description had to move in the direction of the theory that they were ellipses. The refutation of the theory that the heavenly bodies were made of some special stuff, called for a unified theory of the construction of the earth and the heavenly bodies. The refutation of the theory that the world was made of atoms provided indications, that there is no empty space. The refutation that space must be Euclidean gave an indication that newly developed geometries could play an important role in physics.

Examples of Albert's progressive, but problematical theories.

A theory of verisimilitude should be quite generally applicable: It should render it possible that the verisimilitude of as good as all theories which describe aspects of the universe can be examined. Thereby only some theories should be narrowly considered. I do not here offer a broad examination of Albert's broad contributions to the philosophy of science. Rather, I take a look at only a pair of examples, which I regard as problematic, in order to show how these contributions were helpful in bringing us closer to the truth.

Albert's central themes deal with the defense of Popper's fundamental contributions to the philosophy of science with an emphasis on the social applications of these contributions. An important aspect of his discussions consists of his treatment of the relationships between various intellectual fields. On the one hand, there is Popper's methodological individualism and classical economic theory. On the other hand, there are various aspects of their applications in politics, sociology, ethics and religion. These discussions had significant importance because they examined problems which previously were all too little discussed. A few of Albert's suggested solutions are mentioned here and further explanations are given as to why a pair of these problematical suggestions nevertheless led to an improvement of our understanding; They gave indications as to how one could contribute to progress.

Three aspects of Albert's discussion will be briefly critically discussed here. No analysis will be offered of the general importance of these aspects for Albert's contributions. The three aspects are:

The connection between various disciplines through "bridge principles":
The relationship between anthropology and science;
The connection between rationality and religion.

The beginning point of Albert's research in regard to critical rationalism is Popper's *Logik der Forschung*. But Popper's *Die offene Gesellschaft* and his writings about the social sciences are more important. Examples of this research are legal recommendations or social planning in social scientific methods. At first blush his contributions are merely elucidations of ideas, which Popper had already developed. Albert virtually always emphasized that he defended Popper's points of view. But this analysis of Albert's contributions fails to take into account how important Albert thought it was to improve Popper's theories and new directions to add to them. In the end he had to defend Popper's points of view in new intellectual contexts.

An example of such a newly arising problem is the following. Albert's description of social scientific methods is quite limited to that of economics.

He scarcely offers an analysis of the value and methods of other social sciences. But as Albert began to defend Popper's ideas in the German speaking world these themes were central. In view of this situation Albert put the problems, the roles, and the methods of the social science in the center of his research. A Popperian overview was needed in order to portray Popper's ideas as an actual and important (political) alternative in Germany.

This problematic was rendered difficult by the fact that Popper viewed economic theory as an independent field; it should be sharply separated from non-economic factors. How, then, can non-economic and other social entities be brought together within a Popperian perspective? Albert dealt with this problem in that he upheld the separation of various social scientific fields. But at the same time he proposed that their results could be brought into contact with each other with so-called bridge principles.

Albert's reaction to this situation was at the same time conservative and innovative. It was conservative in that he maintained Popper's separation of philosophy of science and other social sciences from another; it was innovative because he elucidated new perspectives concerning the relationships between various social sciences as well as with anthropology and ethics.

His results were weakened by the simultaneous defense of his conservative and innovative results. His holding onto Popper's separation of the social sciences from each other sharply limited his quest for a new alternative. Apparently the only solution for his problem was his theory of bridge principles. In order to build relationship between fields bridges were needed, which simultaneously upheld in some way their sharp separation. Albert did not have a convincing and general theory how this could be possible. In his portrayal the construction and application of such principles is complex and ad hoc. No new clear picture of social scientific methods was developed. And in addition a new, scarcely noticed and hardly discussed problem arose: How can or should the bridge principles change the methods of the social sciences? Insofar as the application of bridge principles portray new ways to new perspectives they must do that. But the assumption, which their application presumes, is that the methods remain unchanged. Albert's problematic theory of bridge principles brings a new and interesting problem in view: How can apparently separate fields be united in some fruitful way?

Closely connected with the problem just discussed is a further difficulty: How can or how should the relationship, between anthropological, i.e., non-empirical theories, with social scientific theories be fruitfully applied? Independently of Albert's research there were only two answers to this question, which had been developed by critical rationalists. Popper had suggested that anthropological theories could serve as a source for various directions of thought. On this view, however, the anthropological and the social scientific theories remain separated; there is no programmatic recommended methodological interaction between anthropological and social scientific theories.

Agassi had made on the basis of his methodological perspective a detailed suggestion. From his point of view anthropological theories could play a unifying role. The integrated scientific theories describe relationships between entities, whose role in the universe is presumed by the anthropological theories. In physics there are, e.g., metaphysical theories which presume the world is made of atoms and alternative theories which presume that the world is made of waves. Scientific theories can further develop one or the other metaphysical theory.

Albert's treatment of the relationship between anthropological and social scientific theories has something from Popper's and something from Agassi's: But he offers no clear alternative. Popper's observation is clearly accepted, and it appears that his view is consistent with Agassi's, at least in so far as he does not deny a methodological interaction. But just how such an interaction should look, is not clear from Popper's theory.

These three incomplete and/or problematical points of view of the relationship between anthropological and social scientific theories give indications of how a better theory of their relationship is possible.

Popper's observation is clear, but too narrow. Agassi brings us further, but his description of the methodological relationship ignores the role of anthropological theories within the social sciences. Albert's open attitude is also an indication, that a detailed description would be desirable. But he fails to say what substance such a description should have. All three alternatives provide indications where we should seek an improved theory: We can, namely, ask what role anthropological theories play by the formulations of new problems.

There are at least two other fields with relationships to the social sciences, which cannot be ignored, if one wants to offer a comprehensive portrayal of the social sciences. One such field is ethics. Normative statements refer to real situations. When one evaluates them one cannot ignore this. For Albert there is here an especially important and difficult problem. He maintains that normative statements are not sentences, that is, they do not describe any state of affairs. At least in regard to descriptions they are neither true nor false. Nevertheless Albert finds relevant bridge principles. One can, for example, pose the question, whether that which normative statements advocate can be carried out. Answers to such questions come above all from the social sciences.

Albert's treatment of the relationship between normative and descriptive statements is rather narrow. It helps us very little, when we try to identify correct normative statements. This limitation obviously grew out of his neo-positivistic of normative statements. But this theory is not needed. The sharp separation between ethics and social sciences create a situation in which possibilities of cooperation are all too little taken into account.

When we consider the fundamental idea of Popper's philosophy of science a better alternative is readily available. According to Popper's philosophy of science the quest for the growth of knowledge lies in the quest to solve problems. In addition it is possible to evaluate problems. There are various standards here. One can formulate central problems, whose solutions bring about widespread changes. One can also formulate problems, whose solutions can save theories, which have been largely successful, but which have fallen upon difficulties. One can also formulate problems whose solutions will unify fields previously separated.

One also finds in ethics such problems. Those normative statements which formulate important personal or human problems and which can lead to solutions should then be preferred. These statements have both normative and descriptive aspects. In this regard they are similar to Popper's methodological rules in his philosophy of science.

A further field which Albert extensively discussed is the relationship between religion and scientific theories. But in this discussion Albert goes in a different direction. As he analysed the relationships between social scientific and non-scientific theories he spoke about bridge principles. But, in regard to the relationship between religion and social scientific theories, he argued that religion should be completely separated from scientific theories. Theological perspectives are allegedly not compatible with a scientific view of the world. If one is to be consistent, one must choose either one or the other field. If one defends both, one will unavoidably end up in contradictions. Albert made many attempts to document such contradictions in detailed ways.

I will not discuss these discussions any further. It might be that Albert's perspective is correct, that no plausible and positive reconciliation between any religious point of view and the social sciences can be constructed. In the past, however, many alternatives, which seemed to do that, were defended. Isaac Newton found a place for God in the universe in that he (God) preserved the order of the universe. Leibniz portrayed God as the foundation of universal principles. Pierre Duhem separated his religious view from his scientific theories and simultaneously defended both. These alternatives are not concrete today. But there is no way of proving that no such alternative could be once again developed. Today it is not possible to sharply and finally separate religious theses from the social sciences with good methodological rules.

Albert's systematic perspective is first, the separation of social sciences from one another and from other non-scientific fields. Secondly, the connections between separate fields should be constructed with bridge principles. Thereby is clear, the separation leads to unnecessary and complicated methodological problems, which can only be treated with ad hoc approaches. In this regard Albert's mistakes demonstrate in a rather clear way, that the original construction of separations and the subsequent construction of bridges portray a mistaken way. The new and valuable problems which thereby arise are those of how the separations may be overcome and, indeed, with solutions which lead to progress.

The theory of verisimilitude.

In his two theories of verisimilitude Popper tried to find a universal standard. In his first attempt he wanted to explain, how the verisimilitude of two empirical theories could be compared. The theories to be compared needed to explain the same phenomenon. This theory did not work, because the results of comparisons could be easily changed, with small changes in the languages used to describe the world. In his second attempt Popper offered an abstract analysis of the meaning of verisimilitude. But this analysis could not be applied to theories to compare their degree of verisimilitude.

These two inadequate theories give a clear indication, where the common mistake lies and thereby an indication of which problems a new alternative must solve. Popper's mistake is that he set the standards for a theory of verisimilitude too high. A theory of verisimilitude is also interesting and useful if it merely opens a critical, fallibilistic discussion of verisimilitude. The results of such discussions do not need to be final or correct. Just as with the discussions of scientific theories the treatment of problems of verisimilitude can consist of open discussions.

In the case of Albert's theories I have explained how his problematical theories could nevertheless be valuable: They offer the basis for the formulation and solutions of new, interesting and valuable problems. I could be wrong about this. One can discuss my judgement of the correctness of Albert's theories or their usefulness. But even then, one could come to the result that some new and interesting themes should be investigated.

It is also the case the theory of verisimilitude proposed here offers neither a definition of, nor a comprehensive theory of, verisimilitude. The verisimilitude of theories can have various aspects. I have here identified and explained only one such aspect, in regard to how progress can be achieved when we examine the verisimilitude of theories. We do not need to assume that these analyses are the only way to proceed, in order to have useful discussions of the verisimilitude of theories.

Interlude

A fallibilist theory of the growth of knowledge raises new problems about how we learn from argument. We learn not merely by proving or refuting statements but also by understanding how and why arguments fail.

2e. 'How Do We Learn from Argument? Toward an Account of the Logic of Problems', with Terry Goode, *Canadian Journal of Philosophy*, XIII, 4 (l982): 673-689.

How Do We Learn from Argument? Toward an Account of The Logic of Problems

Terry M. Goode and John R. Wettersten

From the pre-Socratics to the present, one primary aim of philosophy has been to learn from arguments. Philosophers have debated whether we could indeed do this, but they have by and large agreed on how we would use arguments, if learning from argument was at all possible. They have agreed that we could learn from arguments either by starting with true premises and validly deducing further statements, which must also be true and therefore constitute new knowledge, or that we could start from putative premises and validly deduce false consequences, thereby showing that our premises were false. Our aim in this paper is to suggest a third alternative: we can learn from plausible arguments (invalid arguments which meet some other unspecified desiderata of approximation to valid arguments) through criticism of such arguments, which enable us to discover new problems. We do not attempt to embed such a theory in a more general theory of science as, for example, justificationists have embedded the first view and Popper has embedded the second. We only wish to suggest that the task of embedding our theory of learning from argument in a more general theory to be an interesting one which responds to a contemporary problem situation, i.e., how can we learn, when even the determination of the validity of arguments is conjectural? (Why this determination is conjectural and quite generally deemed to be so will be explained below.)

Our proposal is merely an attempt to generalize our interpretation of some recent work in the philosophy of mathematics and theory of rationality by Imre Lakatos [15] and Joseph Agassi [5] respectively. We are forced to disagree with these authors in doing so however. We will argue that traditional answers to our question (how do we learn from argument?) are inadequate because they incorrectly presume the existence of valid arguments (proofs or refutations) in each case when the advancement of knowledge is attained. Proposals for the use of plausible arguments have attempted to describe and propose the lowering of the standards for arguments in various contexts. This has been done in a justificationist framework within which plausible arguments are deemed sufficient to justify belief. It has also been done in a fallibilist framework within which plausible arguments are deemed adequate for the formulation of criticism. In this essay we do not discuss the traditional problem of plausible arguments, i.e., by what criteria do we correctly judge an argument plausible? Rather, we propose to discuss only the aim of seeking plausible arguments. We propose that we can learn most adequately through the use of plausible arguments, more so than merely with the use of valid arguments, and that a primary aim of using plausible arguments should be to discover problems. This fits nicely with Agassi's view of the use of plausible arguments in the context of a bootstrap theory of rationality, but we wish to say more about problems and less about criteria than Agassi does. We hold that Popper's theory [23, 24] that knowledge can grow by the discovery and solution of problems through criticism of existing theories can be improved if we supplement this process by including the proposal and criticism of plausible arguments as a device of discovering (deep) problems.

In order to place our position and argument as carefully as we can, we wish now to reintroduce our paper. To the question, how do we learn from argument? at least three answers have been (implicitly or explicitly) proposed. The first of these, here called justificationism following W.W. Bartley Ill's use of this term [7], is the theory that we learn from argument by proving theories (true) through the use of valid arguments. Descartes is perhaps the classic representative of this position. The second answer (sometimes called falsificationism) is the view that we learn from argument (particularly in science) by refuting theories. Refutation is always deemed tentative since any statement and thus all premises and also the conclusion of any argument must be tentative. This tentativeness cannot be removed since any argument must involve us in an infinite regress of putative justifications. However, in the falsificationist position, the argument we use to learn is deemed valid when learning occurs in the postulated way. The leader in developing this view is Karl Popper. The third view is that we learn from argument by discovering new problems which are due to inadequacies in our arguments and not merely in our theories. Imre Lakatos proposed this view in Proofs and Refutations. Our essay is an attempt to present this thesis more broadly than Lakatos. We propose to extend his theory of mathematics to other areas of investigation. Our view is that progress in fact occurs in all fields, whether it be mathematics, science or philosophy, because problems are found through criticism of plausible arguments.[174]

Once more let us explain how our position differs from previous discussions of plausible arguments. Thus far all discussions have focused on conditions under which a plausible argument is sound or acceptable. Such efforts include virtually all of inductive logic; attempts such as Carl Hempel's which view historical argument as merely enthymematic; or attempts such as Stephen Toulmin's to find criteria for soundness of arguments in practical contexts [23]. (Toulmin and Strawson have similarly suggested that the desiderata for formal logic and the practice of argument may differ. Toulmin conceives practical argument as analogous to legal rules of argument. We differ from these theorists in having differing desiderata: we seek growth rather than soundness.) Ernest Gellner attempts to find such criteria from a different more anthropological point of view [11]. Joseph Agassi's recent discussion attempts to show that a bootstrap theory of rationality can provide superior desiderata for acceptable plausible arguments than traditional theories can [5]. Unger's book on skepticism, in a similar but more limited way, suggests that arguments for skepticism require that we change our standards of rationality [29].

In this essay we do not reject these efforts. Rather, we claim that, given some plausible argument, given whatever criteria of plausibility you choose the primary interest this argument can have, the possibilities for growth it engenders flow from the possibility it provides for generating new problems. We do not, therefore, adopt the traditional aim, i.e., to make plausible arguments sound, or more sound or sound enough. Rather, plausible arguments may lose interest as they are improved vis a vis traditional standards. This is not to say that we oppose improvement as judged by traditional criteria; we hold that there are other and perhaps conflicting desiderata, which result from viewing plausible arguments as opportunities to generate problems. We hold that desiderata for plausible arguments should be rethought from this point of view.

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[174] Agassi has already pointed out that Lakatos' theory has an inadequacy: lt lacks a theory of problems [5]. Agassi proposes to use his own theory of metaphysics and the choice of problems to remedy this defect in Lakatos view. Peggy Marchi has reworked Lakatos theory from this point of view. [18, 191]. This addition does not however address the issue here, i.e., the use of plausible arguments.

The Breakdown of Justificationism

How do we learn from argument? Standard logic textbooks do not treat this question as problematical. They assume its answer is obvious and explain it: we learn from argument by extending our body of knowledge via deduction. The most important way we learn from argument is by proving theories true. It is also shown of course, that we can refute theories by demonstrating inconsistencies. Argument is thus important for criticism as well.

The poverty of the first part of this position is apparent: It cannot be practiced. If we did learn from argument by proving theories true, we would need to start with some true theories as premises. But if we have to prove these theories, we could never get the enterprise off the ground. We would be blocked by an infinite regress. Hume raised this fundamental problem: How can we have (certain) knowledge without certain and general premises? Moreover, all modern attempts to stop the regress and keep the methodological theory in question have proven problematic. The regress is only stopped by appeal to authority: intuition or innate ideas, sense experience, God, etc. The justificationist methodological theory presumes that all statements which we deem knowledge must be proved. Yet, both the theory that knowledge must be certain and the theory that argument is the tool we use to achieve certainty cannot be proven.

One further difficulty with the traditional approach to learning from argument should be raised here. The traditional view, that we learn from argument by proving statements certainly true, cannot provide an explanation of how we can select theories. The reason for this is that this view cannot be applied in a straightforward way. If we try to apply it strictly and really accept only proven theories we must fail since no theory can be proven. If, however, we lower the standards, we have no alternative criteria by which to judge. Many competing theories would be admissible, but there would be no way to distinguish them. Theorists such as Descartes, Newton and Kant did of course recognize this. But, since their own views do not explain this fact, some new explanation is needed. Justificationism fails because it requires a foundation of true theories from which we can deduce new knowledge and no such foundation can be found.

The Breakdown of Popper's Fallibilism; The Need for Plausible Arguments

Popper attempted to avoid these difficulties with a radical departure from traditional doctrine. Popper tipped the traditional model on its head in order to develop a new theory of how we learn from argument. Popper's theory also presumes a logically perfect model, but the setting is fallibilist rather than justificationist. On Popper's view we may consider the reductio ad absurdum form of argument the model for learning. The following process is paradigmatic. We deduce from a theory a prediction deemed false and thereby refute the theory from which we deduced that prediction. For Popper the key to learning is not the "transmission of truth" in argument, but rather the "retransmission of falsity": knowledge grows by conjecture and refutation. When successful, we learn from (valid) argument by discovering contradictions. The refutations are of course tentative since basic statements or formalizations may be wrong. Progress occurs because our ability to formalize properly and to judge basic statements as true or false is itself open to criticism; mistakes in these areas may also be uncovered via criticism. When a theory is deemed refuted a new theory must be found that explains everything the old theory explained, but which also accounts for the refuting case and more, in order that further tests of the new theory may be made. We follow Popper in this view of progress as due to the discovery of problems, but disagree with his theory of problem formation, because it is too narrow. Valid reductio ad absurdum arguments are not sufficient.

On Popper's theory advances are made in discussions or through argument when (tentative) refutations are attained, i.e., when inconsistencies are found using valid arguments. Lakatos' discussion of the growth of mathematical knowledge in Proofs and Refutations presents a different and potentially more interesting case. On Lakatos' view there are situations where advance occurs due to cases which are problematical counter examples. In these cases we do not have a tentative case which refutes a theory if correct, but a tentative case which may or may not refute a theory if correct. The proof, and not the conclusion, is deemed problematical. The problem which then arises reveals limits to both the theory and the proof. Even though the argument may fail when judged by the standard of validity it succeeds because it uncovers a problem: it shows us where our expectations that a theory solves a problem are not met. We may judge the argument valid or not, but to do so is to make new decisions about our theory. This is so, because the plausible argument forces us to decide if we want the allegedly refuting case to be derivable from our theory or not; in order to make this decision we must uncover what is to be gained or lost by doing so. We need to analyze "proofs" with new conjectures. These new conjectures solve problems uncovered by plausible arguments and lead to still new possibilities for proofs or refutations.[175]

The idea that advances do take place even when validity is not achieved seems to us to be intuitively plausible and widely accepted. It is presumed for example that even though students entering a new subject provide for the most part poor or invalid arguments, they still learn. The most common way this occurs is when attempts at criticism misfire and misunderstanding is revealed. Corrections may be partial and the process continues. It is also commonly thought that advances at the forefront of knowledge may ride on inadequate arguments when judged by subsequent standards and still be progressive when they are first given. Even Euclid's proofs are now deemed to be easily refutable by any competent mathematician. But it is unclear what people make of advances which are made with invalid arguments. One possibility is to presume that knowledge grows only when arguments are given that could be filled in correctly even though they are not. The growth of knowledge by invalid arguments may be either lucky or come about because fundamentally adequate arguments are stated in an incomplete but essentially completable manner. We reject this proposal.

The problem of explaining what occurs when we learn from invalid arguments cannot be solved by the reduction of these cases to the ideal case of learning by valid arguments. Incomplete arguments, as invalid arguments on this view must be deemed, may be developed in any number of ways. When we have an incomplete argument we need to decide what to do with it: Should we improve it or not? And, if so, how? The only detailed attempt to come to grips with this problem without reducing the rough case to some ideal (as for example, Hempel suggests) is made by Lakatos. Lakatos discusses the various alternatives that are used when one finds arguments of uncertain validity in mathematics. According to Lakatos when we discover dubious arguments in formal mathematics we are forced back to informal arguments, back to the place where, on his view, knowledge grows. He attempts to explain the growth of mathematical knowledge, as a process of the discovery of problems with mathematical theory through the discovery of defects in proofs. The

[175] See Philip J. Davis and Reuben Hersh, *The Mathematical Experience* (Boston: Birkhaüser 1980), 345ff for a discussion of Lakatos' view.

discovery of such defects does not simply call for new improved proofs but for a proof analysis and new mathematical theory. When new theory and new proofs are given, we may still find defects. Learning from argument thereby occurs without any strict proof; learning in ordinary mathematics is explained without reduction to some ideal case. We learn by discovering new problems through proof-analysis. The solutions to these problems also increase our knowledge and provide opportunity for the discovery of still newer problems.

Although Lakatos does not generalize this view, we believe that his view of argument in mathematics may be generalized and improved with the aid of an additional theory of problem formation, so as to supply a general theory of a primary way of learning from argument. On this view we primarily learn from argument by discovering inadequacies in arguments which lead us not to removal of simple mistakes, but to the formulation of new conjectures and problems, which are a product of the analysis of the arguments. We learn by discovering problems through the criticism of plausible arguments.

Our Conjecture in Brief

Following Popper and others, we view argumentation in general as primarily a method of discovering new problems and developing conjectural solutions. We propose that the primary aim of argument is to reveal new problems and that this can be done most effectively when we propose and analyze plausible arguments. By plausible arguments we mean arguments which are not demonstrably valid but which are intuitively acceptable by some desiderata which we do not here specify. Plausible arguments may fail to be analyzable as either valid or not due to the uncertainty of conceptions used in a proof. We recommend that we put forth questionable arguments as a means of discovering new problems. We are not of course simply proposing that we make arguments weaker than we are able in order to gratuitously produce problems. Rather, we propose making the arguments for solutions or the criticism of theories sufficiently adventurous so as to strain the application of our standards of validity. This can enable us at times to discover new, interesting and progressive problems. We do not claim that this will always be the case, nor do we explain how to know which plausible arguments will lead to new problems and which will not. Nevertheless, we hold that plausible arguments are potentially the most productive: they offer the best chance to examine the limits of our theories.

This summary view may be developed to a small degree by explaining the limits of our view, i.e., by explaining how we envision plausible arguments to be a needed addition to valid ones and how the use of plausible arguments may be limited by the need to keep the use and goal of validity in many cases. These two discussions enable us to explain our view by showing why we require a broader view of how we learn from argument and yet why it is not too broad so as to make argument empty.

We recognize that the test of validity is useful for discovery of both proofs and refutations. But it cannot be taken as the basis for a universal prescription: always seek validity. The discovery of a valid inference is in many cases the discovery of a dead end. It may be a dead end because it only adds precision to already established views; the one-sided pursuit of increased rigor may restrict us to discussion of established views and in doing so block discussion of problems and new alternatives. Thus, valid arguments may at times lead us nowhere. We may, for example, get mere deductions of new theorems which may not be of interest. This is not to say of course that all theorems are of no interest. When we use reductio ad absurdum arguments that succeed, we may indeed find interesting problems, as Popper has described.

We suggest that problems not accounted for by Popper's theory may be found by extending our repertoire to include techniques of proposing and criticizing plausible arguments. The extension of our techniques to include plausible arguments is valuable because it increases our ability to locate problems in the following two ways. First, we may find problems by discovering putative counterexamples which we cannot make out to be refutations, but which we also cannot say should be interpreted as counterexamples. Lakatos gives examples of this sort from mathematics. The second way in which plausible arguments may extend our ability to identify problems is by enabling us to construct new theories. We may plausibly argue that some explanation is adequate. The criticism of such a defense may enable us to discover limits to our explanation and new problems as a consequence. We will present an example of the discovery of problems through criticism of plausible arguments—the ontological argument for the existence of God—in the following section. We contend that criticism of this argument leads to appreciation of the limits of arguments and theories and, as a result, to new problems of how to deal with and overcome these limits. We propose then that we need also to learn by criticism of plausible arguments.

The extension of our theory of valuable arguments to include merely plausible arguments as quite valuable runs a risk when viewed from another direction. This risk is that we would learn to be satisfied with plausibility when valid arguments could be attained if we had not lowered our standards. This may occur and requires a theory, which we do not now have, of how we judge whether to seek to improve an argument or whether we seek to move on. The problem is not as severe as it might first appear however. This is so because, if we find an interesting problem, this is sufficient. It does not matter too much if we miss some improved argument, if this argument is unnecessary for the discovery of some problem that will enable us to progress. We might even pursue both improved arguments and new problems which arise from the same context. We suggest extending our repertoire; we only mention, but do not solve, the problems of strategy this suggestion raises. Yet, pluralism seems to us the proper framework for the resolution of these problems and also a mitigating factor which makes them less pressing than it might appear at first glance.

There are further and pressing problems for our view such as: By what criteria do we judge arguments plausible? How do we judge problems good or bad? And, how are the two related? We do not propose to answer these questions here either. We only set ourselves the tasks in this essay of stating our proposal, explaining the limits of our view, relating the problems that need to be solved, if it is to be developed within these limits, and providing an example to show that the problems of the view are real: we need to solve them to take account of actual cases. We think actual cases abound as Lakatos' illustrations from the most rigid discipline, mathematics, indicates. We propose a case from the history of philosophy, the ontological argument for the existence of God.

The Ontological Argument.

In this section we will illustrate the theory of learning from argument explained above by examining one version of the ontological argument and some of the reaction to it. The account we offer is necessarily rather sketchy. Nevertheless, it will serve our purpose. We are only interested in illustrating, how some specific responses to the ontological argument were progressive precisely because they provided discoveries of defects in the argument, which raised new and interesting problems. We wish to show through the example that even though philosophers have aimed to get proofs and refutations, they have been successful not because these were attained, but more importantly, because of the new problems that were generated through criticisms of arguments that failed to provide either proofs or refutations. At the same time our example serves to differentiate our position from that of Lakatos and Popper and other theorists of plausible arguments. The example shows that contrary to Lakatos, even in philosophy the criticism of arguments can lead to the discovery of new and interesting problems; and thus, Lakatos' later appeal to a unified framework at a higher level is not necessary. Contrary to Popper, the example shows that we can use merely plausible arguments and criticism in order to discover new problems. Contrary to other theorists, we illustrate the worth of plausible arguments in their usefulness for problem formation rather than their potential soundness. The ontological argument is an appropriate example because the argument was never rendered valid. It thus meshes with our purpose of showing the importance of some merely plausible arguments. Further, we show that the common contemporary view that this argument is merely a dead end is explained (albeit incorrectly) by the acceptance of the view that only arguments rendered as proofs are advances.

The ontological argument for the existence of God was first explicitly used by Anselm. We propose, however, to begin with Descartes' introduction of the argument. Descartes had two reasons for introducing the ontological argument: One was ontological and the other epistemological. The deduction of the existence trivially helped him to develop his theory of what there was, i.e., there was God. Furthermore, it was used to guarantee his epistemology, which quickly enabled him to argue that there was mind and body. This, however, was a mere application of the argument. The second and more important use is that it solved his problem: How can we have certain knowledge? By proving God's existence and appealing to his benevolence, Descartes was able to provide justification for the claim that any clear and distinct idea must also be true. It provided a foundation for his program of proving with complete certitude all desired knowledge.

In *Meditation V* Descartes offers the following version of the ontological argument:

> ... to think of God (that is, of a being completely perfect) as without existence (that is, as lacking a certain perfection) is as impossible as to think of a mountain without a valley ... (And even though) Because I cannot think of a mountain without a valley, it does not indeed follow that there is any mountain or valley in existence, but only that mountain and valley, be they existent or non-existent are inseparably conjoined each with the other, in the case of God ... I cannot think Him save as existing; and it

therefore follows that existence is inseparable from him, and that he therefore really exists. [9] 62-3

We can put this argument quite simply in modern terms in a form which is apparently valid ([20] 218-20):

(1) All supremely perfect beings exist
(True by definition of 'supremely perfect being')
(2) God is a supremely perfect being.
(True by definition of 'God'.)
Therefore (3) God Exists.

Under this interpretation, it seems clearly valid, on a par with:

(1) All men are mortal.
(2) Descartes is a man.
Therefore (3) Descartes is mortal.

Nevertheless it is apparently inadequate in some respect, as many of Descartes' contemporaries pointed out. In particular, Gassendi offered a series of objections to the Cartesian ontological argument. One of the more interesting of which was that if this argument is any good, then so too should we be prepared to accept other arguments that could prove the existence of anything perfect. We can apparently use the same form of argument to prove the existence of any entity, (Gassendi suggested an Island) that we conceive as perfect. This argument of Gassendi's does not show what is wrong with the Cartesian argument, only that something must be. Thus we have a plausible argument with some defect. We also have a problem: Where is the mistake located?

Now, if we accept Gassendi's criticism, as Descartes did not, we might seek to explain the difficulty either as due to false premises or an invalid argument. Neither course was easy for seventeenth century thinkers. One course apparently rejected simple assumptions about God, which nearly everyone accepted; the other course rejected a clearly acceptable form of argument. The Cartesian ontological argument was thus quite plausible for seventeenth century thinkers by whatever criteria one wished to judge and apparently successful criticism of it posed a hard and interesting problem for those thinkers: How can one maintain the argument and the ordinary conception of God?

We suggest that the plausibility of the Cartesian ontological argument and the problem it raised cannot be explained away by merely posing the task of deciding which premises are false or which steps in the argument invalid. This is so because the task of identifying false premises or invalid arguments required new understanding of theories, of God, or of argument or both. Furthermore, once these problems are posed, the primary interest may change. We may no longer care whether the argument is valid or not or whether its premises are true or not. It will have served its purpose by leading us to new problems which will lead to new (plausible) arguments. Indeed, it may be impossible to settle the issue of the specific defect of the old argument: We may simply not have sufficient information or clarity in the old argument to do so. And new arguments will make the issue passe. New conjecture and problems will make decisions about prior versions distinct from future developments.

We contend that the process of discovery of new and interesting problems through criticism of a plausible argument occurred in the case of the ontological argument. In order to illustrate this let us turn to Leibniz. Criticism of the ontological argument, which purported to show that something was wrong, whatever that should be, led to specific and fruitful conjectures. Let us explain. We read Leibniz as adopting a Cartesian program, i.e., he wanted proofs for all desired knowledge. The central problem on his view is: How can we derive all desired knowledge from certain, perhaps merely logical, rules? Now, Leibniz saw that in many ways Descartes' program had failed; his proofs were not convincing. Among those proofs which failed was the ontological proof.

The failure of Descartes to provide a convincing version of the ontological argument was important for at least four reasons. First, the ontological argument was, at least at first reading, a cornerstone of Cartesian philosophy: Only by proving God's existence could Descartes prove to the satisfaction of his audience that clear and distinct ideas were true. Secondly, Leibniz wanted to prove that God existed in order to establish a philosophy which could unify reason and religion. The ontological argument was crucial because only it showed the (rational) necessity of God's existence. Third, the breakdown of Descartes' argument called for a new analysis of God -- an analysis that seemed to lead to a Spinozistic conclusion, that God was identical to substance. Fourth, it led, perhaps along with other breakdowns in the Cartesian program, to a new problem in the analysis of argument. This analysis now had to account for the possibility of surreptitiously introducing contradictory notions. It posed the specific problem for Leibniz of how propositions could be analyzed so as to avoid this possibility. Leibniz accepted criticisms of the ontological argument which purported to show that something was wrong. Leibniz made a conjecture that the argument was incomplete. This conjecture poses an interesting problem, however, because by any traditional standard the argument was already complete. In order to explain in what ways the argument was incomplete, Leibniz needed new conceptions of God, necessity and existence.

In particular, Leibniz made a conjecture that the Cartesian argument was missing or presumed a premise that God is a possible being, that the concept of God is consistent, which when added to the argument would make it sound.

> 'It should be noticed, however, that the most you can draw out of this argument (the Cartesian ontological argument) is that if God is possible, it follows that he exists; for we cannot safely infer from definitions until we know that they are real or that they involve no contradiction. The reason for this is that from concepts which involve a contradiction, contradictory conclusions can be drawn simultaneously, and this is absurd.' [9] 306-7

Leibniz' problem of explaining the defect of the ontological argument and repairing it did not lead to successes in repairing the argument. Yet it did lead to new problems. It was known prior to Leibniz, that from a contradiction, contradictory results could be derived. But Leibniz' critique of the ontological argument raised two problems afresh; it showed that new and better solutions were needed. These two problems were: how can we detect contradictions in our premises? and how can we form, in particular, a consistent concept of God?

The breakdown of the ontological argument and the need to satisfy the desiderata of explaining how we could, on the one hand, refurbish the Cartesian program with new analyses of propositions to explain certain knowledge, and, on the other hand, maintain a sufficiently traditional conception of God to serve the needs of religion posed Leibniz' problem. lt was an important case, on the one hand, for what Russell calls Leibniz' private more Spinozistic philosophy, because here Leibniz needed new analyses of propositions including analyses of necessity and existence, and, on the other hand, for what Russell calls his public philosophy, because here he needed to devise a new conception of God, which was adequate both to some proof of God's existence and to the needs of religion. We will not here discuss his views except to note that in each project Leibniz is nearly led to the Kantian position that existence cannot be a predicate.

The next sustained reaction to the Cartesian-Leibnizian version of the ontological argument comes from Kant, who provided a new conjecture of the nature of the defect of the Cartesian ontological argument. He agreed with Leibniz' conjecture, that in order to show that the ontological argument was valid one would need to show that the concept of God was consistent. Rather than showing that the concept was consistent, however, Kant proposed to show that any use of it to prove the nature of existence would be inconsistent. He did this by exhibiting antinomies, which followed from such use. Kant did not merely try to refute Leibniz' version of the ontological argument; he also sought to demonstrate the impossibility of any version of the ontological argument. Moreover, Kant tied the worth of all other traditional arguments for the existence of God to the adequacy of the ontological argument, arguing that they all depended upon it for their acceptability.

> 'Thus the physico-theological proof (teleological argument) of the existence of an original or Supreme Being rests upon the cosmological proof, and the cosmological upon the ontological. And since, besides these three, there is no other path open to speculative reason, the ontological proof from pure concepts of reason is the only possible one, if indeed any proof of a proposition so far exalted above all empirical employment of the understanding is possible at all.
>
> (But) the attempt to establish the existence of a supreme being by means of the famous ontological argument of Descartes is ... merely so much labor lost; we can no more extend our stock of (theoretical) insight by mere ideas, than a merchant can better his position by adding a few noughts to his cash account.' [91 466-86

Kant's conjecture concerning the error in the ontological argument is that we cannot use logic for establishing what exists; in modern language we cannot substitute 'existence' for predicate variables and retain sound arguments. This criticism of the use of logic leads to far reaching problems in both logic and metaphysics. Most importantly we need to refine our logic to avoid this error. Furthermore, if logic alone cannot show us what exists, we need to rethink, as Kant did, the whole enterprise of traditional metaphysics with its use of proof to establish knowledge of what exists. Kant's criticism is a new conjecture; it requires a new analysis of the argument and as later developments have shown, even a new logic to make it plausible.

Russell incorporates the principle that 'Existence is not a predicate' by making 'existence' a logical operator '$\exists x$' to be attached to open sentences. The effect of this, combined with Russell's Theory of Definite Descriptions, is that any ontological argument becomes question-begging, because they all must assume an existential premise. That is, on Russell's analysis, the crucial premise of the ontological argument is 'God is a supremely perfect being' which Russell (using some obvious signs and symbols) would write as:

S (℩x) Gx. This becomes, through the Theory of Definite Descriptions: ((∃x) ((y)(Gy Ξ (x=y)) & Sx). But this premise obviously begs the question, since it asserts that God exists. Thus, we actually have a whole system of logic, together with a theory of terms that precludes any ontological proof of God's existence by showing that any version of such will be question begging. We can now (within this framework) prove that we cannot prove the existence of God. We are not saying that Russell developed his logic to get this result, but we also do not regard the fact that his system (at this point) represents the formalization of the Kantian intuitions as coincidental. Frege and Russell had to respond to Kant in order to render their theories plausible. They had to refute Kant's theory of the separation of logic and mathematics. Yet they could not return to Leibnizianism. Kant's theory of existence proofs had to be incorporated.

Conclusion

The central point of our paper may now be summarized as follows. Arguments are almost always thought to only be successful when valid. It is commonly thought of course that other arguments such as inductive arguments are properly used in some contexts. The standard approach to the problem this poses, however, is to attempt some theory of soundness or some approximation to validity. Arguments which fail to meet either standards of soundness or validity are deemed failures. We might use them as experiments until we get a proper argument; then we forget them.

On the view presented here arguments which are plausible are themselves interesting. This is so because they enable us to find problems for the development of theories. We can find these problems by analyzing the problematic aspects of plausible arguments. We can then, as on Popper's theory of science, find new plausible arguments (corresponding to conjectures on Popper's theory) and then new problems and so on. Even if arguments are not valid it is crucial that we can appraise their defects. It is only this feature which enables us to find problems. The standards of logic provide desiderata, which enable us to set goals for generating problems, problem solving and theory construction.

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Interlude

Popper's philosophy of science showed that scientific research could not be adequately described merely by some logic. It was guided by methodological rules; these rules are social. This has required making the standards for research more open, as the following essay explains.

2f. 'Against Competence; Towards Improved Standards of Evaluation of Science and Technology', *Nature and System*, 1, 4 (l979): 245-56.

Against Competence: Toward Improved Standards for Appraisal of Science and Technology*

Popular concern with problems of the value and usefulness of various scientific activities as well as with technological innovations have recently led to increased demand for controls and regulations of these activities. Improved performance by the professions employing scientific and technological innovations also has been demanded. But the effect of such pressures and concern is often, quite contrary to its intent, conservative: they appeal to the inadequacy of existing standards to meet established criteria and seek to bring them into conformity with such criteria, so that the demand for reform becomes the demand for higher levels of competence. i.e., higher levels of achievement as measured by established criteria. This demand for increased standards of competence is at least in part a product of the social framework of the society at large, a social framework that incorporates theories of science and technology which deem competence desirable and feasible. The point of view of this essay is quite the opposite. The demand for competence is dangerous. The theories which support the demand for competence are mistaken, and the standards which result from demand for reform in the context of this framework are poor and even damaging.

The common view of increased standards is here portrayed as improvement of standards of competency. This demand is apparently modest and one accepted by all as a good foundation for evaluation and reform. Nevertheless, the conclusion of the argument of this essay is that its dangers outweigh its virtues. This conclusion opens up new problems concerning responsibility, reform, and acceptable performance. This is especially so in science and technology which are, and need to be, largely run by experts, yet must also be open to external evaluation. A sketch of an alternative more liberal and critical approach is suggested.

This essay itself might be described as philosophical sociology. It discusses the situational logic of the use of the standard of competence. It is philosophical in its analysis of the theory and sociological in its analysis of the logic of the situation the use of this theory brings about. The essay does not provide empirical sociological studies; it does provide a theory which is amenable to such studies. The alternative suggested (the theory that standards of minimal performance may be foregone in favor of standard: of problem-solving) is defended merely on the grounds that it provides a framework in which the problems raised by the use of competence as standard either do not occur or may be more easily solved.

It is hoped that, if we understand how technology and science are evaluated and the impact this has on performance, we may better integrate our value judgments concerning the practice of science and technology with our evaluations of the quality of their performance. We may then be more effective using technology and science for achieving our aims. Current practices, including the use of competence, do not take into account aims. The integration of value judgments and appraisals of practice is therefore quite difficult. New alternatives which will make such integration possible may therefore be useful.

The Rationale for Standards of Competence.

It would hardly seem necessary to provide a rationale for using competence as a standard. One cannot defend incompetent scientists, engineers, or even philosophers. The policy of requiring competence does have a rationale, but the rationale is a bad one rooted in a traditional and erroneous philosophy of science. The use of this standard is thought to provide a guarantee, that we can select work and individuals which will satisfy our needs. Competence is thought to be this admirable requirement because it incorporates both minimal and high standards, i.e., standards sufficiently low to be met in principle by anyone and sufficiently high to prevent mistakes. Requiring competence is thus deemed to be nothing more than requiring that blundering and stupidity be avoided. The requirement merely demands that error be avoided. lt is version of the Baconian theory of science that, if we remove the causes of error, we can (nearly) guarantee success, with only extraordinary circumstances causing failure.

This rationale for requiring that performance meet a standard of competence is my explanation of what I take competence to be, viz., a hidden and unjustified attempt to guarantee success. By providing this rationale I believe it is possible to explain how the standard is interpreted and employed. Or, to put it another way, when competence is invoked, the rational explains what is being appealed to and why.

Efforts to insure competence often fail to produce the desired results. We not only have individual instances of failure—which one would expect—we also have examples of widespread failure. At times we question the so-called competence of an entire profession (psychiatry) and at times of scientific research (cancer research). The response to such problems as these is to raise standards of competence, in order to still further inhibit the errors that lead to failure. When this fails, the only recognized response is again to raise standards still further. Let me elaborate.

Competence is introduced as a standard of minimal acceptability; incompetence is simply unacceptable. It is supposed to be protective. In order to serve in such a way the standard has to be sufficiently high to guarantee against failures of various sorts. How is this accomplished?

In order to provide a guarantee against various failures, it is necessary to establish standards which will eliminate the possibility of mistakes. This is impossible, because in any area our knowledge is to some degree limited. In carpentry the guarantee is probably more reasonably sought than in medicine, nuclear engineering, or scientific research; but even in carpentry new developments, e.g., unit construction, may lead to new problems.

So, given one's need to provide a guarantee which cannot be provided, one is forced in two directions: First, no standard is high enough (I will discuss this in the following section); second, all standards must be uncontroversial, since no controversial one could provide a guarantee. This second result forces all work to be evaluated in terms of accepted ways of performing. Accepted ways of performing must be enforced, because only then can any disaster or mistake be explained away on the grounds, that it is an extraordinary or new occurrence. Conservative standards provide protection for the people endorsing the standards just in case the standards fail.

The necessity of using conservative standards in order to have "justified" standards leads to another dangerous result: The standards used are over-estimated. In spite of the fact that standards cannot provide a real guarantee of the removal of error, it may be presumed and claimed that they can. This subterfuge leads to unrealistic claims about the knowledge available to serve as a foundation for standards of competence.

Let me explain the epistemological problem first and then turn to the sociology of the use of the standard of competence. The denial that unrealistic claims are made is based in the view that claims for competence are minimal: They reflect only what is known in a discipline. After one has institutionalized the known, one can proceed to cope with what is unknown. One can build on the known, i.e., a layer of unchanging procedures, rules, and information which is known by every (competent) person in a field.

This epistemological theory is false, whether it is applied to science or technology. New problems and theory force changes not only in procedures at the most advanced level but also at the most rudimentary. Changing materials in construction or changing theory in science (or vice versa, e.g., cheap—now one perhaps should say expensive—printing in science and unit construction in carpentry) may force changes at all levels of operation.

In philosophy it has only recently been noted (under the impact of Kant and Gestalt psychology) that there are no observation terms—that the most rudimentary observations we make may depend on the point of view we take when observing the world. Indeed if even our observations depend on our point of view, it is no wonder that our standards should depend on and change with, our points of view. Since there are numerous discussions of philosophical problems of identifying rudimentary elements, let me tun instead to the new philosophical-sociological problem: What do we do when no rudimentary standards exist? The failure to appreciate the importance of this problem leads to the maintenance of old ideals, among them that of competence. A discussion of the problem can provide an explanation of the unfortunate consequences of the use of the old ideals.

The only recognized solution to this problem, i.e., how do we conduct science and technology without rudimentary standards? has been developed by Michael Polanyi and Thomas Kuhn. Their theory of science reflects the old view that the stability of science rests on a proven foundation of narrow true statements. It rejects, however, the view that the history of science is continuous, with no abrupt changes. They adopt instead the view that science succeeds only by restricting itself arbitrarily to one framework or paradigm. This paradigm then becomes the standard for progress and the tool by which progress is achieved.

This view has the regrettable consequence, a consequence openly advocated by Polanyi and Kuhn, that science must exclude all those would-be scientists who do not follow the socially established paradigm. It is a highly conservative theory, which in all cases must lead to the naive endorsement of those standards which fit the reigning paradigm. It leads to the endorsement of the fashion of the day, whatever that fashion may be. This seems rather vulgar and naive. Yet the theory may be enlightening.

The theories of Polanyi and Kuhn provide a sociological theory of how standard are now set. On this view standards are set in accord with prevailing opinion, thus providing a justification for the endorsement of current fashions: It is necessary for the improvement of standards, since advance is only deemed possible in a unified framework. At the same time they explain how some rationality is possible in spite of these moves, i.e., extension of knowledge and improved accuracy are still possible, so that the most impressive result is an unintended one: The theory reveals the game for what it is. lt reveals arbitrary use of frameworks and discrimination in favor of existing patterns.

The Growth of Standards.

One aim of adopting the conservative standard of competence is to provide reassurance to those enforcing the standard that they will not be blamed in case of error. The use of conservative standards is necessary because there are no reasonable standards of sufficiency. The safest bet for those implementing standards is thus to rely on those standards already institutionalized.

The conservative standards provide the safest policy for the administrators but not necessarily for the consumers. Conservative standards, i.e. those endorsed by the scientific or technological community, may take account of some difficulties only to create some other difficulties and block consideration of new alternatives. Requiring some procedures may lead to problems, which these procedures themselves cause.

Furthermore, existing standards may prevent the introduction of solutions to new problems, because new solutions may require replacement of old standards. In such cases new problems may only be dealt with in an ad hoc way. Finally, existing standards may produce problems due to unintended consequences of their use. Examples of such problems are the damage that may be caused by excessive x-rays or operations, while standards for the use of DDT may lead to the increase of malaria.

The existence of established patterns of insuring success may also block the introduction of entirely new solutions when new solutions do not fit established patterns. For example, birth control research focused exclusively on refining methods of controlling female ovulation, but no research on males was conducted.

These problems and others may make evident that the existing standards are not sufficient by themselves to prevent mistakes. Since big changes are dangerous, the only option left seems to be to increase the extent and detail of the standards. Raising standards that are applied reduces the risk that accrues to those responsible for the standards. This is not simply a personal matter: Those responsible are charged with the responsibility to reduce risk.

Increased detail and scope of standards will often have the unintended consequence of increasing the arbitrariness of the standards. The more detailed they become the less rational is their application. Yet the only option available for increasing the guarantee of safety that the standards provide is to increase the detail with which they are spelled out. This defect can, of course, become apparent. In this case administrators can refuse to spell out standards on the ground that that would make them too restrictive. But this leads to arbitrariness of a different and potentially even worse sort. The administrator may try to reduce risk in each case. So instead of having arbitrary standards uniformly applied, we have arbitrary standards arbitrarily applied. The areas of openness and the means of judging and arguing are neither spelled out nor explained; there is no means of doing so. Thus we are reduced to relying on both experts and conservative standards. The more we seek advance and safety, the more authoritative become the experts and the more conservative the standards.

This means that value judgments of alternatives are precluded, because alternatives are deemed impossible due to the alleged justification of existing patterns. An epistemology with too high claims may preclude the use of value judgments in discussions of science and technology because the high claims can only be upheld by authority. And authority must be univocal.

In sum, the aim of standards of competence, i.e., to prevent mistakes, forces a random and arbitrary growth of standards. The benefit of the doubt is always in favor of raising the standards. Yet the raising of standards in this way can cause problems, which themselves are not amenable to solution.

Obscurantism.

Even though the standards of competence grow in a rather arbitrary and steady way, they still need to be defended. This demand for defense leads to obscurantism. Those who are enforcing or administering the standard are forced to explain them. But they cannot explain them since they grow by gradual accretion. Even if each individual standard has a rationale, maintaining all of them together often will not. Furthermore, the rationale in each case has likely not been critically evaluated; there are no means to do so since they rest on authority and convention. So the "highest" standards are chosen uncritically; they are the safest. Authority cannot be questioned and convention always appears the safest.

The defense of these standards still requires, however, that they be justified on the grounds that they are needed. This is so since all standards impose some restrictions. So, the maximum requirements need to be justified on the grounds that they are the minimal standards! The method for doing so, as noted, is to overstate one's claims for knowledge of the necessity and usefulness of existing standards. (Popper has pointed out, that revolutionaries virtually always claim greater knowledge than they possess about the consequences of their acts. A constant presumption in favor of existing practices leads to the same inflated claims; this can be equally dangerous.)

The obscurantism resulting from the construction of justifications after the fact may take many forms. Dubious statistics is one bureaucratic device of pseudo-proof. Other methods simply are the untested appeal to intuition and the derivation of standards from doctrines modified to serve the specific purposes.

Once the obscurantism is accepted or obscurantist modes of defense are accepted, the standards become even more rigid than before. At times not only are particularly obscure justifications introduced but also standard methods of simultaneously introducing and obscuring justifications are institutionalized. For example, various rationales may be routinely provide but never discussed. Or, standard but vague rationales may be used which could justify any standard. One possible device is to justify standards on the basis of studies and information which are not subject to evaluation. Each individual may recognize the obscurantism in the other fellow's explanation, but believing that no better is available, may tolerate and even endorse the use of these methods. Individually, such explanation may be discounted, even while the method of justification goes unchallenged.

The use of spurious justification provides an even more firm bulwark against the use of value judgments in discussions of science and technology. An obscurantist justification probably cannot be refuted; it can only be faulted for its obscurity. This may mean we have no clear methodological or epistemological evaluation of existing practices. But clear evaluations are needed, if we are to sort out alternative policies from both the perspectives of goals and values on the one hand, and methodological and epistemological points of view on the other.

Standards for Standards.

Standards for the change of standards may come from within a discipline or from without it. If they come from within the discipline, there is little hope for change. There is little hope because the proposed standards are aimed at removing all difficulties. Any change from within has a prima facie case against it: It would loosen standards. It would loosen standards because change, if not simply accretion, would relax some previously enforced standard. Secondly, those engaged in enforcing, determining, and administering the standards have adopted and built their careers on such standards. In order to change them they would have to change the foundations of their careers and their profession. Needless to say this is difficult.

But thirdly, and the real crux is here, existing standards are presumed to be justified. Thus, if members of a profession admit that they are mistaken, they do not simply admit that a single standard has been in error. They admit that the profession is incompetent. If standards of competence are not competently (i.e., correctly) established, the profession itself is incompetently run.

Furthermore, the acknowledgement of a mistake may not only show that the profession is incompetently run, but that competence is not possible. If standards of competence cannot be justified, then no standards of competence are possible; standards of competence require justification. So, if we try to fix standards of competence within a profession, we cannot easily change standards, because these standards themselves determine what the profession is properly about; if they are changed or found inadequate, the whole profession immediately becomes suspect in their own and the public's eyes.

If we attempt to arrive at standards for standards, which would allow for change from outside a profession, things are even worse for justifying or securing the standard of competence. Standards of competence are ordinarily set from within a profession. The rationale for this is that the profession knows how to practice its own business best. If external standards are needed, it must be presumed that the profession is not competent to run its own affairs. Indeed, external standards are usually justified not on grounds of incompetence but of immorality. People prefer to believe that doctors are immoral rather than incompetent.

To elaborate, since standards of competence need to come from those who know most about a given subject matter, when external standards are suggested there is a serious problem; if experts are not competent, how can those outside a profession be competent? Since there is no answer to this question, external standards are reduced to standards of morality. If a profession is moral, it is presumed that it will be competent. This often fails, of course, since lack of knowledge and understanding rather than immorality are often the cause of the difficulty. But no external standard or source can (competently) identify such a defect. The objection that when failure occurs due to lack of knowledge we have a complaint against the incompetence not the competence of the profession illustrates my point: Competence is incompatible with ignorance, yet we are all ignorant. So the false claim leads to problems.

The upshot is that neither external nor internal standards can be appealed to change standards. They can only be used to raise given standards; internal standards may improve practice; external standards may improve morality. But both of these views are conservative and lead to the rigidity and accretion explained above.

The Isolation of Professions.

The above conundrum--both external and internal standards for change are impossible without either admitting incompetence (internal) or employing incompetence (external)—leads to deep isolation of various enterprises. Doctors, nuclear engineers, cancer researchers, sociologists, etc., all set the standards of competence for their own professions. No outside profession can properly offer effective criticism of competence within a profession. Each profession thus becomes isolated from interaction with others because their standards of competence vary. Sometimes these differences are deep, e.g. different sciences post different aims for science (physics nowadays seeks mainly to extend predictive power; biology seeks to understand life or disease, etc.; psychology seeks true predictions that can serve as a basis for future theoretical programs; sociology seeks to find a paradigm for their science); sometimes they are shallow, e.g., the literary style of articles. But in any case these differences block communication.

This isolation would not be dangerous, if in fact professions had firm justified standards for the pursuit of their disciplines. But this is not the case. So, the claim that professions do have such procedures leads to the formation of guilds with their own initiation rites, their own brands of obscurantism, and their own requirements for the endorsement of the rules of the guild.

Even this would not be too serious, if these guilds were not placed at key positions in our society with the power to determine how our society is run. They have the power to control health care, psychological counseling, nuclear power, engineering, etc. In sum, the lack of standards for standards leads to the creation of guilds, which are isolated from external criticism and internal improvement. Yet these guilds exercise considerable power.

Two Explanations of Success.

A reasonable reaction to the above critique is that the use of competence as a standard cannot be that bad: It does in fact prevent harm and encourages the attainment of standards. Indeed, our own society is one of the most successful ever. This is no doubt true, but it does not vitiate the critique. lt does not show that the bad consequences mentioned above do not occur for the reasons cited. It does pose a problem however: how do we explain success?

My first explanation of the success of standards of competence is that some of these standards are designed to solve important problems and are successful in doing so. But this potential is not correctly explained by the rationale for competence. So, even where success is obtained, there is potential danger. There is potential danger, because it is easy to overestimate our standards when they lead to success. We may even lose sight of their original rationale. This can in turn lead to decreased effectiveness and rigidity when they fail.

My second explanation for success is that professions do in fact respond to external pressure. They change their standards, often surreptitiously, when their reputation demands it. When, for example, they are espousing views thought ridiculous by other professions or when under threat of punishment for lack of performance, they may change. So, even though some success is obtained, the problems discussed may still exist. Success does show that progress is possible even within rigid systems—especially in a pluralist society. But it does not remove the problems caused by the rigidity of the system.

How to Improve.

An alternative to current practice would be desirable in order to provide an effective way of appraising existing practices. Instead of seeking a guarantee of minimal or professional competence, we may seek solutions to problems in professions. Alternative standards may be achieved, if we specify problems, aims, and programs for the achievement of aims. The specification of problems, aims, and programs must be institutional. Such specification could take the place of the specification of minimal standards. Any activity or failure to conduct some activity which would render ineffective the particular solution to the problem which requires solution would be unacceptable. Minimal standards described here need not be specified in order to know when they are not being met. Furthermore, minimal standards are relativized to a particular problem situation. The approach thereby builds in a new flexibility; the standards are open to critical debate and change quite easily.

This procedure will not only avoid the bad consequences of requiring minimal standards, but it will also eliminate the task of forming minimal standards as a separate and distinct problem from those normally occurring in a field. The real problems of professions or institutions may then receive more time and interest. And we may substitute problems, which have a clear logic from those which do not. For example, problems of the minimal requirements for entry into professions may be replaced by discussions of how problems in these professions may be solved. The ability to solve problems can be integrated into a general criterion for entry to professions without minimal requirements for entry. People can be restricted to operating within certain areas of their abilities as measured by critical discussion. Specific tasks need not therefore be relegated to certain professions, but qualification would be more piecemeal and measured by ability to solve specific problems.

A second type of example involves problems of specifying minimal requirements for a clearly defined range of activities rather than solutions to particular kinds of problems. We can provide general and minimum standards for performance or we can specify that particular problems must be solved. We might require all cars to have a specific device such as an air bag, or we might require any car to have a solution to the problem of limiting harm in collisions.

The specification of aims and problems needs to be institutionalized, but since this specification may also become too rigid, we also need to institutionalize the criticism of the formulation of problems and solutions. The specification of problems and solutions can make this process more accessible and accountable than it now is. The specifications of clear cut problems will enable us to depend less on authority and to create an improved public forum for evaluation.

At this point it appears that the ghost of competence is rising once more: If standards of criticism are our test of acceptability, is it not necessary to have competent critics? On the suggested view we may proceed with some course of action after a critical discussion of the problems, programs, and courses of action proposed. But it appears that this requires that we set minimal standards for critical discussion before we proceed. This is not necessary, however, because we may have a critical discussion of the need for critical discussion in each case. Thus competent critics and/or minimal standards need not be specified as long as we also discuss the problems of critical discussion and of finding improved critics and criticism.

It is not clear, however, that in all cases for all purposes minimal standards should be removed. To varying degrees and in varying ways the problems of the removal of standards of competence or minimal standards may be intractable. My intent would be accomplished, if setting minimal standards were avoided whenever possible. If the setting of minimal standards were always to be taken as the admission of a problem and not the achievement of a cure, the logic of competence could not function in the unfortunate way I have described. For, from such a viewpoint, increase in minimal standards would always be a sign of incompetence and failure, responsibility would require articulation of problems, aims, rationales, and criticism instead.

Postscript: The Methodology of Against Competence.

In opening this essay I briefly discussed the methodology to be used in it. Here, it is useful to return to this discussion. I have violated at least some common methodological precepts and presented an essay, which is partly philosophical, partly sociological, and partly reform. A central methodological difficulty, which an essay of such a compound character poses, is: How can we evaluate criticism and proposals of the sort I have made? There are accepted standards of how to evaluate philosophical theses, sociological theses, and proposals for reform. But there are two factors which prevent us from using these traditional standards at least without reconsideration.

The first of these is the fact that these various factors are combined in a single analysis, whereas the standards are all designed to be used in distinct theories or at least in distinct ways. The second problem is that the aims of this analysis are new. One such new aim is to analyze the social consequences of the operation of idealized, yet partial, theories.

There are three aspects of the evaluation of this essay which I propose to discuss. The first is: How can we tell if the portrayal of the accepted views is correct? The second is: How can we test any of the sociological theses? The third is: How can we philosophically evaluate the essay? This third question ironically is the easiest to answer—at least on the criteria proposed here.

The essay can be evaluated by asking whether it poses interesting problems, argues plausibly, poses interesting theses, and fits into any broader, yet defensible philosophical position. The first and second questions are thus more interesting. Let us turn to the first.

The first question is: how can we determine whether the view of competence here portrayed is accurate? It seems plausible that, in employing the type of approach I have used, one could read into competence whatever attitudes or viewpoints one wanted to. And by simplifying and abstracting, as I have needed to do, one would present positions not seriously endorsed by anybody. In order to meet this objection, it is first necessary to discuss my problem situation and plan.

On the view presented here many views are institutionalized, i.e., many theories, generalizations, points of view, attitudes are taken to be accepted ways of viewing society and of justifying actions or views in society. Such institutionalized views are, indeed, often amalgams of clear theories, ad hoc additions, and random accretions. It might seem that such amalgams could never yield the rigid and limited standards of competence which I have abstracted from them. Nevertheless, I do presume that in this amalgam there can be found the operation of clear ideas, which do have a logic of their own. And a methodological presumption of this essay is that the very identification of these ideas and their logic will better enable us to understand the actual social processes in which they occur. So my aim is to understand these positions and their logic.

My initial and partial answer to the question of how we can evaluate the portrayal of institutionalized views and, of the logic of the use of these views, is as follows. In my analysis I am attempting not only to portray, but also to clarify and sharpen the ideas used in society. In doing this I am not primarily interested in ad hoc modifications of theories or viewpoints regardless of whether such modifications would make them more humane or defensible, or less. Rather, I am interested in clear and simple statements of rationale. An understanding of the simple views may allow for deeper and more effective change. The aims of descriptions and improvements are here combined. The prior aim of improvement helps to determine my problems; reform could provide tests; and description is needed for understanding.

A further reason for insisting on a presentation of unadorned versions of the rationale for our views is that, if we simply want to understand how the society is operating, we need to know the main forces at work independently of how they are diverted and modified. On the view employed here, the main forces are determined by the logic of the main institutionalized views, which are used to guide action in the society. To understand these views and the forces they create, we must begin by understanding the logic of simple and clear statements of these views.

I have thus far provided a rationale for presenting clear, simple, institutionalized views as a philosophical and sociological endeavor. I have not explained how we can determine if such clear views are correct portrayals. I think that to some extent we are limited in this determination. Confused views—which almost all institutionalized views are to one degree or another—can be read in different ways. But we can determine to some extent whether one reading is better than another. At the moment there is little competition against which to contrast my attempt to adequately capture simple, clear-cut statements of institutionalized views. But we can nevertheless evaluate such statements by asking whether they have explanatory power, i.e., by seeking to determine whether they explain actual trends and problems in society.

Let me turn to the third question: Are such theories testable? The answer to this question is a development of my answer to the second. In a rough sense, the answer is yes. We can test explanatory power by seeing if the views, attitudes, etc., predicted by the theory are present. Such testing would require still further interpretation. We would need to specify in various cases the types of attitudes, rationale, etc. which would confirm or refute the theory. This specification, however, would have to be set in a broad context of aims and degrees of success one would require to pass a test. Refutation would occur if some specified degree of explanatory power were not obtained.

Finally, the processes of testing, improving, and solving philosophical problems are intertwined. As just noted, when we form a clear theory about the use of a standard, we thereby change the nature of the standard and our attitude toward it; indeed, to state an unexamined standard and/or its rationale is already to make such a change. So, at times clear statements of rationale will be more important than tests. If statements of clear rationales lead to the formation of philosophical problems, whose solution can lead to reform, testing becomes of less importance. This is the case because philosophical reform is itself a value, for rationality can by itself increase humaneness.

*Joseph Agassi, Lynn Lindholm, and anonymous referees of this journal have made helpful comments on earlier drafts of this essay.

Interlude.

Popper's methodological rules asserted that one should always favor that alternative to some putatively refuted theory, which was the most adventurous, that had the highest quantity of new empirically refutable statements. But this is too simple: sometimes a small change can produce more significant progress. The facts, that different approaches have different values in various situations and that each approach may be conducted well or poorly, mean that sociological analyses of the consequences of various applications of rules are important.

2g 'On Conservative and Adventurous Styles of Scientific Research', *Minerva*, XXIII, 4 (l985): 443-63.

On Conservative and Adventurous Styles of Scientific Research

We have two images of style in scientific research. In one image, the scientist accepts risk in order to gain knowledge. In the other image, he acts carefully and responsibly to protect existing knowledge and to ensure standards in the acquisition of new knowledge. The growth of knowledge is deemed to be derived both from bold conjectures and from hard and cautious study of the facts. It is accepted that both adventurous new ideas and exigent standards are needed. Yet the two styles seem quite at odds with each other. One style requires the acceptance of risk and, therefore, the acceptance of the possibility of failure as a normal feature of science. The other regards the taking of risk as irresponsible.

The scientist who is an adventurer—not necessarily a genius—seeks to make significant innovations in the face of difficulties. He runs the intellectual risk that his conjectures will turn out to be incapable of solving the problems they are intended to solve and that they will not be accepted by the scientific community. He might also run professional risks, because he is in conflict with established authorities, whether scientific or political. Such a picture of the style of scientific research fits most of the heroes of science. Galileo had to overcome first the resistance of the Aristotelians in the universities and later had to deal with the church; Kepler had to overcome the discouragement of several false conjectures; Faraday developed his views quite alone and in opposition to his colleagues; as a patent officer, Einstein could dare to challenge the scientific establishment; Freud so upset the scientific establishment with his bold ideas concerning sexuality and scientific method, that he was forced to work outside universities. Many great scientists are adventurers.

The other image of the scientist portrays him as one who works slowly, carefully and responsibly. The hallmark of his style is trustworthiness. In his work, clear, objective criteria are severely applied. His results are products of hard and tedious work which removes the unreliable subjectivity characteristic of unscientific knowledge. The rationale for this style is found in the traditional view of scientific method according to which it is accessible to whomever is capable of learning scientific standards, and of adhering to them honestly and carefully in order to contribute to the growth of knowledge.

Science requires new and bold ideas to achieve progress as well as high and rigorously applied standards to maintain the body of scientific knowledge above the level of mere opinion. The two styles complement each other. One common view which reconciles them is that for most of the time scientists are conservative, occasionally closing ranks behind the newly found revolutionary leaders. A new conservative orthodoxy is thereby established. This is the view of Michael Polanyi and Thomas Kuhn.

The Conservative Scientist

The primary aim of both the conservative scientist and the adventurous scientist is to advance knowledge. The conservative scientist thinks that the best way to this objective is to protect scientific standards. He seeks to ensure that these standards are not lowered in application; theories or techniques of research which fall below them must not be allowed to enter the pool of scientific knowledge. He seeks to increase knowledge but only in so far as it does not require risky activities. He seeks to make small additions which adhere to strict rules and preserve already established theory.

Changes in accepted theory may be interpreted as a threat even by scientists who themselves have made substantial contributions to scientific knowledge. Joseph Priestley, for example, sought to develop chemical theory through modification of phlogiston theory, even though Lavoisier's revolution was generally accepted. Priestley believed these standards were right, that they had endorsed phlogiston theory, and he therefore rejected the revolution of Lavoisier. P. W. Bridgman accepted Einstein's revolution but he developed his theory of operational definitions in order to prevent the recurrence of such a revolution. Pierre Duhem, Michael Polanyi and Thomas Kuhn in their theories of science have expressed this attitude: Revolutions should always or for the most part be avoided in order to make possible the development of sound scientific knowledge.

In pursuing his goal of contributing to the growth of knowledge while limiting, if not completely avoiding, mistakes, the conservative scientist requires an appropriate choice of problems and appropriate techniques for seeking solutions. Unfortunately, his choice of both may sometimes be poor and do more to hinder the growth of knowledge than to promote it. Some scientists adopt the conservative attitude not only from intellectual disposition but to advance their careers by the appearance of soundness.

The conservative scientist chooses problems which will enable him to contribute to the growth of knowledge while not requiring solutions which would be dangerous, have unforeseen consequences, or conflict with established theories. The techniques he uses should enable him to find the desired solutions. He chooses problems which may be solved within the body of existing theories. Small extensions of an existing theory offer such opportunities. He tries to stay within accepted procedures and theory while still contributing to the growth of knowledge.

Strict adherence to accepted standards decreases any risk to his reputation as a scientist if things go wrong since in this case he may argue that he has conformed to the standards of the scientific community. Helmholtz appealed to such a standard in his discussion of the work of Robert Mayer. He was above all concerned to show that Mayer's work was not wrongly neglected, since it had not been proven empirically. But he did say that a false calculation by Mayer could not be held against him, since it could be based on an accepted though false result of Gay-Lussac. Helmholtz thereby endorsed the view that error which conforms to accepted doctrine should not be held against the individual scientist making such a mistake. The failure to adhere to accepted standards can bring with it serious problems. Faraday's experimental results, but not his theory, were accepted according to the prevailing standards which were defended by John Hershel, who achieved great eminence without making equally significant contributions as a result of his conformity with conservative standards. His greatest achievement—the mapping of the stars of the southern hemisphere—contributed to the growth of knowledge, but it did not conflict with established theory as Faraday's field theories did. Ohm faced similar difficulties arising at least in part from the theoretical changes he introduced.[176] His career suffered enormously. He became a professor only at the end of this career.

The coincidence of the technique of avoiding intellectual and professional risks with the techniques of contributing to the growth of knowledge does not always occur, as the difficulties faced by Faraday and Ohm show in their different ways. Conservative scientific work may not only be insufficient to further the growth of knowledge, it may also be a hindrance to this growth. Strict adherence to prevailing standards may serve at times

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[176] Schagrin, Morton L., "Resistance to Ohm's Law", *American Journal of Physics*, XXXI (July 1963), pp. 536-547. Further cases see Barber, Bernard, "Resistance by Scientists to Scientific Change", in *The Sociology of Science*, Barben Bernard, and Hirsch, Walter, (eds.) (New York: The Free Press of Glencoe, 1962).

to protect the interests of some scientists but not the interests of scientific knowledge as such. The conservative scientist may not always have the interests of science at heart. He may be conservative merely in trying his own professional interest. This type of scientist is, of course, quite different from the conservative scientist who wishes to protect scientific standards even at great personal risk. The scientist who is a conservative primarily to assure progress in his career is not always easily to be distinguished from the honest conservative scientist.

The self-protection is at a maximum when there is no pressure in the scientist's immediate scientific community to make fundamental contributions to the growth of knowledge. The proposal of "interesting" results within accepted standards invites criticism; the proposal of "reasonable" results requires only that one follow accepted procedures competently. In the cases of some standards, this may be relatively easily done. Other standards, however, can hardly be applied at all without the proposal of "interesting" but dangerous results.

Conservative standards for experimental research allow for interesting results but they often produce uninteresting ones. Experimentation is, of course, the backbone of science and rigorous standards for it are unavoidable, if scientific work is to be done at all. These very standards may, however, be abstracted from their intended purpose and applied merely to demonstrate conformity with them. A striking experimental result, such as Oersted's discovery of an interaction between magnetism and electricity, is an example of adventurous science; experimental results require novel ideas. But acceptable experimental results may be achieved without such intellectual novelty. Standards of importance are vague and difficult to apply while standards of precision and even ingenuity of skill are relatively easy to apply.

The standards of experimental research depend to a large degree on the requirements which scientific journals set for publication. The prime example of the conservative scientific style—perhaps the most important, because of its wide acceptance at various times and in various sciences—is the inductive style suggested by Bacon and Boyle. This style has conventionally taken the following form: a few general remarks, a description of the experiment and apparatus, a report of the results and a brief concluding discussion. It has been recently used in biological journals as well as in many journals in the social sciences and psychology. In psychology, such as journals as *Cognitive Science*, the *Journal of Verbal Learning and Verbal Behavior*, *Cognitive Psychology* and the

Journal of Personality and *Social Psychology* use this style. It is a style which leads to increasing specialization.

This conservative use of the inductive method is based on the view that all facts are important. Little space is therefore given for the explanation of the importance of the facts. The precision with which an experiment is described is a major criterion of acceptability for publication. The standard of fundamental significance is scarcely applied at all.

In psychology, it is more or less expected that experimental results confirm existing theories. Since scientific knowledge should be cumulative, the gathering of confirming instances is deemed the most important activity. Disconfirming evidence might not even be published. Psychologists at times avert their attention from such evidence because it might be blamed on them and not the theory.

Although the inductive procedure makes the application of experimental standards easy as well as safe, other procedures do not make such ease or security impossible. It is always possible to follow the conservative style when the standard of importance is low. Just as the inductive standard of experimental research may serve good purposes as well as being employed to avoid risk, the standards of clarity and precision are perfectly proper although they may be too tightly applied in practice.

In fields other than psychology, and even in some areas of psychology, such critical techniques are quite common. In cognitive psychology, for example, where specific models for language are sought, it is common to find counter-examples to even quite narrow models. The technique is also applied in reviews of books in those fields. Even in fields in which minor criticisms are relatively rarely found in journals which are bound to the conservative style, such critical remarks may be useful in seminars or colloquia. If the criticism is sufficiently minor it may help one's reputation yet cause no trouble through engendering conflict.

The standards of clarity and precision deserve perhaps special mention. Though their goals are quite uncontroversial, their application may not be. Theories of science have traditionally deemed perfect clarity to be absolute and attainable. Precision must also be absolute and attainable on these views since imprecision would damage clarity. Nowadays we are more sophisticated and would not deem clarity, much less precision, as being absolutely attainable. Varying degrees of clarity and of precision are needed for varying purposes. Yet there is a tendency to deem clarity and precision, especially that which is ostensibly obtained through formal or mathematical techniques, to be an end in itself. Edward Tolman's *Purposive Behavior in Animals and Men* was an example of the elevation of the standard of clarity above all others, but it failed to achieve its aim of "producing significant empirical results." [177]

There are standards for scientific research generally accepted by scientific communities which encourage, if they do not demand, novelty. Meeting such standards entails risks, for doing so might result in work which turns out to be clearly mistaken. The honest use of such standards by conservative scientists is important for the growth of science. The conservative scientist may find gaps in knowledge which can be filled without upsetting established theory. John Herschel's description of the stars of the southern hemisphere offers an example of this kind of work. But the more the conservative scientist submits to those standards which encourage novelty, the more he is pressed to become an adventurer. He may go slowly, seeking at each stage to integrate his views more firmly into the established context before venturing to defend them. This can be time-consuming and can distort the ideas if they are genuinely new.

The application of these standards may also lead to the use of techniques which enable scientists to be apparently successful even though these "successes" may be damaging to science. The requirement of testability is old—it was especially emphasized by William Whewell—but it has recently become more prominent in the philosophy of Karl Popper and the endorsement of his view by leading scientists. But before and after the development of Popper's theory, methods were developed and employed which weakened the powers of this standard by enabling scientists to appear to meet it, while not in fact doing so.

There is one main technique for doing so. The standard technique for rendering the requirement of testability ineffective while still acknowledging it as a major part of standards for research is as follows: The requirement of testability says that a new scientific theory should not merely explain a given set of phenomena but should also make new predictions so that it will be refutable. Refutations are regarded as valuable, i.e. successful, since they lead to progress. Even in traditional views under which refutations are not desired, testability is thought desirable as adding power

[177] Berkson, William and Wettersten, John, *Learning from Error* (La Salle, Open Court., 1984), pp. 184ff.

and, when the predictions are true, a stronger proof of the truth of the theory.

Independent tests are supposed to provide both risks and increased proof. They serve to determine which of two theories is better or whether a theory is faced with unsolved problems. The standard way to reduce or even eliminate risk is to allow for corrections of mistakes which need not themselves be independently testable. If a test confirms a theory, no problems exist. If a test leads to a putative refutation, one explains the result with some extension of the theory and treats the result as a sign of the power of the theory. The procedure produces the appearance of success. It is certainly legitimate to seek to explain or even explain away putative counter-examples in order to improve or save a possibly true theory. If this is done too often, however, it degenerates into a merely ad hoc defense of established theory. It may not be easy to judge, whether this occurring. Even our paradigmatic cases are not always so clear-cut. The Ptolemaic system is normally said to have suffered because of its need for ad hoc modifications, which the Copernican theory did not require. But the Copernican system also was not correct and required such modifications and even suffered from more serious problems such as the motion of Mars.

When such independent tests are not available, however, theories may still be protected from refutation, though they may be false. One example can be cited here[178]. My example is Pavlov, who is important for psychology as a result of the methodological model he offered. Pavlov offered a quite simple theory which was intended to be applicable to all psychological phenomena. According to this theory there are innate reflexes to certain stimuli. These reflexes may be conditioned in response to new stimuli. Salivation is an innate response to food. When a bell is rung whenever food appears, a conditioned reflex—salivation—to the sound of a bell may be created. Pavlov discovered also that sometimes such conditioned reflexes could not be created or would not occur. He thus developed theories of inhibition, which appear to be extensions and more precise statements of his view, but which at the same time allowed him to explain any phenomena. Since his dogs often fell asleep instead of becoming conditioned, for example, he simply deemed sleep an inhibition. He postulated varying degrees of strength of reflexes to explain other failures.

[178] Wettersten, John "Methods in Psychology: A Critical Case Study of Pavlov," *Philosophy of the Social Sciences*, Vol. 4, No. 1, March 1974, pp. 17-34.

These additions did not lead to new predictive power. Yet they were accepted as successful.

The standard which requires a clear foundation of the contrast between new theories and established ones may create risks for the conservative scientist. If the new theory is presented as contradictory to established views, it is a challenge to the proponents of the latter. One's view may be thought to be wrong as in the case, say, of Boltzmann; it may also be thought to have gone too far beyond the realm of science. Faraday's theories were regarded with suspicion, Schelling was said to have gone too far, and Goethe was problematic for a while and then rejected. Freud was first rejected and then later integrated. Recently, socio-biology has generated a similar controversy.

It is safer therefore to present theories which will avert disagreement. One way is to present mutually contradictory theories as applying to different ranges of phenomena. Thus a crucial test between the two theories is avoided by assigning the case which is incompatible with one theory to the other. This method appears to add precision by specifying applications.

If we say that one theory explains a wide range of phenomena, we are claiming greater explanatory power for the theory than if we offered two or three theories to explain the same range. Traditionally, the growth of science has been conceived as the process which increases unification. The methods here described go in the opposite direction. The more possibilities we add, the weaker is our claim for general, comprehensive, explanatory power. This is a variant form of conservative research. It runs no risk. In whatever way experiments turn out, their results may be classified as within or beyond specific ranges. Errors, if they occur, can be claimed to be merely those of classification. Even with increased precision and detailed specification of cases, explanatory power decreases. A simple and bold theory is turned into a complex one with steadily decreasing claims for explanatory power.

An example of such a development is offered by the history of learning theory, especially as it has been presented in textbooks. A common view found there is that various views which were originally put forth to account for all the phenomena of learning have in fact been found to account only for limited ranges. These ranges may be partially specified through experiments; new cases need to be classified. Pavlov's theory is said to describe a particular type of learning, but it is insufficient, since for some cases Thorndike's theory of rewards is used. If one takes this as a description of learning theory, one is left with the claim that any case of learning is explained either by Pavlov's theory or Thorndike's theory or by some other theory. The more empirical research is conducted and various alternatives are accumulated, the more secure the theories become, while their range of explanatory power becomes less and less.

Does such research further the growth of knowledge? Even if one concedes that it may at times lead to true predictions, such research is an obstacle to scientific progress.

The requirement of originality in scientific work presents another difficulty for the conservative scientist. This can cause difficulties when the scientist is called upon to explain in what ways his results are original. If his claim is too weak, then its originality cannot be acknowledged. If it is too strong, it may lead to the uncovering of mistakes. That mistakes might be present is, of course, trivial—mistakes are found in almost any serious scientific advance. Not only have scientists of originality themselves discovered and corrected the mistakes of others, but their own further advances have been based on the correction of earlier mistakes

In order to show that he is original while not raising difficulties, the conservative scientist has to justify his procedure. The ideal of originality may be affirmed; even the conservative scientist cannot overtly deny the value of originality. The most obvious technique to use is to make very modest claims, which are sufficient to justify publication and the claim of contributing to the growth of knowledge. A common rhetorical procedure is to claim that progress has been made but that much more work needs to be done before a general appraisal can be made. Thus, the standard of originality and the requirements of a conservative attitude are met at the same time.

The Conservative Scientist and Scientific Progress

The basic attitudes and preferred techniques of the conservative scientist prompt him to resist fundamental scientific change. At his best, a conservative scientist is not a reactionary against any and all progress. He regards science as uniquely valuable precisely because it is capable of growth. He affirms that soundness without discovery is just as contrary to the nature of science as discovery without soundness. But finding those techniques which combine soundness with the growth of knowledge is hard. When one seeks to do something new, the normal standards of soundness, i.e. conformity with established procedures, ideas and categories, break down to some degree. Conservative science requires that changes be legitimated by existing theories; but, in fact, the legitimate theories may be undermined by innovations. The conservative scientist is aware of the necessity to recognize not only Einstein but also lesser but still adventurous thinkers.

Conservative scientists may seek to resolve this conflict by rendering new ideas which are very daring, but have good elements, into respectable theories. This requires ingenious new ideas even though its purpose is to achieve soundness. The research of conservative scientists may attempt to bring a new idea into greater conformity with existing procedures and categories, and then incorporate desirable aspects of the new theory into the established views. This may involve delimiting the range of phenomena over which the new theory is applicable and thereby reconciling it with the prevailing views. The conservative procedure is to identify and assimilate what is new and compatible with existing theory or methods.

This kind of research can, of course, be quite valuable since there are adventurous hypotheses, which need to be refined by removing needless mistakes so as to increase their power. There is in principle nothing wrong with this procedure, but it does offer opportunities for less honorable activities. Since new ideas often challenge established leaders in the field, the latter sometimes desire that these ideas be brought into conformity with prevailing theories, not only because they are in some respects defective but simply because they are new. A new idea is made less disruptive if a modified version of it can be shown to be in conformity with a hitherto prevailing theory. The task can attract able, conservative scientists since it offers hopes of considerable rewards for reconciling an innovation which threatens established views with the views which it threatens.

The Adventurous Scientist

There are adventurous scientists who share the conservative scientist's aim of growth with responsibility. They seek to make discoveries and are undaunted by the risks of failure. There are also adventures in the pejorative sense of the word: They are self-seekers who care little for science. (I exclude from consideration here those seif-seekers who are plainly dishonest and who commit outright fraud.) There are others who seem to overcome the tension between adventure and responsibility. Adventurous science for them is the best way to pursue scientific truth; they conduct their adventure with sureness and peace of mind.

The honestly or genuinely adventurous scientist is the hero of science. Historians of science, even if they do not speak exclusively about adventurous scientists, build their work around their lives, the reactions to them and the progress to which their work led. This portrait of the scientist as a hero obscures the real dangers these adventurous scientists faced. Their mistakes are hardly mentioned. Kepler is perhaps the great exception. His refuted conjectures concerning the motions of the planets are celebrated.

Adventurous scientists often violate existing standards. This has led scholars such as Kuhn to the view that revolutionary changes are beyond standards—they are a new way of seeing the world. According to his view, the adoption or rejection of the new perspective is scarcely to be explained on rational grounds. An implication of this view is that scientific change is fundamentally irrational. Another is that conservative science is the proper model for all science. This attribution underestimates the rationality of adventurous science.

Problems may arise through the identification of inadequacies—perhaps contradictions—in existing theories, refuting instances or mere gaps in our knowledge. In either case, through the identification of problems, specific criteria are identified which new theories should meet. The intellectual tasks of the adventurous scientist require that he discern such problems in the existing stock of knowledge. The adventurous scientist then needs to formulate his own problem and the criteria with which he will attempt to conform. The scientific community may not want to open such challenging problems or it may be unaware of them. In the case of Faraday, for example there was a general, albeit vague, program of finding explanations of magnetic and electrical phenomena, but this problem was interpreted by contemporaries as a problem of extending Newtonian theory. His own discoveries of the inadequacy of this theory and his conjectures as to what an alternative should do gave him competing criteria for a solution. The unwillingness of his competitors to take his criticism of Newtonian theory seriously enough to acknowledge the need for an alternative contributed no doubt to his difficulty in finding a scientific public willing to take seriously his theories as well as his factual discoveries. Einstein, both in his early and his later research, also posed his own problems. His early work was accepted when his problem and solution appeared together. In his later period, his problems were rejected.

The identification of a problem serves the adventurous scientist as stimulus and a test. The success of his adventure depends of course on other factors as well, such as the independent explanatory power of any new solutions and whether they lead to new criticisms of existing theory. If his problems are not acknowledged as problems by the scientific community and if they are persistently resisted by the conservative scientists wishing to remain within the established doctrine, then he will have little chance of acceptance, regardless of his intellectual success.

The adventurous scientist needs more methods than mere problem formulation. He may have heuristic devices or he may use metaphysics as guide. The explanation of such work is quite problematic.

Adventurism in Science

The fact that adventurousness in science is acknowledged by all as important and is often highly rewarded tempts some scientists to be adventurers, with an aim of acquiring the social rewards of science rather than of making a contribution to the growth of knowledge. Adventurers—sheer adventurers—rely on the established methods of science to create their reputation. Whereas the genuinely adventurous scientist seeks quite definite criteria for success of failure, the sheer adventurer wants just the opposite. The former risks failure, but his success will be a valuable contribution to science. The latter prefers vague criteria. Since he aims at social rewards, his objective is reputation for success rather than scientific success itself. It is in his interest to have vague criteria which enable him to excite the admiration of other scientists but which limit the risk that his deficiencies will be discerned.

The formulation of ambitious and good projects is not easy if one seeks objectives which, when attained, yield genuine increments to knowledge and which are in fact attainable. The sheer adventurer, however, has it somewhat easier. It is relatively easy to form ambitious programs if the criteria of their realization are left vague. One may propose, for example, the unification of two disparate fields; Emil Du Bois-Reymond's unification of physics and physiology was such an objective. The proposal of a project is not enough to achieve the aim of the sheer adventurer. He also needs evidence that he is moving towards the fulfilment of his claims. His ability to do that depends on his ability to appeal to established methods and rely on his own social position. Sheer adventurers are sometimes like compulsive gamblers, who play to lose. They may make grandiose claims or raise hopes so that they will be found out and lose. Since the work of the sheer adventurer may be more easily reconciled with conservative science than that of the real adventurers, sheer adventurers may even be preferred, since they may reconcile the tension caused by the desire for new results and integration.

The techniques of the sheer adventurer are designed to create excitement through his capacity to make suggestions and, at the same time, to demonstrate respectability through the use of conservative technique. To gain attention with mere suggestions normally requires a prominent position in the profession. Outsiders or beginners will not be taken seriously, even though they have equally good proposals. In contrast, the real adventurer may attain success without prominence.

The proposals and ambitions of sheer adventurers may encounter opposition, and if they do it may even help. The bulk of the scientific community may adopt the same attitude towards the sheer adventurer in an established position as it does towards the student: It is willing to wait and see. Yet there the effect is quite different, since the person with a platform may benefit from the mere excitement without being called on to show the promised results. If it becomes clear that his promises cannot be realized, he may change them. He may forestall his critics by doing this before they expect it. He may thus cover failure and create new hopes from which he can continue to benefit.

The sheer adventurer must have a plan for what to do when his promises are seen as irredeemable. Unlike the conservative scientist whose promises are too undramatic to arouse high expectations, the sheer adventurer, because of the vastness of his claims, may be called on to show results. This need not be fatal however, if, instead of supplying results, he offers new promises. If he keeps enough promises afloat to sustain the excitement of others, his failure to fulfil his previous promises may be obscured.

The maintenance of high expectations while obscuring past failures may be difficult. The new suggestions must have some plausibility and they must be quickly generated if they are to attract attention. Few can continue this indefinitely. In the end the emptiness of the idea is usually recognized. This may not be damaging professionally to the sheer adventurer, if in the meantime the hopes he raised have enabled him to establish himself in a firm position within the profession.

The Interaction of Conservative and Adventurous Scientists

The two styles are to be found in practically all fields of science, although in differing proportions at different times. The two styles should be complementary when practitioners of each have the common aim contributing to the growth of science. The adventurous scientist recognizes as important the preservation of scientific standards and the need for the meticulous appraisal of new ideas. The conservative scientist not only recognizes the value of large adventurous contributions to science but sees them as a necessary complement to his own work. The adventurer seeks to make great contributions and the conservative may seek to appraise what should be recognized and what rejected.

The sheer adventurer is also not always in conflict with the conservative scientist. The apparent novelty of the ideas of the sheer adventurer can seem to be quite consistent both with the preservation of all that at any given time constitutes established scientific knowledge, and with the methods which are used by the conservative scientist to serve science according to his lights. When the sheer adventurer espouses flashy ideas such as a scheme for the unification of established theories, such schemes not only leave both theories intact, they serve to confirm and reinforce these theories. The sheer adventurer puts forth ostensibly new theories with rather vague content. Such theories call for the use of conservative techniques for assessing small modifications and vague promises of future success.

There is a further complementarity between the conservative scientist and the sheer adventurer in science: The sheer adventurer lends the appearance of novelty and excitement to science. Appearing to be tolerant towards these new and exciting ideas enables the conservative scientist to avoid giving the impression that his conservatism is a hindrance to scientific growth. He does so by appearing to co-operate with the sheer adventurer without endorsing him directly; he can merely point to the interest of the adventurer's ideas. When such a theory fails to yield the predicted results, the conservative scientist, not having committed himself in an outright fashion, can say that some other theory—perhaps equally empty—offers a chance for success. This is a normal reaction of the conservative scientist to any adventurer since he will not want to go against the new idea nor be too quick to approve of it. He will thus appear to be cautious and balanced.

The adventurous scientist and the conservative scientist can also live in a symbiotic relationship. The adventurous scientist requires for his success a receptive audience for his innovations. If he is to demonstrate the relevance of his new theory, he must show that there is some defect in the existing bodies of knowledge. The conservative scientist operates so as to prevent the defectiveness of established theories from becoming apparent.

Yet his defense of established theory may often be too powerful. The severe resistance to the ideas of Galileo, Ohm and Faraday are instances of this. Quite respectable methods of increasing precision of experimentation, of making small progress within established theories, as well as the employment of more dubious techniques of preserving terminology and introducing limitations on the scope of theories, can fulfil these obstructive functions in the face of the ideas of the adventurous scientist.

The genuinely adventurous scientist requires clear criteria for the assessment of the magnitude of his large innovation, while the conservative scientist wants to precisely prevent this. Clear criteria for the acceptability of large innovations pose a danger to the conservative scientist. When a conservative scientist recognizes that established theory is faced with troubles, he may try to limit change. There are, however, scientific objectives which are not easily compatible with such an intention. One such objective is explanatory power. The adventurous scientist and the conservative scientist often have conflicting aims. The conservative attitude of favoring existing categories, existing language and certain results nearly always work to the disadvantage of the adventurous scientist; it does so even if the innovation is of the first order. Major innovations conflict with established views as small changes seldom do. Not only are new ideas hard to understand, they are also sometimes difficult to express in prevailing forms of scientific expression. Problems of expression, of clarity and style, arise for new ideas where they do not exist for ideas which are already established. Furthermore, standards of justification are required for new research, which are not required for established research. Here the view that a theory is justified by well-observed facts is injurious to new ideas: All established theory is more highly confirmed by already made observation than proposed innovation, since even though the new theory turns out in the end to be right, it stands in much need of revision. It is only by ignoring currently accepted empirical justifications that new and better theories can become established.

But adventurous scientists do in the end win in their contests with conservative scientists. A conservative scientist may accept a revolutionary result ad hoc without changing the rest of his scientific beliefs. He might do this because he is persuaded by the evidence, but also for other reasons. A good theory has an inherent advantage. Even though a good idea is not guaranteed quick success, good ideas have a better chance of winning because they are also usable by conservative scientists.

The Adventurousness of Emil Du Bois-Reymond

Emil Du Bois-Reymond represents not only a good example of a sheer adventurer but also shows how conservative techniques may be combined with adventurousness. Du Bois-Reymond sought to create an existing impression of major intellectual progress, while at the same time maintaining that he only used the most careful scientific methods, in order to raise the regrettably low theoretical and experimental standards in his field. He insisted that he presented only results based on such superior understanding and technique. He employed virtually all the techniques; the sheer adventurer—combining the programs of physics and physiology, vague promises of future success in the correct description of electrical processes of the body and the establishment of a law muscle-currents, which he said was fundamental for all scientific research in electrophysiology. He purported to combine this with the conservative image of hard work, while avoiding publishing substantive results. His methods, which were the methods of the sheer adventurer, included the use of empty mathematical formulae, the discovery of small mistakes in the experimental work of his opponents, the argument that the work of others did not measure up to the latest standard of science, and the excessive detail in which his own work was presented.

These latter techniques were needed since his claims for his theoretical research were so high and the quality of his theory was so poor. The emphasis on rigorous science served to cover the weaknesses in his theory as well as to render it respectable.

Du Bois-Reymond began his career as a student of Johannes Müller, as did Herman Helmholtz and other subsequently important scientists. It was Müller who suggested to Du Bois-Reymond that he review the work of Mateucci and pursue the subject. This became his life's work in science. An early article of 30 pages which appeared in the *Annalen der Chemie und Physik* in 1843,[179] his major treatise, *Untersuchungen über thierische*

[179] Du Bois-Reymond, Emil, "Vorläufiger Abriß einer Untersuchung über den sogenannten Froschstrom und über die elektromotorischen Fischen", *Annalen der Physik und Chemie*, LVII (1843), pp. 1-30.

Elektrizität,[180] and his later *Gesammelte Abhandlungen zur allgemeinen Muskelund Nervenphysik*,[181] constituted virtually all his scientific work.

His first essay contained various results, presented as a series of points in good inductive style, with little and poor explanation. The discovery of muscle current and its direction was the major point which he developed in his later work but was not yet the claim for a thoroughgoing revolution such as is found in the later work. The essay evoked no response.

Du Bois-Reymond's next publication was his major treatise, the *Untersuchungen über thierische Elektrizität*. In this book he presented his central results as the foundation of all electrophysiology. The main point of these results was already contained in his earlier article. The treatise was intended to explain its significance and to improve on it. He hoped to show his originality against the claims of Mateucci, who had changed his view as to direction of the current in the muscles, as well to show the better perspectives his view offered in contrast with others. He said that his work would lead to deeper knowledge of electrophysiological processes. The book itself is a combination of review, history and polemics against his opponents, reviews of results existing in other fields—above all in physics and in electrical techniques—and the presentation of his own views. The first volume of 728 pages appeared in 1848; the second of 608 pages in 1849. The third volume of 497 pages did not appear until 1860, after he was appointed in 1858 to the chair of physiology in Berlin on Müller's death. The book is by no means unified. It gives the impression of being a collection of all that Du Bois-Reymond had written on the subject, regardless of how successful it had been or how far the research had progressed. Du Bois-Reymond acknowledged the inadequacies and offers apologies: He referred to the difficulty of his subject and to his other responsibilities and said that the fact that others might receive the credit for his own hard-won efforts had forced him to publish.

In the preface he argued against the theory of the existence of a life-force, which was the view of Müller, under whose influence and at whose suggestion the work had begun. Du Bois-Reymond claimed to have

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[180] Du Bois-Reymond, Emil, Untersuchungen über thierische Elekrizität, Vols 1, II, III (Berlin: G. Reimer, 1848, 1849, 1860).
[181] Du Bois-Reymond, Emil, *Gesammelte Abhandlungen zur allgemeinen Muskel- und Nervenphysik* (Leipzig: Verlag von Veit, 1875).

realized the century-old dream of showing the essential unity of the nerves and electricity. He expressed the hope of unlocking the secret of life.

He did not want to set his own research within the framework of Müller's theory. Helmholtz had published in 1847 his Erhaltung der Kraft which was put forward as a direct refutation of Müller's view. Helmholtz wanted to show that chemical or physical explanations of the functioning of the body did not allow for the operation of a life-force. In any physical process, energy had to be conserved. Du Bois-Reymond saw the need to place his own views within the framework of Helmholtz's ideas. He did so in his preface, intending thereby that his ideas should be viewed as properly scientific. Through his preface and through a sufficiently vague statement of his own program of research, he was able to animate interest as a spokesman of the new reaction against Naturphilosophie, to maintain the dramatic promise of uniting physical explanations with explanations of life-forces, and to make his own program of research appear to be scientifically respectable.

Having raised great hopes in his preface, he needed to show that they were realizable. He had, however, no theory of high explanatory power. He fell back, consequently, on the use of the methods of conservative science to prove the scientific respectability of his program. The fluctuations between appeals to rigorous science, suggestive remarks, polemics, formulations of laws and of hopes, were designed to combine comprehensiveness with rigorous research, large perspectives and portentousness. This allowed him to obscure loose ends, raise expectations without delivering results and to leave arguments vague.

His development of his broadest and deepest theory—the theory of electrical molecules—which should have shown the scope and significance of his hypotheses concerning the existence and direction of muscle currents described in his early work, epitomizes his approach.

The first chapter of the third part began with a discussion of the source of the electrical currents to be found in animals.[182] He wished to show that the currents were primarily muscle currents and that they were not unique to frogs. This was to give a theory of the current in muscles the central place which he wished it to have; it would show that a theory of muscle currents is identical with the theory of animal electricity. His aim was to

[182] Du Bois-Reymond, E., Untersuchungen über thierische Elektrizität, op. cit., Vol. 1, pp. 463ff.

explain nearly all such phenomena in any animal and to show that it applied to all animals. He argued that the current occurred in parts of the frog and that, therefore, not the whole frog but only certain parts alone were the source of the current. He devoted one section to the unity of the current and to Mateucci's doubt about it. He argued that it could not depend on various organs or on the interaction of tissues. He claimed on the basis of this chapter to have traced or reduced the general electrical currents to muscle currents. The nature of these currents, he said, were, however, complex. The explanation was to follow.

The first section of the second chapter served merely to present inconsequential or inconsistent results from the study of currents in muscles from end to end. Experiments which were designed to find such currents gave heterogeneous results. Sometimes the current went in one direction, sometimes in another, without any pattern. He hoped to show how all this would become clear if one studied the current between cross-sections and longitudinal sections.

The next section was the central effort of his whole career.[183] It introduced his law of muscle current, which was supposed to be the fundamental law of all animal electricity, and perhaps the fundamental law of all muscle movement. His theory was that there is a current in the muscle, always present, that the cross-section is negative and the longitudinal section positive. At some point or areas of the surface of muscles, the muscle fibers run parallel to the surface. This, in Du Bois-Reymond's terminology, was the natural longitudinal section. At other points of the surface the fibers end—the surface runs across the ends of various fibers. This, in his terminology, was the natural cross-section. Artificial cross-sections or artificial longitudinal sections could be created by cuts perpendicular to or parallel to the direction of the fibers. The law was extended to include descriptions of weak currents between two points on the cross-section or two points on the longitudinal section, the existence of which he had denied in his earlier work.

Du Bois-Reymond had two main problems in seeking to fulfil the great expectations he had raised. The first was to show that his law was an original discovery and not merely another formulation of similar results obtained by Mateucci. The second task was to show that the law offered a deeper understanding of the working of the muscles and that it was not a mere description of a muscle current. This task was related to the first, for

183 ibid. pp. 498ff.

he wanted to show that his view and not those of his competitors could lead to deeper knowledge, in particular by leading to explanations of currents in animals, which were carried over from physics. After seeking to provide some justification for his theory by confirmations through tests conducted on animals other than frogs,[184] and a discussion of his priority of discovery over Mateucci,[185] he turned to the task of showing how his law led to deeper understanding of physiological processes. He investigated the basic electrical processes of the muscle. This was the core of his theory.

His project was to show how the law of muscle currents could be used as a foundation for the discovery of the basic electrical processes of the body. His conjecture was really quite simple. It was that the currents found to exist between cross-sections and longitudinal sections of the muscles were produced by combinations of entities which made the entire muscle, and which had the same electrical properties as the entire muscle. Thus, one could cut a muscle and it would retain its electrical properties unchanged. This might be done indefinitely. He postulated therefore the existence of electrical molecules with properties of the muscle, negative poles at each end with positive areas in between along the surface. They were, he claimed, muscles in miniature.

Such a conjecture raises the question of what it explained. What arguments are there for its truth? In order to argue for his conjecture Du Bois-Reymond used two major devices. The first was to rule out other types of explanation of the muscle current. Such an effort could hardly achieve success; he would have had to show that only his explanation was possible. The French preferred chemical explanations which, in his later work, Du Bois-Reymond sought to refute. The second and basic device was to introduce primary analogies from physics in order to show that certain patterns which could be found in physics generated currents which behaved just as those of the muscles did. One could, therefore, presume that currents of muscles were caused by systems within the muscles, with the same properties as the physical phenomena. He thought he needed merely to analyze the analogies to bring the two cases closer together.

His method of arguing for his theory of electrical molecules relied on two characteristics of the analogous case. It needed to be recognized by contemporary physics and it needed to produce electrical phenomena

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[184] ibid. pp. 518-527.
[185] ibid. pp. 527-552.

which corresponded to the electrophysiological phenomena. Du Bois-Reymond did not require that the deeper theory of electrical molecules lead to new consequences. It was designed to explain the results he had already allegedly established concerning the electrical currents of muscles. He worked back and forth. He began with his muscle currents and constructed a rough physical analogy. He then tested the physical analogy against the properties of the current, modifying it to remove failures of the analogy. The analogy could never, of course, be complete.

He needed, for example, to be able to divide the source producing electricity indefinitely. None of his new models were so divisible. He introduced, therefore, a new analogy to explain this feature, and then switched back to the old to explain the individual parts. He nevertheless deemed the analogies so constructed to be adequate to support his theory.

Du Bois-Reymond's methods conflated quite different problems. 0ne was the question of whether such molecules could produce the observed current; the other was whether they existed at all. In the end he chose the weakest standards for the demonstration of their existence, i.e. their physiological possibility. His method enabled him to spend most of his effort in a discussion of his analogy, permitting him to demonstrate thoroughness while avoiding any question of the explanatory power of the tests for his conjecture. He thus gave the impression, though not the substance, of unifying fields—the impression, though not the substance, of a fruitful theory.

He began by proposing the search for deeper knowledge, which he had already begun by his studies of muscles; he rejected alternative theories such as those dealing with differences of potential between different organs or tissues.[186] His own alternative was that the muscles, or parts of them, are charged so as to produce specific types of currents. His plan to demonstrate this was to discuss various schemes, which he constructed to show that they produced the currents he had identified in the muscle. He discussed various possibilities, such as a copper cylinder with a zinc surface or a trough with a fluid conductor with zinc sides and copper ends.

After a long and meandering discussion of several models, he turned to the major task of developing and justifying his own theory. He wished to compare a model with the muscle to show that they both produced the same currents, and to find the characteristics that the muscle must have in

[186] ibid., p. 551.

order produce such a current. In order to compare the two, he regarded it possible to deduce from the law of muscle currents what occurs within the muscle and compare it with the model, or to form connections with an arc between various points on the cylinder and then between various points the muscle. In the latter case, if the currents in the conducting arcs we found to be the same in the two cases, he would, he thought, be justified deducing similar patterns in the model and the muscle. The experiments left open possibilities of varying patterns of varying currents producing the same effect in the connecting arcs.

He chose the second method; indeed, the first was obviously not open him. His law was not definite enough to permit deductions. He returned his simplest model—the sheets soldered together and covered by a moist conductor; he then proceeded to analyze what the current in a connecting arc should be, to determine if they were suitable as models for a muscle.

His next task was to test this theoretical result experimentally.[187] He began with the experiments on the simplest model. They produced the wrong results. These, he concluded after a long discussion, were attributable to variations in moist conductors. Since they did not play a role in the muscle, they might be ignored. He then tried to control these factors and claimed he had found the proper curve—given in a seemingly precise diagram—by experiment, though he said it was pointless to give the figures; they were too vague. After further discussion of his models and further difficulties, he turned to a comparison of the results found on the models with those of the muscle. At the end of this section, he claimed success in his comparison as well as in understanding his theory, since both suffered from the same problem: the curves describing the change in currents based in the experiments were discontinuous but had to be continuous. He claimed confirmation because the observations on the cylinder and the muscles were consistent with the same curve.

He concluded that the proposition that the muscle had the same mechanism with respect to arrangement of the various structures was not acceptable: The division of the muscle could not be accounted for.[188] He further mentioned the problem that various mechanisms and various currents could have the same effects on the connecting arcs. He tried further to justify his ignoring of the facts of conduction in the two models.

[187] ibid. pp. 596ff.
[188] ibid. p. 635.

He then turned to the question of whether other arrangements could produce the same currents. This seemed to be a preparation for a critical problem: Could not other models also account for the current? He did not deal with it, for, in consideration of this point, he restricted himself to one model. This was an arrangement consisting of a number of small cylinders of the same kind as that investigated. He said that he would study this possibility and found problems which he explained away. He turned to the investigation of what patterns might produce the electricity, all within the limits of the one model. He proceeded to say that all the possible ways of producing the current were equally good—he merely varied the shape of his cylinder.[189] Physiological considerations would then have to decide. He denied that such physiological considerations could be found in the fiber as he had presumed in his early paper; rather they had to be found in the parts of the fiber. The parts had to have two negative poles at the ends with a positive zone in between, but could have various shapes. He went on to say that, to avoid the charge of groundless speculation, he had constructed such a structure to test it; he reported that it confirmed his results, and discussed once more the various shapes the elements could take.[190]

We come then to the major point in which he puts forth his theory of the muscle molecules.[191] He warned that one should not treat his law as final—it might need modifications and additions with, for example, different types of molecules. It was not to be taken as proven, as one could reasonably believe in view of the care he had taken to develop his theory. He began the section with another analogy, appealing to Ampere and the fact that when a magnet was divided, the parts remained magnetic. This was the heart of Du Bois-Reymond's theory.

Du Bois-Reymond's theory did, of course, meet with opposition. A former student discovered that muscle currents such as those described Du Bois-Reymond were not always present, as his law and theory require. In response, Du Bois-Reymond modified his theory in accord with methods he had long used, and dismissed the research worker from his institute. He still maintained, however, that his own views should not be overthrown t had to be developed in detail if progress were to be made.

[189] ibid. p. 663.
[190] ibid. p. 672.
[191] ibid. pp. 678ff.

His theories could not survive but this did not matter greatly for his career. The excitement he had raised enabled him to gain a university chair in Berlin; he was twice Rector and in 1877, even as his views were proving to be empty, he became head of an institute where he remained even though he did scarcely any more research in physiology. In the afterword to the third volume, written in 1880, Du Bois-Reymond conceded the deficiency of his text; he added an index to help, he says, in the study the history of physiology. He thus more or less acknowledged that his views were superseded, yet he had established his position and given himself a place in the history of physiology. In this he has been successful as the relatively long, sympathetic article on him in the *Dictionary of Scientific Biography* indicates.[192]

In praise of Du Bois-Reymond it is said that he improved techniques of finding out, for example, how to make non-polarizable electrodes. It may also be pointed out that his experimental results have proven correct. It would be a mistake, of course, to classify Du Bois-Reymond as a sheer adventurer merely because his theories were false. Rather, it is because his theories had an apparent power of explanation, and because of the techniques he used to give these impressions without thereby providing a serious content, that he may be considered a sheer adventurer. He used the appearances and techniques with masterful effect to establish a brilliant career as well as a place in the history of science.

The Complementarity of Conservative and Adventurous Scientists

The daring of the adventurous scientist should serve as a balance to the possibly excessive reluctance of the conservative scientist to change, while the caution of the conservative provides tests for the courageous conjectures of the adventurer. Both conservative and adventurous scientists have the same aims: the growth of knowledge, on the one hand, and responsibility, on the other.

This ideal complementarity often breaks down, however. The failure of co-operation sometimes encourages an exclusive emphasis on one style or the other as the only proper way to do science. This complicates the task of their co-operation. The major cause of these breakdowns seems to be the use of conflicting criteria in the evaluations of theories. When the

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[192] Rothschuh, K. E., “Emil du Bois-Reymond”, in Gillespie, Charles Coulston (ed.), *Dictionary of Scientific Biography* (New York: Charles Scribner, 1971), Vol. IV. pp. 200-205.

conservative scientist seeks only the preservation of existing theory or clings tenaciously to existing theory, he cannot co-operate effectively with the genuinely adventurous scientist. When the conservative scientist is guided only by the ideal of rigorous science without the substance, he is, of course, of little use to the growth of scientific knowledge.

The effective co-operation of conservative and adventurous scientists requires, then, that they share the aims of the identification and acknowledgement of the weaknesses of existing theory and share sufficient criteria to enable them to discern such weaknesses and discuss them. This discernment can enable the conservative scientist to apply rigorous standards to existing theory while at the same time accepting and seeking change.

Co-operation between the responsible conservative scientist and the responsible, genuinely adventurous scientist is fostered by open discussions of criteria which existing theories fail to meet and which new alternatives should meet. Here the two types of scientists—to the extent that they have a sense of responsibility—have a common task. The explicit formulation of the criteria, which theories should meet, and the delineation of the weaknesses of existing theories are in the interest of both conservative and adventurous scientists. The conservative needs to do this in order to render standards fair and yet stringent. The adventurous scientist needs to do this in order to find clear standards for the appraisal of his innovations. Scientists may still be more or less adventurous, more or less conservative, but this hardly matters since the important task of the responsible development of problems and criteria is shared and provides a common ground for the appreciation of alternatives.

2h. 'The Sociology of Scientific Establishments Today', *British Journal of Sociology*, 44, 1 (1993): 68-102.

The sociology of scientific establishments today

Introduction I: Can the use of methodologies improve the sociology of science?

Are sociological studies relevant for appraisal of methodological theories? Do methodological theories raise sociological problems? Do they throw light on sociological aspects of science? The dramatic increase in research and interest in the sociology of science has led nearly all concerned to demand that the sociology of science be empirical and that it steer clear of old philosophical questions. But this same interest and the competition between various points of view renders old questions about how sociologists of science and methodologists can benefit from each other's insights interesting and pressing today. Sociologists of science have already shown wide and serious interest in these problems as they have sought to carve out a respected place for their discipline - or their various versions of this discipline. Some philosophers have also shown interest in extending philosophical research with sociological means.[193] But the issues have not been settled. The demand to be empirical and thus to exclude philosophical views is opposed to the obvious interest and relevance of these views for the sociology of science. I would like to make a modest contribution to the solution of the problem, how should the sociology of science be conducted? I argue for the view that methodology and sociology of science need to be actively integrated for the benefit of both. This approach can overcome important difficulties which have arisen

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[193] The major figures here will be discussed below. They are Michael Polanyi and Joseph Agassi. See Joseph Agassi, "Sociologism in Philosophy of Science" in *Science and Society*, Dordrecht, D. Reidel Publ. C., 1981, pp. 85ff. The fact that Agassi has offered a competing program with developed theses has escaped some, who might have noticed. See Peter Urbach, review of "Science and Society", *British Journal of Sociology*, vol. 34, 1983, pp. 151. He states, "There is no overall point of view expressed in this book and unfortunately no introduction drawing its several parts together" p. 151. In the preface to this book, p. xx, Agassi states, "The message of the present volume is this. Science will do better and be more humane if the (inner and outer) democratic controls of the commonwealth of learning improve, become more effective, and apply to wider areas". The introduction, "Science in its Social Setting", explains the background of this point of view in a discussion of Snow's two cultures, of Rationalism and Romanticism and a new rationalist approach.

in the recent history of the sociology of science. It can be used to develop alternative views of science and its institutions which may be appraised both philosophically and empirically. These difficulties are (1) the apparent conflict between the growth and or the change of standards with the objectivity or universality of science, (2) the difficulties of either using prescriptive aspects in the sociology of science or of excluding them, and (3) the difficulty of reconciling the authoritativeness of science with its openness. The view I defend here has been employed by such thinkers as Michael Polanyi and Joseph Agassi. The approach of these two thinkers to the sociology of science is very nearly the same; they each integrate methodological with sociological theories. Their results are sharply opposed. Their approach now finds little support from either side of the divide separating sociologists from methodologists.[194] Popper and most Popperians are rather against it. Inductivists ignore sociological problems. Sociologists seek independence. But the approach of Polanyi and Agassi remains relevant. This may be shown by explaining the capacity of integrated views to solve problems, which have recently become acute due to attempts to exclude methodology from sociology. The use of integrated views to explain science and its institutions would not end debates concerning the philosophical assumptions of the sociology of science; but their quality might improve. Philosophical theories might be recognized for what they are, given their proper place and appraised with the best and most appropriate standards available. Empirical theories might be more interesting due to their connections with philosophical views of science. Some philosophical views might be rendered empirical. And some empirical views might be rendered more theoretical and thus more interesting.

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[194] The Popperians, with the exception of Agassi, by and large ignore the problem. W. W. Bartley III, 'Alienation Alienated: The Economics of Knowledge versus the Psychology and Sociology of Knowledge' in Gerard Radnitzky and W. W. Bartley III, *Evolutionary Epistemology, Theory of Rationality, and the Sociology of Knowledge*, La Salle, Open Court, 1987, pp.423ff, briefly outlined such a program. Inductivists such as Salmon or Grunbaum have no place for it in their programs. Quine finds no particular problems in regard to scientific society. The Marxists or Habermas have sought to include the methodology of science in their social criticism. As Hans Albert has effectively argued, these forays have been based on a naive instrumentalism. Perhaps now Habermas seeks something else as he moves closer and closer to the Popperians in his social theory of communication, i.e. an open society. But he is still justificationist, appealing to conservative methodological views. By and large sociologists of science, although they have been enormously impressed by Polanyi and Kuhn, seek to render their discipline independent of methodology - as is discussed below.

Introduction II: Independence vs. integration

It is quite trivial to claim that sociology alone is insufficient to give an adequate picture of the whole of science. But the view that methodology should be integrated with the sociology of science - an obvious consequence of this trivial remark - is quite controversial. Sociologists of science offer their (sociological) pictures as complete quite independent of any methodological considerations. Some would even claim that no other picture is needed.[195] Attempts to preclude methodologies as background knowledge for the sociology of science today seem paradoxical. The encouragement various methodologies offer this discipline have been gladly accepted. But sociologists of science fear that the integration of specific philosophical background knowledge into the sociology of science would divide the sociology of science into competing intellectual schools and lead it to isolation, should philosophical fashions change, or lead it into a discussion of philosophical instead of sociological questions. As a consequence, however, they have no means of judging whether what they study is good or real science. They confuse the practice with the ideal. The neglect of the sociology of science by methodologists is paradoxical as well. It is trivial to claim that methodology alone cannot give an adequate picture of the whole of science. But methodologists tend to argue that methodology alone can explain the growth of knowledge.[196] The study of any other aspect of science has nothing to do with scientific research per se. But prominent contemporary philosophies of science such as those of Polanyi and Popper have led various investigators quite directly to the sociology of science. Philosophers of science are now often concerned with rules and conventions of scientific research rather than

[195] The move to relativism has been gradual - as I will discuss here. I conjecture that Bruno Latour has made an impact because he let the cat out of the bag - and caused a confused uproar. Should one bite the bullet or has something gone terribly wrong? He is an anything goes Feyerabendian in the sociology of science and is, thus, for some quite fascinating and for others - myself included - quite boring. The debate on the strong program has been going on, of course, for some time now. For a description of one reaction to Latour see B. Elzen, M. Gastelaars and M. Schwarz, “Critical Appraisal of Bruno Latour's 'Science in Action' or 'Can we Learn to Kiss Frankenstein's Creature?',” in E. K. Hicks and W. Callebaut, *Evaluative Proceedings, 4S/EASST, 1988*, Amsterdam, SISWO, 1989, pp 49ff.

[196] From Francis Bacon through the works of John Herschel, William Whewell, John Stuart Mill, the modern day positivists, Pierre Duhem to Karl Popper- to mention some prominent examples- methodology has been separated from social questions and even when methodology was closely related to historical questions as in the cases of Whewell, Duhem or Popper.

with the so-called logic of research alone. Clearly one may study the roles such rules play in science in order to throw light on the growth of knowledge.

Methodologists have nevertheless remained wary of this new source of knowledge about the growth of knowledge. They have tended to keep their distance from the sociology of science in order to avoid its temptations: This somewhat doubtful discipline might lead them from the high road of the study of the proper way of proceeding to the low one of explaining the pursuit of truth away.[197] But this method of maintaining virtue has its price: One cannot look at how science really functions and compare it with norms. This makes learning from mistakes quite difficult. One cannot see where virtue in science is lacking: The idealized image and the practice are confused. One can hardly even judge if the idealized image is possible. The pursuit of purity by the methodologists leads to sterile isolation from some of the real problems of science and thereby to confusion of the ideal with the practice. There is a paradox in the current relationship between the sociology and the philosophy of science. Developments in the philosophy of science have encouraged the growth of the sociology of science by deeming science a social activity. The sociology of science needs methodology to define and appraise its subject matter. Yet the two fields try to keep their distance as best they can. The pursuit of isolation in the two fields is regrettable. There is a loss of enrichment for both sides and each faces deep internal problems raised by the quest for isolation. My puzzle, then, concerning the prima facie sympathy and close connection between the philosophy and the sociology of science, on the one hand, and the very problematical relationship between the disciplines, on the other hand, arises due to ambivalence from both sides. Relationships would no doubt be improved if ambivalence could be removed. The problem, indeed, of the relationship between science as the pursuit of truth - and thus methodology - with science as a subject matter for sociological studies is the fundamental problem of the sociology of science. It is that problem which has rendered the field suspect and hard to integrate in

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[197] Karl Popper has been especially skeptical concerning the sociology of science since it seems to lead to explaining the views of one's opponents away instead of answering them critically. See Karl Popper, “The Sociology of Knowledge” in *The Open Society and its Enemies*, London, Routledge & Kegan Paul, Fourth edition, 1962. Popper here emphasizes the social character of science while criticizing the sociology of knowledge, i.e., the sociologizing of knowledge. He leaves open the problem of how to study the social aspects of science without sociologizing knowledge.

either broader sociological or broader methodological research programs. My argument follows a survey of modern developments as follows. The most prominent attempt to develop an independent sociology of science is that of Robert Merton. The most prominent attempt to integrate the sociology of science with the methodology of science is Michael Polanyi's. Polanyi's view became widely influential only after Kuhn presented a popular form of it.[198] Polanyi had opened up a much broader scope for sociological investigations but had done so at the expense of

[198] Bernard Barber and Walter Hirsch, *The Sociology of Science*, New York, Free Press of Glencoe, 1962 is by and large a Mertonian collection. It includes the work of others who may be deemed independent such as Derek Price, Edward Shils and many others and even an essay by Kuhn. But there is no quarrel with the Mertonian program. The problems concern the relation of science to society or its internal organization. The possibility of explaining scientific knowledge is not addressed. The defects in science are by and large seen as deviations from the established norms, perhaps as consequences of the applications of these norms. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, Chicago and London, The University of Chicago Press, 1962, appeared in the same year. Michael Polanyi, *Science, Faith and Society*, London, Oxford University Press, 1946 and Polanyi's central work, *Personal Knowledge*, London, Routledge & Kegan Paul, 1958, had appeared before Kuhn's text but the gradual attempt to rethink the sociology of science in the light of the views of Polanyi, Kuhn and Popper followed the publication of Kuhn's work. The dependence of scientific judgments on the context in which they were made or on changing standards seemed too many to open up the possibility that scientific views were (also?) socially determined. An example of an attempt to rethink matters in the light of these views is Jerome R. Ravetz, *Scientific Knowledge and its Social Problems*, Oxford, Clarendon Press, 1971. In England sociologists of science found Kuhn's work the basis for an increasingly aggressive and radical attack on traditional views. Barry Barnes, *Sociology of Science*, Middlesex, Penguin Books Ltd., 1972, begins to show a shift. Barry Barnes, *Interests and the Growth of Knowledge,* London, Routledge & Kegan Paul, 1977 seeks to develop a new view of matters. Michael Mulkay, Science and the Sociology of Knowledge, London, George Allen & Unwin, 1979 seeks to rethink matters once more. Bruno Latour and Steve Woolgar, *Laboratory Life*, London, Sage Publications, 1979 is an example of the beginning of more radical moves. These are reflected in Barry Barnes and David Edge, *Science in Context, Milton Keynes*, The Open University Press, 1982. In Barry Barnes, *T. S. Kuhn and Social Science*, London and Basingstoke, The Macmillan Press Ltd., 1982, the revolution is explained as due to Kuhn. In Karin D. Knorr-Cetina and Michael Mulkay, *Science Observed*, London: Sage Publications, 1983, the new sociology becomes dominant. Not all have bought this development as it has been portrayed by the Edinburgh group and their sympathizers. See for example Sal Restivo, 'The Myth of the Kuhnian Revolution' in *Sociological Theory*, San Francisco Jossey-Bass Inc., 1983, pp. 293ff.

introducing methodology into sociology. The problem of how and to what degree this opening could be used centered on the problem of how to (re)interpret the work of Kuhn so as to remove methodology from sociology. Some thought Kuhn extended the scope of sociology by changing its theoretical framework: relativism in science, they have claimed, was given an empirical, sociological foundation. Many thought this change improper. As a consequence the work of Popper became important: he offered the methodological criticism of this reading of Kuhn's reading of the sociology of science.[199] Attempts at resolution of traditional sociology of science (Merton's) with the expanded scope of investigation have been not been successful.[200] The British school has sought to develop a new sociology of science which abandons this attempt and which thereby falls - or jumps - into relativism. Edward Shils has also been enormously impressed both by Polanyi and by traditional views of the objectivity of science. He seeks to save both by developing a new understanding of and norms for the elite - the center, as he says. He seeks to integrate the sociology of science into a broader sociological framework

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[199] The central text for this debate between Kuhnians and Popperians is Imre Lakatos and Alan Musgrave, *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press, 1970. This book was designed to serve as an introduction to Lakatos' own theory, however, which was a mere political and opportunistic move from Popper to Kuhn. Popper discusses "Normal Science and Its Dangers", pp. 51ff. It is popular among the new sociologists of science to dismiss normative questions and to maintain the problems of prescriptive vs. description have been overcome. This I deny: science is not merely a social institution but also a means of gaining knowledge. Such a means may be normatively evaluated just as say, a system of justice, can be sociologically explained but also normatively evaluated. Calling Popperians dogmatists because they are interested in normative questions does not help matters much. Sociologists of science hostile to Popperian perspectives have also not come to terms with the empirical research of Agassi and even suggest that empirical research or those who do it must be Kuhnians. This is not hard-headed science but very soft-headed philosophy. For more interesting discussions of Kuhn see Joseph Agassi, "Kuhn on Revolutions: demarcation by textbook", pp. 117ff. "Kuhn and His Critics: Rational Reconstruction of the Antheap", pp. 315ff. For review of Lakatos see "After Lakatos: the end of an era." pp. 327ff. in *The Gentle Art of Philosophical Polemics*, La Salle, Open Court, 1988.

[200] One school sought to explain these problems away and the other sought to accentuate them. Some of those who accentuate conflicts between Merton and Kuhn have been mentioned above. For some of those who explain them away see Restivo, "The Myth of the Kuhnian Revolution", op. cit.

using his conceptions of center and periphery.[201] But he needs methodology as well which he introduces ad hoc as part of sociology. Joseph Agassi seeks to develop the sociology of science by the integration of Popper's critical views with Polanyi's sociological research. In contrast to other views, the elitism of science is seen not as a solution but a major social problem: science should be democratic. (Functionalist views are rejected in favor of institutional individualism.[202])

The brief survey of developments reviewed here reveals, that the problem of whether and how methodological decisions should be made in the sociology of science has not been resolved by new developments but intensified. The reluctance to integrate methodological decisions explicitly has led to distortions. It has led to (1) idealized pictures of the fairness and objectivity of scientific practice. (Merton) to (2) relativistic views, which explain science away and which, therefore, explain away the sociology of science as some special discipline as well. (Barnes, Bloor, Latour, etc.) to (3) hidden, implicit, uncritical use of methodology (nearly everybody) or to (4) mere ad hoc methodological decisions. (Shils) The explicit integration of methodologies leads to new possibilities. It broadens the scope of sociological explanation without making relativism necessary. Critical appraisal of the institutions of science is opened up to sociological studies. The social aspects of success and failure of science may be studied. And methodological theories may be appraised with sociological means as well as with methodological ones.

Merton: A research program for an independent sociology of science and its limits.

The problematic relationship between the sociology of science and the methodology of science has resulted from difficulties which arise no

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[201] Edward Shils, *Center and Periphery*, Chicago, The University of Chicago Press, 1975.

[202] Joseph Agassi, *Science and Society*, Dordrecht: D. Reidel Publ. Co., 1981. My own work in this field fits into this program as well. See, e.g. John Wettersten, "Against Competence: Towards Improved Standards of Evaluation of Science and Technology", *Nature and System*, Vol. 1, no. 4, 1979, pp. 245-256; "Procrustean Beds of Scientific Style", *Dialogos*, no. 36, 1980, pp. 97-116; "The Sociology of Knowledge vs. the Sociology of Science: A Conundrum and an Alternative", *Philosophy of the Social Sciences*, vol. 13, nos.1-2, 1983, pp.325-53; "On Conservative and Adventurous Styles of Scientific Research", *Minerva*, vol. XXIII, no. 4, Winter, 1985, pp.443-63; "Achievement and Autonomy in Intellectual Society", *Philosophia*, vol. 17, no. l, Jan.1987, pp. 55-75.

matter what position one takes. Attempts to gain independent identity such as Merton's or attempts at integration such as Polanyi's raise difficult problems. Both a secure and independent identity for the sociology of science and real and lasting integration seem elusive. Even though sociologists of science have been exceedingly self-conscious about their own discipline, no consensus about the relationship between the methodology and the sociology of science has been achieved.[203] "It is useful, then, to see how attempts to establish independent identity and attempts to integrate the disciplines have led to problems. The most prominent attempt to establish independent identity in this century is Merton's. The most pathbreaking attempt in this century to integrate the disciplines is Polanyi's. All later views are attempts to solve problems these developments led to. Central problems they have revealed are: Can sociology without methodology explain how science functions? And, can a sociology which is integrated with a methodology be scientific? Can the sociology of science be integrated into broader sociological research programs? Sociologists of science seem to be in danger of losing explanatory power by excluding methodology or of losing scientific status and/or integration in broader sociological research programs by including it. Even though Merton's view is widely recognized as an exceedingly important research program, he has never, so far as I know, presented it as such. Its recognition as a research program with its own special assumptions and problems has grown as opposition to it has become more intense and awareness of alternatives has arisen.[204] We may present

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[203] See for example Hicks and Callebaut, *Evaluative Proceedings*, op. cit. The importance of ethnomethodology or, how the sociology of science should be integrated in sociology proper, seem to be open (methodological) questions.

[204] For one portrayal see Norman W. Storer, "Introduction" to Robert Merton, *The Sociology of Science*, Chicago, The University of Chicago Press, 1973. Storer mentions Barnes and Dolby, "The Scientific Ethos: A Deviant Viewpoint", *European Journal of Sociology*, vol. 11, 1970, pp.3-25 as evidence for the wide recognition of the importance the Mertonian program or paradigm as it is called. In Robert Merton, "The Sociology of Science: An Episodic Memoir", in Robert K. Merton and Jerry Gaston, *The Sociology of Science in Europe*, Carbondale, Southern Illinois University Press, 1977, Merton describes his own and the profession's development and thus his program somewhat. Merton's concerns are different than mine however since he is primarily interested in the institutionalization of the sociology of science and of the methods it uses, whereas I am interested in the theoretical frameworks within the sociology of science or how it may be conducted. Merton there emphasizes that he introduced new methods into the study of science, in particular, prosopography, that is, the study of biographies as a basis for sociological analysis. He sees this as a great advance but

Merton's view as a research program, however, in the following way. There are three types of sociological questions about science. These are: (1) How is science integrated into (various) social environments? (2) What are the universal (sociological) characteristics of science? (3) How does science in fact function to produce knowledge? These three types of questions cover all bases. The sociological study of the content of scientific theory is precluded thereby avoiding old problems of the sociology of knowledge. Science may have differing relations to changing environments but there is only one type of science. The internal workings of science are to be explained functionally: we are to explain how they effectively produce knowledge. This sociological perspective fits nicely with the view of science as unified unique and proper. Conflicts with traditional (philosophical) views of science are effectively avoided. The sociology of science may be demarcated from its dubious cousin the sociology of knowledge and still have interesting tasks to work on. The research program for the sociology of science may be rendered complete. It provides questions concerning the external relations of science, its peculiarity and its internal functioning (above all, its systems of reward and punishment) and it provides for the unification of these in one picture of science: the study of each aspect should complement the studies of other aspects. In answering questions concerning the external relations of science to society, sociology may describe the institutionalization of scientific methods and norms. This may go far toward explaining how science has been created and developed. It may also explain how scientific institutions are supported or not supported by institutions other than science. This may give us important information about our society and how science may be preserved. The view that science has specific universal and unchanging standards opens up the second kind of sociological questions. These standards may be studied from a sociological point of view to explain the uniqueness of the social system of science. We may see how unique social standards are incorporated into the institutions of science. We may also find out more about the social rules of the game of science and how they function to produce knowledge as a complement rather than an alternative to methodology. The demarcation between this endeavor and methodology seems, to be sure, hard to draw. But surely, one might say, sociology must also be able to study these standards even if the results of sociological and methodological studies

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regrets that it has not had as wide a development as it might have had, had the profession been ready to co-operate on laying the basis for it, as Gillispie brought out the *Dictionary of Scientific Biography*. Merton does not tell us, however, just what tasks this method can now accomplish nor does he consider alternatives.

overlap. The study of the third question may be the most interesting for sociologists. Sociology may study how standards function in real scientific institutions. This has been for Mertonians above all a study of rewards and punishments, of priorities, recognition and gatekeeping. We may presume that the institutionalization of specific standards has specific and unexpected consequences even when they are quite proper. We may understand the details of science in the same way the broader society is to be understood. The use of such knowledge to improve science is not precluded but is minimal since such defects as the Mathew-effect or priority fights may be deemed unavoidable consequences of the proper functioning of science. Merton's view on methodology and sociology may be deemed complementary but distinct.[205] They are complementary in that methodology poses normative questions concerning the methods of science and how they lead to knowledge whereas sociology describes how these in fact have been institutionalized, what the sociological correlates of methodological rules are and how they function. Methodological ideas could play at least a heuristic function in the sociology of science. The two fields may nevertheless be deemed distinct. Sociologists of science do not need to do methodology to do sociology. They can discover norms of science by describing the standards they find within scientific institutions. They can presume a unified methodology without saying just what that methodology is.[206] This view is fine so long as there is sufficient agreement concerning the methodology of science. The sociological explanation of these norms by themselves need not fully explain how knowledge is produced. But if there are methodological controversies which touch the assumptions of the sociologists, sociological studies must be subject to methodological criticism. If sociological studies explain the nature and possibility of knowledge, they are already methodological and go beyond mere sociology. If sociological explanations do not do that, they do not (fully) explain how science functions. Due to their use of mere sociological standards for the identification of well-functioning scientific institutions, they may confuse institutions which inhibit the growth of knowledge with

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[205] Merton has expressed the view that the firm establishment of the sociology of science as an independent discipline has allowed it to fruitfully interact with other fields. See Robert Merton, "An Episodic Memoir", op. cit. pp. 67ff.

[206] Bernard Barber, "Toward a New View of the Sociology of Knowledge" in Lewis A. Coser, *The Idea of Social Structure*, New York, Harcourt Brace Jovanich, 1975, pp. 104ff. Barber pleads for a pragmatic view of scientific method, which presumes that science gains knowledge of reality but which does not seek to explain that in detail. In this way methodology or philosophy can, he hopes, be avoided and the sociology of science may concentrate on its own problems.

those that further it. One may claim, for example, that since uncritical adherence to a paradigm is found in 'scientific' institutions, this adherence furthers knowledge in some way. One may set a sociological program of explaining how that occurs without questioning the methodology the program presumes.[207] Methods found in 'scientific' institutions may, however, hinder the growth of knowledge. Problems concerning the tasks and the limits of the sociology of science are thus opened up by empirical studies and by their methodological interpretation.

Michael Polanyi: Subjectivist epistemology and a new sociology

As Merton was developing his own research program for the sociology of science, Michael Polanyi was developing an alternative, integrated view of the methodology and the sociology of science.[208] This alternative has also played an important role in succeeding discussions within and about the sociology of science, even if its influence is of a quite different kind. In contrast to Merton's program, Polanyi's program is based on the assumption that an understanding of scientific society requires an understanding of methodology and epistemology. At the time he began his research Polanyi held that no existing methodology was adequate to the task. A new view of scientific method and of knowledge had to be developed, he thought, in order to explain and defend the autonomy of science. Such a view was needed to solve problems concerning both the place of science in society and the social rules of scientific society. The foundation of knowledge had to be found in personal or tacit knowing, the educated intuitions, of scientists. From this perspective knowledge could be explained psychologically, epistemologically, personally, socially and anthropologically. Knowledge could not, of course, be merely personal: Externally it had to serve as a justification for the independence of science - even to non-scientists - and internally it had to serve to justify the rules of scientific society - even when these appeared dubious. In contrast to Merton, Polanyi sees scientific society beset with two very serious problems. The

[207] This seems to me to be the posture of those who see no conflict between the views of Merton and of Kuhn. See e.g. Joseph Ben-David, "Emergence of National Traditions in the Sociology of Science", op cit. Such a research program is not refutable but also plausible. But it must compete, in my view, with other research programs which make differing philosophical assumptions.

[208] Polanyi began developing his work in the late forties. The basis of his central work, *Personal Knowledge*, was the Gifford lectures of 1951-52. *Personal Knowledge* appeared for the first time in 1958, But in addition to various articles (See *Personal Knowledge*, op. cit. p. x), *Science, Faith and Society* appeared in 1946 and *The Logic of Liberty* in 1951.

first concerns the need to protect the autonomy of science from social planners and/or those who wish to reduce science to a utilitarian pursuit.[209] The second concerns the defense of the rules of scientific society: these seem often rather unfair, arbitrary and authoritative.[210] There is a danger from without that science will be ruined by those who seek to use it to pursue their non-scientific goals and a danger from within that the apparent defects of scientific society will serve as an excuse to reject science. The new 'personal' methodology and epistemology should in the first place explain how knowledge is really produced. The crucial theme is that the objectivity of science should not be overestimated as it has been in the past. This overestimation creates quite misleading pictures of science. If claims are too high, credibility will be lost. But claims should be high enough. The pursuit of scientific discoveries concerning the nature of the world should continue - even if such discoveries never quite reveal the true nature of the world due to subjective aspects of knowledge. The solution to the problem of how knowledge is attained should be used then to solve two types of social or sociological problems. The first are those problems concerning the role of science in society. What relations does it have to the broader society? What should these relations be? How can the proposed view of the relations of science to society - the view science must be autonomous - be justified in terms of personal knowledge? The second type of problem concerns the internal organization of science. If knowledge is personal in the sense intended by Polanyi, then how can we explain how scientific institutions produce knowledge? How should scientific institutions be organized? How can we justify this organization? In sharp contrast to Merton all problems are seen in the light of a specific and controversial theory of knowledge. This theory was designed as a substitute for traditional views such as those presumed by Merton. It leads to a quite different picture of science. Merton's view of science has universal objective standards which can be explained by all, even if the elite are needed to apply them correctly. In Polanyi's view the premises of science are personal and can even change. For Merton the justification of science and its autonomy poses no problem. For Polanyi this problem is

[209] This was the prime motivation. See Michael Polanyi, *Science Faith and Society*, Chicago, The University of Chicago Press, 1964, pp. 7ff. In a more philosophical tone see Michael Polanyi, *The Tacit Dimension*, Garden City, Doubleday, 1966, pp. 3-4.

[210] This second kind of problem was quite evident to him before he began his work in methodology and epistemology, since his own work in chemistry was ignored and then, later, the same results recognized when put forth again by another. They do not seem to play a role in his theoretical presumptions but arise here and there.

crucial: One has to study how the role of science is to be justified in the light of epistemology. Merton views the evaluation of scientific theories as clear cut and problems are mere aberrations. From Polanyi's view point this evaluation depends on personal knowledge and problems are endemic. The same difficulties are, then, explained first by Polanyi as a product of the problematical nature of scientific standards and then by Merton as a product of individual action. On the one hand they are deemed to appear all over; on the other hand they are deemed the (sad) exception.

Thomas Kuhn: Eliminating Polanyi's epistemology

Polanyi is widely known as both a philosopher and as a sociologist of science. He has many admirers due to his fight for the independence of science, for the depth of his theory of science and for his perceptive comments on science. But he has been overshadowed by Kuhn. This is stunning when one notices that the views of Kuhn are in all essential respects identical to Polanyi's prior work.[211] My own conjecture as to why Polanyi's work did not find the success that Kuhn's did is that it is by far

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[211] I will not develop this thesis in detail though one may point to a few important points: Polanyi introduced into modern philosophy of science the idea that any theory faces innumerable anomalies - a sea of anomalies as he put it (*Personal Knowledge*, p. 20; pp. 292-3; he defended the view that there were no general scientific criteria for a change in the premises of science - both the old and the new must face many problems *Personal Knowledge*, p. 18); he deemed these switches to be something like (Gestalt switches and to be psycho-sociological in nature (*Personal Knowledge*, pp.71 ff); he defended the use of "premises" - what Kuhn called "paradigms" to judge theories and would-be scientists (*Personal Knowledge*, pp.160ff.); he deemed, scientific education to be education into the implicit rules of the (scientific) culture. (*Science, Faith and Society*, pp.42ff. *Personal Knowledge*, pp.102ff.) The central problem he, just as Kuhn, faced in defending his view was to defend it against the charge that it was a view of science as merely subjective. (*Science, Faith and Society*, p. 15) Showing that Kuhn defends the theses put forth earlier by Polanyi may be bringing coals to Newcastle. Kuhn mentions Polanyi once in *The Structure of Scientific Revolutions*, op. cit., p. 44 and he there mentions Polanyi's "brilliant" development of a theme - tacit knowledge - which is similar to his own. In the Postscript to the second, enlarged edition, Chicago, University of Chicago Press, 1970, p. l91, he mentions him once again to praise him and to mention that Polanyi holds the same view as him. In Thomas Kuhn *The Essential Tension*, Chicago, University of Chicago Press, 1977, p. 262 he is mentioned once again. This time for his discussion of the scientist's reactions to anomalies. But no systematic comparison is made. See also John Wettersten, "The Fleck Affair: Fashions vs. Heritage", *Inquiry*, vol. 34, 1991, pp.475-88.

superior. It is deeper and more philosophical. The philosophy and the sociology came as a package. Sociologists have sought to accept the one while seeking, if they do not outright reject the other, at least to keep their distance from it. In order for Polanyi's work to be employed in the sociology of science the depth and philosophical aspects had to be removed or declared sociology. Let me explain.

Polanyi uses a clear view of knowledge to integrate his views of how science functions internally, of the place of science in society and of the justification of this place in society. His answers to Mertonian questions concerning how science functions rely on his methodological views which, he contends are needed. These answers to methodological and epistemological questions are quite appealing. Even those of us who favor views quite different from his have appreciated how much we have learned from him. But his view of knowledge seems nevertheless quite dissatisfying. Even though he emphasizes the purely theoretical value of science in rendering the world intelligible, in the end, knowledge is personal. It does not necessarily describe the world but we should be committed to it anyway. It is based in intuition which is not really explained: Are claims for knowledge mere expressions of faith in the intuitions of the leaders of science or are they more? If more, then what? In spite of Polanyi's explicit use of methodology, his theory has three aspects which make it interesting and important for sociologists. In the first place Polanyi acutely discusses the role of science in society and defends a view which is appealing to many sociologists of science: Science must be autonomous. As he does this, however, he does not lose sight of the fact that science is a part of a broader culture, that the support of this culture is needed and that it needs to be explained.[212] In the second place, Polanyi discusses the sociology of scientific change in new and challenging ways. Before Kuhn's work appeared he observed that all scientific theories pose numerous puzzles or face a sea of anomalies. The decision to replace one with another is an intuitive psycho-sociological process. The new theory will suffer from incompleteness and face anomalies just as the old one did. He notes, thereby, that theories embody specific premises, as opposed to universal standards, which are used to appraise innovation. In the third place Polanyi's theory renders clear social problems which science faces. Polanyi recognizes that science may take wrong paths, that it may deny deserved recognition and praise poor work or empty claims and that scientific standards are too weak to prevent that entirely. Polanyi seeks to incorporate the tragedy and mistakes which

[212] Michael Polanyi, "Conviviality", *Personal Knowledge*, op. cit., pp. 203ff.

science contains into his view. He studies the social consequences of scientific organization without apology, even while seeking to explain how the defects he finds are reconcilable with the ideals he loves. Polanyi still seeks to praise and defend science as the pursuit of truth in spite of the blemishes he points to. In the end he remains apologetic.

Polanyi, then, raises interest due to his sociological insight, and tensions due to his subjectivist methodology. Polanyi's view of knowledge conflicts with traditional views of science as the pursuit and attainment of truth. He substitutes subjective for objective knowledge. The use of this view in sociology may seem problematical. It would seem to require that the sociology of science, which was always suspect because it explained the objectivity of knowledge away, has once more returned to this old and problematical view. Polanyi's integration of methodology and sociology of science is internally quite powerful. He reconciles the difficulties of science with its authority. But the methodology required for this reconciliation conflicts with traditional views. Some alternative which preserves Polanyi's sociological insights but which leaves aside the objectionable, too subjective aspects of the methodology seems needed. Kuhn has been interpreted as having provided a way of filling this gap. Kuhn may be seen as lessening the tensions while maintaining the interests raised by Polanyi. He adopts Polanyi's fundamental ideas concerning scientific society but the clear view of knowledge is lacking[213] and the blemishes of science are ignored, explained away or declared desirable. Theological education or excommunication are not merely accepted but are deemed good for science - as in Polanyi. But there is no sense of tragedy and no sense that serious problems for the philosophy of science are thereby raised. Kuhn offers a description of scientific revolutions which follows Polanyi's. But his view avoids the developed theory of scientific knowledge as subjective and it sees science without blemishes. It deems knowledge a sociological phenomenon - or lends itself to this interpretation - and explains it as such. This may seem to many an advantage since they can use the sociology without concerning themselves

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[213] The attempt by Kuhn to defend his view against the charge of irrationalism, while maintaining its central tenets are well-known but not convincing. See Kuhn, "Postscript" to *The Structure of Scientific Revolutions*, op. cit., "Revolution and Relativism", pp. 205ff. He claims his view is not "mere relativism" in *The Essential Tension*, op. cit., esp. 320ff. See also, Joseph Agassi, "Kuhn and His Critics: Rational Reconstruction of the Ant Heap", in *The Gentle Art of Philosophical Polemics*, pp. 316ff. and Alan Musgrave, "Kuhn's Second Thoughts", *British Journal for the Philosophy of Science*, vol. 22, 1971, pp. 287ff.

too much with methodology. Conflicts between sociology and methodology can be dodged. By appearing to allow for the separation of the sociology of science from the theory of knowledge, Kuhn seemed to open the way for new sociological approaches to science, which could employ the sociological insights of Polanyi without having to worry too much about his methodology.

Using Thomas Kuhn: A new research program or new empirical problems?

Merton tells us that both he and Edward Shils encouraged Kuhn.[214] From the very beginning his work was interpreted as an extension of the sociology of science rather than a deep change. For Shils, who seems so close to Polanyi, this can be no surprise; but it is also true for Merton. We find here an apparent coalescence of interests. Merton and Shils sought talent to develop the sociology of science and Kuhn was a find. Kuhn proposed, namely, to describe scientific change sociologically. If not for all sophisticated functionalists, this was a central problem for Shils.[215] The sociological description of such change is a central and difficult task for Mertonian sociologists as well. How are the same standards used first to endorse (as true) and then to overthrow (as false) the same theory? Merton and Kuhn seem to have deemed themselves - or do deem themselves - to be working on the same program and to produce even complementary work. On this interpretation Kuhn's research opened up new empirical questions for the sociology of science. Scientific standards change. Such change should, then, be studied sociologically and, perhaps, reconciled with the universality of scientific standards. The extent of the reaction to Kuhn's work was, perhaps, a surprise. It seemed at first a mere extension of some established views. The reaction was due to the realization that the Kuhn's view raised deeper problems than was at first realized. The separation of Polanyi's epistemology from his sociology did not come as easily as intended or desired. When *The Structure of Scientific Revolutions* introduced views developed in a more sophisticated way by Polanyi to a wider audience, its observations were used not merely as a source of ideas for new empirical research but also to both develop new views of the sociology of science - or of knowledge and to provide a foundation for

[214] Merton, "Episodic Memoir", op. cit., pp. 99-100.
[215] See, for example, Shils "Society: The Idea and Its Sources", in *Center and Periphery*, op. cit., pp. 17ff. esp. p. 33. (First published, 1961).

criticism of Merton's view.[216] The view of the complementarity of the work of Merton and Kuhn was questioned and attacked. The Polanyi-Kuhn view opened up philosophical questions concerning the theoretical framework in which the sociology of science was conducted. Was science unified in the ways presumed and portrayed by Merton? Did Polanyi's or Kuhn's theory offer an acceptable alternative? These questions led to the major contemporary split in the sociology of science. The American-Mertonian program called for more empirical research to enrich the sociology of science, while reconciling the practice of science as described by Polanyi and Kuhn with the assumptions of the established program. The English-Edinburgh program called for a new research program - which they have misleadingly called a demand for empiricism. The new research should not seek to explain how science functions as a unique and rational society but should deem science merely one society among others. Any potential conflict between deeming scientific society something special, on the one hand, and explaining it with established sociological methods designed for any society, on the other hand, could be avoided. The sociology of knowledge received new life after it was long thought dead due to the criticisms of Mannheim's approach.[217] This interpretation

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[216] On the interpretation of the development of the sociology of science given here, the emphasis which has been given to the work of Ludwik Fleck is an attempt to establish that Kuhn's work belongs in a tradition of the sociology of science. This tradition, it is suggested, was not yet developed at the time Fleck published his work, so his book suffered the same fate as Merton's work on seventeenth century-science. Since Kuhn has developed this approach we can see how advanced the work of Fleck was. A philosopher such as Popper on the other hand was merely an expression of the times then and has now been superseded. If Fleck is seen as Kuhn's prime predecessor, Polanyi and philosophy of science may be seen as hardly relevant: the message which is not quite explicitly asserted. See Ludwik Fleck, Genesis and Development of a Scientific Fact, edited by Thaddeus J. Trenn and Robert K. Merton with Forward by Thomas S. Kuhn, Chicago: University of Chicago Press, 1979. See esp. Forward by Kuhn and Preface by Trenn. A different view of matters is offered in the German edition, Ludwik Fleck, Entstehung und Entwicklung einer wissenschaftlichen Tatsache, Frankfurt am Main: Suhrkamp, 1989. See introduction by the editors, Lothar Schafer and Thomas Schnelle. The interest in Fleck was then picked up by Robert S. Cohen. See Cognition and Fact: Materials on Ludwik Fleck, Dordrecht: D. Reidel Publishing Co., 1986, edited by Robert S. Cohen and Thomas Schnelle. See also John Wettersten, "The Fleck Affair: Fashions vs. Heritage", op. cit.

[217] Ben-David suggests that this new movement was possible because those who led it were ignorant of the history of sociology and the discussions of Mannheim's view. Ben-David, "Emergence of National Traditions in the Sociology of Science"

cannot be correct, one might say, since modern sociologists of science have emphasized not only how the research program in the sociology of science has changed but also that one important aspect of this change has been the move to relativism - an obviously epistemological or philosophical theory. This reaction, however, may also be seen as a product of Kuhn's pliability. It is possible to read him this way or that and he does not allow himself to be pinned down easily. But the relativist reading of Kuhn is easier to accept than Polanyi's theory. Polanyi emphasizes the intellectual values of science and tackles these problems. The relativist reading of Kuhn, on the other hand, comes to the conclusion that sociology is enough. Polanyi sought to save the independent values of science and deemed scientific society something unique and of a high cultural value. The new sociologists explain all that away. The exclusion of good philosophy thus demands its price. Kuhn serves the interest of the Mertonians and the British relativists better than Polanyi. The Mertonians can ignore his methodology and the British can deem it sociology. It is Shils, as I explain below, who follows Polanyi more closely. He attempts a third alternative, but he also seeks to re-do Polanyi by developing (ad hoc) a more objectivist view of science as a view of scientific institutions.

Using and abusing Karl Popper: Keeping methodology out and realism in; keeping realism out

The philosophy of Karl Popper is mentioned often enough by sociologists of science[218] to ask what his role in this discipline, if any, might be. He has, to be sure, emphasized that science is social. Robinson Crusoe could not do science and intersubjectivity is needed to avoid subjectivism and dogmatism. But he has developed no sociology of science himself and is even hostile to previous attempts to explain science sociologically. He sees here a danger that science as a pursuit of truth will be explained away. There are nevertheless two quite different reasons why he has been so widely noted in this connection.

Firstly, Popper has developed a challenging methodology and theory of knowledge which is modern but closer to traditional views than Polanyi's and thus appealing. His philosophy of science has been seen as giving

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in Jerry Gaston, *The Sociology of Science*, San Francisco, Jossey-Bass Publishers, 1978, pp. 203ff. esp. p.207.

[218] Merton, "Episodic Memoir", op. cit., pp. 68ff, and Ben-David, "Emergence of National Traditions", op. cit., p. 210, and Harriet Zuckerman, "Theory Choice and Problem Choice in Science" in Gaston, *The Sociology of Science*, op. cit., pp. 66ff.

impetus to this field because, according to Popper's view, science is social. He deems the task of the philosophy of science to be the study of methodological rules.[219] Given such a view it seems an obvious and interesting task to examine how such rules function from a sociological as well as a methodological perspective. Here, then, is a possibility of developing sociological studies using Popperian ideas. His view suggests an interesting if undeveloped research program. But unless one counts Agassi's proposal for the study of scientific society as the (partial) institutionalization of a critical, rational, universalistic endeavor, it has not been developed. Secondly, Popper has become popularly known as a major critic of Kuhn's methodology even though he concedes that what Kuhn calls 'Normal Science' makes up a good part of actual 'scientific' practice. Popper thus offers hope of a methodological answer to Kuhnian doubts about the universalism of science. Popper's defense of the objectivity and universalism of science[220] might complement the sociology of science. Popper's view might offer new support for the sociological studies of the universalism of science.

Although Kuhn and Popper each played a role in encouraging the sociology of science, they did so in different ways. Kuhn was seen as a source of new ideas for empirical research by the American sociologists of science and as a new, relativist research program by the British school. Popper could be used as a confirmation of universalism in science by the

[219] For a discussion of the move by Popper to a view of the philosophy of science which requires methodological rules see John Wettersten, "The Road through Wurzburg, Gottingen and Vienna", *Philosophy of the Social Sciences*, vol. 15,1985, pp.487-505.

[220] Zuckerman, "Theory Choice and Problem Choice in Science", op. cit., p. 67, mentions this connection between universalism and Popper and that the gap between philosophy of science and empirical sociology is wide: The so-called "concept indicator" problem poses an alleged problem in determining if scientists choose problems in the way philosophers say they should. (Ibid., p. 86.) This is one indication that Zuckerman does follow positivist views which deem a common neutral language necessary for science and not more modern views in which this would be no problem. Ben-David, "Emergence of National Traditions", op. cit., p. 210, also notes the opportune connection between Popper's view and the sociology of science. Popper's methodology seems to serve the purpose of removing the methodology from the sociology of science by providing a methodology which accords with accepted intuitions among sociologists. This accords with Barber's proposal in his "Toward a New View of the Sociology of Knowledge", in Coser, *The Idea of Social Structure*, op cit., p. 105, where he recommends the acceptance of a realist view of science as a pragmatic background for the sociology of science.

Americans, who thus saw no conflicts between Popper and Kuhn which could not be resolved. The British school emphasized the conflict between Popper and Kuhn just as they had between Merton and Kuhn. Popper had to be defeated if relativist sociology of science were to be defensible.

Mertonian sociology of science after (or with) Polanyi and/or Popper

Merton strongly supported Kuhn before he wrote his famous *Structure of Scientific Revolutions* and finds this book quite valuable. In many cases we can see that Mertonian and Kuhnian sociology overlap even though they seem at first blush quite distinct.[221] They seem quite distinct because Merton sees in science the application of universal standards, encouraged in seventeenth-century England at least by Puritanism, but which are unchanging from Galileo to the present. He has even tried to say what they are with his now famous four organizing principles of science. Kuhn, on the contrary, says that the appraisal of scientific research depends on the paradigm which is accepted at this or that period of time. So, it would appear, standards are not universal and unchanging. They change at least as often as paradigms do.

Merton's program for reconciling these views might seem plausible. Although he deems standards of science to be universal and unchanging, these unchanging standards are quite abstract. In addition to describing them, he may argue, we need to see more closely how they are applied. Here we might turn to Kuhn (or Polanyi) for insight. We might seek to find out how science changes even though its standards remain the same. Such a view is found in research programs of Ben-David[222] or of Cole and Zuckerman.[223]

In other respects Merton's view of the application of scientific standards is not so different from that of Kuhn's. Both thinkers emphasize the importance of the elite in making decisions concerning how science should proceed or who should be rewarded and who not. Each must appeal to intuition as Polanyi did before, since each maintains that only the elite can make proper decisions. The "old" sociology of science which emphasized the

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[221] Norman W. Storer, "Introduction" to Robert Merton, *The Sociology of Science*, op. cit., p. xxviii defends such a view. See also Merton, ibid. pp. 554ff.
[222] Ben-David, *The Scientist's Role in Society*, Englewood Cliffs: Prentice-Hall, Inc., 1971.
[223] Jonathan R. Cole and Harriet Zuckerman, "The Emergence of a Scientific Specialty: The Self-Exemplifying Case of the Sociology of Science", in *The Idea of Social Structure*, op. cit., pp. l 39ff.

study of the reward system, of priorities and of how views come to be established does not seem at first blush to be in conflict with Kuhn's views. Neither the perspective of Kuhn nor that of Merton, however, yields a reconciliation of the sort sought. To find one from a Kuhnian perspective one needs to explain the nature and role of universal rational standards in the choice and implementation of paradigms. But this is already a quite new and different theory of science. Although Kuhn has worked in this direction, I do not understand how he has, or can, develop a view which reconciles the rationality of science with his view of the role of paradigms. The ambiguity of Kuhn on this point is well-known.

To find a reconciliation from the perspective of Merton one needs to show how this universal standards allow for (some) change of standards. But how much and where are the limits? Here we also find serious problems which are not solved in Merton's system. If Merton draws the limits to exclude some of 'scientific' practice, his view is prescriptive as well as descriptive. If he does not, he has, according to the views of Polanyi and Kuhn, given up his view of the universality of scientific standards.

The integration of Mertonian and Kuhnian views of science has not proved as stable as its defenders had wished. The resolutions which I have sketched have never been successfully developed. The quest for a better and more comprehensive theory of science which includes the effective, proper exercise of power by the elite, the useful development of so-called normal science and the critical pursuit of truth in accord with universal standards continues.

But problems arise as well for Merton's view should he side with Popper. In Popper's view of science there is no place for elitism endorsed by Merton. Popper deems the objectivity of science to be preserved by the openness of science. And this openness is not dependent on the elite but on rules. Anyone can put forth his ideas and have them evaluated on the basis of an open discussion. This view may conflict with practices found in scientific institutions but when this is so, so much the worse for these institutions. One might ask, of course, why does Merton need elitism? The answer seems to be that this is an integral part of his research program. This program sees the granting of recognition rather than the appraisal of ideas as crucial. He studies the processes of recognition in scientific institutions. And this recognition is not, he thinks, democratic; it depends on the gate-keepers in fact and, he might add, for the good of science. But here there is great tension in Merton's view. If his universalistic view of science is true, why do we need gate-keepers? Can and should ideas be appraised in open forum, in accord with universal and impartial standards, as Popper claims or are they appraised by the intuitions of the elite, as Polanyi claims?

Modern British sociology of knowledge: The path to relativism

The difficulties of reconciling a purely descriptive sociology of science with a view of science as the pursuit of truth led many thinkers to give up hope of finding an integration of these views. Instead of solving these problems they have sought to explain them away or have simply deemed them passe. The conflict between descriptive and prescriptive approaches to the study of science has been deemed overcome. When one looks more closely, however, at such claims it is not possible to find any answer which does not deem whatever did happen as correct. The conflict is only overcome insofar as one abandons traditional conceptions of truth as objective and unchanging.[224]

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[224] There are various proposals for overcoming or avoiding the problem of description vs. prescription. Defenders of such views regard those of us, who still deem this a problem to be out of date. I find the proposals here rather thin, however. In response to my defense of views discussed here at the 4S/EASST conference in Amsterdam in 1988, it has been proposed that those who are not ignorant of recent philosophical discussions, such as myself, know that the problem of the boundaries have been overcome. Richard Rorty, for example, had allegedly resolved the problem. See *Evaluative Proceedings*, op. cit., pp. 42-3. Rorty's work will hardly help us, however. See, e.g., Peter Munz, "Philosophy and the Mirror of Nature," Philosophy of the Social Sciences, June, 1984 and in Gerard Radnitzky and W. W. Bartley III, *Evolutionary Epistemology, Theory of Rationality, and the Sociology of Knowledge*, LaSalle, Open Court, 1987, pp. 345ff. Yehuda Elkana also claims that this problem is old hat. See Yehuda El- kana 'Boltzmanns wissenschaftliches Forschungsprogramm und seine Alternativen', in *Anthropologie der Erkenntnis*, Frankfurt am Main, Suhrkamp, 1986, p. 183. He proposes that Lakatos's problem of the rational reconstruction has replaced it. But this has already been seen to be a degenerating research program - as Lakatos would call it. See Joseph Agassi, 'After Lakatos: the end of an era', and Gunnar Andersson, 'Lakatos and Progress and Rationality in Science: Reply to Agassi', in Agassi, *The Gentle Art of Philosophical Polemics*, op. cit., pp. 329ff. Elkana proposes his own two tier theory. Yehuda Elkana 'Anthropologie der Erkenntnis. Ein programmatischer Versuch' in *Anthropologie der Erkenntnis*, op. cit. pp. 1 l . He proposes that the conflict between relativism and realism is not necessary but merely an historical situation. He hopes to overcome the problem with an historical and two-tier approach: We have a framework set historically and we can be realists in the framework. I am not satisfied: The framework or metaphysics deserves, in my opinion, a realist interpretation as well. History is not enough. All such attempts,

In the place of sociology of science, which presumes the objectivity of scientific knowledge, relativist sociologies of knowledge, which presume that scientific knowledge is merely established knowledge, have reappeared. Such sociologies of knowledge are for the most part based on the acceptance of the second reading of Kuhn's approach to the study of science. According to this reading science may be understood in terms of the social conditions and techniques relevant for the fight for power and influence.[225] The attempt to defend realism or the objectivity of knowledge is thereby abandoned or so watered down as to be mere lip-service to the tradition. This separation of methodology from sociology, of normative from descriptive questions, is, they hope and claim, completed.

This approach apparently opens the door for new and more powerful methods borrowed directly for social sciences. Science may be studied as any other social activity. This allows for an easier integration of the sociology of science in the social sciences proper. And sociologists of science have always deemed the development of an independent sociology of science their proper aim. Chief advantages of the new approach are (1) that methods recently developed in sociology such as those of ethno-methodology can be used in the sociology of science without worrying much about problems of epistemology[226] and (2) that the scope of the sociology of science is extended to problems of how theories are chosen, that is, to the content of science.

This extension of the sociology of science to include the task of explaining the content of science is, however, paradoxical. The sociology of science may be extended because the establishment of knowledge may be explained by ordinary sociological means. Science, these theorists say, is like any social activity and may be studied just as they are. But this means that the knowledge is no longer of much interest and may be ignored.[227]

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whether in the sociology of science such as Latour or in philosophy such as Elkana return, whether they want to or not, to Hegel.

[225] Barry Barnes, *Interests and the Growth of Knowledge*, London, Routledge & Kegan Paul,1977; *T. S. Kuhn and Social Science*, London, The Macmillan Press, Ltd,1982.

[226] See for example, Karin D. Knorr-Cetina and Michael Mulkay, *Science Observed*, London: Sage Publications, 1983.

[227] See reviews of Karin D. Knorr-Cetina, *The Manufacture of Knowledge*, Oxford, Pergamon, 1981, J. M. Ziman, "The Adventures of Candide in the Sociology of Science", *Minerva*, vol. 19, 1981, pp. 509ff. and Joseph Agassi, "(Non-) Participant-Observers of Science: trading in absurdities", in *The Gentle Art of Philosophical Polemics*, op. cit., pp. 1 97ff.

Contradictions may also be expected as scientists move from activity to activity, say, from doing science to defending it before the government, and for some that is no problem either. Individual sciences may even have more similarities with other, specific non-scientific enterprises than with other sciences.

The new movement in the sociology of science has two powerful factors in its favor. First, it integrates the sociology of science firmly within sociology. It precludes, thereby, the old problems of methodology and of the integration of methodology with sociology of science which Merton and Kuhn failed to solve. Second, it appears to be science friendly and knowledgeable. Leaders of the movement such as Donald Edge or Barry Barnes were trained in the natural sciences. Even Jonas Salk provides support.[228]

The alleged failure to resolve the problem of the relationship between methodology and sociology of science had direct impact on this development as Barry Barnes' critique of Barber's discussion of resistance among scientists to new scientific ideas shows.[229] In this discussion Barnes argues quite perceptively that Barber's discussion depends on an uncritical acceptance of ideas endorsed by the scientific community: Barber presumes that ideas in science are endorsed or not on the basis of (universal) scientific method. Resistance to ideas which later come to be accepted, then, indicates improper behavior on the part of scientists. Barnes, in contrast, points to the research of Polanyi and of Kuhn and argues that resistance to scientific ideas- even those which eventually come to be established - need not be due to impropriety but rather to the use of different background assumptions, premises or paradigms which scientists may use to appraise research.

Barnes correctly endorses Polanyi's claim that the appraisal of scientific theories is not nearly so straightforward as many, though not all, traditional methodologies had presumed. Whewell is, for example, a great exception. But if we go one step further and fail to discuss whether one set of assumptions or premises or one paradigm is better than another, we run into trouble. Barnes ignores Polanyi's discussion of the justification of these premises and moves on to Kuhn. We have in fact changing

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228 Jonas Salk, "Introduction" to Bruno Latour and Steve Woolgar, *Laboratory Life*, Beverly Hills: Sage Publications, 1979, pp. ll ff.
229 S. B. Barnes, "On the Reception of Scientific Beliefs" in *Sociology of Science*, ed. by Barry Barnes, Middlesex: Penguin Books, 1972, pp.269ff.

standards. The possibility of improper resistance to ideas seems to be explained away in favor of mere observation of differing premises.

If we take this view seriously, however, then Barber's view must be just as good as Barnes': It simply proceeds from different premises and tries to solve the problems in the sociology of science within these premises. Barnes can maintain that he chooses to use other premises, but there seems hardly any reason why we should deem them better or his discussion of Barber any criticism of at all: It is at best a contrast of premises used in the sociology of science. If we have no standards for such, his own view can scarcely make any claims to be progress; if we do, it overlooks the most important part of science: Its capacity to critically appraise alternatives.

But the development of such paradoxes as these for relativistic positions such as that defended by Barnes is really child's play. They have been pointed out often enough.[230] But this has no effect. We are faced with a positivistic Hegelianism which is unimpressed by traditional intellectual standards. It is positivistic because it purports to merely describe and it is Hegelian because it accepts as its subject matter the (contradictory) development (of the Zeitgeist) of science and because it deems facts to be constructed by this Zeitgeist. It is also child's play to defend such a view since contradictions are no problem for it. This is boring. It is also a scandal: The intellectual reduction all relationships to power relationships,

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[230] See e.g., J. W. Grove's review of Barnes' *T. S. Kuhn and Social Science*, "The Social Denigration of the Rationality of Science", *Minerva*, vol. 20, 1981, pp. 550ff. David Bloor, in *Knowledge and Social Imagery*, London, Routledge & Kegan Paul, 1976, pp. 38-9, suggests that the social explanation of the sociology of science poses no problem: It meets the conventions of science just as other sciences do. He then goes on to argue that these are good standards. He can only say, however, that they are established. He thereby slides from deeming science a mere sociological phenomenon to deeming it a good one. In his Hegelian mood anything established is good. In his positivist mood he appeals to standards as normative. The same phenomena is found in Paul Tibbets, "In defense of relativism and the radical programme: a critique of Jarvie", *British Journal of Sociology*, vol. 36, no. 3, 1985, pp. 471ff. who responds to I. C. Jarvie, "Rationality and Relativism", *British Journal of Sociology*, vol. 34, no. 1, 1983, pp. 44ff. Jarvie argues that relativism prevents us from understanding societies, from learning from criticism and from explaining learning. Tibbets says we can do all these things, if we dispense with truth and use such concepts as "credibility", "degree of warrant" or "legitimacy". He gives examples. But this response slides between explaining such techniques away when one talks about the nature of science and sociology, and taking these same techniques at face value when one looks at things from the point of view of the scientist.

the contempt for ethical standards in, say, the presentation of science to the public, which is evident in the acceptance of such behavior as part of science and the rejection of the most fundamental intellectual standard - if two statements contradict each other at least one is false - is the preparation for barbarism.

Edward Shils: The elite working ad hoc on integration

The conflict between followers of Merton and those more or less associated with the Edinburgh point of view has centered on the interpretation of Kuhn, or better, the re-working of Polanyi's epistemology-cum-sociology into sociology alone. Merton and his followers have interpreted Kuhn's results as new challenges for detailed sociological investigations into problems of how sciences emerge or change. The Edinburgh school has interpreted Kuhn's results as a relativist challenge to the Mertonian research program: they show that the presumption of universal standards for science is false and they open up new possibilities for the sociology of knowledge. It can, namely, use normal sociological theories to explain how science functions as well as the content of scientific ideas.

There are other approaches which do not quite fit into this standard schema. Edward Shils, for example, and many of the contributors to his journal have also sought to develop resolutions between changes in science, on the one hand, and the objectivity of science, on the other hand. They have done so not merely as theory but also through the development of practical guides for responsible action by the leaders of the profession.[231] Their program incorporates normative problems into the theory of scientific society; these problems are posed as questions of policy for the elite. The collective or center is identified, explained and its continuance as center defended. In this way normative views, as I explain below, may be intimately integrated into sociological theory. It may appear that the descriptive-prescriptive problem is overcome.

Leaving open the extent to which Polanyi influenced Shils or the other way around, we may note that Shils endorses Polanyi's theory of the role of the elite in science and in (intellectual) society. Shils, however, develops his view of the crucial role of the elite as carriers of standards and thus as integrators of society in the context of his broader sociological

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[231] Edward Shils, "Reports and Documents, The Obligations of University Teachers", *Minerva*, vol. 20, 1982. See esp. "The Academic Ethic", pp. 107ff. and "The Obligation of Knowledge", pp. 145ff.

theory of centers and periphery.[232] Polanyi has no such theory. For Shils, but not for Polanyi, science is a special case.[233] In our society this case is, of course, of crucial importance. It is not trivial to mention that Shils also endorses the view that science should disinterestedly pursue the truth. It must be objective and fair. He shares with Polanyi a sensibility for injustice or unfairness which may occur in science or intellectual life in general and seeks to minimize it. This should be done by furthering the implementation of the proper norms by the elite. And he does not wish the center to be so strong as to dispel diversity.[234] He shares two themes with Polanyi, which are central for his program. These two themes are the integration of society and the overcoming of social abuses or problems. Such abuses or problems may be due to too little integration, too much

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[232] Edward Shils, *Center and Periphery*, op. cit.

[233] Joseph Ben-David deems the approach found in his *The Scientist's Role in Society*, Englewood Cliffs, Prentice Hall, Inc., 1971 - to be followed by thinkers such as Bernard Barber, Robert Merton, Norman W. Storer or Don K. Price. It is he, says, "an institutional sociology of scientific activity." (p. 14). He seems, however, to follow the lead of Shils quite closely, in analyzing the change of scientific centers and thus to apply Shils' view to the special case of science. See also Edward Shils, "Tradition, Ecology and Institution in the History of Sociology", *Daedalus*, vol. 99, 1970, pp. 761ff. Shils applies his own view to the study of the development of sociology. Ben-David, apparently and I presume with the blessing of Shils, does not differentiate between the programs of Merton and Shils. The essay by Shils indicates, perhaps, one difference. Shils contrasts the attempt to articulate a vision, which he finds in Parsons with Merton's call for middle-range theories. Shils is in this respect still closer to Parsons. This reluctance to differentiate is, of course, understandable, if one wishes to keep philosophy and/or methodology distinct from science. Ben-David, in the pages preceding the characterization of his own approach, criticizes such views. In fact, there are studies which may be interpreted by this or that framework or which are not so easily classified. The widely cited study of Paul Forman concerning the social-intellectual influences on physical theory in the Weimar Republic "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment", *Historical Studies in the Physical Sciences*, Third Annual Volume, 1971, pp. 1ff., for example, discusses such influences as are denied by Ben-David to be important, but does not necessarily contradict his view, since they are not totally precluded. Forman does not place his view either; it seems to be a study of influences on science as conceived of by Gerald Holton in his view of themes. Whether there might be another research program for the sociology of science is not clear to me: It is not, I think, well enough developed to say.

[234] Edward Shils, "Introduction", to *Center and Periphery*, op. cit., pp. xxxviii-xxxix.

integration or the wrong kind of integration. In any case, their solution should be found by the study of centers, of the elite.

Shils' central problems concerning science, then, are nearly identical to Polanyi's. In the first place he wishes to protect the autonomy of science. It does not matter much here whether one is concerned more about Marxism in England, as Polanyi was, or McCarthyism in America, as Shils was at first, or with threats from other sources such as student movements, etc., as Shils has later been. Answering such challenges has been a central theme in Shils' work and in his journal. In defending the autonomy of science Shils has not forgotten the responsibilities of science and sought to develop a view of science - really of the elite of science - which is worthy of autonomy. The defense of the autonomy of science, then, is related to a problem internal to science: how can the elite best fulfill their responsibility? Or, how can they insure that science functions, i.e. integrates, as appropriately as it can?

Shils shares Polanyi's sense for injustice and the need for autonomous truth seeking individuals as well. He shares Polanyi's sensibility for the difficulties of scientists and intellectuals which is lacking in Kuhn's popularization of Polanyi's work. But, as Agassi has pointed out, Polanyi's sensibility for difficulties, abuses and the need for control was personal. He had no place for control in his theory.[235] From Polanyi's viewpoint the elite has the last word. Shils seeks to avoid such criticism- I do not know whether he thinks Agassi's criticism on the mark or not - by finding a place for sensibility in the theory of the proper norms for the elite. The theory of centers may bridge the gap between elitism and morality, between an authoritative elite and a decent society. In order for a society to be a society, i.e. to be integrated to this or that degree, it must have a center, i.e. an elite, and this elite must behave properly or the society will change, i.e., the center will change, i.e. the elite will change. The study of the norms of behavior for the elite is then, both a descriptive study of how any science is integrated and a recommendation to the elite on how to preserve that integration. And this integration requires values shared by Shils and Polanyi.

Shils' Polanyiite program, then, is sociological and descriptive on the one hand and normative on the other. Shils applies his general theory of society by developing and strengthening the values and norms of the (scientific) elite. His fundamental idea is that science has centers which

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[235] Joseph Agassi, *Science and Society*, op. cit., pp. xix-xx.

integrate (more or less as the case may be) and that these change. The theory of centers provides a new attempt to develop a highly sophisticated functional view of the hidden structure of society.[236]

Shils has thus made the ethics of science and the appraisal of how well we are doing the subject of an ongoing inquiry. He thinks, of course, that we do fairly well, but is aware of responsibility and threats - as was Polanyi. He seeks to render the implicit judgment of the elite open and subject to ongoing critical discussion. In this respect he moves in the direction of the Popperians. Whether he thereby leaves the Polanyian view for some other is not clear to me. The discussion about how science should be run takes place, of course, among the elite. Perhaps outsiders are not, or should not be excluded, but their participation only occurs ad hoc and due to decisions by the elite.

Here we face our crucial problem concerning methodology in the sociology of science once more. How can one describe the (proper) actions of the elite and develop norms for them, if one does not (explicitly) use methodology? Shils hopes to both describe and to develop the standards by which the elite may promote the integrity and the values of science. These standards are in part ethical in nature; they must also encourage the growth of knowledge. Now Shils does not discuss just which methodology he adopts. His debts to Polanyi and his sympathy for some of Popper's ideas are clear. But how are these competing views of methodology and scientific society to be integrated in a social theory of the intellectual center?

The difficulty may be illustrated by Shils' introduction of methodological standards into his view of the ethics of science. He - along with other members of his study-group- proposes that the ascertaining of truth is always tentative. But truth is sought. This is no excuse for relativism. The best theories of today are replaced by better ones - even if old ones are not proved wrong. Statements which scientists - along with other members of the university community - teach and defend should be as true as possible, based on the most methodically gathered and analysed evidence.[237]

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[236] Edward Shils, "Introduction", to *Center and Periphery*, op. cit., briefly describes here the development of his view from, for example, the view which he developed during his collaboration with Talcott Parsons.

[237] See Edward Shils, "Obligations of University Teachers", op. cit. pp. 107- 108; Shils' collaborator Ben-David endorses Popper. See "Emergence of National Traditions", op. cit., p. 210.

This proposal emphasizes those points made by Popper and not those by Polanyi. It emphasizes that science seeks to get closer to the truth and that its method is critical rather than that knowledge is personal and judgment tacit. But it does not follow through on this view. It by-passes the problem of which conjectures should be examined to find out if they might stand up to tests and how the results of examination should be evaluated. On these points Popper and Polanyi have differing views. For Popper, such evaluations should be shifted to the (scientific) public. For Polanyi, the elite must ultimately decide on the basis of their tacit knowledge: They must appraise situations where the evidence is not sufficient alone to provide clear cut answers. And this difference leads to different views of scientific society - as in the views of Polanyi and Agassi.

Ironically, this difficulty of specifying how new ideas should be appraised in a way which is consistent with Popperian standards, i.e., with his critical realism, and Polanyi's view of scientific society, is a consequence of Shils's partial translation of methodology into social theory. Popper's critical realism is based on a social theory concerning the purpose of the institutions of science. Shils uses this attitude in his ethical view concerning standards for the behavior of scientists - which is a part of his social theory. For Popper the problem of the selection of theories is solved socially and after theories has been presented. But this solution is not available for Shils's Polanyiite program since it by-passes the elite. Such problems are inevitable if one fails to specify methodologies and their differing social consequences.

I see no reason why Shils could not introduce his methodological background explicitly. His program sees sociology in a broader, humanistic context. Tradition is important and methodology is a part of this tradition. But there is also tension. Shils wishes to seek truth, but what counts as true depends on what is institutionalized. If we depart from tradition too much, our ideas will no longer have an institutionalized basis. They might be suggestive to the elite but will not count beyond that. Furthermore, if methodological background were to be explicitly introduced, problems would arise. If Popper's methodology, or one close to it, is introduced, conflicts with his elitist view of scientific society will arise. From Popper's view an open and critical society is necessary for the survival of science. According to him science does not depend on the elite but on institutions and rules which allow for openness, criticism and the discovery of mistakes. If Polanyi's methodology were to be introduced, conflicts with Shils' theory of the objectivity of science will arise. For Polanyi tacit knowledge, and an elite which incorporates such knowledge, is the foundation of science. This foundation, Polanyi argues, is and must be subjectivist. The exclusion of methodology prevents one from examining such conflicts much less from solving or resolving them. Perhaps we can do no better than to save some degree of openness and fairness in science by appeal to what I regard as an enlightened paternalism. Even if one prefers a more democratic stance-as I do-one has to concede, as a matter of fact, that scientific institutions are, in very many respects, elitist in the sense of giving uncontrolled power to the elite.[238] As Polanyi has explained, the power of well-placed and influential individuals to affect the course of a debate and of careers through their decisions - in contrast to their arguments - is enormous. That such individuals should behave properly, should exercise good judgment, should give outsiders a chance, etc. is absolutely necessary if science is to live up to its own standards. Science has, of course, traditions which promote responsible behavior. But, as Shils agrees, these traditions may also be threatened. Is responsible elitism good enough? Is it possible today? Do we need more democratic controls? Are these possible? Do they raise or lower standards? These are problems which interest both Shils and Agassi. Their discussion is inhibited when we fail to be explicit about methodologies and their consequences as Shils, but not Agassi, does.

Shils's merging of Polanyiite and Popperian elements is quite sophisticated. It preserves the self-image of the establishment institutions as the authoritative and responsible carriers of culture as well as the moral demand that all individuals be treated properly. But its merging of various views and its failure to spell out methodological proposals makes it ad hoc and its appraisal quite difficult. Some problems are not spelled out. Just as Polanyi does, Shils keeps a place for elitism, for responsibility, for objectivity and even for some degree of openness. But, just as in Polanyi, this combination is more personal than theoretical and, without explicit and critical use of methodology, it must remain so.

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[238] The criticism of elitism is often taken to be mere sour-grapes and opposition to extraordinary achievement of any kind. Such cheap criticism of scientific institutions no doubt exists; but this response does not meet all criticism of elitism and certainly not Agassi's. For this kind of response to one kind of critique of elitism see Edward Shils, "Faith, Utility, and Legitimacy of Science" in Gerald Holton and William Blanpied, *Science and Its Public: The Changing Relationship*, Dordrecht, 1976, p. 3, "Elitism", *Minerva*, vol. 12, 1974, pp. lff. or P. B. Medawar, "The 'Ultra-Elite' of Science", review of Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States*, *Minerva*, vol. 15, 1977, pp. 105ff.

Agassi: a democratic alternative or taking Polanyi seriously

Various thinkers have tried to integrate Popperian methodology with (Polanyiite) sociology of science. Some hope that in this way the objectivity and/or fairness of science can be given a firm basis while the elitist view of Polanyi and a sophisticated functionalist sociological framework can be used in the sociology of science. The uniqueness of science may be accounted for and the sociology of science integrated into sociology proper. Other thinkers have attacked Popper's methodology due to its conflict with relativistic sociologies of science. They have sought to explain away the uniqueness of science in order to treat it as just another institution. Joseph Agassi is the only thinker who integrates a Popperian, critical approach to scientific method with a Polanyiite sociological approach to explaining and appraising scientific institutions. Instead of abandoning Popper's theory of an open society, he has not only abandoned but also criticized the elitist view of Polanyi. Instead of seeking to preserve some sort of functionalist sociological framework, he has applied a Popperian individualist one.[239] Popper's image of scientific society as open and critical is thereby developed and tested, and used to develop new proposals for the democratization of scientific institutions.

Agassi's use of a Popperian methodology-not Popper's, but one which has grown out of criticism of Popper - enables him to attack head-on those problems which have caused difficulties for the programs of the Mertonians, the British relativists or Shils and his followers. He can only do this because he develops the methodology he uses quite explicitly and opposes it to Polanyi's. He can thus effectively argue that it accounts for both the growth of scientific knowledge and for scientific institutions better than Polanyi's does. He takes Polanyi seriously by offering an alternative to Polanyi's theory of the specific connection between methodology and scientific institutions. He meets this challenge at all levels rather than by seeking to explain the methodological dimensions away.

The work of Polanyi simultaneously challenged traditional methodological and sociological views of science. It did so because these views claimed too much for science. They claimed, namely, that the appraisal of scientific theories depended on the evidence alone. Polanyi knew from his own practice and experience that this view of science was idealized. He

[239] Joseph Agassi, "Methodological and institutional Individualism", *British Journal of Sociology*, vol. 26, 1975, pp. 144-55.

also knew that if one claimed too much for science, science could lose credibility. There were aspects of the Marxist view which justifies social control of science which are correct: Scientific appraisal depends on social and not merely intellectual factors. But this fact should not be used as an excuse to deprive science of its autonomy. A new and more realistic image of science was needed to block this move.

Polanyi's analysis of the possibilities of responding to the demand for social control of science was incomplete. He largely ignored the possibilities which a Popperian view might offer. This approach does not make too high claims for science which lack credibility - at least not in the same way as other views had. Popper offers the opportunity to lower claims about science to avoid lack of credibility but to do so in such a way as to maintain both the idea that scientific society is open when it functions properly - a view which is in some vague way a part of our standard image of science - as well as the view that the aim of science is to describe the world as it is. In his desire to preserve the institutions of science as they are, Polanyi is more conservative than Popper; but in his image of science and his desire to preserve the institutions of science at their best, Popper is more conservative than Polanyi. In order for any Popperian response to Polanyi to be adequate, it must explain how science produces the growth of knowledge. This explanation must take account of the weaknesses in both method and scientific society which Polanyi correctly pointed out and tried to take into account. The Popperian response must explain how realism is possible, how scientific institutions may be open and how the two fit together. The accomplishment of these tasks offers a real alternative to Polanyi. It is one which meets the problems Polanyi sought to solve rather than one which explains them away.

The dominant alternatives fail to adequately come to terms with (1) the apparent conflict between the growth or change of scientific standards with the objectivity or universality of science, (2) the difficulties of either using prescriptive aspects in sociology or of excluding them, and (3) the problem of reconciling the authoritativeness of science with its openness. It is only by the introduction of an alternative methodology and by dealing with these problems at various levels that an alternative sociology of science capable of coming to terms with these difficulties can be developed.

Agassi's program for the sociology of science poses three kinds of problems. In the first place we find philosophical problems of scientific method and of rationality. Solutions to problems of this sort are needed in Agassi's view to break out of the conundrums in the sociology of science, which are based on mistaken views of the rationality of science and the appraisal of scientific research. His studies in the sociology of science thus include studies of research projects, of the logic of inquiry, of rationality, of epistemology and science, of external vs. internal studies of science, of metaphysics and science, of the choice of problems, and of sociologism in the philosophy of science. These studies include answers to central problems of the methodology of the sociology of science. In answer to the question, how can we reconcile the change of scientific standards with the universality of science? he argues that we can improve rationality.[240] In answer to the problem of how prescriptive and descriptive points of view may be integrated in the sociology of science, he argues that science may be conducted in various ways, and we need to appraise these morally, methodologically, sociologically and the three together. In answer to the apparent conflict between the authoritativeness of science and its need to be open, he claims that high standards are met better if we dispense with elitism and become more open.

In the second place Agassi uses, as Polanyi also did, philosophical views to pose sociological problems concerning the historical interpretation of science. These problems are more empirical. They involve the use of new theoretical results to interpret historical episodes in science such as the reaction to Faraday, Galileo's trial, the nature of the Royal Society, revolutions in science, golden ages, or cultural lag.[241] Such problems may be either quite general or may primarily concern interpretations of particular events. The more general problems concern the interface between methodological explanations of the growth of knowledge and sociological explanations of how scientific societies promote this growth. The study of specific episodes allows for critical examination of how scientific societies promote (or fail to promote) the growth of knowledge. The philosophical and/or sociological generalizations may be used to develop new and better explanations of specific episodes.

In the third place Agassi develops proposals for how we might reform scientific society so as to better pursue our goals. Problems of policy or social planning are seen from the point of view of the studies of rationality and methods. Such problems include, for example, the study of procedures

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[240] Joseph Agassi, *Science in Flux*, Dordrecht: D. Reidel, 1975; 'Rationality and the Tu Quoque Argument', in *Science and Society*, op. cit., pp. 465ff.
[241] All these studies are found in Joseph Agassi, *Science and Society*, op. cit.

of publication and refereeing.[242] Above all Agassi is concerned with the problem of how scientific society may be rendered more democratic than it now is. He concedes that scientific society is in fact elitist and authoritarian to a high degree but sees its success in its openness. How it may be rendered more open is a policy question of great importance. And the study of this question requires sociology. We need to see how society now operates and how we might change it for the better.

Agassi's program may not be sharply distinguished from all others due to its assumptions that science and scientific institutions are not the same. Those who defend the views of either Merton or of Shils tend to waffle on this point. They deem methodology distinct. But since science and methodology are studied insofar as they are institutionalized, they tend to be conflated. (Shils, for example, allows for impact of non-institutionalized ideas but only ad hoc.) Rather, Agassi's program may be distinguished from all others with the exception of Polanyi due to its assumptions that one has to (1) explain what science is and what we want it to be, (2) explain how institutions deemed scientific operate and (3) see the two together. Agassi's view differs from Polanyi's quite sharply due to the view of science which he adopts and the differing sociological theses, which emerge as a result.

On the views of Polanyi and Agassi, then, any sociology of science must first specify what science is. Only then can one study scientific institutions intelligently. Only then will one see these institutions as those of science and be able to understand them vis-a-vis the aims that science should serve. Merton's program takes these aims as established and unproblematic. The Edinburgh school takes them as dependent on historical context. But they also deem them unproblematic: Whatever aims scientific institutions pursue and however they do that is scientific. Which institutions are scientific? Apparently any institution is which is recognized in the society as carrying the name properly. On Shils program the aims of science are problematic to a degree but uncertainties are resolved ad hoc.

Agassi does not resolve the conflict between, on the one hand, the methodology, according to which there are objective scientific standards which are properly used in the pursuit of truth, and, on the other hand, the sociology of science which describes the development of science in terms of the use of the personal, tacit knowledge of the elite. He accepts both but regrets the latter and finds science in conflict with itself. His problems are

[242] Joseph Agassi, *Science and Society*, op. cit.

how to explain this conflict, how it arose, how it is defended and developed and how it may be overcome. In order to do that Agassi follows Polanyi's sociological approach. Polanyi intimately connected his sociology of science with his philosophical theory of science and Agassi does this as well. He does it, however, with a different theory of science. Agassi is realist and critical where Polanyi is subjectivist (or conventionalist) and dogmatic.

Today's agenda: Science and its institutions

The sociology of science today has a rich variety of possibilities to explore. These possibilities concern the nature of science as well as the nature of scientific institutions. Rather than being a threat to the sociology of science, the open discussion of these possibilities can lead to new and interesting sociological studies and debates. In order to explore these possibilities the theory of science and the theory of scientific institutions have to be separated and integrated. They have to be separated in order to open the possibility for the use of various philosophical theories of science as background knowledge for sociological studies. They have to be integrated to make this background knowledge useful to sociology and to test it empirically.

The current state of affairs indicates that this has already happened to a large degree. The five research programs discussed above (Merton, Polanyi, Edinburgh, Shils and Agassi) each develop and apply in sociology different philosophical views of science. Yet the competition between them is conducted at a much lower level than need be due to the fact that the defenders of three of these programs do not concede that this is what they are doing. The remaining two - Polanyi's and Agassi's—are now ignored. The Mertonians do seek not to say what science is. And if they were to, their statements would be so abstract and general so as not conflict with any potentially successful view of science. Shils follows Polanyi more closely. But he seeks to integrate views from various sources which he finds appealing rather than to note conflicts. Philosophical propositions are introduced ad hoc and out of context. The new relativistic sociologists of knowledge base their revolutions to a high degree on philosophy, but they pretend that this philosophy is mere empiricism and thus unquestionable. We have the super-sophistication of Merton and Shils, on the one hand, fighting the super, even vulgar naiveté of the relativists, on the other hand.

The upshot of the failure to demarcate and develop competing views of science within the context of the sociology of science is not merely the new concentration on micro-sociology - as one commentator has called it[243] - but very poor standards for theory. Since there is no sharp demarcation between philosophical theories of the nature of science on the one hand and sociological theories about scientific institutions on the other hand, the lack of philosophical standards in the sociology of science leads to poor theoretical sociology of science. The opportunities of exploring various theoretical avenues are lost due to the fear that one cannot admit the existence of such avenues and retain the status of a science. Sometimes the possibilities are noted as Zuckermann, for example, has done. But they are then rejected.

If the sociology of science is to realize its potential, sociologists of science will have to put their cards on the table and say what they think science is and what it ought to be as well as how they explain the functioning or context or whatever of scientific institutions. This means that the views of philosophers which have been extended, or which may be extended, into sociology will have to be put on the agenda as competitors to established programs. And these programs will have to be evaluated by philosophical as well by sociological standards. They will also have to be concerned with the kind of society we want, whether democratic or authoritarian, whether relativist and power oriented or democratic and truth oriented.

The competing views of Agassi and Polanyi show, that the sociology of science can be usefully conducted within various methodological frameworks, and that the results can be tested against each other both philosophically and sociologically just as one pleases. One may seek to study the sociology of science under the assumption that science needs an authoritarian elite or under the assumption that it needs more democratic controls and openness. Only in this way can problems such as those of the universality of science and scientific change, the problem of description vs. prescription in the study of science and the problem of authority vs. openness in science be profitably studied. The debate between competing images of science and society such as those offered by Polanyi and Agassi should be put on the agenda of the sociology of science now.

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[243] J. W. Grove, "The Constructivist Sociology of Science with Science Omitted", *Minerva*, vol. 21, 1983, p. 465. "Micro-sociology" refers here to the new "constructivist" sociology of science.

2i. 'The Legends of One Methodology of Science Used throughout Its History and Its Independence from the Institutions in which Science Has Been Conducted', in Nimrod, Bar-Am and Gattei, Stephano, Eds. *Encouraging Openness* (Berlin: Springer Verlag, 2017b) Chap. 18.

The Legend of One Methodology of Science Used throughout Its History: Its History, Its Defects and the Limitations it Places on the History of Science.

Abstract: Ever since the beginning of science in the 16th and 17th centuries the question of the independence of natural philosophy (science) has been intensively debated. One of the finest statements of the separation was made by Galileo in his letter to Princess Chatarina. Natural philosophers (scientists) struggled to establishment their independence from religion and philosophy; in the 17th century, at the very latest with Newton's success, they succeeded. But this victory has been a Pyrrhic one: Science was often granted its independence but its identity was not clearly established. Methodological debates arose again and again and even then when it looked as if everything had been settled. This history of methodology shows that science cannot be given some unified, established and never changing identity. It shows that methodology and science have to steadily interact and seek improvements. It also shows that to do this is valuable and a crucial and important basis of the growth of knowledge.

A description of the often neglected portrayal of the problems facing the history of science today is followed by a brief review of varying methodological approaches found in the history of the history of science, above all from William Whewell to the present. This shows the wide variations which have been defended and employed. A critical appraisal of Joseph Agassi's thesis that there have been only three methodological variations in the history of science is given. How this thesis conflicts with what historians of science have done will be explained. This breakdown of Agassi's approach, which is the best that has been carried out until now, is, however, no bleak result: Science progresses quite well through its steady and productive interaction with methodological theories. Such theories are tested in ongoing empirical research, improved, and the new innovations once again tested. The social rules of science are reformed. An appendix containing a critical appraisal H. Floris Cohen's comprehensive overview of the history of the history of science follows the text; it demonstrates the emptiness and the incoherencies which follow when one attempts, as Cohen did, to sharply separate historical and methodological studies.

The Legends of One Methodology of Science Used Throughout Its History and Its Independence from the Institutions in which Science Has Been Conducted

1 Introduction: Central Problems of the Historiography of Science Today

Ever since the beginning of science in the sixteenth and seventeenth centuries the question of the independence of natural philosophy (science) has been intensively debated. One of the finest statements of the separation was made by Galileo in his letter to Grand Duchess Christina. Natural philosophers (scientists) struggled to establish their independence from religion and philosophy; in the seventeenth century, at the very latest with Newton's success, they succeeded. But this victory was a Pyrrhic one: Science was often granted its independence, but its identity was not clearly established. Methodological debates arose again and again and even then when it looked as if everything had been settled. This history of methodology shows that science cannot be given some unified, established and never changing methodological identity. lt shows that methodology and science have to steadily interact and seek improvements. lt also shows that to do this is not only valuable but also a crucial and important basis of the growth of knowledge.

William Whewell was the first thinker to notice that the approach of conflating the teaching of science as a discipline, on the one hand, and explaining how science developed, on the other hand, was mistaken. (Whewell 1967a, b) He noticed that scientific theories were not proven by induction, as Hume had previously and so powerfully pointed out, but, he also understood that scientific theories were not created by inductive inferences, as virtually everyone in the light of Newton's fantastic success, including Hume, had long maintained. Virtually all portrayals of science presumed that scientists used induction to discover and prove their results. (Whewell say said that Descartes retained significance in France until around 1750.) But, if this inductivist method of discovering scientific theories was not the method which was actually used to make such discoveries, the development of science had to be described quite differently. There had to be a history of science, which was not merely the cataloguing of important truths, noticing when they were discovered and showing how they were proven by generalizing from facts, that is, by induction. It was not enough to simply describe the results of science; one also had to describe how this extraordinary advance of knowledge took place.

In order to do that, one had to have an alternative methodological theory. This alternative methodological theory could only be constructed if the dominant conflation of the portrayal of the content of science and the history of science was replaced with some other approach. This alternative methodology of science had to be a true methodology in order to describe how scientific discoveries were made in the history of science, or that was, at any rate, what the only person who tried to construct one in the middle of the nineteenth century—William Whewell—thought. A true methodology of science, he thought, would be that methodology which correctly described how scientific discoveries were made. This task of explaining scientific discovery could not be carried out by merely describing the contents of the successes of science. lt required an independent theory of how knowledge was obtained. Whewell set out to meet this new challenge. When his research was largely completed, he claimed that he had met the challenge of describing how scientific knowledge was obtained and, since no other explanation was possible, his own revolutionary methodology was the true methodology. It was, he said, the only methodology scientists had ever used.

The story of how this change in the history of science came about due to Whewell's innovations should be the story of how the framework for the philosophy of science from the middle of the nineteenth century until quite recently was formed. Independently of which specific modern methodological theory one defends. Whewell's approach shows the framework in which all these methodologies needed to compete up until today. They had to clearly portray the methodological theory thought to be true and then show that the methods described by this theory were in fact the methods used by all scientists to make their discoveries. But this fact about research in the philosophy of science remains unacknowledged by the majority of researchers in this field. It needs telling and in such a way, in which it becomes clear that this has been the highly influential but largely unacknowledged framework for philosophies of science for more than the last 150 years.

But this is not the end of the story. Today this framework needs to be changed once more. Karl Popper rediscovered that scientific methodology depends not merely on the correct application of logic, but also on methodological rules. (Whewell and Pierre Duhem had realized this before.) Popper saw that these rules are social; they do not merely describe the application of logic, but are rules which describe how the truth should be pursued. But Popper did not observe that they have changed in progressive ways and that they should further change as science develops. They are, in fact, better or worse and constantly in need of new appraisal as knowledge advances. This means that the correct history of science is not that history, which correctly describes how each advance was made with the application of the single correct methodology, as Whewell maintained. We cannot merely substitute Popper's methodology as a framework for describing the history of science for those such as Whewell's and/or Duhem's (Duhem 1962) which were used before, as Agassi suggests. (Agassi says he was disappointed that readers of his essay did not notice that his view differs from that of Popper. But he nowhere says whether the history of science would be conducted any differently if one used his methodology as a framework rather than Popper's. Because social rules differ and change, we need to show which rules were applied in which historical instances and how these contributed to the emergence of new discoveries, or how they hindered it. The study of the history of science cannot be separated from an ongoing critical appraisal of scientific method. And the appraisal of scientific method cannot be separated from a study of its history. This critical appraisal cannot be carried out by merely describing what happened. It requires philosophical appraisals of these developments as well. It needs to analyze how currently applied rules need to be supported and/or changed.

A crucial aspect of this essay is the demonstration that, though descriptions of parts of the history of science have been excellently described, all attempts to provide a comprehensive overview of the proper tasks and framework for the study of the history of science, from Whewell until today, have turned out to be flawed. A deep-seated reform of the description of the tasks of describing the history of science is needed. lt is desirable to examine how events in this history have been analyzed, criticized and portrayed up to now. This literature is relatively small, and a critical appraisal and overview of it shows the need not only for new studies of the history of the history of science, but also for new theories of how to study the history of science. I will turn, then, to the question of where the historical study of the historiography of science brings us today.

2 Agassi's Historiography of Science: What lt Contributed and How lt Needs to Be Revised

Historians of science have by and large made no systematic effort to appraise and understand the history of their discipline. The only two comprehensive studies are those of Agassi and H. Floris Cohen. (Cohen 1994) Perhaps this neglect has been due to the dominant point of view among historians of science that the history of science is itself a science, in that it does what all scientists have always done: It describes the facts. On this view no separate theories of how science has proceeded are needed in order to understand its growth. Now Whewell agreed that all scientists used the same method. But, he pointed out; this method cannot be the mere description of facts. Historians of science thus need a new theoretical framework in order to describe the growth of science, whereas philosophers of science need to test their theories of the methodology that all scientists have used to achieve knowledge, with historical studies of what really happened. By and large historians of science do not think that science makes advances by correcting mistakes and producing better theories. At best they, following Duhem, presume that scientists make small adjustments to prevailing theories. Since they are also scientists, they must all follow the same procedures. They cannot criticize their predecessors for serious mistakes, without demonstrating that they were not scientific, that their views should be discarded and replaced with serious ones. Philosophers of science, on the other hand, have tended to neglect the study of how the history of science should be conducted due to their view that their proper subject is the logic of science rather than its history and its changing methodological-cum-sociological rules.

The first comprehensive study of the historiography of science was, then, Joseph Agassi's *Towards an Historiography of Science*. (Agassi 2008, pp. 119-243; first published in 1963) This study was a direct consequence of Popper's challenge to the dominant methodological theories of science. In a quite Whewellian fashion Agassi said that they could not correctly describe how science grows, whereas Popper's theory could. But, contrary to Whewell, he made no extensive study of the history of science. There was, then, a Whewellian gap in his theory of the methodology and history of science. Agassi did not try to fill this gap by writing a new history of science, as Whewell had done; he only argued that it existed. He demonstrated with examples from the history of the history of science that developments in this history could be far better understood and described when a Popperian methodological framework was presumed, than could be done when the two established frameworks, that is, inductivism and conventionalism, were used.

Agassi's study makes a number of useful points about the historiography of science, that is, about how specific historical developments could be more accurately understood. But his study also contains significant philosophical-cum-historical mistakes.

3 Agassi's Progressive but Still Faulty Appraisal of the Historiography of Science and Its History

Agassi's essay begins with a characterization of the inductivist-Baconian methodological theory which has been the fundamental theoretical framework for the history of science since its very beginnings. Just when this view became dominant is not explained; the question is not posed. Bacon is given as the only authoritative model. And Whewell is mentioned, by and large, as an inductivist historian, who, to a high degree, followed the Baconian program. Whewell is praised for developing a remarkable history; but his history is not seriously examined; its originality is touched upon but hardly explained. Its excellent theoretical foundation is not explained. Agassi returns to him later as the prime contributor to the development of the emphasis on continuity in the history of science. But this interpretation of Whewell's contribution is not very clear. lt says much too little about Whewell's emphasis on the adventure of discovery, and it does far too little to distinguish Duhem's emphasis on continuity as a series of small steps as modifications of established doctrines from Whewell's theory of the growth of knowledge in historical contexts, which includes false conjectures and criticism, sometimes but not always followed by great gains, as important parts of the difficult quest for truth.

Agassi's criticism of the inductivist approach is launched with a series of examples, which are; for the most part, critical reviews of rather recent histories, that is, those written in the 20th century. This is, of course, all there is to discuss. Outside of his discussion of Duhemian conventionalism, there is no historical analysis of their development or growth. The aim of these histories of science is, by and large, quite simple. Agassi argues quite effectively that the inductivist framework, when used to guide the research of historians of science, regularly leads these historians to make black and white judgments between false speculations, on the one hand, and true theories proven by induction on the other. From the inductivist point of view there is no middle ground. This assumption has led to numerous distorted portrayals of what really happened when the development of science has proven to be far more complex and subtle than the inductivist methodological assumptions allow.

As a matter of fact the development of science regularly involves false conjectures and approximations and criticisms and improvements, as Whewell pointed out although Agassi does not give him the credit that he is due. But the inductivist historian of science has no option other than to use curious descriptions and evasions, which are designed to sweep these complexities under the rug. Their existence cannot be admitted, even when they are virtually always present. From the inductivist point of view, admissions of their existence call the scientific nature of science into question. But the avoidance of pointing out the pervasive and serious difficulties scientists have faced leads to quite unrealistic portrayals of what really happened.

After Agassi has completed his critical portrayal of inductivist histories of science, he tums to the second of the two major theoretical frameworks used to set the tone for histories of science up until the time he conducted his analysis. Agassi does both, but there are a few exceptions to this generalization, such as the research of Alexander Koyré who found that during the early history of science it was integrated with philosophy and theology (Koyré 1955, 1958, 1965, 1971, 1973) and of Arthur Koestler who argued that scientists were sleepwalkers: Their innovations could, Koestler argued, not be explained. (Koestler 1963) But Agassi pushes his threefold division of the historiographies of science too far; he erroneously claims, for example, that Koyré is a Popperian simply because he noted the importance that false theories have played in the growth of knowledge. What he fails to note is the sharp conflict between Koyre's view that science from Copernicus to Newton was above all a philosophical-cum-mathematical problem of finding good mathematical readings of reality, on the one hand, and Popper's theory that the key to scientific progress is the construction of highly falsifiable theories, on the other.

The second major alternative to inductivism is the conventionalist approach introduced above all by Duhem. Conventionalism is the theory that the only aim of science is to find true predictions. On this view science does not, and should not, seek true theories. Conventionalism is an old theory; it is already found in the Preface to Copernicus's magnum opus. But Duhem was the first to turn it into a quite general methodological and historical theory of science. Both the earlier version of conventionalism and the latter one shared the aim of removing any conflict between religion, which should provide the true theory of the world, and science, which should provide everyday tools for effectively dealing with it. Duhem's innovations to this traditional view were above all twofold. First, from a methodological point of view Duhem argues that the only acceptable scientific method is that of making small modifications to established theory. When one tries to do more, the unity of science breaks down; philosophical schools are formed and the divisions between them cannot be resolved with rational approaches. Secondly, from an historical point of view, Duhem argues that the history of science is continuous: All true scientific innovations are small modifications of that which went before. Descriptions of so-called revolutions in science are misguided; they simply ignore the continuity between that which went before and that which came afterwards. This continuity applies even to the beginning of science. The beginning of science was not a revolution away from the natural philosophy of the Middle Ages, but rather it was many small modifications of theories already at hand in the so-called dark ages. On Duhem's view the failure to recognize this development leads to a quite mis-taken view of the limitations of theoretical developments before what is now regularly called the scientific revolution. The preceding theories were allegedly far better than acknowledged by those seeking to find a revolution from philosophy to science at the end of the fifteenth and the beginning of the sixteenth century.

In introducing this approach Agassi once more mentions Whewell as an important contributor to it. This is correct, as he and I have elaborated elsewhere (Wettersten and Agassi 1991) and I in a rather different way. (Wettersten 2005) Whewell's contribution did not occur in the way which Agassi describes in this essay. Agassi claims that Whewell was an inductivist; Whewell allegedly believed that, when attempts to refute conjectures confirmed the conjectures, they were, with sufficient successes of this kind, proven to be true. Under this assumption Whewell's influence could not have been due to his call for a significant alternative to inductivism. Agassi needs an alternative. He claims that Whewell pointed to the continuity of scientific progress, even though he was an inductivist. His influence then allegedly occurred because other historians learned from him about this continuity. Duhem is the most prominent example. And Duhem taught many other succeeding historians of science, how to portray the history of science as a continuous process with small changes.

As a matter of fact Whewell rejected inductivism. And while nobody adopted his powerful alternative explanation of how scientific knowledge grew, some important thinkers saw that he was right to seek an alternative to inductivism. They sought to provide their own innovations. They gave no credit thereby to Whewell for what they had learned from him. There were various reasons for not doing that. Whewell was, after all, rejected by the scientific and the philosophical establishments. Siding with him gave no one any advantage. For two French innovators—Pierre Duhem and Claude Bernard—the fact that he was English and they were French may also have had some influence on their reluctance to mention him. At any rate they said nothing about this connection. And this fact blurred the history of the history of science considerably.

In limiting the historiographical alternatives to inductivism and conventionalism, Agassi is by and large correct. Nobody tried to explicitly build on Whewell's alternative, so the dominant active approaches were inductivist and conventionalist, although there were important exceptions, as I further explain below. And Duhem's conventionalism was the strongest alternative to Whewell that was constructed. Some alternatives, such as that of Charles Sanders Peirce, never got off the ground. (Wettersten 2005, pp. 100-106) Others, such as that of Ernst Mach, were never clear (Mach 1970, 1974; Cohen and Seeger 1970) Mach futilely tried to give inductivism a solid psychological foundation. And this is not possible.

Agassi proceeds with a critique of Duhemian conventionalist histories of science, which parallels his critique of inductivist histories of science. And this critique hits the mark quite well. He concedes that conventionalist histories of science have been better than inductivist histories of science, because they take account of the fact that each development builds on theories which preceded it. But, he nevertheless shows how conventionalist histories of science also distort the history of science. They do this, because their framework says that all advances are relatively small changes made in previously established theories. But many advances which really occurred in the history of science were not small and, indeed, rested on powerful criticisms of their predecessors. Conventionalists have to level out these changes; in doing so they often significantly distort the historical developments they want to describe. This approach blocks explanations of the deep-seated problems which progressive scientists have found in die established doctrines in their fields, the enormous difficulties they faced in their struggles to overcome them, and the significance of their broad advances.

After his discussion of conventionalist historiography Agassi turns to the task of explaining how the history of science should be written. His historiographical theory is an application of Popper's methodological theory to the history of science. It is a small attempt to do for Popper's methodological theory one of the things which Whewell had done in very large measure for his methodology, that is, to show how it can both reform how the history of science is written and thereby significantly improve portrayals of it. Agassi's result is more or less a standard application of Popper's methodological theory. Popper said his theory should and can describe the history of science. But Agassi was the first to develop this methodological theory as a historiography of science at any length.

On Agassi's Popperian view, scientific research begins with problems which are based on criticism of the best theories available. Criticisms of established theories can point to counter-examples or vagueness or incompleteness or inconsistencies. New proposals are made which are designed to overcome such difficulties; these are in turn subject to the same sort of criticisms. In portraying the history of science, it is of great importance to understand the views taken by scientists at any time. lt is important to make these presumptions as they were then conceived as clear as possible, even when, or especially when, they are now seen to be false or untenable or utterly confused. Only then can we effectively understand how science has developed, that is, just what problems scientists have faced and how they have, to this or that degree, overcome them.

Agassi gives some examples of how historians of science had, until he wrote his essay, regularly failed to describe the history of science in realistic ways. They distorted what happened either by romanticizing the successes or underestimating the importance of mistaken theories (inductivists) or by papering over serious changes (conventionalists); they thereby rendered the history of science quite difficult to understand. Two of his significant examples of such distortions are, one, how historians have dealt with the transition from phlogiston theories, on the one hand, to Lavoisier's alternative, on the other hand, arid, two, how they have portrayed Örsted's demonstration of the connection between electrical currents and magnetic forces.

He shows how Priestley's criticisms of Lavoisier's proposal led Lavoisier to real improvements and he shows how Orsted's theoretical background, even when false, was of considerable importance for his empirical research. In both cases he makes interesting contributions to the history of science; but in neither case does he develop an alternative historical portrayal, which satisfies the high standards he sets for historians of science. This was, of course, not his purpose. He only intended to illustrate how portrayals failed to take into account important aspects of these developments, and how this failure led to mistaken or vague portrayals, which could even be hard to understand.

One crucial aspect of Agassi's historiographical study is his assumption that the history of science can only be written within some specific philosophical frame-work, which explains with a methodological theory how science grows. He here follows Whewell's profound innovation in the historiography of science, which Agassi failed to acknowledge. Historians of science have followed it, as Agassi shows, in various ways with varying frameworks. No one followed Whewell's methodology. Agassi works in the tradition of Duhem, who agreed that the history of science should be written within the framework of a correct methodological theory, but who also (implicitly) claimed that the correct theory is not Whewell's but rather his own version of conventionalism. Agassi says, no. lt is his own version of Popper's methodology translated into a proper historiography.

4 The Need to Take Methodological Change in the History of Science into Account.

When we ask what standards or goals we should seek to realize when participating in scientific institutions, we must take into account the fact that science has changed in significant ways, often for the better but sometimes for the worse. Traditionally the pursuit of the correct methodology has been the pursuit of that which logic demands. Induction has been, of course, deemed a version of logic. The social rules of science were thought to be those rules which followed the logic of science.

Whewell broke with this tradition. His methodology explains how scientists pursue the truth with activities such as making conjectures, which are not merely applications of logic. De Morgan's criticism of Whewell's theory in which he said that Whewell offered the best psychological theory of scientific practice but no logic of science accurately mirrored what Whewell had done. (Wettersten 2005, pp. 58-63) And Duhem followed him in this regard by saying that a crucial rule of scientific methodology is that only small changes to established theories are allowed. (Wettersten 2005) Charles Sanders Peirce clearly recognized the superiority of Whewell's theory over that of his chief competitor, John Stuart Mill, but he also saw the gap between logic and scientific practice in Whewell's theory. He tried to find unity, but he had no success (Wettersten 2005, pp. 100-106).

After these thinkers Popper was the first to clearly break with the tradition of seeking a methodology of science which was merely the application of logic. He saw that his first methodological problem, that is, that of describing the logic of research alone, was not formulated adequately. The logic of science, that is, deducing the consequences of a theory and finding a contradiction between a consequence and a fact, allowed for ad hoc modifications. And these could not be a part of proper science defined as die proper application of logic alone. In order to keep ad modifications out of science one needed, in addition to the rules of logic, social rules, which he then added. The most significant additional rule he added is to always select that not yet refuted theory with the highest degree of falsifiability.

Agassi knows all this as well as anybody. In his study of Faraday he pointed out the unhappy influence of inductivist dogmatism (Agassi 1971) and in his study of Boyle in which he points out that Boyle instituted rules of crediting priority and public recognition which led to an exaggeration and perpetuation beyond reason. (Agassi 2008, pp. 388; 114) But he does not notice the extent to which the existence of social methodological rules requires changes in the philosophy of science. We cannot find a correct theory of science by only studying its logic. Proper science is not that science whose methodology conforms to logic. No science does merely that. Each science, and/or each period of science have their own rules which are better or worse. Whenever we pose the problem of how science should be conducted today—the problem on the table in Agassi's response to critics—we have to ask how the rules of today can be (somewhat) improved. They cannot be made perfect; such rules do not exist. Because they do not exist we cannot set our task as getting as close as possible to them when we do science. We can only say that this rule in this context brings us forward, try it out, and see if things improve or not. Things might even get worse. Even when the problem posed is not reformulated in this way, some of the deliberations may touch on it. But what will be claimed about good scientific behavior will not be specifically limited to specific social conditions, but universalized as applying to all of science, from Copernicus until today. This needs to be changed.

A second important innovation in Popper's philosophy of science is that science is social. Robinson Crusoe could not do science, Popper said, because he had no one with whom he could have critical discussions. (This is false and a bit racist: Friday was there after all. But the point is clear.) This means that to analyze how science should be conducted one must analyze how scientists interact with one another, and not merely how each scientist behaves correctly. Agassi's writings sometimes go in this direction, as in his discussion of Lavoisier and Priestley, but he does not go far enough. Actions will be socially analyzed but for the most part depend implicitly on being individual contributions, rather than being often small or partial contributions to positive social change.

5 Mach, the Social Victory of Inductivism, and Koyré's Innovation.

In the twentieth century Duheimian, conventionalist methods were introduced into the study of the history of science; they became widely practiced. But many historians retained the traditional inductivist approach. Some mixed their methods here and there. But nobody explicitly took up Whewell's approach; nobody tried to develop further a Whewellian history and methodology of science. A general discussion of alternatives was effectively blocked. And this was desired by a majority of historians, because the inductivists, led by the resistance to Whewell of John Stuart Mill and John Herschel, had determined in the middle of the nineteenth century that Whewell's non-inductivist theories could gain no open and acknowledged foothold in the traditions of scientific thought. (Wettersten 2005, pp. 35-57) From merely sociological and historical points of view, though not from an intellectual one, the inductivists won the day. Their conservative view dominated the discussions well into the twentieth century. But a closer look at the discussion reveals the considerable theoretical influence that Whewell had in the late 19th and earlier twentieth centuries. I have shown this influence in the philosophy of science elsewhere (Wettersten 2005).

The only historical study, which sought to give a serious methodological support to the social victory of inductivism, was Ernst Mach's approach to the historiography of science. His project failed, but he had influence. The most important twentieth century innovation in the history of science was Koyré's approach to sixteenth and seventeenth century natural philosophy. But before Duhem adopted Whewell's approach to the relationship between methodological and historical theories, Mach combined an historical study of the rise of mechanics with his own methodological theory of science: He sought to show how his anti-metaphysical methodological-cum-epistemological theory could show how physical theories could be properly interpreted. The key to doing this, he thought, is to preclude from them any non-empirical aspects.

Mach's historical research preceded that of Duhem by several years. And the two views have similarities due to the fact that they both reject metaphysics in science; each wants science to simply deal with facts. But they are, nevertheless, quite different. Mach seeks to portray the history of science as the construction of true statements about the relationships between facts—and only the facts. He presents the history of mechanics as a series of discoveries of how the relationships between facts could be described. When he describes Galileo's research he portrays the factual discoveries which Galileo made. His theoretical breakthroughs are hardly mentioned. When discussing Galileo he claims to be using Galileo's own descriptions of the facts, or, at any rate, of those of contemporaries who set them down in some simpler way. When he comes to Huygens, he says that Huygens' language and mathematics are all too complex and embedded in the time to be used in a more modern history. So, he translates them into a modern language, claiming that he thereby can more simply and clearly portray the relationships between facts as Huygens allegedly did.

Mach also makes attempts to describe quite generally the path of scientific development. In a somewhat surprising way this description comes quite close to that developed by Whewell. He says that there is an initial stage of searching for statements which describe the facts. This is followed by the simplification of these statements and, then, finally by the extension of the statements to other facts. This description comes close to Whewell's observation that there is first a quest with conjectures and corrections, followed by the formulation of fundamental ideas—the true scientific theories, whose applications are then extended and rendered more precise in a third stage. But the direct influence of Whewell on this aspect of Mach's theory is not clearly stated; it remains circumstantial.

Among historians of science who have made significant studies of important aspects of the history of science, Koyré stands out. And one of his most important contributions to the history of science is the significant comments he made about the historiography of science. He pointed out, for example, that the studies of philosophy, theology and science were unified before Newton (Koyré 1958, p. 4). In this attitude he followed that of E. A. Burtt, who first published his study of this connection in 1924. (Burtt 1954) But Burtt did not make any significant innovations in the historiography of science. He simply showed how science had changed our view of man and the world in such a way that God and the importance of man no longer played a central role. For religious reasons he wanted to find a way to recapture this role. Koyré observed that in the seventeenth century there was hardly any history of science. But Koyré's significance was not only because he pointed out the integration of philosophy, theology and science in die fifteenth, sixteenth, and seventeenth centuries.

He also explained why the significance of the scientific revolution did not rest above all on empirical research: It lay in their rethinking the philosophical framework which was used to understand the world. They moved from an Aristotelian framework, which emphasized the qualities of objects and the relationships these qualities had to each other, to an abstract mathematical framework. They sought then to find the mathematical laws which objects obeyed. This new framework led to revolutionary theoretical advances, which also had some very significant empirical consequences, and it led to important observations such as those of Galileo's of the surface of the moon and of Jupiter's planets. But it was the newfound application of mathematics to the world, which produced what later became characterized as the scientific revolution. In this respect he is the opposite of Mach's portrayal of Galileo's results as improved descriptions of the relationships between facts.

Agassi comments on Koyré and praises him highly. But then he says that Koyré was a Popperian although his research was by no means based on some Popperian framework. And, indeed, it contradicts it. For Koyré emphasizes the importance of a mathematical framework, while limiting the importance of empirical research. Popper emphasizes the importance of the empirical tests of all new scientific theories. Agassi's characterization of all histories of science as being inductivist, conventionalist, or Popperian breaks down in the case of Koyré's research, as it does in others as well. I.B. Cohen's study of Newton and Franklin is one such further example (Cohen 1953). Koyré mentions Popper once, but then he points out Popper's view of thought experiments. This is, perhaps, a bridge between his view and Popper's: Empirical research is rendered theoretical and non-empirical.

The above overview of the historiography of some of the leading historians of science shows how the program introduced by Whewell has dominated the field, ever since he explained it to a hostile audience. But Whewell says the aim of history of science is the correct description of one methodology which was adhered to by all scientists. Koyré discussed the methodology of science in its beginnings. But he did not universalize this view to all of science. Whewell did not simply use his methodology to guide descriptions of scientific research; he called for the development of one universal methodological framework for the conduct of the history of science. Whewell thought, of course, that only his alternative was acceptable. But those following him used his procedure to develop their own points of view, whether it was Mach, Duhem, Bernard or Peirce. Historians of science have by and large accepted the frameworks put forth by these leading philosophers-cum-historians in the late nineteenth and early twentieth centuries. This logic has been somewhat noticed by some; but its central factor in the historiography of science has not.

6 Methodological-Cum-Historical Pluralism Today.

The history of the history of science shows the lasting, important and close relation-ship between the methodology of science and the history of science. Each discipline has been vastly improved by this cooperation. It also opens up interesting relationships between the varying institutions in which science has been conducted and their methodologies. These connections are, of course, not the only important aspects of the history of science. There are many other interesting problems which are valuable parts of the both the history and methodology of science which are not treated in this essay.

The variations of methodologies of science and their differing institutional contexts are important areas of study for the history of science. There is no proper, comprehensive list for what aims should be pursued: The imagination of historians in finding new aims in regard to ever developing institutional contexts should be encouraged. All attempts to formulate one aim or even a few aims end up setting limits to the study of the history of science, which preclude interesting problems.

The alternative to some specific methodological theory combined with a specific set of aims as a framework for the study of the history of science offered here is a theory of science as an institution. Science has not been one constant and unchanging institution since it began, say, in the sixteenth century. Rather, institutions in which science has been pursued have changed as both the understanding of how to pursue the truth has changed and the social contexts in which truth has been pursued, well or poorly, has changed. By ignoring the close social interaction between what scientists have engaged in and the social contexts in which they have carried out their research, historians of science have romanticized, downplayed, and/or ignored many interesting problems. They have romanticized conflicts between non-scientific institutions such as the church and scientists, they have downplayed or ignored social contexts which favored some alternatives and rejected others, and they have ignored borderline aspects of the influence of science on other fields or the influence of other fields on science.

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PART 3

METHODOLOGICAL REFORM: SOCIOLOGY AND POLITICAL THEORY

The rejection of methodological individualism in favor of the social nature of individual rational practice has far reaching consequences for sociology and political theory. The fact that fallibilist rationality is social has parallel impacts on various social sciences, which shows it strength. The need for a new social theory of rationality can be demonstrated by showing how Popper's own methodological individualism conflicts with his theory of rationality as the pursuit of truth.

3a. 'Die Inkompatibilität von Poppers Theorie der Rationalität mit dem methodologischen Individualismus', in Eds. Reinhard Neck and Harald Stelzer, *Kritischer Rationalismus heute*. Frankfurt: Peter-Lang-Verlag, 2013, pp. 79-108.

The Incompatibility of Popper's Theory of Rationality with his Methodological Individualism

Introduction: Two Theories of Rationality

On the basis of Popper's research on the methodology of the natural sciences but after a long hesitation a general theory of rationality arose. This theory explains how individuals pursue truth and manage to get closer to it. This theory was developed by a number of thinkers. But its basis was Popper's famous theory that we learn from our mistakes. We make conjectures and criticize them in order to discover mistakes, to formulate new problems, and to suggest better conjectures. According to this theory rational practice is embedded is social systems, because only under these conditions is criticism at hand. The quality of rational practice—perhaps even its existence—is dependent on criticism and the answers to it which grow out of it. Robinson Crusoe, Popper emphasized, could not have done any science and therefore could only think rationally in limited ways.

On the other hand, Popper said in regard to the development of social scientific theories that rational action occurs when each person perceives the logic of his or her situation, pursues his or her own goals, and acts in accordance with his or her beliefs in order to achieve them. Neither criticism nor discussion plays here any role. Popper emphasizes the differences between these theories. (Popper, 1985, p. 365). Only the direct perception of the logic of the situation and the beliefs and aims of the individual, which are relatively independent from the direct logic of the situation are relevant for rational action.

At first blush one might think that these two theories do not conflict with each other, because they are solutions to quite different problems. They could, indeed, supplement each other, as Popper apparently thought. The first theory describes how scientists and/or philosophers get closer to the truth; the second generally describes how each individual in everyday life arises at his or her decisions concerning his or her actions and how these actions can be understood. It does not deny that individuals in everyday life can also get nearer to the truth. It only explains how social scientists can explain individual action. It borders highly on Weber's methodological individualism, which also sharply separates human action and the formation of beliefs. According to Weber belief arises from the ability of charismatic leaders to convince others what they should believe. The decision to follow some charismatic leader is not rational. One can, for example, ignore the influence of John Calvin, when one explains the social consequences of his beliefs. In order to explain the actions of humans one needs only to describe their beliefs and the consequences of these beliefs for their actions.

This suggested reconciliation of the two theories of rationality is, however, suspicious. Scientific research presumes individual actions. Examples of such actions are the choice of themes of research, the quest for money, and so forth. An adequate theory of the quest for truth has to take these facts into account. The second theory of rational action presumes that rational action occurs in socially isolated attempts of individuals to pursue the correct plans to achieve their aims in view of the logic of their situation. But everyday action includes the quest for truth every bit as much as scientific research does.

In order to understand the processes of scientific research according to the first theory, one needs to understand the rules which scientists apply in their research. Popper presumed that scientific rules are the same everywhere. On his view differences arise when they concern some problems specific to some discipline. The sociological study of these rules is, then, not needed, because the methodology already describes these rules. But when the rules vary from time to time, from field to field, from scientist to scientist, new and interesting problems arise for scientific sociology. All scientists are neither adventurers, as Popper claims, nor are they virtually all conservative as Kuhn claims. The determination of both types of applied rules and the consequences of their competition pose important tasks for sociologists of science. Scientists apply both kinds of rules. Rules which are exclusively adventurous are just as unacceptable as rules which are exclusively conservative. (Wettersten 1981; 1985a; 1995a)

Popper's observation that rational practice is social has the consequence that the difference between the social study of scientific institutions and the social study of other institutions cannot be so significant. Humans speak with one another during their decision processes—with friends, partners, colleagues, children, parents, experts, neighbors, etc. These conversations have quite obviously a significant influence on their decisions, which is in no way reducible to their perception of the logic of their situation and to the setting of aims and plans. The rules of scientific institutions may be more clearly defined. This difference is, however, one of the degree of specificity of the established rules of rational practice rather one of than one of the existence of a completely different process, which needs to be investigated with different means. In both cases we need to investigate the nature and the degree of the rational practice which is at hand.[244]

In everyday contexts rational practice is social, as Albert has portrayed it (Albert 1978). Albert saw here a problem and an opportunity for political theory. This theory should take into account the necessity and opportunity

[244] Agassi and Jarvie have developed the idea, that rational practice can have varying degrees. In a series of important articles on the rationality of magic, dogmatism and irrationality they have described differing thought strategies and have found varying degrees of rationality (Agassi and Jarvie 1987, 361-394; 431-452). These essays have prepared the path for this essay. But they treat the degree of rationality of individuals. The subject of this essay is institutional rules, which partially determine the degree of rationality practiced in institutions and how the social scientific study of these rules can be conducted.

of criticism. But Albert saw no problem for social scientific methods (Wettersten 2006a, pp. 121ff.). The problem that the social role which criticism plays requires new social scientific methods is the theme of this essay. In order to understand everyday decisions one must—just as in the study of scientific practices—understand the rules which humans follow in their considerations, conversations and critical discussions. One finds such rules everywhere. But in different contexts one finds significant differences.

The incompatibility of Popper's theory of the quest for truth with his methodological individualism is above all driven by varying philosophical anthropologies. According to the first theory all rational activity is partial. When humans seek the truth they use incomplete and confused ideas.[245] Nevertheless some people make progress. According to the second theory each person has clear perspectives with clear consequences, which he or she simply needs to apply. In the social sciences these two theories lead to differing research programs. The first program presumes that we should investigate the logic of the situation within which we can describe the beliefs and aims of individuals from which we can derive the social consequences which arise from them. The second program begins with institutional rules and asks how humans in varying institutional contexts form problems, seek solutions, and criticize suggested solutions.

In order to clarify my approach I want to add, that my analysis follows Agassi's theory of metaphysical research programs. On Agassi's theory some episodes in the history of science are characterized by competition between differing metaphysical programs which are used to construct empirical theories (Agassi 1971). A metaphysical theory according to which the world is made of waves requires empirical theories about the behavior of waves. A metaphysical theory according to which the world is made of atoms requires empirical theories of the behavior of atoms (Agassi 1971). Metaphysical theories can thereby be evaluated as research programs by examining how fruitful they are in producing empirical theories. In my case I presume two differing philosophical anthropologies or theories of rationality, which could lead to varying empirical social scientific theories. Eventually the competing programs can be appraised

[245] Popper was rather ambivalent in regard to the possibility of overcoming confused ideas. He presumes that they could always be at hand. He simply recommends that, when they arise, one should clear the misunderstanding and afterwards proceed. This is not a bad suggestion. But it overlooks the fact that we can always falsely interpret the ideas of others (Wettersten 1978).

on how well they solve theoretical problems as well as how fruitful they are for the social sciences.[246]

We find in Popper's theory of rationality as the quest for truth the best research program for the study of social rules of rationality. Popper had, of course, by no means either defended or developed such a program. He always harbored the fear that every social scientific study of science in regard to belief would devalue rationality by viewing it as a merely a simple social phenomena. Belief and thought of individuals would then be viewed as merely a function of their social situations; the objectivity of rationality would be removed. Relativism with all of its negative consequences would be the result. For that reason Popper clung to methodological individualism as a necessary support of the open society. He saw the application of the principle of rationality as the only way to develop social scientific theories, which could not themselves be empirically proven (Popper 1985, pp. 360ff.) He never considered the possibility of examining alternatives in view of the fruitfulness, because in light of the all-or-nothing theory of justification he saw no alternative to methodological individualism.

The roots of methodological individualism in the justificationist theory of rationality.

The individualism defended by Popper is a continuation of the traditional democratic political theory. A central problem of this tradition lies in the attempt to find a justification for political systems. The tradition presumes that justifications are only then possible when a source for justification is found. In epistemology these sources are either obviously true ideas or veridical sensations. On this view the source of justification for political systems was sought in the endorsement by citizens. Thereby these authors did not have the intention to offer an historical perspective. Hobbes, Rawls and Nozick, in order to just mention a few examples, postulated imaginary beginning points in order then to ask, how a government could be justified by completely isolated individuals. Such formulations of problems are not theories of how a government really can be justified. They are merely a series of thought experiments which should show where the justifications of governments should come from. But they show the background of philosophical anthropology in this tradition. Each person should be viewed in principle as an atom, who with his agreement portrays the source of justification of a political system.

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[246] For a further plea for this approach see (Wettersten 2007b),

The attempt to view humans as completely independent entities arises out of the need to portray individuals as sources of justification of political systems. According to the theory of rationality in this tradition justifications require clear, identifiable sources. It cannot be doubted where these justifying elements are to be found.

This observation of what the traditional theory of rationality demands for justification follows from a thesis put forth by Agassi. He pointed out that the traditional theory demands an all-or-nothing point of view (Agassi 1977; Wettersten 1982; 1985b). When we remain within the borders set by this theory, we can only allow as possibilities truth or falsity, reality or appearance, etc. As a consequence, in social theory we have to decide between a radical holistic theory and a radical individualistic theory. There is no possibility that an entity with a bit of both could play the role of justifying element. After one decides for one alternative or the other, one has to make the best of it.

Although Popper had some time before abandoned the aim of justification, he wanted to defend individualism as the only civilized alternative to holism. He saw no alternative to a radical traditional individualistic alternative. Therefore he sought to bring his own fallibilist theory of methodology in the natural sciences in agreement with the traditional individualistic political and economic theory. Just as his friend and supporter Hayek he worked in the tradition of John Stuart Mill and Smith. But in view of his fallibilistic perspective this tradition and to be renewed. The quest for a source of justification no longer played any role. For that reason radical individualism was no longer a sensible alternative. But Popper saw little need to reform anything in the political theory, as he had seen in regard to the methodology of the natural sciences and the holistic tradition. He wanted to derive out his revolutionary philosophy of science new arguments for a lasting and successful individualist democracy. For that reason he tried to force his new perspectives in a procrustean bed of a traditional justificationist theory.

After *Logik der Forschung* Popper did not try to develop a new philosophical alternative. Rather, he wanted to carry out various tasks with the help of his philosophy of science. He thereby without further ado and without mentioning it used the perspectives of his time, which appeared to him to be acceptable. He began his intellectual career by seeking to develop an alternative to the philosophers of the Vienna Circle, to Reichenbach, Carnap, Schlick, Neurath, etc. He did not notice that he thereby introduced a revolutionary philosophy. I have elsewhere portrayed how Popper changed and developed his theory as his critics from the Vienna Circle reacted to his initial attempts (Wettersten, 1985c; 1987a; 1988; 1992; 2005). Agassi adopted my portrayal of this development as his. He pointed out that Popper noticed for the first time in England the broad significance of his perspective (Agassi 2008, p. 295). During this time he began to further develop his own philosophical perspective which following Popper we today call critical rationalism. I have elsewhere argued that, for example, his theory of the attractiveness of the closed society was rather typical for his time, but that it does not fit in his fallibilist, critical theory of rationality (Wettersten 2006b; 2007a). According to the theory of the attractiveness of the closed society we try to avoid rationality, that is, criticism, while in his fallibilist theory of rationality he emphasizes how satisfying the quest for truth found in science can be. Popper's methodological individualism is a further theory of this kind, in that he takes over all too much from a tradition near to his research. On various positions he overlooked the deep changes his fallibilism demanded in political and methodological theories.

The necessity of Popper's later interpretation of the rationality principle for the completion of his theory of social scientific methods.

Popper developed his theory of rationality in three phases. The first phase was *Das Elend des Historizismus*, the second was *Die offene Gesellschaft und ihre Feinde*, and the third was his essay on the rationality principle. The first two phases may have been much broader and far more important for his development. But in the third phase he tried to solve important problems. He tried there to improve his methodological individualism in light of his philosophy of science, or better said, to bring into agreement his philosophy of science with his methodological individualism as it is applied in modern economic theory. While carrying out this task he forced his revolutionary philosophy of science in the Procrustean bed of modern individualist theory of method. The problems which arose out of this move were considerable. If Popper's solutions to these problems could not be maintained, he would need a new alternative to methodological individualism.

Popper took up above all two tasks in *Das Elend des Historizismus*. The first consisted of the development of a critique of holistic and/or historicistic methods in the social sciences. The second was a defense of the application of the methods of the natural sciences in the social sciences. In both cases his theory of the methods of the natural sciences came to his aid. Through the application of his criticism of traditional theories of the methods of the natural sciences he could show that historical laws could not be found. There could, therefore, not be any historicistic social science. With the help of his fallibilist methodology he could also show how empirical tests of social scientific laws could be carried out.

In *Die offene Gesellschaft und ihre Feinde* Popper, on the hand, analyzed the political consequences of what he called the historicist theories of the social sciences, in order to fight this direction in the social sciences and the social consequences which followed from it, and, on the other hand, plead for an individualist and modest theory of social reform. In the context of his political considerations he also developed his theory of rationality as a social theory (Jarvie 2001)[247] He suggested that the tasks of the social sciences were attempts to identify unintentional consequences; social reform should be as good as exclusively modest. In contrast to earlier individualist theories he emphasized the importance of institutions (Agassi 1987; 2009). These theses fit nicely with his fallibilistic theory of methods in the natural sciences.

According to the point of view defended here Popper's theory of institutions borrows all too much from traditional theories. Especially his theory of the use of the rationality principle should be set aside in order to develop a better approach to the study of effects of institutions. Insofar as the view defended here the central role of institutions by the development of social scientific explanations as well as by the planning of reforms it remains from these points of view in agreement with the view Popper developed in *The Open Society*.

The important difference between Popper's theory of institutions and the theory defended by Popper and Agassi, on the one hand, and the view defended here, on the other hand, is, that, although Popper, Agassi and Jarvie emphasize the importance of the analysis of institutions and their

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[247] Jarvie emphasized, that Popper developed a theory of science as an institution, which followed its own rules. From my point of view Jarvie's contribution lies in his portrayal of Popper's philosophy of science in *The Open Society* and to have defended his view against those who have not seen this fact (Wettersten 2006a). A number of Popper's defenders have taken as a theme this institutional aspect of Popper's theory. I have described the development of Popper's theory as he introduced methodological rules in view of the German literature (Wettersten 1985c; 1987a; 1988; 1992; 1005a) and defended the theory that science needs methodological rules in opposition to other interpreters such as Gunnan Andersson (Wettersten 1995b). The research in the sociology of science undertaken by Agassi and myself presume this interpretation (Wettersten 1993).

consequences in regard to the evaluation and planning of social reform, they have not seen that carrying out these tasks requires changes in the individualistic theory of methods. For that reason Popper largely ignored the problem of how social scientific theories could be developed in the best way in *The Open Society*. But implicitly he laid the already existing individualist research program as the groundwork for research in the social sciences and/economics.

Popper's interpretation of the rationality principle and its conflict with his fallibilist theory of rationality.

When Popper began in the 1960s to develop further his methodology of the social sciences, he tried to satisfy to types of needs (Popper 1995). On the one hand he had the intention present a methodology of the social sciences which would supplement his methodology of the natural sciences. On the other hand, he wanted to orient this methodology on the dominant methodology in national economics. These two conditions cannot be simultaneously satisfied. According to his methodology of natural sciences all rational practice is faces mistakes. A rational person is, as Bartley formulated it, someone who holds all his beliefs open to criticism. But according to methodological individualism each person is rational insofar as he or she has coherent ideas, specific aims, and devises plans that should enable him to realize these aims in view of the logic of his or her situation. Judged on the standards of the first theory is the second theory simultaneously too strong and too weak. It is too strong, because no human has coherent ideas which he or she can use without intellectual problems to attain his or her aims. The theory is too weak, because each decision process embodies critical phases, which deals with vague and confused ideas as well uncertainty regarding the specific aims which should be reached.

Popper's interpretation of the application of the rationality principle too strong because individuals are attributed rational actions which admittedly cannot be achieved. As a consequence Popper's theory can scarcely be distinguished from Weber's theory of ideal types. Only when one, just as on Weber's theory, presumes the existence of a coherent world view can one on the basis of beliefs explain how a society should appear. On this point, Weber fell back into a holistic theory, as Agassi and others have pointed out (Agassi 1987). Weber tried, for example, to describe how a capitalistic society should function as a whole. Popper did not, of course, want to go down this path. Instead he suggested that the social sciences construct models, which obviously take only aspects of societies into account. He had above all national economics in mind. In my discussion of Jarvie's defense of this program treat this suggestion and the possibility of realizing it.

It was also important to Popper to present a realistic interpretation of the social sciences. This task is by no means easy to carry out. At first blush there is scarcely a difference between Milton Friedman's instrumentalist theory of models in the social sciences (Friedman 1953) and Popper's. Friedman and Popper agree that models take into account only specific aspects of societies. Both authors see models which are more precise and more comprehensive as superior. They are constructed according to each author with nearly the same methods, that is, on the basis of the rationality principle each person uses coherent plans to pursue the realization of specific aims. Friedman maintains that models do not need to correspond to thought processes which are really at hand. It is merely a case of whether they can be used to make true predictions. They are tools.

Popper tries to avoid this instrumentalist view in that he views the rationality principle as false but also as nearly empty. Social scientists should dogmatically hold to the rationality principle even though it cannot exactly describe reality. Only then, he says, can that find social scientific explanations. One can improve social scientific explanations in that one conducts tests, which are with the formed with the help of the rationality principle. This claim is dubious because models always describe only a portion of reality. When social scientists explain the failed attempts of individuals to achieve their goals, they can according to Popper introduce the following argument: Although the failed attempts were appropriate in the context of the model, they nevertheless failed because the model does not take into account important aspects of the situation in which the actions took place. In a wider and therefore more realistic model these actions can be understood as rational. Insofar as social scientists follow Popper's advice and investigate unintended consequences models would always be judged on how well or poorly they steer actions. The models might be true but nevertheless remain all too schematic in order to appropriately steer appropriate actions. They do not have meaning because they are true or false, but rather whether they are good or bad tools. There seems to be no sense to make models so precise as possible and therefore so near to the truth as possible. All models remain to this or that degree schematic. This result is a consequence of Popper's changes in the tasks that methodological individualism should carry out. Popper uses a method which was constructed to investigate the functions of economic systems. But he uses it in order to carry out other tasks, that is, he wants to investigate the unintended consequences of rational actions which are beyond a clearly defined system.

Popper's attempt to construct a realistic interpretation of social scientific methods has rather little to do with his theory of the natural sciences. In the natural sciences theories with more explanatory power replace theories with less explanatory power. We thereby develop a deeper understanding of the world. In this way we come closer to the truth. In the social science each model, whose application is wider or more precise, is considered closer to the truth. Because each model describes this or that aspect of a society, models do not need to have anything to do with each other. The do not build any system. But they should be able, to create an increasing number of true predictions, just as by Friedman.

Popper's theory is consistent and also, as some have doubted, in agreement with his theory of the natural sciences (Wettersten 2006a, pp. j45ff.). The argument, that social scientific explanations would only be possible with the rationality principle leads to a dogmatic defense of the rationality principle. But this transcendental argument is not convincing, because there are alternatives.

Jarvie: The disappearance of critical institutions in the minds of humans.

In order to look more closely at Popper's methodological individualism, I will examine Jarvie's defense of his theory. (Agassi has also pointed to Popper's emphasis on institutions. But his commentary is above all a discussion of ontology, that is, of the nature of institutions. He does not deal with the methodological consequences which arise out of this situation.) Jarvie tried above all to interpret Popper's philosophy of science as above all a theory of scientific institutions and to build on this thesis a portrayal of Popper's social scientific methods as a theory of the investigation of institutions. On his point of view Popper's portrayal of social scientific methods is above all a further development of his theory of the methods of the natural sciences.

Jarvie maintains that he brings something new in the interpretation of Popper's theory in that he points to the importance in Popper's theory of its institutional aspect. This, he says, had hardly been noticed before. Jarvie does in fact offer a number of enlightening observations concerning the importance which Popper gave to institutions. But Jarvie adds too little to Popper's theory to render it useful for the investigation of institutions.

At the same time he all too much ignores the prior discussions of this institutional aspect. In this regard there is Agassi's research in the sociology of science, as well as my own work on furthering this area of research; Agassi research in the philosophy of technology can be added to this list. Albert has applied Popper's perspective to social philosophy and jurisprudence; I have investigated Popper's introduction of social rules[248]. Jarvie mentions some of these inquiries but he gives the false impression, that there is a wide neglect of this aspect of critical rationalism. He did want to do a service for critical rationalism in that he pointed out an alleged neglect of an ignored aspect of Popper's philosophy of science, but he conveys the false impression that there had previously been no studies of this institutional aspect of critical rationalism and that all that is serious in this field can be found in his book.[249]

The theme of Jarvie's book is above all the development and the meaning of assumptions, which are found in Popper's statements about science and research in the social sciences. His study elucidates Popper's methods of the social scientific investigation of institutions. He holds to Popper's theory of the rationality principle as the central element of Popper's theory of social scientific research, which has been rejected here.

Jarvie emphasizes, that Popper rejects the traditional reductionist theory of society, whereby the institutions can be reduced to the actions of individuals. He thinks that in its place Popper emphasized the moral aspect of individualism (Jarvie 2001, p. 124). Jarvie observation, that Popper thought that the appraisal of the consequences for individuals is a moral standard. But this point of view does not separate him from the traditional individualist tradition. In the framework of the theory of justification, traditional individualism constructed utilitarianism[250]. The third support of

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[248] Jarvie tries to explain why Popper introduced methodological rules in his philosophy of science. But he thereby ignores the transition from *Die beiden Grundprobleme der Erkenntnöistheorie* without Kapital V to *Logik der Forschung*, which I have precisely documented (Wettersten 1985c; 1992).

[249] The mentioning of other interpretations of Popper's research (Jarvie 2001, pp. 32-33) as well as the observation that some other thinkers this aspect (perhaps) had noticed (Jarvie2001, p. 47) does not make this omission much better.

[250] Popper also took over a great deal of the traditional ethics. He saw, that the utilitarians correctly saw, that the consequences of actions have to be used as a moral standard. He also saw that this standard is not sufficient, because there are principles which moral actions cannot violate. He partially portrayed this second aspect of moral action, by observing that one must be rational, that Is, critical, On my reading adopted the view that one should be as rational as possible. This

the traditional theory is methodological individualism[251]. Popper's acceptance of this support conflicts with that, which he developed in conclusion of his rejection of the first support—the ontological support.

Jarvie dedicates an entire capital of his book to the methodology of the study of institutions (Jarvie 2001, pp.88ff.)[252] One can divide the methodology described there into two parts. In the first part we find Popper's theses about holistic theories and the methodological problems which arise out of them. In this part there are not differences of opinion between me and Jarvie which are worth mentioning. The theses defended here are either a part of his ontological theories of institutions or a part of his critical theory of rationality.

In order to solve the problem of how societies should be studied, Popper says, that methodological individualism should be applied. Above all in *Das Elend des Historizismus* and in his essay on the rationality principle Popper describes how that should be done. Jarvie defends these aspects of Popper's theory as the best and/or the only acceptable method for studying institutions.

The application of the rationality principle for the study of institutions reminds one of William Wundt's physiology of the beginning point and foundation of his psychology, which then should be studied with the means of associationist psychology. In regard to Wundt's procedure Wilhelm Frege pointed out, that approach sawed off the branch on which he sat (Frege 1969, p. 155ff; Wettersten 1995c). Wundt had namely built all of psychology on physiology. But then he claimed that all knowledge

decision is the foundation of his work ethic. As Agassi has reported, he could not do anything which could not be interpreted as work, as if his duty to serve the open society would be damaged if he did something only for fun. This attitude is a remnant of justification theory.

[251] Jarvie thinks that methodological individualism is a direct consequence of Popper's philosophy of science (Jarvie 2001, p. 102). Through the application of methodological individualism one can build falsifiable theories. He thereby ignores, that the thinkers who intensively applied this theory, had considerable difficulty to render their models testable, as Boland has portrayed in a detailed way (Boland 1989). One can also construct falsifiable holistic theories. Marx developed falsifiable and falsified theories, according to which the unavoidable communist revolution would begin those countries within which capitalism was the furthest developed. I offer here an alternative individualist theory, which I call fallibilist institutionalism and within which falsifiable theories can be developed.

[252] In the table of contents capital 3 is called "Problems in a Science of Social Institutions", but in the text "The Methodology of Studying Social Institutions".

of physiology was based on sense impressions. As a consequence of this method one hat no knowledge of anything besides sense impression, that is, only ideas. Popper begins with a realistic theory of institutions. The logic of the situation is treated as an objective fact. But then he says that social scientific explanations are not based on the logic of the situation as it really is, but on the perception of the logic of the situation. This completely removes his realistic theory: One has no realistic situation, but only perceptions of situations to which individuals react.

After institutions are lost in the heads of humans one has to bring them out. In order to do this there are two traditional methods. The first method is Weber's. Weber studied so-called ideal types. These were in the end the structures of complete societies and/or cultures, which were to be studied by looking at the dominant beliefs in them. The capitalist society could be studied in this way in that one studied the beliefs of the Calvinists or of representative individuals such as Benjamin Franklin. But this alternative is not acceptable to Popper, because from his point of view there are no holistic societies.

Popper favors a second method, which has been applied by economists. Economists build models of typical situations, above all of markets. Jarvie defends this as a method, which avoids the subjectivism described above (Jarvie 2001, p. 103, 126, 132ff.) Popper's begins neither with holistic societies nor with the beliefs of individuals as by Weber, but with so-called typical situations as by the economists. Because Popper says nearly nothing about the characteristics or the choice of such situations, it is difficult to interpret his preferred point of view. But he borrows heavily from the modern economics dominated by neo-classical methods. These economists construct their typical situations in that the take the market as the basis of their considerations then question, how rational individuals in specific constellations of the market act.

In *Das Elend des Historizimus* Popper calls these methods the Null-method and makes them central of his methodology of the social sciences. In order to build a social scientific explanation one presents a typical situation and asks, how a rational person would act in such a situation. One presumes that the acting has "all information" and judges it in a coherent way, in order to achieve his or her normally recognized aims. The hypothesis which then arises one can call the zero-hypothesis and then test, which individuals really do act in this way. In economics the typical situations are market situations and the rational person is a person, who tries to maximize the usefulness of the results of his or her actions.

Following Popper portrayal in *Das Elend des Historizimus* and his article on the rationality principle, Jarvie claims that assumptions made about the rationality of individuals are minimal.

In *Das Elend des Historizismus* Popper mentions an article, in which one finds the application of his preferred method (Marschak 1943). In this prime example of Popper's method Marschak uses a method found in statistics in which a zero hypothesis will be hypothesized and tested against alternatives. On Marschak's zero hypothesis is the effect on the demand when incomes are reduced or prices are raised the same, if the reduction of incomes is proportional to the increase in prices. Popper chose this article as an example of his favored method, because Marschak's zero hypothesis describes how rational individuals would react.

In the following discussion Popper poses the question whether this widespread technic can be properly used as methodological model for all social scientific research. (Popper was the first "economic imperialist".) There are at least two difficulties with the application of this model of research for all social scientific research. First, the proposed situations are not really typical. Second, the assumptions made about the rational actions of individuals are not minimal. The so-called typical situations are above all artificially constructed market situations. They place actors in some ideal market situation with presumed characteristics in order to then ask how a rational person would react in this situation. In comparison to real situations they are schematically portrayed in order to make rational action at all possible. In addition they are statistical. They can then only play a role, when the completely rule out that which Schumpeter called "creative destruction". But "creative destruction" is the norm rather than the exception. These artificially constructed situations are dependent on the assumptions of neo-classical economic theory. When one tries to apply these methods in other fields the construction of models is more artificial as in economics, because clear definitions of situations are lacking.

Popper maintains that, although this method is not appropriate for the study of individual cases, it is necessary for the development of broader social scientific theories. The opposite may be true. The construction of especially clearly defined situations forces social scientists separate rather widely from the reality. In some individual cases this is not the case, as a lovely example developed by John Watkins shows. Watkins portrayed how a completely ridiculous catastrophe at sea as an intended consequence of the rational actions of two captains can be rendered understandable

(Watkins 1970). But he could do that only because he exactly knew, first, what the logic of the situation was, and second, how a captain would think in such a situation. He wanted to show how effective methodological individualism could be. But he identified an assumption in which the logic of the situation and the thought processes of individuals clear and plain to see. These assumptions confirm he rule that the method is not applicable in normal situations.

The suggested typical situations largely ignore the deep influence of institutions on individuals, when they formulate problems and evaluate the alternatives which are taken into question. Difficulties arise when, for example, humans follow rules under whose influence it is not clear how they pass in some plan whose application should serve the attainment of the personal aims of individuals. The models overlook that fact that the rules are in the heads of individuals. In her anthropological research Mary Douglas describes this phenomenon in an impressive way (Douglas 1987; Wettersten 2000a, pp. 185ff.). The rules partly determine how humans in differing ways think in various contexts.

One could answer this criticism by pointing out that the application of the zero hypothesis only fills the aim of finding out whether factors not taken into account by the zero hypothesis play a role in real situations. But when one determines that there are such factors, one needs a method for identifying them. The application of the zero hypothesis cannot do that. One can also compare the zero hypothesis with others. When the predictions of the zero hypothesis differ from the predictions of some other hypothesis, one can determine whether the alternative has more explanatory power than the zero hypothesis. But that provides nothing concerning the type or the content of alternative hypotheses.

When one determines what the rational reaction to a typical situation is, one should make only minimal assumptions action to be determined. One presumes that each rational thinking person in a typical situation comes to the same conclusion with "all information". Jarvie thinks that this assumption is not special. It is only used as a beginning point of social scientific research. Afterwards, one inquiries to what degree the actual actions vary from the ideal actions and what unintended consequences arise out of the actions.

But why should there be such a beginning point? The assumption, made for methodological reasons, that the acting person has "all information" is simply nonsense, when there is no inductive method or no inductive proofs. The assumption should serve to show that there is some ideal thought process. But that cannot arise out of the collecting of data. One would still have a never ending number of alternatives in each selected situation.

Methodological individualists concede that the assumptions made are false, that each action occurs with a deficiency of relevant information. In order to use this method a source of plausible hypotheses one must at least presume that the rational actions which are portrayed do not normally differ all that much from the norms of rationality. Otherwise nothing could be explained. One would then have to come to the conclusion that individuals act irrationally—as Jarvie says. For this reason economists try to explain how individuals come to more or less correct appraisals of a situation and thereby to the most effective reactions. Hayek suggests, although each in a market acting person has no sufficient information about businesses, demand and supply, he or she can nevertheless act rationally because the price of goods provides him or her with a summary version of this information. But this theory is circular, because the price can play this role only when the acting individuals already have the necessary information to pay the right price. This is often not the case. George Soros points out that there are reflexive processes. A buyer of stocks observes competitors are buying stocks and the price rises. On the basis of this observation he or she also buys. The original buyer who had already bought sees that others follow and buys further. They swing each other higher, even though no meaningful information is at hand. Herbert Simon maintains that, although one never has sufficient information or enough time to exactly find the right answer, one can reach satisfying results with a rule of thumb. But this theory employs the inductivist assumption that there exists a correct solution when one has enough data. Gerd Gigerenzer says that the evolution has supplied us with a simple heuristic, which we apply in appropriate contexts. But how do we know which heuristic is appropriate in which context? Either we have a heuristic to decide, which leads to never ending regress, or we have inductive proof. In his article on the rationality principle Popper goes from the obviously inductivist assumption of the presence of "all information" to the vague assumption of adequate knowledge. But one still has to presume that there are rationally based justified best solutions. The theory of rationally acting individuals was not only developed in the framework of justification theory; it presumes the existence of a method of justification. For this reason its application always leads back to unsolvable problems of justification.

Lawrence Boland emphasizes and describes how established economic methods apply psychological-cum-inductivist and inductivist assumptions, in order to describe so-called rational actions (Boland 1986). Boland would like to substitute in economics for the established inductivist theory a theory of learning out of mistakes. But this program cannot be carried out because it contradicts the statistical equilibrium of neo-classical theory. The neo-classical theory especially presumes that rational actions move the market the direction of equilibrium. In order to do this the actors need to possess more or less correct ideas what damages to equilibrium exist, for example, where business managers can increase income by raising their prices and where they cannot do this. But no theory of learning from mistakes provides the actors with even conjectures about such relationships. They can only be rational the sense that they are critical and learn from their mistakes, without taking as a collective the best course.

The economic methods favored by Popper and Jarvie presume, that there is a normative of best perception of each typical situation. Each (ideal) rational thinking person will share this perception. But, why? In each situation individuals can perceive the logic of the situation in varying ways. In order to solve the problem of why all rational thinking individuals should perceive each situation in the same way, the situations are intentionally not realistically described, but are radically simplified. In contrast to real situations the situations which are portrayed are static. Ronald H. Coase has called this result "blackboard economics" and Albert model Platonism. In order to render it clear, that in a typical situation there is only one rationally based reaction, the situation must be described so schematically as possible. The clearer the alleged rational reaction is made, the wider it is separated from reality.

It might appear that my criticism of the application of methodological individualism in the study of institutions oversees strength of this methodology. Methodological individualism presumes that individuals take into consideration the logic of the situation. Within the research program of methodological individualism one must take into consideration the institutional rules. They form an important part of the logic of the situation. Jarvie emphasizes how strong and rich Popper's treatment of institutions is (Jarvie 2001, p. 127). But the planned approach of methodological individualism cannot adequately grasp the influence of institutions on individuals. The application of the rationality principle which is recommended by methodological individualism leads to the conclusion, that the widely described by Popper critical aspects of institutions are largely banned from social scientific explanations. The perceived institutions are critical, well or poorly defined and constantly changing. But in the heads of individuals they are washed clean. They occur in the thoughts of individuals as static, clearly defined and simple.

Due to Popper's skeptical methodological theory, he quite correctly finds a major task of the social sciences to explain the unintended consequences of rational actions. But the creators of methodological individualism did not view these tasks as central. The importance of unintended consequences goes back to Smith. He emphasized, that an unintended consequence of the actions of bakers, butchers, etc., when they pursue their own economic interests, is the creation and maintenance of a well-functioning system. Weber also explains that the attempt by Calvinists, certainty about their fate after their death, had the unintended consequence the creation of a capitalist economy. Traditionally methodological individualists have investigated how systems function well. Popper saw the problem completely differently. He wanted to understand, which consequences arise, when there is no well-functioning system.

Traditional methodological individualism can be used to investigate unintended consequences of rational actions insofar as these consequences occur within economic systems. Popper's methodological treatment of this problematic seems acceptable, insofar as the as the independent consequences arise out of coherent plans which can be coherently carried out. The assumption, that all unintended consequences should be investigated, would follow his claim, that social scientific explanations are only possible through the application of the rationality principle.

Can one with success the old methodological individualism also use to carry out the new tasks of investigating unintended consequences, when the consequences arise from false perceptions of the logic of the situation and or from confused ideas? Popper says, yes. He explains this in an answer to a commentary by Winston Churchill, who said, that wars cannot be won, but through bad planning can be lost. Popper thinks that the application of methodological individualism can make such mistakes understandable. One could, albeit with great difficulties, determine whether a mistake, on the hand, was based on an inadequate model of the situation or whether, on the other hand, based on the inappropriate actions of the individuals. But no method exists to distinguish between these alternatives. The space for the construction of alternatives is too large.

Popper's critical social theory of rationality as a research program for the social sciences.

We can leave aside Popper's theses about methodological individualism and the rationality principle and substitute for them a more promising program. This alternative avoids problems in the older research program, but opens new problems and supports a strong theory of realist social science research. We can thereby use Popper's theory of critical and social anthropology as a beginning point for this new research program for the social sciences. In order to render this perspective fruitful for social scientific research, we do not begin with the beliefs of individuals and/or ideally constructed logic of the situation. In we ask, on the one hand, how institutions within which individuals act effect the formation of problems and, on the other hand, which methods institutions offer for the favoring of alternative plans[253].

In order to carry out this program we place on its head Popper's theory that by the construction of social scientific explanations one should place as much as possible of the (subjectively perceived) logic of the situation. Instead we try to put as much as possible of the (objectively described) logic of the situation in the institutions. In this way we can determine the effects of the institutions on the individuals in their area of influence. We set ourselves the task of describing the really applied social rules of rational discussion. We can also evaluate them critically. In this way we can hopefully render decisions easier to understand. One aim of this approach is to see how the applications in social contexts of certain rules have consequences.

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[253] I do not maintain that studies of approach portrayed here have never be carried out in the past. One example of this is the excellent study of Michael Segre of Italian science after Galileo, in which he describes how societal conditions led to the decline of Italian science (Segre 1991). A second example is the study by Agassi and Laor of medical diagnoses (Agassi and Laor 1990). A third example is a study of families which shows how families can better understood when one studies the tasks which each member of the family carries out. It renders possible the refutation of Parson's claim concerning the disappearance of the extended family (Litwark, et. al. 2003). A fourth example is Jacoby's investigation of the influence Japanese and American personal politics on firms in the relevant country (Jacoby 2005). I only maintain, the philosophical anthropology proposed here can such research better interpret and can better steer new tasks than previous points of view, including those of Popper, Agassi and Jarvie (Wettersten 2007b).

In order to improve the portrayal of this suggestion, I offer an example. The German school system is divided into three levels. After the fourth grade the teachers decide, on which level each child can or must continue their education. This system gives teachers certain tasks to carry out, which have deep consequences for the future and social context of each child. The relationships between teachers and parents suffer regularly due to this arrangement when the parents are not in agreement with the decisions of the teachers. This social system can be socially analyzed on the basis of the tasks carried out by teachers and the social consequences of their being carried out. One does not need to bring the beliefs of individuals into consideration (Wettersten 2002).

Popper always harbored the fear, that the social scientific research of science of the thought or beliefs of scientists was a danger for science. But in this context this fear has no basis. Social scientific investigations of rationality can, indeed, be used the rules to identify and to endorse in order to render more secure and to expand political and social openness. (In the example just mentioned it is clear that the rules employed by the German school system harm the openness of society, in that they impede the personal development and the path to integration of all too many children all too early.) The Popperian theory of rationality does not merely have the capacity to open the social investigation of rationality but also can show that such investigations are needed. It also shows how such an investigation is possible without falling back into a functionalist view of society and without the need to view existing rules as the best possible ones.

The changes in fallibilist social scientific research suggested here move in the direction of those theories, which have their beginnings in the investigation of institutions. Traditionally the strongest theories of this kind are holistic. This kind of theory has been effectively criticized. Falling back into this perspective would be fatal. But we can formulate our research program, in that we start with institutions without ending in any holistic theory.

Consequences of the new research program for social scientific research: Old tasks changed and new ones formulated.

According to the research program proposed here all rational actions follow established social rules to a certain degree. Rules are applied in order to pose problems, to exercise criticism, and to carry out plans. One can, indeed, understand the degree of rational practice, when one knows the rules which individuals apply in certain contexts. A central example of this kind of understanding is constructed by the sociology of science which investigates the scientific rules applied in research, criticism, publications, discussions, etc.[254]. But this discipline is only an example of the broader investigation of institutional rules; the program can be carried out almost anywhere. When Popper's theory of rationality is used as a framework for social scientific research new perspectives for old tasks can be opened and new tasks can be formulated. I can only sketch a few examples here. Insofar that they can lead to progress must be, of course, piecemeal determined in practice.

The difference between actions which are the results of individuals pursuing aims in accord with plans and actions which merely follow rules has a rather strong influence of social psychology. There it is often presumed, that actions which are conducted independently of any rule are rational, whereas actions which follow rules diverge from an ideal of rationality. On can study how and to what degree the social situation or established prejudice about sex or status etc. influence decisions. But this program completely ignores, that social rules are always used whether or not the better or worse and that they, as one can see in science, are necessary in order to think rationally. A consequence of this observation of Popper is that there are scarcely any meaningful tasks for social psychology alone. These tasks include: Under which social conditions is rational, that is, critical thought furthered the best? Which social rules further and which block such thought? Which group actions or group influences, say, workshops, increase the degree of rationality of individuals and which do not? (Wettersten 2008) The sharp division between thought psychology and cognitive psychology is removed. All cognitive processes have social components, which possibly by their study cannot be ignored without distorting them.

Also in political science the new research program offers new tasks. Defenders of democratic forms of government presume that an open conversation renders it possible to find the best solutions to political problems. But Watkins has pointed out, that the traditional theory of rationality allows no theory of the agreement of various points of view, because it allows only all or nothing judgements: A theory is justified or it is not (Watkins 1987). There is no place for differing good, but contradictory, alternatives. I add to that, that the discovery of good

[254] From my point of view the best sociology of science is in the application of a Popperian perspective and was introduced by Joseph Agassi (Wettersten 1993).

political solutions does not merely depend on each person being free to form his or her own opinion and to express that opinion. How good or bad the points of view, which are expressed, are depends to a high degree one how good or bad the rules the participants use in their discussions are. These depend in turn on institutions such as newspapers, parliaments, television, religions, etc. Defenders of democratic governments cannot exclusively strive, that citizens are free. They must also take note of which rules institutions apply, in order to encourage or to limit the open debate.

The use of the traditional theory of rationality can also bring with it terrible consequences. When the standards for political opinions criticized by Watkins are applied, extremist points of view are favored, because it is easier to provide radical points of view a coherent, systematic form. Non radical views which take criticism and problems seriously must regularly make do with compromises and partial solutions. But this is not a loss, because the degree of rationality is higher when criticism is taken seriously (Wettersten 2009). There is here important tasks for political science, whose investigations through the application of traditional epistemological theories in connection with the traditional individualist theory of action have been hindered.

National economics is more than any other social science dedicated to methodological individualism. It builds the proudest economic accomplishments. But economics have recognized that the research program of national economics has been too narrow because it has not taken adequate account of the role of institutions in economic processes. Modern economics has rejected traditional theories of institutions, such as those from John Commons, Gunnar Myrdahl, Thorstein Veblen, or the Frieburger School. They have maintained that these “old institutions” provided no correct theory. For that reason the so-called “new institutionalism” was developed by such thinkers as Schumpeter (1993), Buchanan and Tullock (1962), Downs (1968), Olson (1985; 2004) or Douglass North. The new institutionalism should above all use the methods of modern economics, that is, the neo-classical economy, in order to adequately take the roles of institutions into account.

The so-called new institutionalists try to explain the creation of and/or changes to institutions in that see are viewed as direct consequences of attempts by individuals to maximize their usefulness. From the perspective developed here such attempts cannot be successful. The reason for this is that institutions give to a high degree form to the formulation of problems, the solutions to them, and the appraisals of solutions. The problems which individual formulate, the solutions they consider, and the critical methods they employ, are all to a high degree influenced by the institutions in whose context they are carried out. There exists no individualist source for the judgement of institutions which is completely independent of institutions. One cannot ask was usefulness a person has in some situation without taking the institutional context into account. To ask was usefulness persons derive and to understand this as source of the change in institutions leads to a circular process, whereby institutions serve as their own justification, because they exert so much influence on the individuals judging them. When individuals can freely choose, they often prefer institutional arrangements, whose preference is determined independently of their direct personal interests. In Africa persons of choose tribes, whereby democracy can lead to tribal conflicts. Institutions are not above all the consequence of attempts by individuals to maximize their usefulness, but rather attempts by persons to formulate problems, to find solutions, and to judge them in institutional contexts. Attempts by individuals to realize their own aims offer no independent basis from which plausible explanations of the formation and changes of institutions can be constructed. One begins, rather, with the characteristics of institutions and then asks how they influence individuals, when they formulate their problems, seek solutions to them, and appraise alternatives. This perspective allows a critical approach to institutions. For that reason it can better further democracy as the attempt institutions to justify with the agreement of individuals. The former method ends with the agreement of individuals in given situations. It does not ask whether the institutions can be reformed so that better conditions for the construction of perspectives could be brought about. This proposed method begins with the question, how the formation of perspectives can be improved with better institutions.

Institutions cannot build any holistic system. They are results of historical processes, which are often ordered by many contradictory influences. Neither evolutions nor rational actions can lead to their harmony, even in non-economic processes. Their interaction can only be understood piecemeal. It serves no economic aim such as equilibrium but various ones, which are often in conflict with one another. How far economic laws can be abstracted from historically determined institutions is an open question. The effects of individual institutions has to individually investigated in order to determine to what degree economic expectations in areas of influence can be fulfilled.

Conclusion: For what is philosophical anthropology advantageous?

From the critical perspective defended here the purposed of philosophical anthropology lies above all in the building of research programs. The research programs should carry out two tasks: First, they should interpret well current research and second, they should open new perspectives for the construction of social scientific theories. In order to determine how well or poor a research program carries out these tasks it must be tested in various contexts. The application and the evaluation of such programs occur piecemeal. The better philosophical problems are solved within such philosophical anthropologies the better are the chances that they interpret good research programs and new perspectives open.

I have tried here above all to show the philosophical advantages of a reform of individualist methodology. This reform has the purpose to render useful for social scientific research Popper's theory of rationality as social, critical and fallible. It offers a simpler research program, which also opens the possibility of putting forth a better interpretation of realism in the social sciences. Social sciences can then better study institutional rules and attain- deeper and hopefully more general knowledge about how they work. The major difference between this and other programs, which presume that institutions consists of rules, lies in the fact that program brings into connection the rules of rational practice, which partially determine the practice of individuals. Other programs simply presume that the rationality of individuals is simply at hand and can be employed in various situations. The theory proposed here presumes, as Agassi and Jarvie have emphasized, that rational practice has varying degrees. It can be adventurous or conservative in that piece for piece theories are improved. Rational practice has thereby various styles (Wettersten 1995a). Rationality is nevertheless in that sense universal in that each person tries with his or her rules to formulate problems, to suggest solutions and to engage in criticism.

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Interlude

A theory of how social science can be reformed by applying the theory of rational practice as social in place of methodological individualism is needed.

3b. 'Beyond Methodological Individualism: Social scientific studies of rational practice', *European Journal of Sociology*, LIII, 1, 2012, pp. 97-118.

Beyond Methodological Individualism: Social Scientific Studies of Rational Practice

Abstract

Standard versions of the sociology of rational practice assume justificationist theories of rationality: all rational beliefs are justified and rational individuals do not believe any non-justified statements. This theory appears to some to offer the possibility of finding "deeper" insights into social behavior: some actions presented by actors as "rational" can, in fact, be explained as non-justified and, therefore, as mere consequences of Prestige and/or power conflicts. When, however, it turns out that no theories can be justified then all theories are irrational. This leads to relativism. The possibility, that we may profitably construct alternative theories of rationality is, in contrast, raised nearly uniquely by fallibilist theories of rationality. In order to take advantage of this, an alternative to the dominant methodological individualist theory of rational action is needed and possible. According to this alternative, rational action consists of problem-solving within institutional contexts without justification. Such non-relativist sociology of rational practice can be enlightening and useful. Differing institutional contexts offer differing standards of rationality.

Keywords: Methodological individualism; Rationality; Fallibilist theory; Relativism.

Justificationist theories of rationality lead to the view that all sociological studies of rational practice are relativist.

ACCORDING TO ESTABLISHED justificationist theories, standards of rationality are standards of last resort; they are presumed to be the supreme courts of appraisal, which cannot themselves be called into question. Indeed, what standard could we use to appraise these (ultimate) standards? We either have a dogmatic assertion of some other standard than those now being used, or we have an infinite regress, in which each new standard is questioned and calls for a further standard by which we may justify it, or we have a circle, in which any proposed standard is used to justify itself.[255] It appears, then, that any attempt to develop a sociological theory of rationality within the framework of the justificationist theory is doomed to failure. When failing to show how any theory is justified, it saws off the branch on which it sits, by viewing social standards of rationality not as universal—they cannot be, because there is no universally applicable justification for any of them—but as merely local phenomena. Under these conditions the appraisal of rational practice can only be a local assertion about some narrow standard used in some circle: each standard of rationality merely reflects a peculiar culture or a special interest.

For this reason any attempt to develop sociology of rationality such as Karl Mannheim's sociology of knowledge or the Edinburgh School's strong program in the sociology of science has met with stiff resistance.[256] They were attractive to some, because they seemed to provide a deeper understanding of intellectual battles which are erroneously presented as quests for truth. From a deeper perspective provided by the sociology of rational practice, such battles can arguably be seen to be something quite different. They can be revealed as, say, battles for prestige, political quests for power, or cultural conflicts, which have little or nothing to do with truth. But, in the end, these sociological views are disappointing. They destroy the common institutional and intellectual ground, which is the basis for the unity of science and of mankind. They are destructive of the hope for mutual understanding and cooperation between individuals and cultures, which most social scientists think only science and rationality offer.

This unhappy consequence is a direct result of the traditional and established equation of rationality with justification. If all and only justified views are taken to be rational, there can be no (rational) variances between either the standards for rationality or those views which are held rationally. All those views which are justified by some proper standard are rational, and all those views which are not so justified cannot be held rationally. Any attempt to describe varying standards of rationality and appraise them can, then, only be descriptions of variances from the proper standard of rationality. Such studies cannot tell us anything about divergence from properly established standards of rationality since these cannot be

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[255] Hans Albert calls this the "Münchausen Trilemma" and uses it as the cornerstone of his philosophy (ALBERT 1985, pp. i6ff.).

[256] For a survey of alternatives in the sociology of science including Agassi's, see Wettersten 1993.

identified. Such an attempt can, then, only explain how decisions are made on the basis of social standards without application of any viable rational standards.

Thus, for example, when Tversky and Kahneman conducted their empirical studies of how individuals solve some specific problems, they did not study how good or poor particular standards of rationality were. They were only interested in the question whether or not individuals as a matter of fact thought rationally and, if they did not, whether there were systematic (psychological) divergencies from known standards of rationality. This program does not allow for any divergencies in rational standards: they are either rational or not. Divergencies cannot be better or worse, but are appraised with an all-or-nothing standard.

This specific research program has been developed further by Richard Nisbett and Lee Ross. These thinkers pose their problem as that of investigating how well or poorly individuals in a wide variety of situations follow agreed upon rules of scientific method. That which is allegedly agreed upon is deemed proper, with an observation that some small changes might occur. In order to carry out this program it is, of course, necessary to specify just what these agreed upon rules are. The authors presume that an inductivist theory is true, as it is allegedly established by consensus. (In fact, it is true that a vast majority of social scientists do hold to this view; but this, by itself, is no basis for assuming that it is true.) According to this theory, facts must be correctly perceived and properly chosen for tasks at hand. The authors further erroneously presume that this is, from a methodological point of view, by and large possible, if one is not led astray by psychological factors. They seek, then, to identify just what those factors are which needlessly lead to mistakes. Observed mistakes are, then, generally presumed to be failures to correctly apply the proper and viable inductivist method (Nisbett and Ross, 1980).

Popper has, of course, over many years argued quite impressively that this view is false. But he is only secondarily and very briefly mentioned as someone who discusses tests. His view is presented, then, quite erroneously as a mere addition to, and complementary aspect of, the inductivist consensus. Hardly any mention is made of the fact that Popper's central point was, that the method, which is presumed to be consensus and thereby correct by Nisbett and Ross, was not and could not be used. This traditional approach was vehemently rejected by Popper virtually throughout his career. Popper did, indeed, show that the inductivist approach presumed by Nisbett and Ross not only was by no means effectively applied in scientific research, but also that it could not be successful *From a Logical Point of View*. The errors, which the authors explain from a psychological point of view, cannot be avoided with any scientific method at all. The basis for the development of Popper's view was the thought psychology of the Würzburg school, above all that of Otto Selz; the Würzburg school rejected Wundtian associationist psychology, and with it the inductivist methodology, which had led to this erroneous psychology. They did not, however, as Popper later did, reject the theory that scientific theories were somehow justified.

In a further study of how individuals think, John H. Holland, Keith J. Holyoak, Richard E. Nisbett and Paul R. Thagard discuss "deductive reasoning" (Holland, Holyoak, Nisbett, and Thagard, 1993, pp. 23-41). The task they set themselves is to empirically examine how and/or to what degree individuals use deductive reasoning. Traditionally, thinkers who have emphasized that thought is deductive have rejected induction both as a viable theory of proof and as a method which has been, and can be, used to guide thought processes. But this approach is not taken here. Rather, with various examples, the authors seek to show that deductive reasoning, as found in the application of deductive logic, is not the method that individuals normally use to solve problems or reach conclusions. Rather, it is some version of inductive reasoning, such as the inference from many repetitive cases to some general statement. The explanation for this lack of the use of deductive reasoning is the failure to use inductive inferences to infer and discover deductivist rules.

This is a very curious treatment of thought processes. All adults have learned how to think. They have done so not merely through practice, but also through learning from others. Sometimes this instruction is quite definite and sometimes it merely arises by imitation of the thought techniques of others. The bulk of any knowledge used in thought processes is not achieved by individuals learning afresh; and it is by no means achieved through induction. Induction cannot yield proof and it can only be applied when assumptions are made which renders it deductivist. We do not simply observe one swan after another, until we come to the arguably justified conclusion that all swans are white. We see a white swan, presume that all swans have the same characteristics, and then deduce that if one swan is white the others will also be white; we adhere to this assumption until a black swan appears on the scene. In everyday occurrences these, in principle deductive approaches, are neither spelled out nor thought through in ways which would satisfy a logician. But that does not change the fact that they are based on the application—good or bad—of what is taken for previous knowledge. The arbitrary assumption, that both science and rationality are inductivist, does not change this fact, nor does it give any reason for preceding to search for inductive thought processes and especially not for the inductive justification of deductive thought processes.

Another attempt to explain whether or not humans are rational has been undertaken by Edward Stein (Stein 1996). In order to decide, whether humans are rational or not one must first, of course, explain what humans would do, if they were rational. One may then ask whether they in fact do that. But the question of what rational human beings do, or would do, is a philosophical problem with competing theories. Stein seeks to avoid this conundrum by swimming with the tide: Whatever a majority of thinkers say is rational behavior is rational behavior. So, he simply puts forth his problem as one of determining whether humans follow the rules of logic and use those techniques of finding truth that the establishment endorses. Now, it is clear that no individual always follows the high rules in such a clear-cut way, that this individual could, without further ado, be deemed rational. So, Stein proceeds with various ways of testing the actions of individuals to see if they follow the rules closely enough to be deemed rational. Not surprisingly, he decides he does not know. Indeed, in order to decide, he would have to say just what degree of following a set of established rules is good enough to be deemed rational. And he has no theory which answers this question.

Instead of posing the essentialist problem as Stein does, it seems far more sensible to first say a few obvious things. We do not agree on just what procedures rational individuals follow. We all follow, even those rules we endorse, to some degree, but by no means with perfection. We can all encourage a better discussion of just which rules are good or bad, and avoid the slavish acceptance of whatever happens to dominate the discussion today. And we can all improve our degree of rationality to some unknown degree by posing the questions concerning which rules we should follow and how we can realistically improve our ability to do so.[257]

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[257] Critics of earlier versions of this essay have objected to the fact that I do not, from their point of view, sufficiently discuss and appreciate various alternatives to the fallibilist approach discussed here. Some of these alternatives, such as Feyerabend's *Against Method* approach, are not in my opinion worth a more detailed discussion. Others, such as Fuller's social epistemology, with the influence of Kuhn's methodology, or John Searle's analysis of concepts, or Tuomela's discussion of “sociality” or Manicas's “realist philosophy of social science”, I have

A new framework for the study of rationality: rationality vs. theories of rationality.

Fallibilist theories of rationality have introduced radical changes in our understanding of how individuals can and do pursue truth. These views no longer simply oppose rationality to irrationality, but rather they view rationality as partial. This approach opens up the possibility of studying how the degree of rationality practiced by individuals in any institutional context can be appraised. It may, thereby, offer the possibility of identifying those standards which should be retained, while offering good reasons for doing so, as well as the possibility of improving poorer ones with specific proposals.

The modern criticism of the efficacy of the traditional view of rationality is not merely a repetition of a traditional philosophical debate between skeptics and non-skeptics. Skeptics accept the justificationist standard of rationality but regard it as unattainable, just as Hume accepted induction as the proper method for doing science even though he thought it could not lead to proof. According to the new fallibilist views, we have alternative approaches to rationality, which do not merely question the efficacy of traditional methods of justification; these have regularly been questioned. The new approach goes beyond such skepticism and offers new alternative methods, which are better at getting closer to the truth than the use of traditional justificationist views.

If we admit, on the one hand, that there are alternative theories of how the truth may best be sought and, on the other hand, that the application of these various theories lead to results which are better or worse, but neither perfect nor useless, we have a problem of deciding which among these alternatives are the best, and perhaps which is the best under which circumstances. We can study these questions, if we agree that that theory, which solves problems and withstands criticism the best, should be tentatively regarded as the best. Fallibilists have no problem using these modest standards; they will not ask which theory is best justified, because they regard all pursuit of justification as futile. But justificationists may also, and often do, use modest standards to ask which theory solves

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discussed elsewhere. Various approaches to social scientific research, influenced by various philosophical frameworks, are worth discussing and a pluralist approach in this regard is of considerable value, not merely for philosophical background but also for empirical research (Wettersten 2007). However they cannot all be dealt with in one fell swoop.

problems and withstands criticism the best. They normally view good results in this regard as at least progress toward some justification which may follow. Many justificationist scientists, for example, follow Popper and advocate the use of the standard of falsifiability in both natural and social sciences even when, quite contrary to Popper, they regard tests which are passed as movement toward justification. There may, then, be some common ground between justificationists and non-justificationists in regard to the search for the best methods for the pursuit of truth. This common ground would consist in the quest for that theory which best explains how truth may be pursued, and which best withstands criticisms of its proposals now. There remains, of course, the serious disagreement whether the quest for better theories of rationality is the end which we pursue, or whether this quest is only a step on the way towards the justification of the true theory of how to be rational by justifying theories. Perhaps we may put this question aside for the time being and seek modest progress where we can achieve it.

Sociologies of knowledge and/or science, developed within the framework of justificationist theories, end up substituting mere social standards, which can be found, for standards of logical justification, which cannot be found. This is a consequence of setting unattainable standards for the necessary conditions for rationality; they see the hallmark of rationality as lying in the ability to identify true theories. This means that social standards of rationality must be those standards which correctly identify true theories. But, we have no such standards. This inevitably leads to the substitution of unattainable absolute standards of justification with established social standards of justification; this is to fall back into some kind of relativism. Justificationists may argue that, even though no universal standards can now be found or presumed to exist as a regulatory idea, justification within some context is good enough.[258] But then we need to describe why some theory is good enough as justification. We have no such theory. This consequence of some sociological studies of rationality may be avoided if a fallibilist framework instead of a justificationist one is used. Within a fallibilist framework one does not ask which theories are justified—either quite universally as true or locally by

[258] This tendency is found in the methodology of sociology developed by Anselm Strauss, Barney Glaser and Juliet Corbin. They seek to explain how "qualitative research" is "grounded" in the process of its development, without providing any theory of induction or justification which can solve traditional problems (Strauss and Corbin, 1990).

some social standard[259]—but rather, which theories solve problems and stand up to criticism best. This approach to the sociological study of rationality is an offshoot from, but certainly not a part of, Popper's fallibilist theory of rationality.

Methodological individualism has blocked the study of social practices of rationality.

Building on Popper's view of science as social, Agassi has developed a fallibilist sociology of science (Agassi 1987; Wettersten 1993). I have followed him in this (Wettersten 2010) and Jarvie has also rather recently endorsed it (Jarvie 2001; Wettersten 2006b). But neither Agassi nor Jarvie has posed the question of what changes this innovation calls for in the methodology of the social sciences. In this regard they have adhered to Popper's endorsement of the methods of economics, that is, of methodological individualism.

One ignored consequence of Popper's theory that all rational practice—not merely scientific method—is social, is that the door for positive social studies of rationality is opened; such studies do need, as has hitherto been the case, to study above all breakdowns of methods of justification. These new sociological studies of rational practice correspond to the more special studies in the sociology of science. In order to carry out this program for the social study of rational practice, one has to presume that the style and methods of rational practices differ from one social context to another. But all of these practices are partial and subject to improvement. Looking at rational practice from this point of view is blocked, however, as long as one adheres to methodological individualism: this view presumes that rational action is based on rational planning and this rational planning is identical for all individuals under all conditions. Under Popper's influence this theory has become the standard fallibilist view of methods in the social sciences.

Methodological individualists do not offer, and see no need for, any program for studying how individuals think: The way individuals think is taken as given. Popper calls this assumption the "rationality principle". Along with many economists he presumes that individuals pursue their

[259] We may of course use local social standards when they are clearly specified as such and are not designed to take the place of universal standards, as in the law or in publication standards or in rules for debate. We may also seek to improve such standards using the program here proposed. We may study, for example, which standards for publication best encourage a high level of critical discussion.

own aims in the logic of the situation (as they perceive it) with plans they have devised to achieve their goals. Even when this assumption does not accurately describe individual thought processes, says Popper (along with Milton Friedman), it should be adhered to as a framework for social scientific research, because without it we cannot construct social scientific explanations (Popper 1985).

This method is supposed to achieve understanding not merely of why particular individuals have acted the way they have but above all of social events. Popper takes over his approach from economics and calls the social events to be explained "typical social situations" (Popper 1985, pp. 358ff.). His uncritical attitude toward economics is curious, since contemporary economic methods are riddled with inductivism, as Lawrence Boland has so nicely shown (Boland 1986; 1989). In economics "typical situations" are for the most part markets and individuals that are presumed to pursue their own economic ends in the logic of the situation. This was the model he took over from J. Marschak in *The Poverty of Historicism* (Popper 1964, p. 141; Marschak 1943). Since both the logic of the situation and the rationality of individuals are defined by economists in narrow ways, one can analyze what happens when all individuals in the given (idealized) situation act rationally. Whether such thought experiments apply to the real world is another matter. No one claims that they always do, but economists—the paradigm example is Friedman's famous essay on "positive" economics—do claim that trained economists can judge in which instances particular models correspond near enough to the real world to be useful. This claim appears rather daring in the light of current economic problems, in which economists were largely incapable of sufficiently understanding economic developments in order to make accurate predictions about what should come next.

But, what about the social scientific study of institutions? Methodological individualists treat institutions as, above all, consequences of rational behavior. They are built up and/or maintained because they serve the interests of individuals, as thinkers such as Anthony Downs, (1957), James M. Buchanan and Gordon Tullock (1962), and Mancur Olson, (1965) have maintained. But some have observed that this is not all they do. They also have unintended effects which thwart the aims of individuals. Thus Douglass North, for example, points out how particular institutions, such as the way land was owned by the monarchy in Spain in the 6th century, led to a reinforcement of the very institution which prevented the development of better ways of satisfying the needs and wishes of individuals. As northern European countries moved economically forward, southern

European ones declined. This was not due, according to North, to the fact that institutions were not preserved by individuals pursuing their own aims, as the standard theory asserts they are. But, contrary to expectations, the preservation of the institutions as a means of furthering some individual aims was detrimental to those very individuals who were reinforcing them. The institutions, which were detrimental to the well-being of all, were reinforced by the actions of individuals pursuing their own best interests in the logic of the situation they found themselves in (North and Thomas, 1973, pp. 2off.; North, 1981 pp. 24ff.; 1990 pp. 92ff.). In some institutional settings, then, rationally pursuing ones own's aims in the narrow sense described above can, in the long run, be detrimental to all.

North's interesting theory was developed in the context of the standard theory of rationality. As a consequence it has a relatively narrow scope (Wettersten 2006a, pp. 91-94). North recognizes this when he calls for further study of ideologies, which he hopes can be carried out within the confines of the standard theory. But this study views ideologies as products of individuals who are maximizers. Why indeed is anything broader needed? Why is it not possible to adequately understand the good and harm that institutions do within the framework developed by Buchanan, Tullock, North and others? This is due to the fact that institutions lead people to act in particular ways quite regard-less of their pursuit of personal interest. Weber dealt with this problem by deeming the choice of social framework, above all of religion, to be arational: It is due to the acceptance of the message of some charismatic personality. This theory was a consequence of Weber's limited theory of rationality. He thought that science sought and found the truth. But he knew, as he explained in his essay on scientific careers, that there was no proof of this hypothesis. And he further knew that social scientists had to explain the social consequences of non-provable social frameworks, above all, those of religions. But this view pushes him backward in the direction of functionalism: Each society functions in accord with some social framework, most often, religion. The consequences of following such a religion, such as the capitalist consequences of Calvinism, can then be explained.

Weber's methodological approach in regard to this point still has its modern-day followers. Samuel P. Huntington's view of religious clashes is such an example (Huntington 1998). And, Lawrence E. Harrison and he have offered a collection of essays which should show the need for, and the strength of, a Weberian approach (Harrison and Huntington 2000). But this approach is quite limited and does not fit with modern theories of rationality (Wettersten 2006a, 15ff, i 64ff.).

The same can be said for the historical-philosophical-sociological research of J.L. Talmon. Like Weber he views all choices of frame-works as beyond rationality. Rationality itself is a mere secular religion. This leads to a breakdown of any attempt to use rationality to help believers in differing faiths to live together in tolerance (Talmon 1952; 1961; 1963; 1970; 1980; 1987; Wettersten 2012). These views cannot, as Weber intended, be easily combined with methodological individualist theories. The resilience of institutions such as religions, for example, cannot be explained as a consequence of individuals pursuing their personal well-being by guiding their actions in accord with the rationality principle.[260] At times individuals accept and defend religious principles, even at the cost of their own lives. Such instances are not simply aberrations but the norm under some conditions. And their influence extends to all areas of life, even economic ones. If our understanding of societies is to be improved, this phenomenon of the commitment of individuals has to be understood, not as an aberration of rationality but as part of the way in which individuals are rational.[261]

Some theorists such as Stark and Bainbridge have tried to explain religions as mere products of individual choice (Stark and Bainbridge 1987). This would avoid Weber's use of the hypothesis of the arationality of the choice of religion and contribute to the reduction of all social scientific research to the use of well-established methodological individualism. But it fails to overcome difficulties facing methodological individualism, as the one and only methodological approach to social scientific research. The authors begin their book with explanations of methodological assumptions, which are to be used to develop their theory. This starts out, rather surprisingly, in a Popperian way, that is, it expresses their aim to

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[260] In a recent study Jeremy Ginges and Scott Atran show how Palestinians follow the social norms of their cultures and/or religions, even at the cost of their own interests. But they still presume that such action is not rational because it is not in accord with rational choice theory (ATRAN and GINGES 2009; GINGES and ATRAN 2009).

[261] With colleagues North has recently proposed a quite new approach, which begins more closely to resemble Popper's (North, Wallis and Weingast, 2009). He does not say whether he wishes to adhere to his traditional methodological individualism nor whether he has given up on the hope that cognitive psychology could be used to significantly improve our understanding of economic performance over time.

formulate testable theories and, then, to hope that the proposed hypotheses will, indeed, be subject to empirical tests.

But the authors do not want to merely begin with a conjecture. Rather they propose a method for formulating a theory which may not be perfect, but which comes close to a scientific ideal. In order to do this they seek to formulate their theory in a clear-cut deductivist way, one which takes mathematical formulations of scientific theories as an ideal. In order to do this they begin, really after the style of Descartes, to provide definitions of the concepts which will be used in the formulation of a set of axioms, from which conclusions about religion will be deduced and which can then be tested against sociological observations.

The definitions selected cannot, of course, be arbitrary: they define just which kinds of social objects should be explained. The authors claim that they start with assumptions which humans can easily accept, though they see a need to refine these assumptions in order to develop a good social scientific theory. In fact, the assumptions they use are by and large taken over from the methodological individualist version of economic theory, that is, individual action is to be explained as an attempt to maximize rewards and minimize undesired consequences. Now, in economic theory such rewards and undesired consequences are above all taken to be economic ones. Economists do not necessarily hold that this is, in fact, always the case but, because it is such a dominant force, it is quite appropriate to assume that it is. However, this approach has to be revised if methodological individualism is to be applied to religion: Individuals do not believe in some religious doctrine, or join some religious organization, above all for financial gain, even if it can play a role, as de Tocqueville noted in the U.S. The transition from the purely economic use of methodological individualism to its application to religion is carried out by defining a particular kind of "payment" made by religions. This religious "payment" is defined as a "compensator". A "compensator" is some promise of reward invented by religions. (The authors insist that they make no assumption about whether the statements made by religions are true or false.) lt can be, for example, the promise to believers that they, and perhaps only they, will enjoy an afterlife in heaven. Such a "compensator" provides no reward here and now. Religions then are those institutions which offer "compensators" and individual members of religions are individuals who weigh the costs and benefits of acquiring compensators and decide that the benefits outweigh the costs.

This methodological individualist definition of religion determines just what social entities the theory which follows should explain. That is, it does not explain any entities that some may view as religious but which do not offer "compensators" and whose social qualities cannot be explained as the consequences of individuals seeking to maximize their rewards. An institution not reducible to one which awards compensators and whose members do not guide their relations to this institution by attempts to maximize received values is not a religion and not a subject of the current theory. The authors note from the beginning that some readers have first rejected their definition and then criticized their results. This kind of response is not allowed.

Unfortunately the determination of the degree to which individuals guide their religious behavior by the quest for personal goods (or "compensators") is just what is at stake. The fact that some religious behavior can be interpreted with the use of the extended assumptions of methodological individualist assumptions by no means shows that this explanation is true. (It could under specific conditions be quite false and quite useful if it made some true predictions, and just where it did that was known. But that is not what the authors seek.) So, the authors stress that they seek to offer a testable—really a refutable—theory, but they formulate their theory and its application in such a way that no test of its general assumptions is possible.

One question, then, is: do the authors provide enlightening (conjectural) explanations of aspects of the formation and maintenance of religions? They only do this if one accepts their definition of religion as a supernatural supplier of valued goods. But, this does not answer questions about other possible aims. It also by no means deals with the question of when religious individuals as a matter of fact follow the methods the authors presume they do. Theorists such as Atron and Ginges, who have discussed these matters directly with religious individuals, have found out they do no such thing. If they are right, the best such a theory as this could offer would be to make true predictions in some cases but by no means explain what was really going on. One further question is: Do these explanations provide any grounds for the methodological hypothesis that all social aspects of religions can be explained with methodological individualist explanations? It does not do this simply because it begins the exercise by excluding, by definition, any aspect of some putative religion which does not conform to their methodological individualist standards.

In order to study the consequences of the use of different standards of rationality, methodological individualism needs to be revised.

Methodological individualism places undesirable limitations on the tasks set by the social sciences. These limitations block the social study of rational practice. They can, however, be overcome. There are at least four ways in which improvement can be achieved. Whereas methodological individualism tries either to move in small steps toward comprehensiveness (North 1996) or produce models whose application in any specific instance may only be judged by intuition (Friedman 1953), a modification of methodological individualism can produce partial theories, which can be tested now. Secondly, a modification also allows one to study how rules affect thought processes rather than merely presuming that all individuals think in the same way. Thirdly, methodological individualism unreasonably presumes that social scientists can know, or can reasonably assume, how individuals perceive their situations, whereas a modification can make the nature of their perceptions of their situations subject to empirical inquiry. And, fourthly, methodological individualism must assume fully rational behavior whereas a modification can explain even some behavior, which seems irrational on the traditional view because it is partially rational.

The first way to improve research is to avoid the blockage caused by methodological individualism, which results from the pursuit of comprehensive theories or limited models whose application in specific contexts is judged by intuition. In contrast to these approaches one can take problems of social scientific explanation to be problems of explaining how aspects of existing institutions produce the results they do, and/or how reformed institutions might produce different, hopefully more desirable, results. One may then study aspects of social structures as given, in order to develop specific theories of the consequences of the partial maintenance and/or change of institutions. Changes may then be planned, but not in the sense of holistic social planning, so devastatingly criticized by Popper and economists, but in terms of piecemeal reform, which is advocated by Popper and many economists. This can be done when one studies how institutions lead individuals to pose problems and solutions as well as how institutions encourage or hinder critical appraisal of them.

Methodological individualism does not take into account differences in problem-solving approaches,[262] which thereby serves to block social

[262] I have argued that although Popper and Amartya Sen have remarkably similar points of view, they are both blocked by their adherence to methodological

scientific research in a second way. Problem-solving differences exist and are to a high degree dependent on the social contexts in which they occur. Whether one is critical or dogmatic, and under which circumstances and how, depends on social rules and on having learned how to operate properly in given social contexts. In order to understand how institutions lead to certain results, it can be useful to understand these differences. Social rules which lead individuals to solve problems in specific, in good or poor ways, may be identified and evaluated.

The methodological individualist approach to the study of institutions blocks research in a third way because it is too weak; it normally postulates the importance of economic aims while ignoring others. This is not a consequence of the theory itself, which merely states that individuals pursue their aims in accordance with their beliefs. The use of the traditional rationality principle enables one to take all factors into account that any individual employs when building some model of his situation. One may take into account his religious beliefs, his moral compunction, his commitment to friends and family, etc, in order to understand his actions. But there is no sensible way of building a model of social situations, when such a broad range of factors are taken into account. Each individual's situation turns out to be different from that of his neighbor's, much less from those farther from him in belief and social situation. For this reason the use of the rationality principle leads quite directly to the application of this principle in terms of maximizers, that is, of those who maximize their (economic) utility while minimizing their cost and risk. But this leads in turn to blocking out real factors in any social situation. Pure economic situations hardly exist and are not stable since various factors can always impinge in situations which seem, at first blush, to fit some model. On a large scale it is hardly possible to accurately say which goals individuals are pursuing. It is impossible to know, on any large scale, how individuals perceive the logic of their situation and whether this perception is approximately correct. Aims are simply postulated in accordance with the narrow theory of rationality employed in economic research and advocated by Popper and his followers. Just as the methods which individuals use to solve problems are to a large degree a product of their social context, so are the goals they pursue and their perception of the logic of their situation.

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individualism and their failure to take this adequately into account in their research programs (Wettersten 2009).

Methodological individualism presumes that all individuals think alike, and thereby blocks in a fourth way empirical research into the differences that exist between problem-solving approaches. If we do not start, however, with presumptions about how individuals always think, but rather with institutions whose rules are relatively stable but which vary from context to context, we may presume that individuals are rational in the sense of trying to solve problems in institutional contexts, that is, in accordance with those rules which these contexts impose on them with more or less rigidity. We may then gain partial understanding of institutions and how they operate, while taking into account various economic, but also non-economic, factors. We may do this without exaggerating the degree of rationality, which is presumed in our models, as the use of the traditional principle of rationality forces us to do—as has been described above. Those who defend causes at great risk to their own personal welfare can be highly rational in that they have critically studied alternatives both in terms of their own values and their situation. Individuals may also be partially critical or follow plans at some times, but not at others. They may use methods of appraisal which are good or bad. None of these variations are signs of simple irrationality; all are rational to a degree. And the fact that they are rational to a degree makes them understandable to a degree. No one is in fact perfectly rational. We therefore have to settle for explanations of partial rationality and results to which the exercise of partial rationality leads. This is possible when we modify the rationality principle as suggested in this essay.

The theory of rationality vs. the theory of rational action: How can the use of differing theories of rational action be studied?

Instead of starting, as methodological individualists must do, with constructions of "typical situations" and asking how any rational person would act in those situations, one begins with institutions, that is to say, with the rules which institutions impose on individuals as they pose problems, create solutions and critically evaluate them. Instead of asking how rational individuals respond to the logic of their situation, one asks how institutional rules determine how individuals pose problems, construct solutions and critically appraise them. We may form, then, theories about these rules, even when they are not used by individuals to achieve their own (private) goals in the logic of their situation. Individuals may use such rules to seek to preserve their societies and/or to improve them even without regard to their private welfare.

The above proposal for a new theory of rationality establishes the tie between institutions and individuals, which is lost when, using the established theory, anything broader than problems of simple economic advantage are considered, that is, in most cases. It does this by inquiring how as a matter of fact institutional rules affect the formulation of problems and solutions. One may look for regularities among individuals with differing aims and problems, including social ones, rather than merely individual, economic aims, as they pose their problems, construct solutions and evaluate them. The connection is not between a typical situation and a single rational response. Rather the connection is between the rules of institutions as they effect individuals in various situations. Rules may, for example, be very useful to individuals posing problems of one sort and very damaging for those who pose problems of a different sort. Rules may, of course, hinder those who want to start a business, but help those who want to find security for their families. Which rules will be supported by citizens will depend on which problems individuals choose and whether they understand which rules will help them and which rules will hinder them in solving those problems.

If we presume that rationality is social and critical, we may reformulate the tasks of social scientists. Instead of investigating the consequences of rational actions given the logic of the situation, social scientists may investigate the impact of institutional rules on the actions of individuals and the consequences that thereby ensue. Since these institutional rules incorporate various views of and/or standards for rationality, there is no reason to exclude them from, and every reason to include them in, social scientific studies of how these standards influence individuals and, as a consequence, societies. The methods for the study of ordinary rules of behavior and that of rationality are only different insofar as the study of the latter is of the most general, thereby perhaps the most important, standards which guide individuals in their choice of problems, solutions and methods of criticism. Dahrendorf's contrast between the rational "homo ökonomicus" and the role playing "homo soziolgicus" is overcome, (Dahrendorf 1959) because all rational thought and action is shaped to a high degree by social rules.[263] The method proposed here is

[263] The social study of standards of rationality is not an entirely new development. Ernest Gellner, for example, in his path-breaking book *Legitimation of Belief* described in quite general terms those standards which are used in Western society and which are being taken over in virtually all societies today (Gellner 5974). He also discussed the con-sequences of the use of these standards. His study still remains, however, too much under the influence of traditional functionalist views

always piecemeal. It seeks to describe the effects which particular institutions have on particular aspects of society. It also seeks to find out what happens, when institutions influence the same aspects of societies in different ways. If general theories are possible, they describe the way institutions do this, but they can only be applied to particular societies in light of which complementary and/or competing institutions are present in specific cases (Wettersten 2010).

The explanation of institutions as consequences of rational action vs. the study of how institutions lead individuals to think in various ways.

It might appear that the proposal offered here is hardly new. According to this proposal a prime task of the social sciences is to first study how institutions lead people to pose problems, to select solutions and to critically appraise them and, second, to ask what the social consequences of these thought processes are. On the one hand one may ask: how is this different from the proposal developed by Popper, Buchanan and Tullock, Downs, and North according to which one explains institutions on the basis of how individuals use them to further their own aims? Both views emphasize that individuals pose their problems in view of the logic of the situation, that is, in terms of their institutional contexts. On the other hand, one may observe, social scientists have in fact carried out such inquires as those proposed here by asking, say, how individuals view their situations and what consequences result.

The view developed here sets the task of explaining how institutions affect the way individuals pose problems, choose solutions, and critically evaluate them. This task is quite thoroughly ignored when one presumes that all individuals think alike. A second task is to investigate consequences which follow from these decisions based on adherence to specific sets of rules of thinking. We should not then have partial non-testable "explanations" of specific institutional developments, but testable explanations of how

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in anthropology. He has, to be sure, not only rejected but effectively criticized this view, but did not break free of it enough in this study of rationality. He does not see the use of the standards he described as constituting a coherent whole. But he does see them as determining the whole, without a theory of how the competition between various standards might play out in societies and how standards of rationality might be improved to overcome any unfortunate results. 1 made this point previously (Wettersten 1979) and Gellner responded to it (Gellner 1996). He claims that I have attributed views to him which he does not hold, but I find his response in-adequate because he does not explain how the degree of rationality practiced by individuals may be improved (Wettersten 2006a, pp. 154ff).

specific institutions steer events through the influences they have on how people think. This information should enable one to make some predictions about future events. The crucial difference between these predictions and those made by models hitherto developed in the framework of methodological individualism is that the institutional contexts in which they apply can be specified. Theories about specific institutions may be refuted and replaced with better ones. There is to be sure still no guarantee that these predictions will be true. But they may enable one to prevent some unhappy events from occurring, and they may enable one to steer events in particular desirable directions. If this were to be the case, the lower ambitions of this program would lead to more powerful results.

A criticism of my claim that this approach is methodologically superior to that which now dominates much of social science in the name of methodological individualism and/or rational choice theory is the following. My claim that this method leads to testable theories and the ability to make responsible prognoses far better than alternatives is belied by the fact that I also have to use a ceteris paribus clause. For, any institutional practice may have various results in various contexts. And these contexts can never be completely spelled out. Indeed, when some rule is seen to lead to some result, which some individuals do not want, those individuals may very well succeed in devising new strategies to avoid it. The difference, however, lies not only in the increased capacity to specify those institutional contexts in which a theory should apply, it also opens the way to discovering how and why it applies in some contexts with specifiable features, but not in other contexts with differing specifiable features. We may form testable theories about which factors need to be taken as constant.

The claim that such inquiries have already been carried out is rendered weaker than it might appear at first blush. Social scientists who leave the narrow bounds set by the economists' use of methodological individualism regularly presume they are also leaving aside the task of explaining events as a consequence of the rationality of actors. If individuals do not adhere to the rules of rational-choice theory they are mistakenly presumed to be acting arationally or even irrationally. This blocks understanding of their behavior as partially rational. Secondly, a new proposal for an alternative theoretical framework for the conduct of social scientific research need not call for the change of all social scientific research to be valuable. It is sufficient if it describes good research, explains problems with other research, and sets new tasks as, I claim, this proposal does.[264]

Social studies of the practice of rationality today.

Fallibilist theories of rationality have opened the path to a non-relativist, critical sociology of science and rationality. We can conduct sociological studies to better understand the impact of social rules of the practice of rationality in both everyday life and in intellectual endeavors. This possibility has been opened up because of the discovery that rationality is by no means an all-or-nothing affair as all previous (non-fallibilist) theories have assumed. These theories lead to the view that, if justification is not possible, we must make do with relativism and/or we must accept the substitution of power for reason in the fixation of belief. The fear of this result was movingly expressed by Bertrand Russell as he despaired about the failure to find a solution to the problem of induction (Wettersten 1985); it seems to be the result of the sociology of knowledge or strong programs in the sociology of science (Wettersten 1983; 1993).

But this fear and this result can be avoided with the realization that degrees of rationality can be achieved, maintained, and even improved, if we note what is possible with criticism in the absence of justification. Still further, and the main point of this essay, is that this possibility can best be taken advantage of by noting that all rationality is not merely critical and a matter of degree, but also social. The tasks of maintaining and raising degrees of rationality are not primarily problems of individuals doing better, working harder, or concentrating more intensely; they are institutional. They are problems of improving the quality of institutions, that is, their ability to encourage criticism of a high level.[265]

[264] For a discussion of this use of philosophical anthropology, see Wettersten 2007.
[265] In the case of the use of the traditional rationality principle in the social sciences we may see that it leads to an isolation of disciplines — economics, political science, social and moral theory — from one another and that it leads us farther and farther away from the construction of realistic models. It does this by forcing social scientists to accept exaggerated assumptions about the degree of rationality individuals practice and by the need to reduce the factors which are built into models in order to construct "typical cases" which in practice turn out not to be typical at all.

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Interlude

Reforming the social sciences, in light of the social character of the practice of rationality, is quite simple from a theoretical point of view: One needs merely to revise the rationality principle so that it emphasizes the social aspect of the practice of rationality.

3c. 'How to Integrate Economic, Social and Political Theory: Revise the Rationality Principle'. In *The Impact of Critical Rationalism*, Eds. Raphael Sassower and Nathaniel Laor (Cham: Palgtrave Macmillan, 2019) 81-94.

How to Integrate Economic Theory with Social and Political Theory: Revise the Rationality Principle

Abstract: Economic research is isolated from social and political deliberations and both are isolated from normative social inquires. Politicians put the distinct results together in ad hoc ways. This state of affairs is explained as a result of the equation of rationality with coherency and justification; the rationality principle according to which social facts are to be explained as the result of coherent plans of individuals is singled out as the prime source of the division. A revised version is proposed which may remove theoretical barriers between economics, social and political theory, and normative theories.

Introduction.

Economic theory today is isolated from both political thought and normative social theories. In order to overcome this isolation an analysis of why it has come about is needed. These disciplines have a common ground in the theory of rationality. Since it is disunity rather than unity which is to be explained, this common ground may seem a curious place to look for understanding. But the pressures towards isolation found in various fields have the same source in demands for coherence and justification as conditions for rational thought and action. By loosening standards for rational thought and action, the rationale for separation of economics from other social sciences and from normative social theories may be overcome. No easy path to integrated solutions to social problems is thereby attained. But a research program for the partial integration of economics with other disciplines emerges. Under reduced assumptions about conditions which rational thought and action must meet, the partial rationality of individuals may not only be understood. It may also be used both to explain social events and to provide proposals for social reform The specific proposal for the revision here proposed, that is, that the rationality of individuals be explained as the attempt to solve problems within given institutional frameworks, has been developed elsewhere. (Wettersten 2006) According to this principle institutions set the kind of problems individuals seek to solve and the desiderata they use to appraise solutions. Instead explaining how institutions are produced by the rational actions of individuals, institutions are taken as given and their impact on how individuals pose and solve problems is studied. The purpose of this essay is to apply this view to, on the hand, the explanation of the separation of economics from social and political theory and from normative social theory and to, on the other hand, the task of overcoming these divisions.

1. The Separation of Economic from Social and Political Theory and from Normative Social Theory.

The separations of economic from social and political theory and from normative social theory are due to, on the one hand, attempts to make economics as well as social and political theory scientific, and, on the other hand, attempts to find justified moral and political propositions. Those interested in social planning hope that scientific economic judgements may be coherently combined with justified moral ones. But attempts to combine a scientific social science with justified normative social theory lead to ad hoc political decisions. And these may appear corrupt because they are not based on universally applied standards. Interests seem to take the place of agreed upon standards. This occurs when economists, social and political theorists and normative essayists seek within their disciplines to meet high demands.

1.1 The Separation of Economic from Social and Political Theory and from Normative Social Theory: Economics.

In their quest to make economics as scientific as possible, economists have sought to meet especially high standards which cannot be matched by other social sciences. In order to do that, they isolate their own discipline. Their strategy is determined by their aim of finding economic laws. Economic laws, they plausibly presume, describe causal relations between differing economic factors. An analysis of how this occurs can only be possible if economic systems function in law-like ways. It must be the case, for example, that increase of supply will lead to decrease in demand in economic systems. The ideal was formulated by Leon Walrás, though even he did not think it was possible to use this approach to describe the real world. There are simply too many variables in the real world to include in one system. So, economists presume that there is an ideal coherent market system. This ideal can be used to describe aspects of real systems, but only if they are taken piecemeal and isolated from non-economic factors.

This program poses the problem: How many real causal factors can be left out of any ideal functional (coherent) system without destroying the ability to use the ideal to describe real processes? The borders of economic science have been drawn and re-drawn as new factors which influence economic processes have been added to deliberations. Coherent models of relations between increasing numbers of economic factors should lead to models which come closer to describing real world situations. Institutions, for example, are powerful forces in shaping the course of economic events. The movement to distinguish more sharply the science of economics was fueled by opposition to those thinkers such as John Commons and Thorsten Veblen or the historical school in Germany. This movement was deemed necessary because those thinkers, who are now known as the old institutionalists, allegedly had no theory of institutions. As a result they could offer historical and/or social analyses but no scientific laws. As it became increasingly clear that attempts to make economics scientific and capable of describing real social processes could not entirely ignore institutions, new ways were sought to reincorporate them into economic models without rejecting the neo-classical functionalist approach. The so-called "new institutionalists" sought to do better than the old ones by incorporating institutions into the new theoretical models. Gordon Tullock and James Buchanan and then James Buchanan, for example, sought to explain the creation, maintenance and change of institutions on the basis of the individualist presumptions of economic theory alone. I have argued elsewhere that their efforts are empty: Their results are compatible with the maintenance and/or formation of any institution at all (Wettersten 2006). At best they provide methodological individualist explanations of institutions but not of how institutions steer events.

Influenced by Joseph Schumpeter, Douglass North tried to fill a gap between economic theory and the history of economic activity. He began his career by applying ideas of Tullock and Buchanan to account for that history. But by the end of the 80's he had come to the conclusion that Hans Albert had reached in the early 70's: a new explanatory approach was needed, if real economic phenomena were to be explained and, like Albert, he has called for a new kind of explanation in economics which takes its cue from psychology. (Wettersten 2006) I am not sure just how this project is supposed to look, since psychology is today so far away from economics. Albert points to James Duesenbery's theory of consumption which is certainly better than Friedman's theory that consumption can be explained as a direct consequence of income and price, because it includes social factors; North goes in the direction of recent developments in cognitive psychology, and Viktor Vanberg, who sees himself as continuing Hayek's advocacy of the use of psychology to inform economic theory, emphasizes evolutionary epistemology and psychology. But no one starts with psychological processes and describes how economic processes proceed as a consequence of them.

Ronald H. Coase has tried to make models more realistic by adding transactions costs. And Amartya Sen has pointed various limitations of economic theory and has proposed adding to these the roles which freedom and process effect economic outcomes. But no comprehensive system has emerged.

Some economists have also dealt more intensively with the obvious fact that politics influences how economic systems function. But they have had trouble reconciling democratic politics with free-market economies although this reconciliation should be easy, since both are individualist. Joseph Schumpeter suggested that capitalist societies are simply too risky for most individuals and as a consequence cannot survive democracy. F. A. Hayek sought to isolate economics from politics even to the point of apologizing for Pinochet's dictatorship, because Pinochet secured economic freedom. Hayek also attempted to devise a political theory to protect economic policy from any too direct democracy. But his attempt is quixotic, because it exemplifies a social planning in political theory rightly rejected in economic theory, and anachronistic, because it flies in the face of modern movements such as that found in America to revise its constitution to make democracy more, rather than less, direct.

Economists have hoped to draw attention away from serious internal problems their discipline faces with "imperialist" attempts to incorporate other disciplines within their own. They hope to extend the use of economic methods, above all model-building under some equilibrium assumptions, to all social sciences. The fundamental problem that societies are treated as functional systems due to the high standards set by the underlying theory of rationality is not overcome.

An alternative to theories which seek to incorporate social factors into economic models has been offered by Milton Friedman: Economics is scientific because it creates models which are useful, but the judgement of just when and where they are useful must be left to the inarticulate knowledge of trained economists. Although this theory is by no means enlightening about the impact of social factors on economic processes nor plausible in its claim that economists know intuitively how and when these factors are relevant without being able to explain why, it is the best stop-gap measure economists have been able to come up with.

1.2 The Separation of Economics from Normative Social Theory: Normative Social Theory.

The attempt to isolate economics from social and political theory as a consequence of the equation of rationality with coherence and justification has its mirror-image in the work of social and political theorists. They ignore economics when devising their theories of how societies ought to be organized. The underlying cause is the same: In order to "justify" their moral and/or political theories, they isolate moral, normative questions, from descriptions of the real world. Any universal, coherent and justified moral theory must be applicable to all individuals in all societies. It should give a definitive answer to any moral question. Since it must be applicable to all societies, it cannot depend on descriptions of any particular society. Once such a system has been constructed the remaining moral problems are merely those of applying to particular cases.

Moral theorists concentrate on theories of rights and/or proper distribution of goods. As Sen points out they ignore the significance of how these states of affairs are to be brought about in the real world or what actual consequences their application might have today. As a consequence the theories they produce are by and large useless for politicians. Indeed, these theories often do damage by demanding too much, or by leading politicians and political activists to seek to move in the direction of some utopia with no idea of how to reach it or whether the short term problems encountered in the first steps taken toward some utopia far outweigh the long-term benefits of reaching it, which in the rule will never be achieved.

The difficulty of applying theories of ideal societies to real ones may also be found in economic theorists who turn to political-cum-moral questions. I have mentioned Hayek's political theory above, which may designed to serve the interests of a free-market but avoids any discussion of existing political institutions and how they may be changed. North criticized Stiglitz's moral-cum-economic theory of the proper role of the state for neglecting problems of changes in economic systems, while dealing with the apparently static problems of the reallocation of wealth. But the fact that the problem of distribution is treated as static is a direct consequence of the theory of rationality which leads to the treatment of societies as systems.

Albert does far better by advocating and explaining the use of fallibilist principles in social policy, but he does not overcome the dichotomy between economic and social theory, because he clings to neo-classical economic theory. There are two Alberts, one is a progressive Popperian social philosopher, and the other is a sympathetic but critical commentator on neo-classical economics. But he fails to connect the two, because he fails to overcome the use of a theory of rationality in economics which he rejects in social theory.

1.3 The Separation of Economic from Normative Political Theory: Ad hoc Combinations.

Although economic theory seeks to abstract from or declare irrelevant all moral and social theory and moral and political theorists ignore social reality to find abstract ideals of what is good and moral, every politician has to be aware that free-markets carry costs which cannot be ignored and democratic politics demands that needs of individuals be fulfilled with or without reference to free-markets. The costs of free-markets include economic ones such as unemployment among particular social groups, but also non-economic ones such as environmental damage and social upheaval. Today conflicts between free markets and adequate security have become especially important. Sometimes the unintended consequences of unfettered economic activity have a moral or political nature, since measures intended to further economic growth can conflict with views of what is fair or just. If politicians are to retain their democratic support they cannot ignore these costs, and if economists are to offer theories which describe the real world, they cannot ignore social and political problems.

Faced with conflicts between desirable attempts to provide for the best economic results and attempts to maintain standards of justice and fairness as well as attempts to prevent environmental damage and other unintended, but undesirable results, political decisions are regularly made ad hoc. We now have no non-ad hoc approach to resolving conflicts between policies to further effective economic institutions, on the one hand, and policies to solve other pressing problems on the other. This regularly leads to impressions that politics is corrupt, since decisions which in fact favor one interested party or another and which cannot be given some encompassing rationale will be seen as mere attempts to satisfy the interests of one group or another at the expense of the common good. One of the central political problems in America today, for example, is how to deal with the influence of lobbyists who are often defending economic interests and often using free-market theories to do so. They have left the widespread impression that they corrupt government in an unbearable way by furthering special interests at the cost of the general welfare.

No comprehensive resolution of such conflicts for any specific society is possible, since only a functionalist view of that society, a view of it as one integrated whole, or as moving toward the realization of one integrated whole, could supply such a view. Attempts are regularly made to do that: neo-conservatives have tried to do so in America and Mullahs have tried to do so in Iran. But their efforts inevitably fail on their incapacity to deal with the unintended consequences of attempts to transform existing societies to meet standards of favored utopias. So, to a large degree we make do with ad hoc procedures to resolve the most important political conflicts in contemporary societies.

2. The Source of Difficulties and a Path to Partial Solutions.

Any attempt to overcome the separation of economic theory from normative political theory may seem to be damned from the start. It is now conceded by everyone that wholesale economic planning is a fool's errand, which regularly does more damage than good. Yet the maintenance of democratic societies requires that methods be available for resolving conflicts between economic and non-economic interests. The conundrum caused by the apparent economic necessity to avoid social planning so that free markets may provide for economic well-being and the social need to engage in it to overcome environmental and social problems leads to contradictory recommendations. Economists by and large advocate keepings hands off social developments while groups concerned with the environment, social justice and other problems demand just the opposite.

Although the dominant theory of rationality is shared by the relevant disciplines, it has led to sharp disagreement rather than to agreement in regard to social planning. Economists by and large believe social planning to be damaging to the well-functioning of economic systems, and do so because of the theory that these systems satisfy demands of rationality. Those interested in social justice take their cues from moral theories which demand that moral principles determined independently of the practices of real societies be adhered to, also on the theory that only then can true and justified principles be followed. If we replace this dominant view of rationality with a fallibilist approach, we may make progress in the direction of a unified approach for solving economic and other problems without ad hoc procedures This approach has not been sufficiently taken advantage of to resolve the conflict between economic theory and political theory, because theorists of all stripes have either clung too closely and too resolutely to the rationality principle, which is the main source of the difficulty. What is needed, however, is a revision of the principle. When this principle is revised in a way which thoroughly fits Popper's and/or Agassi's fallibilist philosophy, difficulties can be lessened. In this second part I will discuss why the rationality principle is the source of the separation, explain my proposed revision of it, and defend it against the charges that it is trivial and/or useless.

2.1 The Rationality Principle as the Source of the Separation.

Traditional theories of rationality presume that all rational thought and/or action is coherent and/or justified. Because it has been assumed that in the social sciences only rational thought or action can be explained, coherency in human thought and action has been presumed. One way of finding coherency in human action is to presume that the society itself is a coherent whole, as functionalist social sciences have done; the only other way is to presume that individual thought is coherent. The former is largely passé except in economics. It is still doing well there since neoclassical equilibrium assumptions require it. John Harsanyi, for example, suggests that there is no conflict between individualism and functionalism in the social sciences, because the unhindered rational actions of individuals lead to coherent social systems. Since, however, functionalist systems in economics are considered consequences of coherent individual thought and action, we may concentrate on the individualism of economic theory.

Economists presume that individual action is the consequence of coherent thought about how to achieve goals. As a consequence they limit what individuals are rational about to the pursuit of economic goals. Given standard theories of rationality it would not be possible to portray economic activity as rational without this limitation. If one tries to take into account the moral, social, or religious beliefs leading to action, no functionalist system can be described as emerging out of rational actions. Reaction to specific economic situations can be found. Economics is thus isolated, because only by isolating itself from social, moral, religious factors can it find in the coherent action of individuals the basis for well-functioning economic systems.

The application of traditional theories of rationality in ethical or political theory leads to a similar result. In order to find a justified view of what is good and right, theorists have felt forced to abstract from any given social situation. Universal and justified standards are sought or, alternatively, relativism and/or subjectivism are endorsed as a consequence of the failure to justify. Thus, for example, Rawls constructs an artificial choice of a neutral observer of various societies in attempt to show that any rational person would choose only one type of society or Nozick imagines an imaginary starting point of social interaction in order to find a method of determining what rights individuals have. The details of these theories are unimportant here: the only point I wish to make is that the demand for justification isolates them from any real moral choices

2.2 The Rationality Principle Revised.

If we take rationality to be partial—a proposal developed by Agassi—we can explain action as rational to a degree without assuming the coherence and/or the justification of the actor's beliefs. The rationales for both extreme attitudes towards explanation in the social sciences, that is, functionalism which assumes the coherency of social systems and methodological individualism which presumes the coherency of individual belief, collapse. And with them both extreme attitudes toward social planning collapse. These extremes are the theory that social planning should be used to control all elements of the society in order to create a coherent social system and the theory that it is to be completely avoided so that the rational action of individuals alone will lead to a coherent social system. We can neither plan with assurance that we are on the right path nor can we leave things alone and expect things to work out right.

According to the rationality principle all rational actions are consequences of what goes on in the heads of individuals. From one perspective this is trivially true, since without specific thoughts there can be neither institutions nor rational action. But it is nevertheless false, insofar as it presumes that the contents of thought are primarily results of individual deliberations. The far larger part of the contents of thought consists of mere expressions of social situations which are adopted by individuals willy-nilly, as Mary Douglas has so forcefully argued. If we revise the rationality principle to take this fact into account, we can overcome the dichotomy between the strict rejection of planning in economics and the strict endorsement of planning in politics. We do not need a large revision of the rationality principle, because a small revision has very wide consequences.

If instead of saying that all actions are the result of the beliefs of individuals, we say that all actions are the result of attempts by individuals to solve problems within institutional contexts, we may solve the methodological problem posed by the separation of economic, social and political theory. We may then pose questions which are neither simply economic nor simply social nor simply political. Individuals pose problems which seek to incorporate both economic and non-economic aspects. If we ask which problems individuals seek to solve in specific institutional contexts and how they may be aided or hindered by institutions in their pursuits to solve them, the problems which politicians and which economists try to solve turn out to be of the same kind.

We start in the manner which anthropologists do, that is, by looking at institutionalized views and how they shape the way individuals view their problems and solutions they consider. We cannot of course do this after the fashion of functionalists who seek to describe the whole intellectual framework of some society and how it determines the lives of its citizens. Rather we look at this or that institution, at businesses or schools or unions or churches or combinations of them to see how they typically shape problems of individuals under their influence. We do not treat institutions as actors. As Agassi has pointed out societies do not have aims, only individuals do. But we may see how institutions set aims, problems and desiderata for solutions to problems for individuals.

This approach allows us to avoid assuming the coherency or comprehensiveness of the views of individuals while still viewing them as rational to a degree. We may see, for example, that groups of individuals try to solve some problems while meeting incompatible socially established standards, say, those imposed by their religion and those imposed by their profession. But we may still explain their moving from one standard to the other as rational to a degree by explaining how they avoid conflicts by switching from one belief system to the other as the occasion demands. We may be able to explain how they move from one context to another and presume competing standards. We do not need to integrate or separate economic from political or social demands but study how individuals integrate or fail to integrate various aspects of their lives as they attempt to use the institutions available to them to solve their problems.

We may start with questions about how institutions influence the course of events, whether this influence is economic, social or political or, as is normally the case, some combination of the three. By describing the assumptions which institutions lead individuals to make because they identify with them or which they are required to make at their own peril, we may examine how these problems lead individuals to particular paths of action. These paths of action are combinations of economic, social and political endeavors but they do not serve to integrate the society, but only to allow the individual to take the various aspects of his problem situation into account as he proceeds. The fact that Jews in Christian societies or Chinese in Muslim societies have to take account of the religious views of their fellow citizens does not mean that the result is some functioning whole, but it also does not mean that we cannot see their action as rational, as the use of the social means at their disposal to solve their problems.

Problems may be chosen in a very broad or very narrow way, depending on our interests. Ernest Gellner, for example, proposed to study how beliefs are legitimized in Western culture. He says that in Western societies we have three standards, one is intellectual, demanding system and coherency, another is empirical, demanding correspondence to the facts, and a third is mechanical, demanding causality. But he does not say that we have, thereby, a coherent system. We simply apply three standards. I previously found this view quite unacceptable because of the primacy it gives to an anthropological study of how theories are legitimated, rather than to philosophical, normative theories. But I now see the method—I do not wish here to discuss Gellner's results—as the proper way to begin the social scientific study of rationality. Gellner, I think, does not go far enough. Having started with such an anthropological study of how standards are used one may proceed to study how individuals make use of such standards, what problems they face when they do, and what social consequences result from their attempts. We may also discuss how such standards may be reformed to improve the search for truth. In this way we are not simply studying the consequences of the beliefs and actions of individuals in accord with them, but rather the consequences of the use of established doctrines by individuals acting partially rational. They are partially rational, because they will rarely have comprehensive and often not even coherent views on which they act, yet they do act on understandable presumptions given their social contexts and their action can be understood as attempts to solve problems in their contexts, whose social consequences can also be understood.

We may also study narrow problems with the same approach. We may ask, for example, whether one school system is better than another by looking at the differing problems which they impose on teachers, students and parents. (Wettersten 2006)

On this view social scientific explanations are always piecemeal to this or that degree. But they always explain how individuals use social frameworks peculiar to their social position to pose problems, to devise solutions to them. Their problem-posing and solving activities interact with those of other individuals who may very use differing assumptions because of their differing religions or differing political parties. And such interaction may be happy but can also be disastrous.

2.3. Between Functionalism and No Social Structure at All.

One might suggest that this proposal is trivial because the standard rationality principle already says that humans act in view of the logic of their situation, which is just that which I emphasize. In order to respond to this suspicion let me compare the proposal to Weber's integration of economic and social theory, on the one hand, and Popper's endorsement of the rationality principle, on the other. Weber's is defective because it is too functionalist, whereas Popper's is defective because it fails to take account of social structure at all. The proposal presented here can take account of social structure without falling back into functionalism.

The use of sociological frameworks to set a context for economic theories is not a new idea. The most prominent representative of this approach is Weber's explanation of the growth of capitalism as the consequence of Calvinism. But economic theorists today have sought to free themselves from the yoke of sociological frameworks and to establish their science as an independent discipline much after the model of physics. The Walrasian ideal of describing economic processes in a systematic way is still influential regardless of the details. And economics has gained the reputation as an imperialist discipline because it has blocked the use of methods from other social sciences to study economic processes by staging an offensive: The methods used by economists should be used in all social scientific research.

But others move back in the direction of Weber, who explained the growth of capitalism as to a large degree the product of the Calvinist belief that those who accumulate wealth are chosen by God. To a degree the proposal made here also turns back to Weber. But those who share this desire to move back and incorporate social and culture frameworks in the explanation of economic events get stuck, because they share the high standards of rationality of neo-classical economists. They only way to use social and cultural frameworks to explain economic systems, given their theories of rationality, is to return the functionalism of Weber. Weber could only explain the rise of capitalism as a product a unified society. He knew that society was not always or completely unified because he saw that non-Calvinists such as Jews were a part of this society. But all he could say about them amounted to a few anti-Semitic remarks about their freedom to charge usurious interest rates because they were not part of the system. This view was, I think, more a consequence of his theory of rationality than some underlying anti-Semitism, to whatever extent that may have been present.

Modern theorists who wish to go back to Weber run up against the same kind of problem when they discuss the Chinese in Malaysia vs. the Chinese in China and handle it no better, as is evident in essays in the recent collection of essays edited by Harrison and Huntington. They seek to explain, say, economic success as a direct product of a set of beliefs without asking whether acting on the same set of beliefs in different social contexts might have quite different consequences.

The return to a Weberian use of sociological frameworks in the construction of economic theories is only possible, if we adopt a fallibilist view of rationality along the lines posed by Agassi. On this view both individual rationality and social explanation are partial. We do not need to presume that a whole system is a functioning system, but rather only at how aspects of societies lead people to certain problem situations and explain how they act on the basis of them. We will regularly find them getting into trouble because different institutions will regularly impose conflicting demands on them. In such cases we do not need to presume that individuals develop some coherent view. They do not. They muddle through. But we can understand the rules they use when they do muddle through, when they follow the rules of one institution in their society and when they follow the rules of another and what rules they have to decide. And their following of rules is not opposed to their rational actions as is regularly supposed, given traditional views of rationality. Rather, they follow rules because these rules are needed to pose problems and posing problems and seeking to solve them is the core of rationality. We will also of course find some unpredictable behavior. Our explanations will, then, always be partial. On a fallibilist view of rationality as partial this is inevitable.

Now many will find this quite dissatisfying and will insist that we do better. But we have seen that trying to do better leads us to do worse. Friedman's often used defense of modern economic method is forced to fall back on the claim only the educated economist—anyone with a Ph.D.

will do—can judge whether a model will work in a given case and he can only do so with the aid of inarticulate knowledge. Alan Greenspan never grew tired of saying that the models could not be trusted to explain how the economy was functioning: He became a super-guru, a substitute for real knowledge, whose vast knowledge of the facts—a good dose of inductivism remains—and deep insight would lead him to the truth. Partial and criticizable theories which explicitly take into account the reasons the models do not work, that is, the social conditions in which they are embedded, are certainly superior to this.

I wish also to contrast this proposal with that of Popper's. Popper was also to a high degree a follower of Weber. But whereas other thinkers who look back to Weber for guidance take too much of the functionalist aspect of Weber's theory Popper takes too much of individualist aspect, of the theory that each individual has coherent plans which guide his actions in the pursuit of his goals. He ties himself in knots in trying to defend his non-refutable and empty rationality principle. I can find no contradiction in his defense but it is not pretty, sliding as it does from an a priori principle to what sound like conventionalism to a defense of the claim that models constructed using his principle are realist. (Wettersten 2006)

From the perspective taken here Popper made the central mistake of presuming that social scientific explanations were only possible when each individual actor met high standards of rationality. We should put, as he said, as much as possible into his head. On the view proposed here the opposite is true. We should see how much of individual behavior is an attempt to apply socially established rules to solve problems of individuals. But it does not say, as have functionalists in anthropology and sociology, that we explain at least some aspects of behavior as mere rule following or carrying out the duties of their station. Rather, we preserve individualism by explaining how social rules determine the way individuals pose problems. I do not mean that individuals cannot disregard established rules when posing problems, only that we can explain social events when we notice how as a matter of fact established rules lead people in certain situations to pose problems in specific ways.

Weber's analysis of the influence of Calvinism can be taken as an example. Insofar as Calvinism did lead individuals to pose their economic problems in terms of the accumulation of wealth without the aim of enjoying much more than its accumulation, his view explains how Calvinism influenced economic behavior. But this analysis cannot be taken as explanation of how the society functions. It is at best partial. The interaction of Calvinists and non-Calvinists must also be explained, what rules do they develop in interaction with each other, for example, in order to explain an historical event as complex as the rise of capitalism. All institutions in modern societies are partial and in conflict with other institutions. How individuals deal with these varying, sometimes competing institutional contexts, must be taken into account to explain economic, social and political affairs. By and large to the degree that one separates them one distorts them.

Weber's explanation also purports to begin with the beliefs of individuals and to use these beliefs to explain societies. The proposal made here reverses the procedure and take the established beliefs in any society to explain how this society is maintained and/or changed. This reversal maintains individualism, because only individuals act by attempting to solve problems, but it is also crucial, because it enables one to look at conflicting demands on individuals, on socially established standards which in the rule do not constitute some coherent belief system, and still explain how individuals are rational and how their (partial) rationality leads to social stability or change. On Weber's and/or Popper's and and/or the rational choice approach, that is, by beginning with the rationality of individuals this is not possible, because with this beginning point only coherent beliefs can be taken into account.

Conclusion:

On the proposal made here all problems of economic, social and political theory use the same methods of explaining how institutions lead individuals to pose and to seek to solve problems in particular ways. They do this by setting the frameworks which individuals use to pose their problems and the desiderata which acceptable solutions must meet. It may thus serve to unify these various endeavors.

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Interlude

The recognition that rationality is social opens new tasks for social scientists.

3d. 'Eine aktuelle Aufgabe für den kritischen Rationalismus und die Soziologie', in Hans-Jürgen Wendel und Volker Gadenne (eds) *Kritik und Rationalität* (Tübingen: J.C.B. Mohr(Paul Siebeck) 1996), pp. 183-212.

A real task for critical rationalism and sociology today

Through the development of critical rationalism—especially in the research of Hans Albert—sociology and philosophy of science have played a closely related and important role. Some thinkers think this fruitful connection is exhausted. But this is by no means the case. For the shared work of sociology and philosophy of science, there remain important tasks. In appraisal of the work of Albert and at the same time as a possible continuation of his research I wish to sketch such a task, which can only be taken up with integrated research of both areas. Although its formulation is closely related to the development of critical rationalism, the task has been by and large ignored. This task is the investigation of standards of rationality with sociological methods. In what follows I will describe the development of critical rationalism and render clear the state of research today.

Above all through the writings of Albert his critical rationalism in the '60s, '70s, and '80s in Germany has been established as (1) a challenging philosophy of science, (2) an important social philosophy and political theory, which supports democracy, and (3) a controversial theory of rationality. But now it appears that many thinkers think it has gotten on in years. The philosophy of science of critical rationalism is to be sure still thought of as a challenge; and its thesis, that falsification is important, is recognized by many. But many think it has done its job in that it has shown the borders of rationality. But in its rejection of all attempts at justification many think that it is all too one-sided. The fundamentals of its political theory is still endorsed but viewed as not very relevant for today's research in the social sciences and, as a consequence, largely ignored. And because the critical rationalist theory of rationality is considered too radical and one-sided is viewed quite skeptically very few attempts are made to develop it further without at the same time attempting to complement it with some kind of positive theory of justification.[266] From

[266] Examples of such research conducted by former critical rationalists are: John Watkins, *Science and Skepticism*, Princeton, Princeton University Press, 1984; and Alan Musgrave, *Common Sense, Science and Skepticism*, Cambridge: Cambridge

this perspective critical rationalism appears as a point of view, which has, indeed, contributed an important element to contemporary research, but which cannot serve as a promising framework for the formulation of significant up-to-date problems. It shows no path to the future.

As Albert has himself emphasized critical rationalism does not offer a final system, but rather a perspective which can be further developed.[267] The contribution which critical rationalism can offer in the future is not above all to seek in its widespread acceptance or that its critical aspect will still stronger be stronger developed, say in the rejection of the quest to find historical laws. Critical rationalism can contribute more when it is used to formulate new problems. Such a possibility will be portrayed and discussed here. Out of a look at its development these problems will be portrayed and out of this development a fascinating, actual and promising problem will be proposed.

The most important aspect of this development is the theory of methodological rules. This development led to the discovery of problems of rationality, which widely took the place problems of scientific methods. Of interest here are problems which are not limited to scientific methodology. They are of a general nature and remove the borders between philosophy of science and sociology. They have both philosophical and sociological aspects. In the overview presented here the movement of problems of scientific method first to those of rationality and then to those of the society will be portrayed and defended. It is to be hoped, that the study of these new problems can contribute to the growth of knowledge.

It is emphasized that this is a violation against traditional so-called serious research. According to these standards each scientific field should be sharply separated, in order avoid every irrelevant influence, which walls off the quest but could block certain knowledge. According to critical rationalism it is on the contrary desirable to include such knowledge which goes over borders. If one wanted to justify theories in one field with reference to results in some other field but then also used the results in the first field to justify results in the second one ends up in a fatal circle. But when the results in one field are used to criticize those in another and the

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University Press, 1993, with the exception of David Miller, *Critical Rationalism*, LaSalle: Open Court, 1994, who tries to closely adhere to Popper's rejection of justification. For a discussion of Musgrave and Miller see John Wettersten, "After Popper" in: *Philosophy of the Social Sciences*, Vol. 26, No. 1, 1996, pp. 92-112.

[270] Hans Albert, Kritik der reinen Erkenntnislehre. Das Erkenntnisproblem in realistscher Perspektive, Tubingen J.C.B. Mohr (Paul Siebeck), 1987, p.4.

other way around, there are various possibilities to make progress with changes in this or that field. As Albert has pointed out each field can be used as a provisional foundation.

From *Logik der Forschung* to the study of the rationality of methodological rules.

When Popper began to develop a new philosophy of science he wanted to follow the positivists in so far as he talked exclusively about the logic of science.[268] It was, indeed, the members of the Würzburg School which motivated him and awoke his interest in the problems of knowledge, but the greatest challenge came above all from the positivists. It was they who had posed the task of constructing a new philosophy of science on the basis of the new developments in logic which have been above all developed by Bertrand Russell. They wanted to at last either explain away the traditional problems in the philosophy of science or to finally solve them. The program of the positivists was very sharply conceived: they wanted to solve the problem of induction and the borders of science exclusively with the means provided by logic. At first Popper tried to work within this program. But one of the positivists—Hans Reichenbach—pointed out to him that logic alone was not sufficient to describe the methods of science, at least not then when one understood the logic of science as Modus Tollens: the application of this logic does not preclude ad-hoc methods.[269] As a response to Reichenbach, Popper added to his philosophy of science methodological rules, such as the rule that, in response to a refutation of a theory, one should adopt as its replacement that theory with the highest degree of falsifiability.[270] These rules were

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[268] In regard to the development of Popper's philosophy of science see John R. Wettersten, *The Roots of Critical Rationalism, Schriftenreihe zur Philosophie Karl R. Poppers und des kritischen Rationalismus*, hrsg. von Kurt Salamun, Amsterdam und Atlanta, Rodopi, 1992.

[269] Popper then viewed the logic of science as modus tollens. He later found out that this was a mistake. The logic which he wanted to describe was the fact that when the conclusion of a valid argument is false, at least one of the premises of that argument must also be false.

[270] Gunnar Anderson tried to return Popper's philosophy of science to its original form by arguing that methodological rules are by no means necessary to prevent the use of ad-hoc hypotheses. See Gunnar Andersson, *Kritik und Wissenschaftsgeschichte*, Tübingnen: J.C.B. Mohr (Paul Siebeck) 1988. For a discussion of Andersson's suggestion see John Wettersten, Braucht die Wissenschaften methodologische Regeln? In *Conceptus*, Nr. 73, 1995.

intuitively quite acceptable and they built with his logic of research a unity. And from the perspectives of the members of the Würzburg School—Popper was one—they did not appear to be a significant innovation.[271] Nevertheless, during this time Popper had moved so far to the positivists, that there seemed no place in his theory for methodological rules. His theory of these rules in *Logik der Forschung* appeared so thin, because he wanted to remain as close as possible to the description of logic. In regard to methodological rules he merely said that they were necessary, useful and fruitful. But whether they were to be understood as merely descriptive or prescriptive was not so clear. And the methods of their evaluation as well as the role they played in a comprehensive theory remained in darkness.

Because Popper's theory at first received so little recognition, little to no progress was made in this regard. But when Popper returned from New Zealand after World War II, he attracted a group of talented students who seriously discussed the problems within his theory. These problems had above all to do with the fact that his theory as he put it forth in *Logik der Forschung* was rather positivistic even though the basis of his theory—a basis that was also to be felt in *Logik der Forschung*—stemmed from the Würzburger School and was rather directed against the positivistic perspective. He then defended a realistic view of science, even though it was not then clear to him how this view was to be made consistent with his rejection of essentialism.[272] For this reason and others he said that his realism was merely his personal point of view and not part of his philosophy of science.

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[271] It may seem an exaggeration to view Popper as a member of the Würzburg School. Popper did not view his research as a continuation of the research of that school. But his earlier defense of Otto Selz, his education from Karl Bühler as well some of his later publications such "The Bucket and The Searchlight: Two theories of knowledge" in *Objective Knowledge*, Oxford: Clarendon Press, 1972, pp. 341ff. show the deep roots of his philosophy in the Würzburg School. From my point of view it is appropriate to understand him as a member of this school.

[272] Popper sought later to overcome this difficulty. See "Three Views Concerning Human Knowledge" in *Conjectures and Refutations*, London: Routledge & KeganPaul, 1963, pp. 97ff. See in this regard John Wettersten, The Roots of Critical Rationalism, op.cit. p. 172 ff.; 212ff. It is regrettable that Popper did not portray his view of essentialism, that is, as a part of his own development. It is clear that he never held an essentialist view. But it he was first later able to reconcile his realistic methodological theory with the realism of the essentialist theory. For that reason he first emphasized his realism later.

In the fifties he tried to find a clear position about realism in order to defend one important aspect of his philosophy of science. He thereby developed new perspectives on philosophical and metaphysical problems. He tried to explain how they could also be treated rationally. And quite naturally the problem of how methodological rules could be handled rationally appeared as important: How can they be evaluated, developed and improved? I abandon here the history of the development of Popper's theory[273], because it was above all his students and followers who developed the theory of rationality which is of interest here. Popper's own development was increasing hemmed in, because he tried too hard to justify the theories he put forth as a quite young philosopher.

Methodological rules and the theory of rationality.

In the 50s and 60s it became increasingly clear, that Popper's demarcation theory was questionable: The demarcation suggestion, on the one hand, and the recognition of the important role which nonscientific theories played in science stood in apparent or real conflict. The relationship between metaphysical and scientific theories had to be investigated. Agassi sought to develop a theory of metaphysical research programs in order to explain how metaphysical and scientific research played common and cooperative roles or combine these two types of research and thereby to describe the resulting methods.[274] Bartley tried to develop a comprehensive but still coherent theory of rationality.[275] Albert tried to clarify the situation with an attempt to describe so-called bridge principles between

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[273] I have portrayed this development in far greater detail in *The Roots of Critical Rationalism*, op. cit. PP. 137ff.

[274] Joseph Agassi, "The Nature of Scientific Problems and their Roots in Metaphysics", in: Mario Bunge, ed. *The Critical Approach*, New York: Free Press, p. 189-211. Republished in *Science in Flux*, *Boston Studies in the Philosophy of Science*, 28, Dordrecht: Reidel, 1975, 1971, Joseph Agassi, *Faraday as a Natural Philosopher*, Chicago and London: University of Chicago Press. For a discussion of philosophical anthropology see *Towards a Rational Philosophical Anthropology*, The Hague: Martinus Nijihoff 1977. See in that regard John Wettersten, review of *Towards a Rational Philosophical Anthropology* by Joseph Agassi, *Zeitschrift für allgemiene Wissenschaftstheorie*, Band XVI, HKeft 1, 1985, pp. 167-176 and John Wettersten, "Schritweise Rationalität als Theorie über den Menschien" review of *Towards a Rational Philosophical Anthropology* by Joseph Agassi, in: Grazer philosophische Studien, HIeft 15, 1982, pp.163-174.

[275] William Warren Bartley III, *The Retreat to Commitment*. Öpmdpm_ Cjattp & Windus, 1964. "Rationality vs. The Theory of Rationality" in: *The Critical Approach*, ed. By Mario Bunge, New York: The Free Press, 1964.

various fields.[276] In all these attempts there was a movement of the problems of the philosophy of science from methodological problems of the demarcated science to the problems of rationality. In order to portray the development of the theory of methodological rules it is of course useful to review these three theories of rationality of Agassi, Bartley and Albert. We can then clarify the problems which grew out of this development.

Bartley tried not only to show that a theory of rationality was necessary, but also to show that a critical perspective of rationality could meet this need. All traditional theories of rationality, he determined, viewed rationality and justification as equivalent. But none of these theories could meet this standard: no theory of rationality was justified. They were not justified, because each possible justification of such a theory had to presume that the standard to be justified was true. But that is just that which should be justified. If a theory of rationality did not do this there would be an infinite regress or a dogmatically presumed point of beginning. His own theory on the other hand, according to which a theory is held rationally when it is held open to criticism or a person acts rationally, when he or she holds his or her opinions open to criticism, can meet its own standards: it can itself be held open to criticism.

Although this theory is coherent, it is too one-sided. In order to comprehensively explain rationality, it is necessary to say much more. It is

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[276] Hans Albert, “Theorien in den Sozialwissenschaften“, in: *Theorie und Realität*, 2. Veränderte Auflage, ed by HIans Albert, Tübingen: J:.B. Mohr (Paul Siebeckl) 12972. In this essay Albert speaks about the problems of bridging the gap between the theoretical social and cultural sciences, on the one hand, and politics, on the other hand, but not about the gap between the social sciences and philosophical anthropology. But, as he told me personally, this does not mean, that he made an exception for the later. Philosophical anthropology plays an important role in his unpublished dissertation, which he wrote before he knew Popper. See *Rationalität und Existenz, Politische Arithmetik und politische Anthropologie*, Köln, 1952. The second part of this dissertation was published in a modified version under the title *Ökonomishce Ideologie und politische Theorie*, Göttingen, 1954. Bridge principles could also build connections between falsifiable and non-falsifiable theories. See also Hans Albert, "Ideologie und Wahrheit“, in: *Konstruktion und Kritik*, Hamburg: HIoffmann und Campe, 1972l, pp. 41ff. In his *Traktat über kritische Vernunft*, op. cit. P. 58, he prefers a research program which largely corresponds to Agassi’s program. See also *Kritik der reinen Erkenntnislehre*, p. 69, 193. His own research includes the treatment of metaphysical foundations of various knowledge research programs such as the Scottish moral philosophy, Bentham’s utilitarianism or Marxism.

obvious that criticism can be good or bad. But which critical methods are good and which are bad? And what can one achieve with criticism? New theories which distinguished between good and bad methods of criticism were needed.[277] Out of this need problems of the logic were moved to problems of the best methods of criticism as well as problems of their sociological characteristics. These problems were studied above all by other thinkers.

As Bartley developed his theory of rationality Agassi had begun his inquiry into the relationship between metaphysical and scientific theories. He defended the thesis that scientific research was especially interesting, valuable and progressive, when it was conducted with close contact to metaphysical theories. The metaphysical research programs supplied the conditions for developing scientific theories with a high degree of explanatory power. If this is the case, said Agassi, one has to revise Popper's theory. Popper had, namely, identified the degree of falsifiability with the degree of explanatory power. But if the explanatory power of a scientific theory depends on integration in a metaphysical theory, then one has to judge the degree of falsification and the explanatory power of a theory separately. It is not then sufficient to judge a theory on its degree of falsification. One must also seek theories with high degrees of explanatory power. Agassi then suggested that the refutable theories with the highest degree of explanatory power be judged the best.

One can thereby by no means presume that theories which are not falsifiable can provide scientific explanations. Rather it means that one raises the explanatory power of testable theories by interpreting them with the help of metaphysical theories. Metaphysical theories provide the basis assumptions which are used in explanations of specific phenomena. Examples of this phenomenon are Newton's atomic theory and Faraday's field theory. Such falsifiable theories, which explain phenomena as examples of the working of basic principles formulated in metaphysical theories, will have higher degrees of explanatory power as those which cannot be interpreted as applications of such basic principles.

[277] Joseph Agassi, ‚Rationality and the Tu Quoque Argument', in *Inquiry*, 16, 1973, pp. 195-406. Republished in *Science and Society*, op. cit., John Wettersten and Joseph Agassi, "The Choice of Problems and the Limits of Rationality," in *Rationality: The Critical View*, Joseph Agassi and Ian Jarvie (eds.) Martinus Nijhoff Publ. (Dordrecht) 1987, pp. 281-296.

Agassi's theory led to further studies of methodological rules, which aimed to describe how they may appear in practice while at the same time furthering criticism and progress in science. It should, for example, be allowed to explain away apparently refuting observations or experiments, but, of course, not merely with ad hoc hypotheses, but rather with hypotheses which are independently testable.[278] It could also be useful to develop metaphysical theories, which could realistically interpret scientific theories. Only in this way can one be clear to what degree theories are testable. One should thereby by no means confuse the metaphysical research programs with the scientific programs because they have differing standards.

These studies of methodological rules and the reasons in favor of or against them led to further studies whose aims were to describe the metaphysical and scientific research programs. The theory which grew out of these studies is known as the boot-strap theory of rationality.[279] It can

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[278] Joseph Agassi, "Towards a Theory of ad hoc Hypotheses", in *Science in Flux*, op. cit. John Wettersten, 'Free from Sin: On Living with ad hoc hypotheses', in *Conceptus*, Jahrgang XVIII, Nr. 43, 1984, pp. 86-100; "Welche Problem stellen Ad Hoc Hypothesen heute?" in: *Zeitschrift für philosophische Forschung*.

[279] Joseph Agassi, "Testing as a Bootstrap Operation in Physics" in *Zeitschrift fürf allgemeine Wissenschaftstheorie*, 4, 1973, pp. 1-24. Republished in: *Rationality: The Critical View*, ed. by Joseph Agassi and I.C, Jarvie, Martinus Nijhoff Publ. (Dordrecht 1987, pp. 249-263.) See also the essays by I.C. Jarvie and Joseph Agassi, ibid. and L. Briskman, "Historical Relativism and Bootstrap Rationality," ibid. pp. 317ff. Albert's theory of rationality and philosophy of science is very close to this theory, in that it also recommends the interaction between science and philosophy of science. See Hans Albert, *Kritik der reinen Erkenntnistheorie*, op.cit. p. 60. He says, "Der kritischer Rationalismuss überantwortet dazu (zu der Auffasung, die wissenschaftliche Ergebnisse seien nützliche Machwerke, die mit anderen Weisen gewonnen werden, J:W.) die echte Erkenntnis nicht einer den Wissenschaften voroder übergeordeneten und von ihnen scharf abgrenzbaren Metaphysik. Er gesteht ihnen vielmehr selbst die Möglichkeit zu, solche Erkenntnis zu erreichen, und sieht in metaphysischen Auffassungen mögliche Inspirationen für die Entscheidung zwischen solche Auffassungen. Die Metaphysik skizziert unter Umständen (…) mögliche Erklärungen, die in der wissenschaftlichen Forschung ausgearbeitet und geprüft werden können." (Critical rationalism hands over to scientific results their usefulness, which have been achieved with other methods but not to a higher placed metaphysical perspective. It gives it rather itself the possibility to achieve such knowledge and sees in metaphysical perspectives under some conditions inspiration for decisions between such perspectives. Metaphysics sketches possible explanations, which can be developed and examined

be viewed as generalization of metaphysical research programs. On this theory one first seeks to develop a metaphysical theory whose ability to interpret empirical is then evaluated. Or one begins with a scientific theory and does not merely try to refute it but also to interpret it in the framework of a metaphysical theory. One goes, then, from the lower level upwards or the other way around. Thereby it is—as Albert also emphasized—not important how these levels are chosen or described, but only, that one uses the results of one level to criticize the theories of another level. Albert also argues that in the development of science it is not only the scientific theories but also the scientific methods which are improved.[280] This point of view can be applied in the discussion of rationality.

As mentioned above this development shifted the discussion from the demarcated scientific method to problems of rationality. One no longer tried to demarcate science, to describe the methods within this demarcated discipline, and to exclude the methods beyond the borders of this discipline. The good in contrast to the bad methods, metaphysical and scientific, should be identified and the good should be developed and recommended. But the methods to be examined had inseparable scientific and metaphysical aspects. Thereby it is extremely important how these aspects are connected and then their depended on how well or poorly the whole metaphysical-cum-scientific process proceeded.

In his own further development of critical rationalism[281] Albert tried the rules suggested by Popper to examine and improve. He leaved it open how far he thought how far the rules suggested by Popper or Agassi or by others were correct. Instead he tried on the one hand to develop a theory of rationality in view of the already proposed rules and, on the other hand, he wanted to construct a theory of the relationships between various sciences and between sciences and nonscientific theories. He was thereby above all concerned with relationships between social scientific theories and relationships between social scientific theories and nonscientific theories. He concentrated above all the relationships between and normative and descriptive statements in these theories. This theory should explain what

in scientific research.) He refers here to Agassi's theory of the relationship between metaphysics and the sciences.

280 Hans Albert, see for example, "Science and the Search for Truth" in: *Rationality: The Critical View*, op. cit. pp. 79ff.

281 Hans Albert, *Traktat über kritische Vernunft*, 5. Afu9lage, 1991, Tübingen: J.C.B. Mohr (Paul Siebeck), 1978.

can be achieved by the application of such rules both within and without the sciences; it should also portray the borders of rationality.

As he started his first larger attempts to systematically portray, to defend and to further develop critical rationalism, there had already developed in this regard, which had brought progress to the study of methodological rules as well as the theory of rationality. Thereby Albert had the opportunity to extend the newly one knowledge of rationality, so that a comprehensive, theoretical, moral and political perspective could be formed. The pressing problems in various specific fields could be overcome in a systematic way. But the development of critical rationalism had not only opened this possibility, but also posed some new difficulties. It was, for example, not clear, how Popper's older philosophy of science could be brought into agreement with the new developments.

Albert's first task consisted of developing a systematic concept of critical rationalism, which (1) laid down the foundations for rejection of theories of justification, (2) which showed their capacity to further and improve rational methods in various fields, and (3) which integrated the newly achieved points of view within critical rationalism. The foundation, which Albert formulated as trilemma, was close at hand and not controversial among critical rationalists: If one tried to justify a theory, one dogmatically presumed some point of view, or one landed in a never ending regress, or one landed in a logical circle. Since no satisfactory justification is possible, it is necessary to develop a new theory of rationality and, indeed, one that does not exclusively consider rationality within the sciences, but also the possibility and borders of rationality beyond the sciences. Albert here took over the central points of view previously made by Bartley, who saw rationality as above all to consist of holding all theories open to criticism. As I have already pointed out, this theory was not enough, because it failed to offer any sufficient and definite theory of rational methods.

In this way new problems of how the methods of criticism can be evaluated, how new methods can be developed, and how they can be systematically portrayed, insofar as this is possible. For Albert's research program this development raised significant problems of the relationships between the social sciences and nonscientific fields.

One central problem for the intention to provide a systematic portrayal of critical rationalism lay in the problem of the demarcation of science. In his original theory Popper emphasized the problem of demarcation as central.

But then he had a much more skeptical view of rationality outside of science as he later had. As the fallibilistic theory of rationality was generalized in order to develop theories of rationality, this aspect of Popper's theory had to be newly thought through: Was it in various situations still appropriate to retain a sharp border of the sciences?

In regard to this question, whether the demarcation of the sciences is desirable, remained Bartley unclear. The demarcation was for him not from great importance, but he never clearly rejected it.[282] Popper tried to show that the question was still important and actual. But he portrayed the problem in a new way in order to take account of the new situation.[283] Agassi sharply criticized Popper's position and rejected his portrayal of the problem[284]; the demarcation of science served neither progress in science nor rationality outside of science. Our aim should be a mutually fruitful integration of the differing areas of research.

Albert took a fourth way in that he did not reject the demarcation of science, but nevertheless proceeded to bury it. I can find no detailed discussion about how far his theory is in agreement with either Popper's or Agassi's. He treats to be sure demarcation as preliminary and relevant to the division of labor in the practice of gaining knowledge.

In this connection one has to differentiate between two parts of Albert's treatment of the so-called 'bridge principles'. His theory of bridge principles is one aspect; the other is his theory of the application of metaphysical and/or philosophical-cum-anthropological considerations. He uses the theory of bridge principles is special cases such as ethics and theology. But I find in Albert's discussions neither a detailed explanation of the difference between these two differing treatments of the bridge principle problems nor an explanation of why he needs these two parts.

In regard to ethics he is in agreement with analytical philosophers who hold that descriptive sentences can be true or false, but normative sentences cannot. Normative sentences are not cognitive but value standards or

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[282] William Warren Bartley III, "Theories of Demarcation between Science and Metaphysics", in: *Problems in the Philosophy of Science*, ed. by Imre Lakatos and Alan Musgrave, Amsterdam: North-Holland Publ. Co. 1969.

[283] Karl Popper, Postscript to the Logic of Scientific Discovery, Vol. I, Realism and the Aim of Science, Totowa: Rowman and Littlefield, 1983. See John Wettersten, *The Roots of Critical Rationalism*, op. cit. pp. 192ff.

[284] Joseph Agassi, "Popper's Demarcation of Science Refuted" in: *Methodology and Science*, 24, 1991, pp. 1-7.

maxims. In order to judge descriptive sentences we have the methods of science at our disposal. The tasks of the science are exclusively those of investigating the truth or falsity of these sentences. But this does not mean that social, ethical or normative problems remain fully untouched by the sciences. One can bring the descriptive sentences in contact with normative sentences with “bridge principles”. One can accept as a bridge principle, for example, the normative sentence that one should not do, what one cannot do.[285] And through empirical, scientific research one can determine what one can and cannot do. In this way one can integrate results of scientific research in nonscientific discussions. An important consequence of critical rationalism is, of course, the maxim: One can never know what one can, before one has tried. From this point of view it seems to always be a challenge to find new knowledge, which can correct our ethical perspectives.[286]

With his theory of “bridge principles” Albert did not consider the bridge problem to be solved: We also need to bring philosophical anthropology into consideration. Here Albert defends a point of view—as far as I can see—which is identical to Agassi’s. He emphasizes that metaphysics and/or philosophical anthropology should be used to develop new perspectives which can then lead to empirical theories.[287]

In order to clarify the logic of the problem situation we can compare three perspectives: Popper’s, Albert’s, and Agassi’s. Popper had recognized the value of metaphysics. But he saw this value as above because it filled an

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[285] Albert would not say that this statement is true, but rather that is a recommendable Maxim, because he holds, that ethical statements can be neither true nor false. A “bridge principle” such as “Should implies can” is such a statement. It deals with standards of value or maxims, which actions should adhere to. See HIans Albert, "Ethik und Meta-Ethiik“ in: *Konstruktion und Kritik*, Hamburg: HIoffmann und Campe, 1972, pp. 127ff. A part of his theory of the rationality of “bridge-principles” is also the part of his theory of the rationality of ethical principles. This theory does not treat metaphysical or philosophical-cum-anthropological or nonscientific statements which are not ethical ones. But this is not in the sense of limiting the interaction between philosophical or anthropological and empirical theories.

[286] On this point a careful reader of Albert should be bothered a bit. The statement “Should implies can” is regularly used to sympathetically portray the behavior of the Germans under the Nazis. One sees here that such a sentence cannot be taken out of context. Philosophical anthropology must be brought in to organize the rational implementation of such principles.

[287] Hans Albert, *Traktat über rationale Praxis*, op. cit. Kap. II.

important heuristic function is science. It served, namely, as a source of ideas, which could be developed in science.

Albert wanted to extend this theory in that he tried to describe the interaction between scientific and nonscientific points of view. For some fields he called in his theory of “bridge principles”. In some of his inquiries this theory is the core of his treatment of bridge problems because they treat important fields. And it appears to presume separations between various fields. The conception of “bridge principles” plays an important role in his theory because the demarcation between descriptive and normative sentences as well as the importance given in the German speaking world to the application and rationality of normative sentences. Because his major competitors fused together descriptive and normative sentences he could clearly distinguish his own approach from theirs.

In addition to his theory of “bridge principles” he had developed a theory which was as good as identical to the view developed by Agassi. Both thinkers recognized that Popper’s theory of metaphysics and philosophical anthropology had to be reworked: Metaphysical and philosophical anthropological theories do not merely serve as heuristic, but also as research programs, which interact with sciences.

On my view Albert’s two sided treatment of bridge problems is too complex. When we recognize the role of philosophical anthropology in the social sciences, we do not need any “bridge principles”. Without such a theory there is no need for any rational appraisal of “bridge principles”. When we add the theory of the role of philosophical anthropology in the social sciences, we do not have any special “bridge principles”. The “bridge” is then already at hand and can be used to reach an improved cooperation between sociological and philosophical anthropological research. The theory of rationality can, for example, also lead to changes in ethics.[288] A further reason to do without a theory of ‘bridge principles’ is that such a theory gives the impression, that a sharp separation between philosophic anthropological theories and scientific theories is possible, which can only be overcome by a special type of principle—"bridge principles”. This impression is not in agreement with Albert’s philosophy.

When the task is to construct a realistic theory of the social sciences requires a theory of the role of philosophical-cum-anthropological theories,

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[288] For a suggestion in this direction see John Wettersten and Joseph Agassi, “The Choice of Problems and the Limits of Reason”, op. cit. pp. 293ff.

this task cannot be carried out with the help of 'bridge principles' alone. "Bridge principles" only play a role when we solve nonscientific problems with the help of scientific theories and/or when we want to critically connect sciences. In such cases the sciences work completely independently of such principles. The "bridge principles" are only brought into play when a connection between already obtained scientific results and nonscientific problems and/or connections between sciences should be created.

Sociology and philosophical anthropology.

The opinion defended by Agassi and Albert of a close relationship between philosophical anthropology and science must also be applied to sociology. Just how this application of their view to sociology looks needs to be clarified. On this point of view a close relationship between philosophical anthropology and sociology advantageous for methodological reasons. But, do various theories of rationality exclusively build various frameworks within which social scientific research can be conducted? Or can they in addition to subjects of social scientific research? There are indications that (1) a closer relationship and advantageous connection as that which has up until now been noticed between philosophical anthropology and sociology can be constructed and (2) new empirical studies of rationality can be conducted which fit well into the views of methodology and of rationality suggested by Agassi and/or Albert.

As has been elucidated above Agassi has presented a developed theory of metaphysical research programs which can play important roles in the choice of problems in the natural sciences. He extended this theory in that he explained philosophical anthropological theories can play the same role in the social sciences as metaphysical theories can play in the natural sciences. Whereas the metaphysical theories explain the nature of the world—examples are Newton's atomistic theory and Faraday's field theory—philosophical anthropological theories explain the nature of humans. Examples of such theories are the perspective that humans are mere machines or that they are mere functions of their societies.[289]

If philosophical anthropological theories should play this role in the social sciences, the discussion of both kinds of theories needs to be discussed in close working relationships. First the basic assumptions of social scientific

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[289] See Joseph Agassi, *Towards a Rationality Philosophical Anthropology*, op. cit. capitols 1 and 2.

explanations need to be clearly formulated. Among other qualities they offer answers to the question of how social phenomena should be explained.[290] When one proceeds in this way, the influence of nonscientific theories on scientific ones is not limited to practical factors such as political ones, but extends to interests in knowledge: One wants to achieve deeper knowledge of the nature of man.[291]

The close connection between the social sciences and philosophical anthropology appears, then, above all necessary if the social sciences want to develop realistic theories, that is, when they want to shed light on the nature of societies. It has been determined that a realist philosophy of science has to take into account of the role of metaphysical and/or philosophical anthropological theories in the formation of problems and the construction of theories. One can, of course, try to block this role, to explain it away, or to construct impassible borders between fields. But with the help of philosophical-cum-anthropological theories, which can serve as background for the interpretation of social scientific theories, it is easier to construct more convincing theories concerning the fundamental connections in social reality as well as to critically judge them. There are, then, reasons, to prefer this role and to build it up in order to further social science itself.

That criticism of nonscientific theories can be furthered through contact with the sciences appears less problematic as the claim, that the sciences themselves can be furthered with such contacts. It is obviously convincing to inquire whether a metaphysical theory contradicts a scientific theory, as Bartley and others have pointed out. But this widely accepted approach is not the only one, which we have at our disposal. As has been explained above, Agassi has, for example, that criticism is possible in a further way. We can, namely, evaluate metaphysical theories on the basis of their ability to interpret scientific theories.

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[290] For a discussion of the relationship between theories of rationality and formulation of problems see John Wettersten and Joseph Agassi, "The Choice of Problems and the Limits of Reason", in: *Rationality: The Critical View*, op. cit., ppl 281-296.

[291] This perspective is in agreement with Albert's. He emphasizes that science is embedded in society and that social and/or political problems direct its development. This direction can be a product of philosophical anthropological theories. See, for example, Hans Albert, *Kritik der reinen Erkenntnislehre*, Tübingen: J.C.B. Mohr (Paul Siebeck), 1987, p. 171. Science has influence on society and the changed society on science. This corresponds to the interaction discussed above.

The sciences can better fulfill their critical appraisals of metaphysical theories, when their research is from the beginning directed by these theories as when they are only called into consideration after the scientific research has been carried out. We can only then appraise the ability of metaphysical theories to interpret when we also have made attempts to use them in the construction of appropriate scientific theories.

This steering of the sciences is only then possible when one abandons attempts to demarcate the sciences. It is above all about the steering of sciences whose methods go beyond their own: The content of the sought for theories will be partially determined by philosophical-cum-anthropological theories. The close connection between sociology and philosophical anthropology opens the possibility to examine theories of rationality with sociological means, in that these will be used to form and to interpret empirical theories.

Is there here something new, which differs from previous methods of proceeding? In sociology various social scientific theories are developed on the foundation of various theories of rationality. This fact is traditionally recognized. But each defender of this or that theory of rationality presumes that research is only then scientific when it is conducted in the framework of his own theory. One presumes, for example, that rationality above all lies in the society, as traditional functionalist theories do. Many have also presumed that rationality lies above all in actions conducted by individuals seeking to achieve their own aims; many have maintained that we understand quite well the nature of this rationality. The presumptions about how individuals act in light of the logic of the situation and specific convictions in order to achieve their aims are taken to be unproblematic. One always presumes that individuals try to maximize their gains and minimize their risks. But when one presumes that (correct, scientific) social sciences are not bothered by differences in opinion regarding rationality, one assumes that man has an accurate theory of rationality, which serves as a framework for social scientific research. Under such circumstances there would be no need to study the problems of rationality in the social sciences. These problems would be solved prior to the conduct of social scientific research. If all (proper) social scientific researchers were convinced of the same assumptions one could keep problems of rationality out of social scientific research.

In contrast to such approaches the use of philosophical-cum-anthropological theories, which is preferred here, envisions the empirical study of assumptions of rationality. It should be investigated how the rational practice introduced by these assumptions functions. This sociological inquiry of theories of rationality can be easily brought into agreement with Albert's perspective. He emphasizes, that science also has sociological aspects and that it pays off these to study from a sociological point of view. Science must also be seen as part of society. The relationship between science and society raises interesting problems.[292]

It is not controversial that there are various interesting theories of rationality. There are also various methodological suggestions; there is, however, no common opinion about which one of them should be institutionalized. These differing methods and/or methodological rules and/or rules of rationality play important roles in modern societies. The differences between them can also be relevant for fundamental questions of rationality.[293] It seems clear that it can be useful these aspects of science to investigate from a sociological point of view. An understanding of our society presumes that we understand such methods and/or rules. We should understand these rules and the consequences they have.[294]

The fact that there are controversial points of view about which methods have which consequences, that these problems are relevant today, can be shown in the following way with examples from the sociology of science. We find there various perspectives with considerable differences concerning the correct scientific methods, the methods, which are in fact used, as well as the consequences of using varying methods. As differing analyses show, these differing opinions should be analyzed as consequences of research programs which have both philosophy of science and sociological aspects. One can show this with examples of the controversial

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[292] Hans Albert, *Kritik der reinen Erkenntnislehre*, op. cit., Kapitel V, Erkenntnis, Kulur und Gesellschaft, p., 144ff. See footnote 24.

[293] John Wettersten, ”Styles of Rationality”, in: *Philosophy of the Social Sciences*, Vol. 25, No. 1, 1995.

[294] Hans Albert has pointed out that Ernest Gellner among others has shown how interesting such inquires can be. Hans Albert, *Kritik der reinen Erkenntnislehre*, p. cit. p. 144. See for example, Ernest Gellner, *The Legitimation of Belief*, Cambridge University Press, 1974. *Thought and Change*, London: Weidenfeld & Nicolson, 1965, *Relativism and the Social Sciences*, Cambridge: Cambridge university Press, 1985. See John Wettersten, Ernest Gellner: A Wittgensteinian Rationalist, Review of *Cause and Meaning in the Social Sciences, The devil in Modern Philosophy Contemporary Thought and Politics* and *Legitimation of Belief*, in: *Philosophica*, Vol. 8no. 4, 1979, pp. 741-769; Republished in: *The Social Philosophy of Ernest Gellner*, ed. by John A Hall and I.C. Jarvie, Rodopi.

points of view of Michael Polanyi and Joseph Agassi.[295] One can also investigate, which consequences the simultaneous use of differing methods has, as I, for example, have done in my analysis of adventurous and conservative research methods.[296] When through a glance at the general society, we will easily notice various fields in which such inquires could be interesting and/or have already been conducted.[297] Methods and appraisals of the consequences of technologies or art, how we deal with them, are obviously important fields for such analyses. Thereby we need social technologies, which have been developed in frameworks of theories of rationality.[298] If we want to further follow the theory and practice of such investigations we need to develop a close cooperation between various theories of rationality and sociology.

The possibility of theoretical sociology within the borders of social scientific research proposed by Popper.

Popper's inquiry into social scientific methods leads to results, which, at the first look, have strong negative consequences for the social sciences. These results are, indeed, so negative, that it seems that there is scarcely

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[295] John Wettersten, 'The Sociology of Scientific Establishments Today', in: *British Journal of Sociology*, Vol. 44, No. 1 1993, pp. 68-102. Joseph Agassi, *Science and Society: Essays in the Sociology of Science*, *Boston Studies in the Philosophy of Science*, 65, Dordrecht: Reidel, 1981. See especially 'Sociologiism in the Philosophy of Science', pp. 85ff.

[296] John Wettersten, 'On Two Non-Justificationist Moves', in: *Synthese*, Vol. 49, 1981, pp. 419-421. Republished with erroneous title 'On Two Non-Justificationist Theories', in: *Rationality: The Critical View*, eds. I.C. Jarvie and Joseph Agassi, op. cit. pp. 339ff. 'On Conservative and Adventurous Styles of Research', in: *Minerva*, Vol. XXIII, No. 4, Winter1985 pp. 443-463.

[297] From the point of view of critical rationalism it is interesting to ask, whether an increased emphasis on problems could improve the standards in various fields. As a preliminary inquiry for carrying out this task see John Wettersten, 'Against Competence; Towards Improved Standards of Evaluation of Science and Technology'. In: *Nature and System*, Vol. I, No. 4lk 1979, pp. 245-256. See also Joseph Agassi, 'Minimal Criteria for Intellectual Progress', *Iyyun*, Vol.43, 1994, pp. 61-83.

[298] See Joseph Agassi, *Technology, Philosophical and Social Aspects*, Dordrecht: D. Reidel Publ. Co. 1985. In this book Agassi develops an integrated picture of the social philosophy and sociology of technology. He pleas for a stronger emphasis on social technology in connection with physical technology. See John Wettersten, New Problems in Technology: A Comprehensive Analysis, review of *Technology, Philosophical and Social Aspects*, by Joseph Agassi, in: *Zeitschrift für allgemeine Wissenschaftstheorie*, Bd. SXVIII, Nor. 1-2, 1987, pp. 322-331.

any place for theoretical sociology. Popper suggested himself that sociological laws in contrast to natural scientific laws are often trivial.[299] In the social sciences it is really a matter of applying these laws in order to understand historical events and the unintended consequences of rational actions. When a suggestion for theoretical sociology from the perspective of critical rationalism is proposed, one needs to pose the question, whether the proposed research remains within the boundaries set by Popper or whether it goes beyond them.

Popper's rather pessimistic attitude toward theoretical social sciences is based in an excellent criticism of the philosophical-cum-anthropological theories, which then served as the foundation for the formulation of social scientific laws. Two aspects of this analysis are worth mentioning here. The first is Popper's criticism of so-called historicism, that is, of all those theories, which claim to have found some law of historical development. He shows among other points, that these theories cannot account for either the growth of knowledge or natural events. His criticism was, of course, developed in the context of the idea, that individuals can pursue their goals by applying new and unpredictable ideas and decisions.

A second aspect of Popper's analysis of social scientific methods and/or philosophical-cum-anthropological theories in the social sciences is his analysis of so-called functionalism. He shows here that a society cannot be viewed as an organism, which has its own aims and laws. Such a theory can neither explain the actions of individuals nor their consequences, as, for example, I.C. Jarvie has demonstrated in a convincing way.[300]

In German sociology after the Second World War there were extensive efforts to construct a comprehensive theory of society, which at the same time would be a moral criticism, a convincing theory, and an empirical sociology. Under the influence of Popper and Albert one gradually distance oneself from the historicism and functionalism. But one tried as before in the framework of the theories of this tradition. It was above all Albert who clearly showed that this new theory of society could not be

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[299] Karl Popper, *The Open Society and Its Enemies*, Vol. II: London: Routledge & Kegan Paul, 1962, pp. 261ff.

[300] I.C. Jarvie, *The Revolution in Anthropology*, London: Routledge & Kegan Paul, 1964, and 'Theories of Cargo Cults, A Critical Analysis', in: *Oceana*, Vol. 34, pp. 1-31; 108-136. In this essay Jarvie shows that the explanation of so-called cargo cults demands taking social and individual aspects into account.

upheld.[301] He showed among other points how deeply these new proposals were driven by assumptions, which had been previously effectively criticized. He also convincingly showed why sought for comprehensive theory of society could not be attained.

But his positive program had little to contribute to finding new paths to sociological theories. (I leave here open how far sociologists may not have noticed fruitful ideas.) As a consequence of these developments sociology in Germany became divided between those who concentrated on empirical research but had little interest in theoretical sociology and gave it little hope and those who rather pursued a philosophical social theory without any capacity to connect it to empirical research.

A connection between the philosophy of science and sociology with the aim of researching the standards of rationality offered a new, interesting alternative which maintained the borders of social scientific research which had been drawn by Popper. It presumes neither the existence of historical laws nor a functionalist view of society. The problems of how various measures of rationality function can nevertheless be posed. In this way the intuition of functionalism concerning the aim of social scientific research can be taken into account, though with the modification suggested by thinkers such as Robert Merton and Ernest Gellner that one also needs to examine how functionalism fails.

A consequence of Popper's analysis was the sharpening of a dilemma. It looks as if the social sciences must either explain away the quest for scientific laws of individuals' rationality or they have to find it very difficult to formulate social scientific laws.[302] But the quest for social scientific laws is an important example of exercise of rationality by individuals. This seems to lead to a paradox: The assumption of rational action explains away such actions.

This dilemma arises in the following way: When one explains social phenomena as consequences of rational actions, these actions are themselves not predictable. It regularly happens that individuals employ new knowledge and/or new strategies. No social law can take account of these advances in knowledge and/or new strategies. Thereby there cannot

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[301] Hans Albert, ‚Ein hermeneutischer Rückfall: Habermas und der kritischer Rationalismus', in: *Kritik der reinen Hermeneutik*, Tübingen: J.C.B. Mohr (Paul Siebeck) pp. 230ff.

[302] John Wettersten, 'How is Rational Social Science Possible?' in: *Methodology and Science*, Vol. 15, nol. 1, 1982, pp. 35-53.

be any social explanation of them. When one presumes the opposite, that there are social laws, these laws must preclude such unexpected events from happening, more accurately said, they cannot occur in those areas described by laws.

One can try to avoid this dilemma in that, just as Weber, one distinguishes between rational actions which happen within some established framework and arational actions which happen outside of such a framework. Weber's theory provides space for rational actions only within some established framework. A consequential rational action, one that changes the framework, can only be carried out by a charismatic leader, and his action is arational. Weber's theory of social change is correspondingly very low in content. The biggest changes are the consequences of actions by charismatic leaders and their actions are arational.

Popper's methodological individualism emphasizes, that the social consequences of individual rational actions must be explained. He also leaves little room for theoretical sociology. Social theories are only with difficulty brought into compatibility with rational actions of individuals. Such actions constantly change social conditions in unpredictable ways. The sociological study of institutionalized rules of rationality opens a space for theoretical sociology without explaining away the rationality of individuals in any way.

Methodological Individualism Offers no Adequate Framework for Social Scientific Explanations.

Critical rationalism has two aspects which are closely tied to one another—the theory of rationality and the so-called methodological individualism—which are separately from one another developed. In light of the task here described they are in contrast very closely tied to one another. It pays to pose the question, whether the development of the theory of rationality, which has grown above all out of the investigation of methodological rules, makes it necessary to revise methodological individualism. Just like other theories of social scientific methods methodological individualism has assumed that the theory of rationality offers the stable background of social scientific research. That the rules and/or the methods of rationality could themselves be problematic within the social sciences is on this presumption as good as excluded. On this view rationality consists on the one hand of social scientific methods and, on the other hand, in the unproblematic attempts by individuals to realize their aims. It is thereby presumed that scientific methods and the nature of the rationality of individuals are known.

Agassi's suggestion clarification and/or modification of this theory as one of institutional individualism may build a path to a new, sought for alternative. On Agassi's version, which he presents as a reading of Popper's theory, the institutions are real, although they do not possess any aims. In connection with the individuals' pursuit of their aims, their consequences must be taken into account.[303] The logic of the situation partly touches them and they are not reducible to the interactions between individuals. They must be considered as independent factors when one wants to explain sociological phenomena as consequences of attempts by individuals to achieve their aims.

Even when institutions are regarded as real, that is, as not reducible to the convictions and actions of individuals, it might be that our knowledge of them is trivial, as Popper perhaps thought. We would then need to take them into consideration but we do not need to develop any new theories of them. More interesting is the possibility that, not only are explanations of individual actions possible, but also theories about institutions, about their nature and consequences. This alternative does not contradict Popper's theory. But it requires a further development. Albert suggests that such knowledge can be achieved by the application of his version of philosophical anthropology and national economy. Gellner's suggestion is close to the task suggested here and Agassi had, for example, a theory of the logic of extremism developed, which also begins a path in this direction.[304] But there is here a further possibility to enrich institutional individualism with help from the new results in the theory of rationality. A theory of the institutional standards of rationality could bring us forward, in that it could explain the close connection between various theories of rationality and rational practice, which might have desirable consequences for both sides.

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[303] Joseph Agassi, "Methodological Individualism", in: *British Journal of Sociology*, 11, 1960, pp. 244-170. Joseph Agassi, "Institutional Individualism", in *British Journal of Sociology*, 26, 1973, pp. 144-155. These two essays are republished under the title, "Methodological Individualism and Institutional Individualism", in *Rationality: The Critical View*, op. cit. See also I.C. Jarvie, *Concepts and Society*, London and Boston: Routledge & Kegan Paul, 1972. Jarvie, *Thinking about Society*, Dordrecht: D. Reidel Publ. Co., 1986.

[304] Joseph Agassi, "The Logic of Consensus and Extremes", in: I.C. Jarvie and Fred D'Agostino, eds., *Freedom and Rationality*, Dordrecht: Kluwer Academic Publishers, 1989.

But such a theory has to distance itself from methodological individualism insofar as it makes rationality itself an object of research. It is not a question of explaining the consequences of individual actions as results of attempts by individuals to achieve their aims. Beyond this task the methods of rationality they apply should be studied with sociological methods. Such a theory begins with a close connection between the philosophy of science and sociology. The identification of various standards of rationality as well as theories of their consequences presume that we have theories of how specific social standards can contribute to finding solutions to problems of expanding our knowledge and/or other problems.

To what degree is the proposed task sociological and to what degree is it a problem for the philosophy of science?

On the task proposed here theories of rationality should be empirically examined. This task requires a connection between general theories of rationality and rational practice. In order to find such connections one can formulate new empirical theories as bases for already existing theories of rationality, one can interpret already existing empirical theories as applications of theories of rationality, and/or one can formulate new theories of rationality. I only want to say here, that, from the point of view of critical rationalism it is interesting to point out such connections and to make them as useful as possible. Insofar as such inquiries further knowledge will be first determinable through further studies.

Unfortunately it is not possible here to treat this theme more deeply and to develop further examples. I have, however, mentioned various examples in which results have already been obtained. The work of Gellner belongs to the most important of these. But in his analysis of rational practice he assumes principles which are in conflict with the assumptions suggested here.[305] He views rationality as largely set by societies and as scarcely possible to improve with rational methods. But in this regard I also cannot diverge further here.

Albert between Popper and Weber.

Max Weber hat social scientific research fundamentally influenced. His studies made a significant contribution to our understanding of rational human action. But it also has important difficulties, because it strongly

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[305] See John Wettersten, "Ernest Gellner, A Wittgensteinian Rationalist", op. cit.

limits rationality and offers no convincing explanation of social change. Especially after the Second World War one began to take an interest in these difficulties.

In his model of social scientific research Weber portrays rational individual action as the attempt by individuals to realize their aims within socially established frameworks. But such frameworks are not produced by rational actions. They find their content through the action of charismatic leaders (in Nietzsche's sense) whose actions are arational. Weber's perspective provided significant progress in the analysis and explanation of individual actions within a given framework, but the institutionalized acceptance of such a framework by individuals of a society remains irrational. Thereby no clear explanation of social change is possible.

A tension arises between the arationality of a social order and the rationality of individual actions in such a social order. The tension between the arationality of social orders and the scientific claim to be rational was made still stronger when one saw, that Hitler had to be seen as such a charismatic leader and his actions as irrational whereas only the actions of his followers could be explained as rational.

Popper took on the difficult task of improving Weber's theory in that he removed the theory of charismatic leaders while maintaining the theory that human actions can be explained as rational; the latter is his methodological individualism. He thereby achieved important results; some of them have been explained above. But he failed to completely solve the problem of the borders of rationality. He had, namely, viewed the acceptance of rationality as irrational. He had tried to construct a theory of social change, which explained such changes as unintended consequences of rational actions, that is, of actions with which certain aims in light of the logic of the situation should be achieved. That this theory required institutional as well as individual factors became clear through the work of Jarvie and Agassi. But even this clear portrayal could not completely solve the problem of understanding the rationality of social changes and/or social orders. There was still a place for the study of changes of institutional standards of rationality.

Albert also made an attempt to further develop Weber's theory.[306] He showed which changes are necessary in order to make Weber's perspective

[306] Albert and Agassi posed differing problems concerning the study of Weber's theory. Whereas Albert tried to show to what degree Weber's perspective could

a foundation for modern social science. He showed, thereby, on the one hand, which changes in national economics are needed if Weber's approach is to be taken into account. Normative assumptions should be removed and the explanations of rational actions should not be exclusively deemed attempts to realize personal aims but also as results of given social orders. The independence and/or the irreducibility of social factors should be thereby rendered clear. In addition the hierarchical assumptions of Weber's theory should be removed. Among the assumptions to be removed are Weber's theory of charismatic leaders and his theory of social orders which constitute a given context for individual rational actions. The perspectives of Popper and Weber still allow for viewing rationality as a non-universal institution. The established forms of appraisal of theories and problem formation are examples of this possibility.[307] In place of Weber's theory of charismatic leaders Albert employs a new theory of social order which allows, for example, explanations of political changes of social frameworks as a result of rational criticism.

An important aspect of this approach to social scientific research lies in the emphasis of the irreducibility of social and/or institutional factors to individual factors and the irreducibility of individual factors auf social and/or institutional factors—view that Agassi and Jarvie have also developed. A second important aspect lies in the assumption that rationality is social and can have various forms.[308] This can be furthered

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still serve as a model for social scientific research, Agassi attempted to show which problems in Weber's theory could be overcome. Albert honored Weber by modernizing him; Agassi in contrast analyzed his historical role. See Joseph Agassi, Bye-Bye Weber, in: *Methodology and Science*, Vol.21, No. 1, 1991.

307 See the essays by I.C. Jarvie and Joseph Agassi in: *Rationality: The Critical View*, ed. by I.C. Jarvie and Joseph Agassi, op. cit. Part III, Rationality and Irrationality, pp. 361ff.

308 Hans Albert, *Kritik der reinen Erkenntnislehre*, op. cit. He says there: "Wenn man ein besonderes Merkmal institutioneller Natur anführen soll, das dies Züge der abendländischen Sonderentwicklung gemeinsam kennzeichnet, dann ist es die Institutionalisierung von Konkurrenz und Kritik, die die Lebensordnungen europäischen Ursprungs in hohem Grad auszeichnet (….) Vor allem Max Weber ist die Einsicht zu verdanken, maß man den institutionellen Aspekt dieser Problematik nicht vernachlässigen darf, wenn man den Formen der Rationalität, die sich in der Geschichte der verschiedenen Kulturen finden, gerecht werden will (…) Unter verschiedenen sozial-kulturellen Bedingungen ergeben sich, für rationales Verhalten ganz verschiedene Arten, Problem zu lösen" (pp. 157-158).

> "When a special feature of an institutional nature should appear, which characterizes the special development of Western society, then it is

when we integrate various forms of rational practice in the institutional factors and make objects of social scientific research—when, for example, we pose the question what consequences specific methodological suggestions have when they are institutionalized. Methodologies and conceptions of rationality, on the one hand, and social scientific research, on the other hand, need to be treated as two aspects of the same task. The social scientific inquiry of institutionalized methodological proposals for the practice of rationality will then be an important task for critical rationalism and social science.[309]

institutionalization of competition and criticism, which characterizes to a high degree the ways of life of European origin. We have above all Max Weber to thank for the perspective that one cannot ignore the institutional aspect of this form of rationality, which is found in the history of various cultures. In various forms of social-cultural conditions quite different types of rational approaches to solving problems arise."

[309] I am thankful to Joseph Agassi and Hans Albert for detailed criticisms of an earlier version of this essay. Through discussions with both of them a great deal became clearer. This does not, of course, mean that either of them agrees with my results.

3e. 'Popper and Sen on Rationality and Economics: Two (Independent) Wrong Turns Can Be Remedied with the Same Program', in Zuzana Parisnakova and Robert Cohen, eds., *Rethinking Popper* (Berlin. Springer, 2009): pp. 369-378.

Popper and Sen on Rationality and Economics: Two (Independent) Wrong Turns Can Be Remedied with the Same Program

Abstract:

Karl Popper and Amartya Sen have developed social theories which are very close to each other, though neither has taken notice of the other. The independent programs they propose for the development of their theories go astray, because they build on standard economic methods, albeit in different ways. A better approach for the development of each program can be found by using Popper's important, but in the methodology of the social sciences hitherto ignored discovery, that rationality is social. Important contributions of Sen to economic theory may then be developed in ways which also contribute to Popper's social theory.

1. An alternative to Popper's approach to the methodology of the social sciences is needed.

Although Popper said that economics was the best social science, the rationality principles which economists use are quite unrealistic: they presume (1) that individuals act in ways they cannot act at all, (2) a very narrow view of the aims of actions, and (3) that the sum of individual rational actions in a free market is a well-functioning system. Popper rejected these assumptions, but he nevertheless tried to save a version of the rationality principle, which was close to versions of it used in economics.

Popper took a clear position against (1) all functionalist versions of social scientific research, that is, all theories which presumed that societies were entities which could be treated as functional wholes which obeyed their own laws, against (2) all theories which sought historical laws of social development, above all, Marxist theories, and against (3) all those theories which claimed that social scientific theories had to be constructed with interpretative methods designed to look at events from "within".

In *The Open Society and Its Enemies* Popper developed a social philosophy which grew out of his studies of knowledge. But he remained comparatively silent on individualist social science, especially economics, such as that practiced by his friend and supporter Hayek. He set the prime task for social science to be the discovery of the unintended consequences of rational action and he defended methodological individualism as the proper method for the social sciences. As Jarvie has pointed out, he portrayed his theory of science as social. (Jarvie 2001) Although Popper later praised economics as the best social science that we have, he did not explain himself. The closest he came to doing so was his defence of the use of the rationality principle in social science, a defence which raised difficulties, since he argued that this principle must be dogmatically assumed even though it did not correctly describe much of human behavior. (Popper 1985) His argument is not only convoluted; it diluted his fallibilism and his realism (Wettersten 2006, pp. 45ff.).

A different approach to this nest of difficulties is to use new normative theories of rationality developed by Popper's followers to develop new descriptive theories of rational behavior. In the light of these theories, we may ask, can rational behavior be explained in some better and more hopeful way than the established rationality principles allow? (Wettersten 2006, 2007b)

On this approach the prime task of economic theory is the study of the consequences of rational action in institutional contexts, that is, the study of how specific institutions steer events by shaping the problems individuals pose, the solutions they select, and their critical methods for appraising both. This proposal fits far better not only with Popper's thesis of the limits of rationality and the need for the social sciences to discover unintended consequences of social policies, but also with his important thesis that rationality is social. It also fits quite nicely with Sen's studies of the needs of individuals to control their own fates and to take their institutional context into account. It extends the range of events which may be explained as rational in a realistic way, as Sen also wants to do.[310]

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[310] Popper's theory that science is social grew out of the need to add methodological rules to a study of the logic of research. He discovered this need after he wrote his first philosophy of science. In his first view—*Die beiden Grundprobleme* without Chapter V—he presumed that basic statements were veridical, and he ignored the possibility of ad hoc defences of theories—as Reichenbach pointed out to him. (Reichenbach 1930-31) He then added methodological rules to remove difficulties which arose for his first view.

2. Popper's defence of the rationality principle ignores his most important contribution to the theory of rationality.

Popper maintained that rational thinking is a social process of making conjectures and criticising them so as to improve them. This process enables individuals working together to improve the knowledge of all. Without it no science is possible. But in his philosophy of the social sciences he left aside his social theory of conjectures and criticism. Other than its appearance as a warning that even rational action can go astray and as a recommendation that social scientists should investigate unintended consequences of actions, it is not treated as relevant to social scientific explanation. He restricted social science to tracing the consequences of individuals pursuing aims and following plans in accordance with their beliefs. This stance brought him very close to economists, where he wanted to be, but it failed to integrate his fundamental discovery, that we learn by criticism in interaction with others, into his methodology of the social sciences. Why should we ignore this crucial fact about learning and society in developing social scientific theories?

(Wettersten 1985, 1992, 2005) As Jarvie has recently emphasized, Popper developed his view of science as social in *The Open Society and Its Enemies*. (Jarvie 2001; Wettersten 2006b) Joseph Agassi introduced the idea that rationality is partial (Agassi 1981) and Jarvie and Agassi together have developed this view by explaining the rationality of magic, dogmatism, and irrationalism. (Agassi and Jarvie 1987) But they do not make the study of varying rational practices in various institutional settings a task for the social sciences. The best description of Popper's theory of social scientific research in the sense of being the most sympathetic to him is Agassi's. (Agassi and Jarvie, 119-150) But he also ignores too much Popper's thesis that rationality is social. On my view in contrast to Popper, who said we should put as much as we can into the heads of individuals to construct social scientific explanations, we should put as much as we can into institutions to explain how they steer events. This should be done by, on the one hand, studying how institutions shape the problems individuals seek to solve, how they influence the content of solutions individuals choose, and how they enable or hinder the critical appraisal of both and, on the other hand, using results of such studies to explain consequences of institutional arrangements. Agassi's study of medical diagnosis carried out with Nathanial Laor is an example of the research which the program here suggested advocates: They study social rules of diagnosis, the problems they pose for individuals, and the consequences of adhering to them. (Agassi and Laor 1990) A further example of such a study is Michael Segré's portrayal of the decline of science in Italy after Galileo as a consequence of the institutions of the time. (Segré 1991) For further discussion see (Wettersten 1996; 2006a; 2006c; 2007a; 2007b).

On the face of it, it seems that Popper desired to offer a methodological approach which would be simultaneously consistent with his own philosophy of the natural sciences, on the one hand, and with neo-classical economic research on the other. The result is curious. It does not merely ignore his significant discovery that rationality is social and critical, but it also is quite convoluted. He asserts that the rationality principle is “almost empty”. But it is hardly clear what that means. It sounds very positivistic where “empty” can mean “non-empirical”. But Popper does not accept any such theory. Social scientists should never deem the rationality principle refuted; they should presume it is true when constructing models of social situations. The models, he says, should describe reality. For this reason he claims to be offering a realistic theory of the social scientific research. But he apparently views model construction itself as a rather ad hoc procedure: one seeks in various situations to build models. He gives no theory about whether models should be connected with each other, thereby building more comprehensive social theories, or how one chooses which social situations should be modeled.

Though at first glance Popper’s theory may appear to be internally inconsistent and/or inconsistent with his theory of the natural science, it is neither. But it is very complex and says little that is useful about how to do social science. It is above all a defensive effort to reconcile his philosophy of the natural sciences with established economic methodology. Far more progress can be expected if we look for conflict as well, and then ask what will have to give.

3. Sen’s two-sided view of research in economics.

Before correcting Popper’s proposal for methodology in the social science by incorporating his view that rationality is social, on the one hand, and an explanation of how this correction may be used to develop Popper’s own social theory, on the other hand, I turn to the economic theories of Amartya Sen: He has developed virtually the same social theory as Popper and has also provided a poor approach to developing his theory because he clings to established approaches in economic theory. The social theories of Sen as well as those of Popper can be developed with the same methodological approach because their perspectives are so close to each other, as I explain below.

Sen has observed that there are two kinds of research in economics today. On the one hand economists use rationality principles to construct elegant mathematical models. These models may presume the existence of systems in, or moving towards, equilibrium or they may attempt to describe a society with a proper distribution of goods—a so-called welfare function. On the other hand economists take account of social contexts which are not so easily put into the Procrustean bed of neo-classical economic theory. Sen's view of progressive economic societies lies on this side of the divide and is virtually identical to Popper's theory of the open society. His major interest lies in showing the limits of the former traditional side in order to make room for the latter progressive side. The latter is needed in order to understand economic development and to find some acceptable measure of social welfare. He hopes to preserve the ideal of the elegant, formal side of economics by extending it take account of a wider range of rational behavior. Although he finds standard views of rationality limited, he not only does he not reject them; he seeks to save them by extending the standard approach.

One of the crucial limitations of standard economic theory Sen finds is its ethical theory, that is, utilitarianism. In, for example, *Ethics and Economics*, he gives an explanation of why the assumptions made in ethical theory are too narrow. The standard approach to economics requires that all rational behavior consists solely of attempts by individuals to maximize utilities. The utilitarian approach is thus needed in order to develop models of economic systems. Sen stresses that only a wider view of the rationality of human action can take into account the appreciation of values which individuals exhibit as well as their desire and ability to act autonomously by choosing their own actions. Actions are often pursued even at the expense of those sorts of well-being which are easily expressed as utilities. He finds that individuals have commitments which are quite different from self-interest and these commitments in addition to the pursuit of self-interest guide their planning and choice of actions.

But, after convincingly arguing for this point of view, he adds that he hopes to extend the ethical theories of economics rather than to replace them. People do seek to maximize utilities, but that is not all they do. Sen does not say how the neglected aspects of moral, rational human behavior should be integrated with those that are taken into account in standard theories. But he emphasizes, that he and others are working on the project of developing a more comprehensive and coherent view.

When discussing how we can tell whether individuals are rational, he offers no extension of standard views. He simply proposes that individuals are rational if they have subjected their views to critical scrutiny. And when discussing justice, he does not hope to have a precise theory of the just society. But he says it is sufficient if we can say that some conditions are quite unjust. A society which tolerates famine is unjust.

If we look at the content which Sen places on the two sides of the division, he describes (an imprecise description of economic behavior on the one hand and the formal apparatus used to develop economic models on the other) we can see that a new framework, not merely an extension of the existing framework is needed. The theory of rationality on the mathematical side is too narrow to take into account of the description of actual economic behavior. On the informal side of Sen's divide we find such factors as the interest of individuals in controlling their own fate, their interest in both the process by which decisions are made and their autonomy in setting the direction they choose, and the need to take into account how real economic systems are regularly mixed, how, for example, family based economic conditions interact with markets in specific societies. In his discussions of the elegant side of economics Sen discusses above all how theories are limited, because they do not take such factors as these into account. The theory of individual decision making does not take into account the importance for individuals of the process of decision making and attempts to find a social welfare function do not take into account the value which individuals place on the process by which decisions are made. The former should be explained and then incorporated into some proper welfare function, according to his program. When he comes to discussing what should be done, however, he says we need to take the facts more comprehensively into account. He does not say we need a new theory, though he does offer his own theory of the role of freedom in development as an alternative program.

4. Sen's progressive program in economics parallels Popper's ideals.

Sen emphasizes autonomy, rationality as critical scrutiny, social evaluation as the identification of unbearable conditions, the importance of effective institutions for economic activity, the importance of taking unintended consequences into account, and the importance of democracy as a learning process which contributes to economic development. Both Popper's social theory and his theory of rationality fit extremely well with all of these points.

Sen contrasts rational behavior as postulated by standard theories of rationality with the behavior of individuals seeking autonomy. He sees this latter behavior as rational, but only in an intuitive sense. He offers no alternative theory of rationality which explains how and why such behavior is rational. He observes that it is not described by the standard principles of rationality which describe individuals as setting priorities and choosing those which have some desirable combination of the satisfaction of personal utilities and some probability of success. This theory is too narrow, because individuals pursue aims which are not merely personal utilities. They attempt to solve problems in institutional settings which are defined not merely in personal terms but also in moral, family, or other social terms. Individuals have commitments which they use is making their plans.

Sen describes those problems individuals face in attempting to come to terms with their institutional contexts. He describes their desires and hopes to choose direction, rather than to simply have economic alternatives open to them in the sense of having various paths to financial well-being as measured in the amount of goods they have at their disposal. He also takes into account their desire for achievement and autonomy, their desire to solve problems.

The activities he describes are examples of the exercise of rationality as fallibilist theories envision it, that is, it is problem-solving activity which involves learning from mistakes and setting new goals which should solve problems. Sen takes no notice of either this or of the rationale these theories offer for viewing rationality as he does. Indeed, even though his numerous publications contain an unbelievable number of references, he avoids any mention of Popper or fallibilism. He never considers revising the rationality assumptions of established economic theory in order to improve his research program. He merely notes that his contributions do not fit standard theories very well and expresses conviction that seeking to reconcile the differences will lead to progress.

From a moral point of view the activities Sen describes are those called for in a fallibilist moral theory. They are autonomous activities which require that individuals take responsibility for their actions and learn from their mistakes. This moral view goes well beyond the utilitarianism to which economic theory is tied. Sen realizes this and hopes to extend utilitarianism, but this is not possible: the activities he describes are not merely extensions but conflict with the moral judgment of the utilitarians. The normal theory can be extended in some easy ways. But it cannot be treated as a catch-all for all moral perspectives. Sen claims that Nozick and Rawls each takes account of some important moral facts, but each ignores those facts which the other takes into account. But these so-called facts cannot simply be added together and then accounted for in some comprehensive theory. They are statements of competing moral perspectives.

5. Sen's methods cannot reconcile old principles with new results.

Sen hopes to reconcile standard approaches to rationality used by economists with his own innovations in economic theory by extending the former to include the latter. In doing so he uses an inductivist method, pursues the theoretical ideal of a complete system, and presumes a functionalist social theory. In the end he rejects all three as unrealizable: he knows that his inductivism cannot produce a holistic theory of a functionalist society. He suggests that we should approach as near as possible to the ideal until we find Arrowian inconsistencies, a procedure he calls 'brinkmanship'. We may then not have a perfect system, but we will have the best possible system.

Sen's inductivism is evident, above all, in his critical method. This method is to show that current systems do not take specific facts into account; they must be extended to remedy the deficiency. Although he never explicitly states the inductivist assumptions that facts can be gathered without theoretical guidance and that all facts gathered should be incorporated into some ideal system, he offers no standards by which to judge whether some theory which does not take some facts into account should be deemed incomplete. Rather he adds facts he takes to be important. Although he uses his theory to gather and choose facts—when talking about freedom, for example, he argues that it is important for economic development—his criticisms of various alternatives are treated as mere observations that some criticized theory is incomplete. Nozick accounts for rights, and Rawls accounts for distribution of goods, so neither is wrong, but both are incomplete. Becker accounts for human capital as part of the market but neglects the value of freedom itself. His view is not wrong but incomplete. Sen does not explain why he dismisses the normal view that these are simply contradictory theories.

By demanding that a true theory should account for as close to all the facts as possible Sen adds to his implicit inductivism the theory that the true theory will be an all-encompassing coherent system. A theory which accounts for all the facts will, we may presume, describe societies in comprehensive ways. He does not single out which aspects of some particular societies or of all societies he intends to account for. Having no standard to select those facts which should be explained as part of economic systems, and contending that theories are inadequate for not taking some facts into account, the only plausible interpretation of his critical approach is that it presumes that any social theory which fails to take some social facts into account is to that degree inadequate. This approach precludes the construction of adequate theories of aspects of societies. Each proposal should be subjected to the systematic analysis offered by the methods of the elegant side of economics. (He does this in essays in *Rationality and Freedom*.) If it fails to meet these standards it must be extended to avoid inconsistencies.

Any holistic aim in the social sciences presumes a functionalist view of societies. Only societies which are functional, or functional under certain conditions, could be truly described by some comprehensive and systematic social theory. Functionalist social theory has been effectively criticized from both within and without the social sciences, but it is not surprising to find an economist taking it for granted. Neo-classical economic theory presumes that under certain assumptions societies can be described as well functioning systems. Sen is not, of course, satisfied with a merely economic description, because he realizes that no economic description can be adequate which does not take into account the moral dimensions of human beings. Only then can their rationality be properly understood. But this does not lead him to question the functionalist assumptions of neo-classical theory, but rather to call for their further development. In doing so he lands pretty much back where sociology under Parson's leadership was. A complete social system should be constructed which can be applied to describing how each society functions. The only caveat is that no such system is possible, so we try to find out how close we can come to it.

The difficulty facing Sen's program for finding a social welfare function which his moral theory is intended to serve becomes clear at the end of his essay, 'Social Choice and Individual Behavior' in *Freedom and Development*. He suggests that there are three reasons why one should not view the construction of a social choice function to be impossible. He wishes to answer each. The first reason for deeming such a function impossible is that Arrow's results show the impossibility of rational social choice. He suggests Arrow's negative result merely shows that not enough information has been incorporated. The remaining problem is merely one of incorporating enough information. He assures us that this is possible, but no test of this hypothesis is suggested. The second reason for deeming a social choice function impossible is that rational social choice cannot take account of unintended consequences. He suggests that the problem can be resolved if one takes into account the unintended and predictable consequences of social action. He seems to assume that there is no serious problem in identifying these results, and no position is taken about the possibility of unpredictable unintended consequences or what the consequence for theory construction should be if there ae such. The third reason for rejecting the possibility of social choice function is that rational social choice does not take account of human motivation. He suggests that rational choice need not be so narrow as to be restricted to the pursuit of individual interests, that ethics plays important roles in all economic systems. But he does not explain how to extend the standard approach to take that into account.

6. Fallibilism can further the programs of Popper and Sen.

Problems which Sen's studies raise include those of how to improve the opportunity and capacity of individuals to think better, of how institutions impact their capabilities to pose and solve problems, of how critical appraisals about what is to be done can be made, and of how one can cope with unintended consequences of social planning. All of these problems can be handled nicely in the context of a fallibilist theory which recognizes that all judgments are provisional, are made in social contexts which set parameters for them, and are subject to criticism in institutional contexts. These problems grow quite naturally out of the studies of both Sen and Popper. But neither has developed methods for dealing with them within the most promising framework, perhaps the only framework, for dealing with them. This is a framework which builds on fallibilist theories of rationality. Unfortunately both Popper and Sen have concentrated on reconciling their own views with traditional views of rationality and economic methods. Popper has emphasized methodological individualism at the expense of his social theory of rationality. Sen tries far too hard to put them in the Procrustean bed of standard economic theory.

An alternative program may avoid both the problems faced by Popper in his convoluted theory of the use of the rationality principle in the social sciences and those faced by Sen in trying to reconcile his progressive ideas with standard economic theory. This program takes account of (1) how institutions lead individuals to pose problems and to select solutions, of (2) how learning is hindered or furthered by institutions, thereby providing social accounts of rational practice in differing contexts, of (3) how institutions interact either by complementing each other, that is, by improving the ability of individuals to pose problems and solve them, or by hindering them (Wettersten 2006a; 2007b).

This program can be carried out if individual problem solving is viewed from the perspectives of the varying institutions in which problems are posed, solutions sought, and criticism of alternatives developed. Instead of using individual problem solving to explain institutions, we may use institutions to explain how individuals pose problems, how they solve them and how they critically evaluate alternative solutions to them. Institutions are not merely blocks in the road which have to be overcome after problems are posed or roads which make solving problems easy. They determine which problems are posed, which solutions are selected, and how individuals learn. The social theories of Popper and of Sen may be developed by the construction of empirical theories of how various institutions do these things.

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Interlude

The recognition that the practice of rationality is social leads to the conclusion that the study of institutions is far more important than on traditional views. It is not merely describing a logic of a situation, which is quite similar to, say, weather conditions or social relationships or political power structures, but also involves the study of which social rules of the practice of rationality institutions embody and what the good or bad impact the adherence to these rules brings about.

3f. 'Aus dem Irrtum Lernen: Ein institutionelles Problem', in Otto Neumeir, Hg., Was aus Fehlern zu lernen ist—in *Alltag, Wissenschaft und Kunst* (Wien-Munster: Lit Verlag, 2010b): pp. 55-76.

Learning from Error: An institutional problem

When Popper learned from a criticism of Hans Reichenbach, that learning from error in theoretical science is only possible through the application of methodological rules the institutional problem of learning from error emerged. The determination of these rules turned out to be an ignored and difficult problem for the sociology of science. For the social sciences the problem of determining the advantageous rules of rational practice is even more comprehensive and deeper. It has not only to do the sciences, but all institutions. An important example is the problem of understanding the influence of the social sciences on politics. Progress in the solution of the problem, which institutional rules encourages learning from error is needed. Thereby one has to overcome the much too strong and lasting influence of theories which equate rationality with justification. In addition one needs new social scientific methods. They should further the inquiry into the social consequences of institutions rather the consequences of individual actions.

Learning from Error: From commonsense to social rules.

In everyday life we all presume, that we learn from errors. When a person reacts in some unexpected way we immediately correct our false expectation. When we notice that some political party which we support adopts strategies, which we find false, we support some other party. When we notice that products, which we have purchased, have defects, we avoid the company which produced them. Unproblematic examples of mistakes which are not only easy to identify, but also easy to correct can be easily found. They are examples of errors of mistakes which are both easy to identify and to correct. In this sense learning from error poses no problem, and the idea that we in this learn from errors is practiced by virtually everybody, regardless of philosophical perspective.

If we expect more from learning from error as examples such as these illustrate, we in contrast face interesting problems. The idea introduced by Popper, that learning from error constitutes the core of scientific method has been and still is conservatively discussed. Popper (1963; 1982) did not merely defend the trivial hypothesis that we can learn in this way, but also that we can only learn in this way. This perspective radically changes the recognition that we learn from error. The traditional theory according to which scientists learn by providing proofs for various ideas, leads to a theory of learning from error according to which one makes mistakes and corrects them until the truth is found. This is the core of William Whewell's philosophy of science (Whewell 1967a; 1967b; 1971).

Whewell's theory describes a large number of cases of learning from error. One can find in biology a number of examples, Harvey's theory of the circulation of blood, the theory of DNA or even the theory of evolution are theories which can be partially improved and/or refined, but no one thinks that these theories have to be fundamentally changed. The fact that nearly half of all Americans do not believe in evolution is not a refutation of this observation. These theories are not accepted by critical thinkers, because logically impeccable proofs are at hand, but rather because no one can offer a suitable alternative, which has anywhere the explanatory power as the established theories can. If someone could offer such an alternative, one could reevaluate the theory in question (Wettersten 2007b).

Examples of learning from error such as Whewell's proceed without strong borders into the problematical cases raised by Popper's theory. But Popper's theory of learning from error raises new problems. In many cases after the discovery of some error, we have to decide how and what we could learn from an error. And answers to such questions are by no means obvious.

Popper's first suggestion as to how we can exclusively learn from error without reaching some end-point at which we can prove the truth was quite simple: Science only makes progress when theories are refuted (Popper 1979). But he quickly learned that learning in this way was not without further ado so simple. In a critical observation of his publication in which Popper suggested this theory (Popper 1932-33), Reichenbach (1932-33) pointed out, that one can make small changes to an allegedly refuted theory. In this case one learned hardly anything from errors. For that reason Reichenbach had long before rejected learning from error as a theory of science, Reichenbach (1930-31) had maintained instead that there is no logic of research, that is, there are neither proofs nor refutations. One has to make do with computations of probability alone.

Popper did not give up so quickly on his idea, that there is a logic of research, and this logic was the retransmission of the falsity of the conclusion of a valid argument to at least one of the premises of that argument. In spite of Reichenbach's plausible criticism of his first attempt, he tried to explain how scientists could learn from error. In order to do that he introduced various methodological rules in the methodology of science (Wettersten 1985b; 1992; 2005b). In this context the most important rule is that in reply to a refutation one should favor the easiest falsifiable alternative. In this way scientists can insure that one learns as much as possible from mistakes. Ad hoc changes of falsifiable theories, which lessen and/or prevent learning from error, would be blocked. In order to make learning from error in science possible, Popper thinks, we do not merely need criticism and the discovery of mistakes, but also rules which enable us to make criticism and the discovery of errors fruitful.

Popper hoped that these rules could be identified in an unproblematic way. It has turned out, however, that they are by no means obvious. Popper's theory of learning from errors in theoretical science presumes that all learning can be judged in that the predicted empirical observations of each theory is determined and compared with its alternatives. Scientists should always favor the non-refuted theory, which implies the greatest number of falsifiable statements. Popper maintains that all the valuable characteristics of a theory can be reduced to this one characteristic, i.e., falsifiability.

In his historical-cum-methodological inquiries, however, Agassi determined that explanatory power and predictive power could be different. This case occurs, when a theory with a low degree of predictive power can be better embedded in a metaphysical theory than a competing theory with a higher degree of predictive power. The better embedded theory provides a better understanding of real relationships than the theory with a higher predictive power but which is not as well embedded. Agassi suggested this alternative as he developed his theory of metaphysical research programs in the case of Faraday's use of such a program. Faraday's empirical theories created more understanding, because they fit better in his theory that the world consists of waves as the competing Newtonian theories fit in their metaphysical theory.

According to Agassi's research there are in regard to learning from error in science at least two competing strategies with alternative social rules. One strategy recommends that one always favors that theory with the highest degree of falsifiability. The other demands that one develop empirical theories which can be interpreted in a unified way by some metaphysical theory, and even then, when one must accept a loss of falsifiability in order to achieve this aim.

I am not here simply interested in defending Agassi's suggestion. It suffices if one accepts it as a serious alternative. In this case it is clear, the one already finds in theoretical science a social problem, of how one learns from error. In order to find better solutions to this problem, sociology of science must examines various social rules in the practice of science in order to determine to what degree they promote learning from error. We have then an institutional problem which requires social studies of scientific practice.[311]

Two examples of learning from error in the history of science can render this problematic somewhat clearer. When Whewell presented his philosophy of science which observed that science made progress when scientists tried out ideas, criticized them, tested them and refined them, until they found the true idea, David Brewster had an apparently effective criticism. He pointed out that the scientist Robert Hooke defended a theory of light interference, which Newton afterwards rejected, but which Young reintroduced. How could it be that science shoved aside a true theory on the basis of experimental observation, substituted for this theory a false theory, and then finally came back to the theory, which had been previously rejected? (Brewster 1842, 288; Wettersten 2005a, 73ff.).

John Worrall introduced nearly the same objection to Popper (Worrall 1982). According to Worrall a theory such as Popper's according to which false, but not true theories can be refuted, cannot be reconciled with the history of the theory of light. Young developed a wave theory which was rejected in favor of an atomic theory. Afterwards a new wave theory was successful. Popper's descriptive theory of scientific progress appears to be false because it gives no place for the mistaken rejection of a true theory. His practical suggestion for learning from error is not progressive, because it cannot without further ado be correctly applied.

From my point of view, both Whewell and Popper had good possibilities to meet this criticism, although neither did. In answer to Brewster Whewell could have said, that the true theory can first time be discovered, when a theory is adequately developed, as the earlier wave theory of light was not. It was only later, as various theories had been examined, that this process could find an end. Popper could have said in response to Worrall, first, that the first formulation of a fundamental idea was not the same as

311 It has been determined that Popper's treatment of ad hoc hypotheses is all too simple (Wettersten 1984; 1998).

its later formulation,[312] second, that the later theory arose through a series of conjectures and refutations, and third, that the later theory had a higher explanatory power than the earlier one had. This process is to be sure not lineal in the sense that the general assumptions remain unchanged, as the details were worked out, but we learned from our mistakes in that we refuted theories and replaced them with new theories with higher explanatory power.

If one wants to employ these responses to Brewster's criticism of Whewell and of Worrall's criticism of Popper, one must however concede few things. One concedes that Whewell's and Popper's theories of the process of learning from error are all too simple. There are difficulties in the details, one tries to determine the best to use in order to learn from errors. One can find mistakes, where there are none, and one can reintroduce prior mistakes when one tries to correct them. Such problems pose tasks for the sociology of science, which has the task of examining the effectiveness of rules.

The Tasks and Methods of the Sociology of Science.

From the point of view of critical rationalism the tasks of the sociology of science consist above all to raise the effectiveness of methodological rules to produce criticism, and to develop further methods which enable us to learn the maximum from the identified errors.[313] The sociology of science investigates, for example, whether adventurous or conservative strategies can generally be viewed as the best. The results of my studies lead to the conclusion that those rules are better which encourage competition between such strategies than those rules which prefer one or the other strategy—as Popper and Kuhn did. Beyond that one can investigate from a sociological point of view how this competition and cooperation looks in the practice of science (Wettersten 1985a). It is also a matter of favoring those rules which maximize the openness of science while simultaneously providing for discussions of high quality—aims which are at times in conflict. More detailed questions concern the rules of publication in journals or by publishers and the distribution of money.

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[312] According to Worrall the first wave theory held that light had no pressure, whereas the second wave theory presumed the opposite.

[313] Agassi started and developed the sociology of science from the perspective of critical rationalism (Agassi 1981; Wettersten 1993) I have provided a methodological portrayal and defense of the critical rationalist sociology of science (Wettersten 1996) and conducted further studies (Wettersten 1978; 1979; 1980; 1985a).

Because there are various theories of rationality, which help or hinder us to learn from error, the problem arises whether fallibilistic sociology of science can serve as a model for broader sociological inquiries. Can we determine which rules we can use in other areas? And how do we distinguish these rules from those in theoretical science?

Learning from Error as a Comprehensive Institutional Problem.

According to traditional philosophy of science theoretical science discovers truths. The applied sciences and/technology apply these truths in order to solve practical problems. The appropriate rules for the choice of applied theories are then rather easy to find: each justified theory of theoretical science can be applied in order solve practical problems. But when no theories are justified this theory is empty. If Popper is right, that the sciences consist of bold conjectures, we cannot apply the methodological rules of theoretical science in order to solve practical problems. In our attempts to solve practical problems we do not want, as in the theoretical sciences, to aim at bold conjectures, with the hope that our plans fail so that we can learn something.

As Popper noticed, the applied sciences need new rules. But his solution to this problem is scarcely adequate. He only said that we can trust those theories which have up until now been the severely tested without being refuted. This is merely a small modification of the traditional theory which can hardly take adequate account of the new situation. On Popper's philosophy of science the fact that a theory through empirical research has not yet been refuted is scarcely a reason it as true or its application trustworthy.[314]

When we try to solve practical problems we have no alternative to learning from the mistakes or previous approaches. Only in this way can we to some degree shield us from the failures of our plans. Normally we try to notice which mistakes have been made in the past in order to protect ourselves from them in the future. But this process is by no means obvious. Attempts to avoid failures made in the past can lead us to introduce even worse new mistakes. In addition we must presume that there are unknown dangers about which we need to find out as much as possible.

[314] Agassi was the first follower of Popper to have pointed out his problematic and to offer an answer to it (Agassi 1975, 282-337).

Agassi pleaded that in this situation we should use democratic processes, in order to determine which risks effected individuals are willing to take. A critical and open discussion of risks is clearly appropriate. Without institutions which can lead to fruitful discussions they will not bring us very far. The estimation of risks in science is such a detailed process, that it is only open to experts. But we cannot trust the judgments of individuals. We need provisionally determined transparent institutional processes which experts should follow. These processes should grasp the constant exercise of criticism in which no decision can be viewed as final. Mistakes should thereby be laid open and new plans should be held open to criticism before they are carried out.

These aims are, of course, not new. In many fields the experts conduct rather good work. Above all in technical fields we rely on experts. Bridges are not without errors but normally we can trust them. In Europe tunnels have now and then significant difficulties, but generally they are rather good, insofar as rules are followed and we can trust both trains and planes. Nuclear power and new developments in biology give rise to political as well as technical problems of acceptable risks and inspection. In these fields the existing standards need to be regularly improved in view of technical advances, higher expectations, and newly appearing problems. But there are still more problematical cases.

These cases concern the interaction between the social sciences and politics. Especially today, since our economic institutions show severe deficiencies it is clear, that our capacity to learn from error in this field is rather limited. In regard to the connection between politics and economics we have had massive difficulties to make reasonable prognostications, that is, to learn from our mistakes in the past. Just as the majority of economists thought they had identified the mistakes of Keynesian economic theory and had overcome them, the relevance of this theory came to be clear. This process reminds us of the repeated disagreements concerning the wave and particle theories of light. Both cases throw open the question, whether we can improve our institutions so that we can better learn from our mistakes.

Can we improve Institutions?

In order to investigate the possibility of the improvement of institutions, we can ask, which defects our institutions show, which hinder us from learning from our errors. There are at least two typical defects: The first is that we all too often and all too quickly presume, that after we have discovered a mistake, that we have discovered the truth; thereby is further criticism blocked and no longer effective. The other typical defect lies in the fact that we all too often all too intensive concentrate on individual thinkers. We ignore the problems of the quality of institutions, because the traditional justificationist theory of rationality continues to exert far too much influence.

The central reason for the first defect is that a Whewellian instead of a fallibilist theory of learning from error is applied and we thereby assume that the correction of mistakes brings us to the truth. After encountering criticism theoreticians move too quickly to the naïve assumption, that the truth has been discovered. After this discovery we need only apply the newly discovered truth, as the traditional theory presumes. Criticism on new developments is all too lightly shoved aside, until new, perhaps worse errors appear; once again, an all too limited amount of time for criticism is conceded. An overpowering example for this is the development of economic theory over the last twenty years as well as its application.

The second typical defect, that is, that we all too often concentrate on how individuals can better think instead of how institutions can be improved, is above all a consequence of the nearly universal application of theories of knowledge that rationality consists of justification. When one seeks justification, one needs a theory of how much proof is needed in order to finally prove some theory. Sources of evidence can vary, for example, sensations. After some source of truth is presented, each individual should see the truth of the proven theory. This process is an individual one, even then, when one person presents the alleged proof to another. The success of the quest for truth after good advice and learning from error depends on whether individuals are at hand who master evidence and in accord with appropriate standards examine and evaluate alleged proofs. For this purpose we have presumed experts, whose opinions we should trust. For a long period of time in the USA one waited for the opinion of Alan Greenspan because he allegedly as Head of the American Central Bank had so much information and as a person of so high a capacity to judge, that he must come to the correct results. Some pointed out his mistakes, but scarcely anyone took their criticism seriously, although it is now conceded that in at least some cases they then were correct.

An example of this mistaken individualist formulation of the problem is offered by Philip E. Tetlock in an interesting study of the success of public commentators to decisively influence public opinion (Tetlock 2005). As Tetlock determined, their long term success is not at hand. For the most part arbitrary conjectures were every bit as good as the opinions of the so-called experts. Tetlock also found out, that those who used comprehensive systems—people who in Isaiah Berlin's terminology think as hedgehogs—have poorer results as people who think pragmatically—people who in Berlin's terminology think like foxes. I presume that Tetlock is right, that foxes tend to think more critically than hedgehogs.

For me it is characteristic, that Tetlock formulated the problem as one of finding the best experts, say, that one substituted the fox for the hedgehog. One thereby compares differing methods of justification. He wanted to find methods of identifying the correct solutions and/or prognostications. Of course, he finds problems with each method. Some attempts try to proceed systematically in order to think coherently and to understand. Critics of this view say they are dogmatic. Others try to adjust their views to constantly arising new evidence. Critics say they abandon good ideas and perspectives all too quickly. Viewed in this way the best what can arise from such procedures is, on the one hand, caution by the application of the best methods, and, on the other hand, hope that there are individuals with a good but inexplicable ability to judge. This is the despairing solution which Milton Friedman in regard to application of economic models recommended (Friedman 1953; Wettersten 2006a, 275).

From the point of view of fallibilism the problems lay elsewhere. The central aim should be to create the best institutions, that is, institutions, which lead to the best criticism of all opinions under consideration. Although Tetlock moved closer to a fallibilist perspective, in that he warned against all too high expectations from experts, he withheld the individualist assumption of justification. He tried to describe methods which the best thinking persons use. But we have to abandon attempts to describe the methods the best thinking persons use. We can, of course, still take into consideration, which institutions help individuals the most to think critically.

Popper's Perspective on the Role of Institutions as a Protection of the Open Society.

In *The Open Society and Its Enemies* Popper emphasizes how important institutions are for the protection of the open society. This theory is, of course, by no means new, but its emphasis is nevertheless important. Popper explains that an open society can only be maintained when a leader can be replaced by citizens. But Popper's treatment of this theme by no means go far enough, because says much too little about what characteristics such institutions should have. But in order to learn from our errors we do not only need the possibility to replace bad leaders. We also need to know how we can in the future make better decisions when one leader is replaced by some other. Sometimes we succeed in doing this. Although it is still too early for a clear statement, perhaps the replacement of Bush by Obama is such a case. But some already feared while he was in office that on the basis of the false analysis of why Bush failed according to which he (Bush) followed an ideology, he (Obama) avoided any ideology in favor of a pragmatic approach. But according to this false analysis of Bush's failure Obama should have tried to employ a better ideology. In view of the victory and failure of Donald Trump all of this needs to be thoroughly rethought.

When the Germans brought down the Weimar Republic in favor of Hitler many thought at first that they had learned from an error, that is, the support of democracy. In any case we need institutions that do not merely allow us to replace bad leaders, but which allow provide us the situations in which we can critically appraise all actual and potential leaders. We need institutional standards which allow us to avoid repeating mistakes. In any case they should prevent us from substituting one mistake with a still worse mistake.

According to Popper the maintenance of the institutions which protect an open society depends on the willingness of individuals to maintain a critical posture. And he added that this posture is not natural. As a consequence of evolution all humans have a strong need to live in a tribal, that is, a closed society. In spite of this need each individual should carry his cross and remain critical and autonomous.

When Popper emphasizes how important it is for individuals to exercise their willingness to contribute to the functioning of an open society he is rather one-sided. First, the basic assumption is false. Individuals can live without special difficulties in open societies (Wettersten 2006b; 2007a). Participating in a critical conversation can be every bit as enjoyable as playing chess, or tennis, or music. In order to maximize this pleasure one needs to exercise. The same is true for critical thinking. But learning to think critically and the ability to enjoy exercising it depends on our institutions. The maintenance of open societies depends less from the willingness of individuals to carry out an unnatural and difficult attitude as from the kind of institutions which are at hand and/or which can created in order to promote autonomous critical thinking. Persons must, of course, accept responsibility. But to carry this responsibility does not mean carrying a cross and the preservation of open societies does not so one-sidedly depend on how ready populations are to think critically. It depends rather on how good or bad institutions are constructed.

In contrast Popper placed the task of institutions protecting the open society above all in their ability to limit the power of governments. This task is decisive but sufficient. An additional, but every bit as necessary task consists in how institutions are developed so as to render possible for individuals scientific, philosophical, and political perspectives to criticize, to correct and to develop.

The weak characteristic of Popper's methodological individualism for the study of the effectiveness of institutions.

In order to solve the institutional problem of learning from error we need methods for studying institutions. We can begin with Popper's methods. Popper is not only the leading personality in the development of fallibilistic philosophy. He has also emphasized the importance of institutions for the social scientific explanations. Popper's central thesis in regard to learning from error is that we should take care to look for unintended consequences of social planning. The important task for the social sciences explained here is the investigation of the effectiveness of institutions in the furtherance of criticism. Popper's methodological individualism is not appropriate to carry out with success either the one or the other task.

How can one maintain a look out for unintended consequences? Popper took his methodology from neo-classical economics. This methodology was developed in order to study the characteristics of economic systems. It was attractive to Popper, because it was closely connected to the democratic tradition of the open society. But in economics it was above all used to describe the consequences of economic factors, which were abstracted from other social or natural conditions. At first the application of this methodological approach in economics ignored institutions. But Popper wanted to defend a broader approach than that exercised by the economists. Already in *Das Elend von Historizismus* Popper defended that view which has today become known as economic imperialism, that is, the application of methodological individualism in every social science.

Because Popper and some of his followers did rather little to defend his theory[315], I use in addition to his discussions Ian Jarvie's relative extensive portrayal and defense of this method (Jarvie 2001; Wettersten 2006c). With this method, Jarvie says, one begins with the null hypothesis, which describes the action a rational acting person. One then investigates whither individuals really do act in this way and, if they do not, to explain the differences with social scientific explanations. This explanation corresponds with the first example of the application of this method gave in *Das Elend des Historizismus* (Popper 1965, pp. 110ff.; see also Marschak 1943), and later in his discussion of the rationality principle (Popper 1985, 359). But this method is only then applicable, when there is a null-hypothesis whose application can be clearly appraised. This is virtually never the case. The application of the null-hypothesis can only be clearly appraised when a clear advantage for an individual is given by one action in a specific context. In order to identify such a clear advantage, one must make rather strong assumptions about the situation, the aims of individuals and the knowledge of the acting individual. The economists, for example, always presume that the central aim is the improvement of wealth, even when they are aware that this aim is not always the most important one for the acting individuals. They also presume that the acting individuals have enough knowledge to identify that action which best serves their interest. The truth of such clear assumptions can, however, almost never be determined. The situations in which individuals act are normally quite complex. The aims which individuals try to achieve vary from time to time, from person to person, and from situation to situation.

[315] Next to Popper's discussion in *Das Elend des Historizimus* and *The Open Society,* Popper's short discussion of the rationality principle is the most important source (Popper 1985). Agassi also defended Popper's theory. He emphasizes, that Popper emphasized the importance and reality of institutions, but he says little about Popper's method (Agassi 1987). Albert applied Popper's ideas to politics and the theory of justice and critically discussed political economy, above all because economic theories were all too often not falsifiable or were platonic (Albert 1977; 1978b; 1987; 1994). He failed, however, to connect the former with the latter, so that he contributed nothing new to the methodology that was not in agreement with Popper's suggestions (Albert 1972b; 1978a; 1998; Wettersten 2006a, pp. 121ff.). Albert also discussed the relationship between economics and politics with the result that—in agreement with Hayek and others—for the existence of a market economy a legal framework is necessary. Finally Lawrence Boland investigated the falsifiability of economic theories and the role which inductivism played in economics; he has wanted, however, above all to introduce Popper's theory of learning rather than his theory to defend or criticize (Boland 1986; 1089).

In addition individuals so good as always consider various alternatives in order to reach their goal including appraisals which include factors which are not portrayed in the models employed by social scientists. And the acting individuals seldom possess the knowledge and the analytic capabilities presumed by social scientists (Popper 1985, p. 362), but without the application of the rationality principle the construction of models would be rather arbitrary. But even the construction of models with the help of the rationality principle is rather arbitrary.

Popper touches on the problem of constructing enlightening models, when he mentions Churchill's observation that wars are for the most part when the enemy makes a mistake as through intelligent planning (Popper 1985, p. 362). His answer to this problem is that the actions of generals can be explained by their mistaken grasp of the situation. But this theory presumes that the generals fail to possess creativity which cannot be taken into account by the social scientists, that they make no false assumptions not understood by the social scientists, or that they do not change their minds about the questionable situation. In addition this example is relatively easy to portray because the aims and problems in a war situation are relatively easy to analyze compared to other situations.

Popper holds that the assumptions of the rationality principle are rather minimal (Popper p. 365). He assumes that our actions in view of our perceptions of situations in which we find ourselves are adequate. With this observation has is more or less correct, insofar as one considers the individual alone. In order to understand how this relationship between perception and action appears one has to learn of the world of thought of the individual. And in order to use the principle as the basis for social scientific explanation one needs to infer the world of thought from the situation and the actions. The assumptions which are needed to do that are by no means minimal. Each model constructed with the help of the rationality principle is more or less arbitrarily limits the factors considered by individuals[316].

One can see that this method is quite problematical on the difficulties which economists face. Especially in regard to the role which institutions

[316] John Watkin's fascinating study of an apparently unexplainable and tragic ship accident is an exception, which confirms the rule. He can reconstruct the erroneous perceptions of the captains involved in the accident under the supposition that they strictly held to the rules applicable to their situations, because they were known to him as a former marine officer (Watkins 1970).

play in economic processes the narrow limits of this approach have been discovered. In attempts to explain political institutions as processes in which individuals aims to achieve their goals are expressed empty theories have resulted which predict virtually nothing (Wettersten 2006a, pp. 75ff.) One cannot explain why individuals vote in democratic systems: the costs are all too high and the benefits are all too low to justify the costs (Green and Shapiro 1999). Popper's results fail to offer any solutions to many problems which arise in economic theory by such thinkers as Douglas North, Friedrich von Hayek, Anthony Downs or James M. Buchanan (Wettersten 2006a, p. 33ff.). By his attempt to portray Popper's method as sufficient Jarvie completely ignores the problems in economics. In the end Popper's theory is scarcely better as Friedman's unsatisfactory attempt to explain why models constructed with the assumption of the rationality principle can even then be used when the actions to be explained cannot be traced back to rational considerations and when one does not have any understandable standards for the applicability of each model (Wettersten 2006a, p. 275ff.).

Jarvie attempts to defend Popper's method in that he claims it can be used to explain the emergence of institutions. In his criticism of psychologism Popper correctly argued cannot be fruitful. His observation is also appropriate when one tries to explain the emergence of institutions as a product of various individual actions. One must first analyze the institutional context before one can understand individual actions. It is much more promising to take the institutions as given and then to ask, how they steer events.

The difficulties in the applications of methodological individualism discussed here can be overcome when one begins with the investigation of the effects of institutional rules. The description of the rules, which models take over in methodological individualism, can be relatively easily empirically examined. The same holds for theories of their consequences. When their strong directions are not wanted problems arise as to how they can be reformed.

Popper's theory of is not especially appropriate to solve the institutional problems of learning from error, because the central problems of learning from error lay in the institutional evaluation of ideas and in improving the effectiveness of intellectual strategies with which one reacts to mistakes. They do not directly have to do with the attempts by individuals to reach their individual goals, but rather with the problems of correcting mistakes and choosing how to improve institutional ideas. These problems correspond more closely to the problems of the sociology of science. The methods for the investigation of these problems should rather be oriented on those of this field as on methodological individualism.

An Alternative Method for the Investigation of Institutions.

In order to find better institutional rules, we need an alternative to current social scientific methods such as methodological individualism. An alternative can look like this: One can investigate institutions by describing the rules that they prescribe. One can then explain how these rules affect the behavior of individuals. One can above all ask, how they influence the problems individuals impose, even how they at times determine them. One need only presume that people are rational in that they pose problems, seek solutions and critically analyze them, as Popper's psychological and methodological theories of learning from error maintains[317]. One can then ask how far certain institutions give individuals the freedom to formulate, evaluate and solve problems.

It is thereby advantageous to ask how far established institutional rules further, block or limit the evaluation and criticism specific considerations. Popper's psychological and methodological theories of learning from error are by no means adequate. Popper made the important observation that rational thinking is a social process. But he failed to integrate this discovery in his theory of social scientific explanation. We have to make up for this neglect.

This theory of social scientific methods is an application of the fallibilist research program in science. According to this theory the formulation of scientific problems depends on differing theoretical frameworks. From this perspective one can view institutions as analogies of research programs. In science on introduces a research program to formulate specific problems, whose proposed solutions bring us forward or not. In an analogous way one can view institutions as frameworks for the problems of individuals. We can then analyze whether they do this well or poorly.

The, from Popper and his followers presumed more or less sharp separation between personal problems, on the one hand, and institutions, on the other hand, is removed. Institutions decidedly affect the problems

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[317] (Berkson and Wettersten 1982; 1984; Wettersten 1987; 1988). I have elsewhere compared the assumptions of rationality in this sense with the assumptions of rationality in methodological individualism. Popper defends both points of view and maintains they are compatible. But they are not.

and aims of people. They are not merely a part of the environment, with which people must deal, in order to be able to solve the problems they have formulated quite independently from them (Wettersten 2006a).

Learning from Error: a Long-lasting and Ignored Social Scientific Problem.

It has been determined, that learning from error is difficult. It is all too easy not to notice mistakes and to make erroneous inferences from mistakes. These difficulties have been recognized, but their existence has led all too many thinkers to demand even more than correct answers be produced. These attempts do not limit the number of mistakes. Instead they lead to hope that ever more theories can be used to find the truth. But the pursuit of so-called proofs leads us away from the task of finding better institutional rules for the never-ending process of examining theoretical and applied theories.

By our attempts from errors to learn the striving for proofs and/or clever authorities leads to giving criticism far too little a role. It leads us especially away from the knowledge that the quest for truth is a social exercise. For this reason we shift out concentration all too much on individuals, who can allegedly help us, instead of on institutions, which can help us further criticism and learning.

The neglected tasks touch the social sciences and begin with the sociology of science. These tasks are much wider and more pressing and have consequences for other fields such as the interaction between politics and science. With a look on the USA Joseph Nye complains that scarcely meaningful discussions occur between social scientists and politicians. Either the social scientists say virtually nothing about pressing political problems public commentators defend perspectives determined by ideologies. This process leads to sharp but hardly progressive debates (Nye, 2009). Agassi makes the same sad determination in regard to Israel. Problems of this kind can be found everywhere, even then when they show significant differences in various institutional contexts.

The treatment of these problems demands new procedures in the social sciences. We need to begin with institutional rules and investigate their social consequences. In addition we need to introduce changes in the rules which further the discovery of errors and the construction of appropriate answers to them. Because new situations demand new rules and lead to new mistakes, this process has no end. A permanent skeptical attitude is needed. But this lasting skepticism cannot end in either resignation, in that one abandons the pursuit of improvements, because one cannot in any case make it right, nor can it end in a defiant reaction, in that one simply says one knows the truth by intuition. As Seth Freeman reports, he has become conscious through new economic developments, how incapable economists are to make halfway realistic prognoses. At a conference he thereby confronted a colleague with the question, from where he knew, that his views about the current situation were correct. He received the answer, that his colleague knew, but did not have the ability to present his method of proof (Freeman 2009). As the observations of Nye and Agassi mentioned above illustrate, is this defiant reaction rather typical. Although it is quite damaging, Friedman institutionalized it by economists, as he claimed that educated economists could determine with their intuitions under which social situations models are applicable.

The task of the sociology of science and/or methodology consists in describing the existing rules of research, of publication and the distribution of money, differentiating the good from the bad rules and developing suggestions for reform. One finds the same tasks in the general investigation of rationality. Some existing institutions fulfill their tasks rather well. For these institutions it is at times difficult to develop suggestions for reform, which do not run the danger of unintendedly making things worse. Others fulfill their tasks with rather weak results. For this reason critical investigations of institutions are needed, which should raise the needed level of rationality. Such investigations can confront us with many problems. But hopefully they can help us to solve various problems, so that we can better learn from our errors and lessen the dangers which arise out of such mistakes.

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Interlude

The usefulness of the fallibilist theory of rational practice as social is clear when it comes to the problems of studying the strengths and weaknesses of religions. Such problems regularly degenerate into all or nothing quarrels between differing religions or between rationalist non-believers and adherents to some religion. By looking at the rational practices encouraged by religious institutions one may seek to improve such practices with partial solutions, which modify religious rules without judging religions in some all-or-nothing way.

3g. 'Ein beschiedenes aber schwieriges Projekt für den kritischen Rationalismus und die Religion: Die Einbettung der Religionen in offene Gesellschaften', in Ed. Giuseppe Franco, *Sentieri aperti della Ragione verità methodo scienza* (Penza editore, 2010a): pp. 463-480.

A Modest but difficult project for critical rationalism and religion: The integration of religions in open societies

Introduction: How far can and should religion be subject to criticism?

Critical rationalism puts forth a philosophical approach and moral attitude, which is relevant for all fields of human experience. It has strongly attracted humans of various cultures and backgrounds. Among these are religious individuals, atheists, agnostics, non-believers with sympathy for religion and opponents of religion. Thereby various thinkers have tried to clarify the relationship between critical rationalism and religion. They have defended widely different perspectives. The central problems lay in the question: How far can and should religion be subject to criticism? Some, like Popper, want to preclude the question as private. Some, like William Warren Bartley III, want reconciliation. Others, such as Joseph Agassi, want to achieve mutual respect and tolerance. Some, like Antiseri, want to use critical rationalism to build a protected place for religion. Some, like Hans Albert, want to eliminate religion with criticism. Here the thesis will be defended that the rationality of religion lays in its institutions and the appropriate aim of critical rationalism can and should be to raise the degree of rationality of these institutions. In this way one can, as believer or non-believer, meet religion with tolerance and sympathy without giving it a free pass from criticism of its doctrines or actions.

The background: Popper, Bartley and Agassi.

In the late fifties and early sixties there arose in discussions and arguments between Popper, Bartley and Agassi three different versions of critical rationalism; they have strongly characterized the development of critical rationalism until today. The three differing versions of critical rationalism are of significant meaning, because they lead to varying formulations of the problems which lay in the relationship between critical rationalism and religion. Popper wanted to reserve religion for private decisions; he did not wish to speak about it. Bartley wanted to use critical rationalism in order to deepen the relationship between rationalists and believers, but also to defuse the conflict. Agassi took on Popper's hard puritanical attitude to rationality as a cross that one had to carry; he suggested all humans are, as a matter of human nature, rational to a degree.

Popper wanted to both encourage and to exhort humans to be rational in order to protect the open society and to reform its institutions, including religious institutions, in ways which would reduce suffering. But religion remained separated. Bartley wanted to show how humans could fulfill the comprehensive demands of critical rationalism with a special reference to religion. Agassi wanted to take into account the partial rationality of each person and thereby show that humans could raise their varying degrees of natural rationality even without demanding a high level of rationality.

Popper: Religion left aside as private.

After a delay of over 30 years critical rationalism grew rather directly out of Popper's philosophy of science. When Popper first published *Logik der Forschung* in 1934 it failed to win any followers. During the Second World War and in relative isolation in New Zealand Popper turned his research to the social sciences. He analyzed above all totalitarian politics, its attraction and the possibility of fighting it.

In order to carry out these tasks he had to rethink the limits of rationality. In *Logik der Forschung* rationality is limited to, on the one hand, empirical theories and, on the other hand methodology, i.e. the study of the methodological rules of science. This drawing of the borders of rationality was obviously too narrow. It placed all political discussions beyond the limits of rationality. Popper's major aim was to set rationality against the (irrational) totalitarian movements and politics.

Since Popper viewed all attempts at justification as hopeless, he had no alternative but to find rationality in criticism. He extended his theory of rationality so that political and moral perspectives could also be criticized, at least in as much as one studied the consequences of their applications. But one could not demonstrate that perspectives with these or those consequences were false. Even Auschwitz could not serve this purpose. If one is ready to accept the consequences of some perspective, the thought, one could not say any more.

Popper could nevertheless use his rather narrowly drawn borders of rationality in order to carry out his fight against totalitarian politics. Totalitarian politics, he showed, was always holistic, and that means one needs more knowledge than humans have in order to successfully carry them out. Every political plan has unintended consequences. Due to this fact one can only make progress with small steps, if one wants to avoid catastrophic consequences. Still worse, every attempt to carry out a holistic plan in an already open society necessarily leads to the use of barbaric means to fight the unavoidable critics and the resistance of activists.

Popper's conclusion is full of pathos. Humans, he thought, have an inbuilt tendency to see their own well-being in a closed society. This is allegedly so because open societies have grown out of tribal societies. Humans try to recreate the lost and missed characteristics of tribal societies. He pleads, then, that one should carry ones cross, i.e., although one would rather live in a society without criticism and in which one is embedded in a stable and ordered world, one must nevertheless face criticism, exercise criticism and further criticism. According to Agassi's critical interpretation of Popper's theory, Popper developed a secular religion or a secular version of Puritanism. One should know that every person has tendency to sin. Every person wants to live in a closed society. He or she needs to constantly strain him or herself in order to resist this tendency.

But what does this mean for traditional religion? Popper considers religion a merely personal affair; he separates this private religiousness from religious institutions. This separation may have something to do with his problematical relationship to religion and especially to his Jewish heritage, which he never denied but also did not actively live.[318] He dealt rather unwillingly with those problems which religion called forth in various fields. Agassi reports that when he was Popper's assistant he tried and failed a number of times to engage Popper in a discussion about religion (Agassi 2008, p. 59).

Popper's treatment of religion was an unavoidable consequence of his theory of the open society. In this theory Popper portrayed the open society as an abstract structure. There are rules for trains, contracts and criminality. These can be contrasted with real social groups. These are rather face to face relationships from friends and colleagues. Institutions set rules for abstract and impersonal relationships between individuals. They treat all humans in the same ways.

[318] He may also be influenced to a certain degree by Sören Kierkegaard, whose books he found in his father's library and preserved.

Popper's separation of the real, private relationships from the abstract public relationships can only then be carried out, if religious attitudes, actions and rituals are viewed as private matters, which in effect have little to do with the open society. If this were not the case religious institutions would have to be viewed just as any institution would be. They then would be, perhaps should be, open to piecemeal reform. Popper took the methods, which he favored by such reforms, from the economists. One should use the aims and the logic of the situation of individuals as the basis for social scientific explanation as well as a model for the formulation of suggestions for reform.

On this point Popper's theory cannot be maintained. For the majority of believers is religion by no means a private affair. For the majority of believers a religious life includes the membership in a social group. Their attitude towards this group, as well as toward religious institutions are by no means that of individuals, who are trying to reach their personal goals, unless one extends the meaning of 'personal goals' so far, that the expression no longer has any meaning.

Bartley: The application of rationality to religion.

At the end of the '50s and the beginning of the '60s Popper attracted various talented students. A number of them found his views exciting but also not satisfying. In regard to the theme of critical rationalism and religion the most important of his followers were Bartley and Agassi. Simultaneously they developed their own alternatives to Popper's theory of rationality. Bartley Popper's main focus lies in his theory the decision to apply rationality lay beyond the borders of rationality. Agassi saw the central problem in Popper's portrayal of the application of rationality as a secular religion.

Bartley's is the most studied fallibilist alternative to Popper's own view. Bartley was above all disturbed, that on Popper's view rationality was based on a decision that lay beyond the borders of rationality. At first blush Popper's theory looks weaker than traditional views. Traditional theories claimed that they were justified. But Bartley saw here a chance to turn this appraisal on its head.

Traditional theories have maintained that all justified theories and only justified theories should be accepted. But this standard could not itself be justified. One lands in that which Albert called the 'Munchhausen Trilemma': all attempts at justification end in (1) a circular argument, (2) an infinite regress, or (3) a dogmatically determined point of beginning. Traditional theories of rationality can by no means claim that their beginning point in rationality is better than others, such as religious ones. All beginning points are mere decisions which are made beyond the borders of rationality.

With this result alone Popper's theory would be set at the same level as traditional theories. But Bartley was not satisfied. He argued further, the decision to be rational in Popper's sense, that is, that all views are held open to criticism, can be rationally made. Just as with all other decisions or points of view one can hold these open to criticism, that is, hold them rationally.

In regard to the theory of rationality this is his central point. He came to this point of view through discussions with both believers and non-believers. On my reading of Bartley he wanted with this proposal to provide reconciliation between these groups. The fundamental idea would be that one could be religious without violating any proper standards of rationality. He did not presume that a critical treatment of religion necessarily results in a catastrophic result for religion. He had sympathy for the theologians, who set themselves the task of showing how an intellectually acceptable modern religion, which did not take a stance against religion, could be developed.

My reason for this interpretation of Bartley's intent is above all personal. I cannot defend it on the basis of his writings. But I had a conversation with him in which he perhaps revealed his attitude, although not directly. About twenty years after I had read his book with enthusiasm I led a seminar with him in Alpbach. During our conversations I told him the story of how important his book had been for me as I began my graduate studies in philosophy. With considerable interest he asked whether my book had helped me in developing my relationship to religion. I said no; I had already rejected religion as I read his book. He was obviously disappointed and did not want to continue the discussion. The real reason for my initial enthusiasm was that Bartley had overcome Popper's pathetic thesis about the irrationality of the decision to be rational. He opened for me a much more optimistic perspective.

Bartley's theory of rationality did not only extend and systematize Popper's theory of rationality. He held that the application of rationality to religion was obvious. Religion was not for Bartley, as it was for Popper, a private matter about which one did not speak. His tone is also much softer than

Popper's. He never spoke about life demanding considerable effort. But his emphasis, that rational practice should be comprehensive gives just as by Popper the impression that the exercise of a Popperian style of rationality is a rather difficult task.

Agassi: Partial rationality and the raising of the degree of rationality as natural.

Agassi disliked above all Popper's puritanical attitude toward the practice of rationality. This attitude was above all a consequence of his theory that all humans had an innate desire to live in a tribal society.[319] He had no other choice than to preach rationality. Each person has to carry his or her cross. This attitude is still to be found by Bartley, although not so strongly. In part as a reaction to this attitude Agassi argued that the tu quoque argument was not so important (Agassi 1981a; 1981c; Agassi and Wettersten 1978). According to this argument any person, whose basis assumptions are characterized as not allowable from any other person, with a 'you too' answer. Agassi thought that we are all rational to this or that degree. The exercise of criticism does not conflict with out nature. It is rather something like loving or seeing. Although the thought of each person shows deficits, we should not react thereto by exercising pressure, demanding perfection or portraying hard work as necessary. Each person can raise the degree of his or her rationality, when he or she wants, and can do that without Popper's demand for hard work. One does not need to meet higher standards. Sometimes higher qualities of results can be achieved with relaxed and easier approaches (Agassi 1981b).

Agassi's theory lets Popper's secular religion, with its invocations to work always harder and harder, fall.[320] The open society can be protected with other means. He rejects Bartley's emphasis on the unavoidable necessity to hold all points of view open to criticism. Instead he finds that progress can be achieved in more humane and more effective ways when humans follow their innate tendency to learn, also when they discover mistakes and correct them. They should do that when and where they find it necessary, interesting or amusing. When humans are free to do that, it will be sufficient to achieve progress when each individual and all of us together do that. But the open society is necessary in order to given humans the freedom to live critically.

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[319] For a discussion see (Wettersten 2006b; 2007)

[320] Agassi explains his attitude towards Popper's treatment of religion in (Agassi 2008, p. 580). The portrayal of Popper's extensively follows Agassi's analysis.

Agassi found no reason for the rejection of religion. He did not try to deny anyone his or her religion. On this point his attitude but not his theory agrees with Bartley's. Insofar as problems arise out of the social role which religion plays he stays with Popper's methodological individualism. But this method cannot master such problems (Wettersten 2006a).

Contra and pro religion: Albert and Antiseri.

The points of view of Albert and Dario Antiseri concerning the relationship between critical rationalism and religion stand at opposing extremes. For a long period of time Albert argued that religion should have no special place beyond the borders of rationality, that is, of criticism. That is placing a dangerous and arbitrary limit on rationality. He tried to apply Bartley's comprehensive theory of rationality to religion. He did find, however, that there could be any reconciliation between religion and rationality, but rather that religious doctrines are always indefensible.

In contrast to Albert Antiseri tried to find free space for religious belief, which Albert thought was intellectually inacceptable. Antiseri bases his view on the fact scientific knowledge cannot be justified and for that reason cannot be used to block free space for religious belief. This would be an absolutist position, which contradicts the epistemological modesty which characterizes critical rationalism. One can decide with the same justification to be for or against religion. In addition he tries to find in parts of history, (above all German) philosophy, and in the application of a Gadamerian heuristic, an understanding of the opening for religious belief.

Albert: The fight for the consequential application of critical rationality on religion.

In *Traktat über kritische Vernunft* Albert laid the foundation for his treatment of the relationship between critical rationalism and religion. He defends there a Bartleyan theory of rationality, that is, all views of a rational human must be held open to criticism. In his exposition of this point of view he takes a close look at religion and theology in order to determine if theological doctrines can withstand criticism or how far the defender of religion can meet the obligation of each rational person to hold their views open to criticism.

Wherever he looks he comes to the same result: Religious doctrines can only be defended when they are protected from appropriate criticism. Thereby he tries to engage in discussions with those theologians who are interested in developing a modern theology, a theology which is consistent with modern science the established contemporary cosmology which has been developed by science. He regularly thereby notices the Christian doctrines contain cognitive aspects, which cannot be separated from a Christian message. Without the cosmological theories found in Christian doctrine the whole breaks down like a house of cards.[321]

In my opinion Albert was for the most part right with his analyses. There are philosophical theories in Christianity which play important roles in this religion, which is emphasized by defenders of all shades of Christian belief. In addition Albert shows without much difficulty, that these cognitive aspects of Christianity can only be defended today with changes in the standards of criticism. But this weakening of standards of criticism is not something that would embarrass theologians, as if they were caught cheating. They seek to formulate interpretations of their religions which establish a separation of science and rational thought from religion.

For this reason there is much which Albert points out which has little to do with the disagreement between, on the one hand, religious thinkers, who do not want to deny science and, on the other hand, scientific thinkers, who find no place for religion in their world, because there are not willing to sacrifice their intellectual integrity. It is really a matter of whether humans should separate their religion and their science—as Galileo, Kant, Duhem and many other scientists and theologians have maintained—or whether when evaluating religion one should use the same standards which one uses in science.

We face here the limits of Bartley's attempt to defend rationality as comprehensive. He showed that at least in principle rationality could be rationally accepted if one held all ones religions points of view open to criticism. He took thereby an argument away from the believers: Their rational beginning point cannot from the point of view the theory of rationality be just as acceptable as the beginning point of a thoroughly rational person. But this argument still leaves open whether even without this argument for belief based on the tu quoque argument someone still

[321] In contrast Antiseri wants to reduce this aspect to a minimum. Christianity should be built on the person Jesus and not, say, on Aristoteles. He thereby mentions Ernst Renan. Renan belongs to a theological school whose members wanted to build their belief on the historical Jesus (Renan 1863). But this theological direction came to an end with publication of Albert Schweitzer's *Die Geschichte der Leben-Jesu-Forschung* (Schweitzer 1984). Without their philosophical doctrines Christian belief is rather thin.

wants to be religious and whether one wants to treat this religious belief critically.

Antiseri: Can critical rationalism be used to make free room for religion on the other side of scientific rationality?

Antiseri wants to draw a place for religion which is also respectable for scientists. On the hand, the epistemological possibility of such a free space should be determined; on the other hand, the unavoidability of using such a space in the religious need of an absolutely meaningful perspective of the world or attitude toward the world should be clearly portrayed. Antiseri joins a large tradition which goes from Osiander through Galileo, Kant, Duhem and many others, who have used analyses of the borders of scientific knowledge to find a place for religion next to their scientific interests without any conflict between their two intellectual or spiritual worlds. But in recent times there has been only one influential program which tried to achieve this aim. The basic theory of this perspective presumes that the exclusive aim of scientific research lies exclusively in the construction of true predictions. Scientific theories cannot be viewed as true or false descriptions of reality. They are mere tools which are used to arrive at practical goals. One can seek the truth in religion.

What is challenging in Antiseri's approach is that he breaks with this conventionalist tradition, but nevertheless wants to meet the goal of believing scientists. On the basis of Popper's philosophy of science he seeks to explain how and why the quest for truth in science does not or should not touch the content of religious life. In order to use Popper's philosophy of science to create a free space for religion, he argues that such a space can only be impossible through "absolute" philosophies. Such theories maintain that they attain the truth and that there is no other way to do so. Religious scientists have seen in each philosophy of science, which maintains that it explains the path to truth, a danger for religion. Antiseri avoids going down this path in that he maintains that there is no "absolute" philosophy of science. And here he can very well insofar support his belief with Popper's theory in that Popper has shown just this impossibility. Popper remains an agnostic. This perspective is not consistent with Antiseri's, but it gives him the apparent opening to believe, where Popper held back his belief.

This analysis leaves out of consideration just that which Bartley and Agassi in their discussions of critical rationalism and religion emphasize: A serious critical rationalist holds all his beliefs—including his religious beliefs—open to criticism. The theory explained above concerning the free space for religion offers no explanation why one holds off criticism, at least with normal criticism one finds in science, where religion begins.

Antiseri portrays the decision, whether one believes in God or not, as one which lays on the far side of limits of rationality. But the development of critical rationalism since Popper shows, this is a mistake. In fact there are many non-believers who defend their position with critical discussions, for example, Walter Kaufman (Kaufman 1961; 1963), Bertrand Russell (Russell 1917; 1957) Richard Dawkins and, of course, Albert.

Antiseri: What purpose has heuristic?

In part of the history of philosophy of the 20th century Antiseri finds a development, which allegedly holds the path to religious belief open. He comes to this result with an application of Gadamer's heuristic, which he defends as an example of learning from mistakes. Gadamer allegedly employs the same methodological principles as Popper: He tries to ascertain the meaning of texts with conjectures and criticism from his own perspective.

This might be true. Popper views the history of philosophy as a series of theories which are at times enlightening and at times damaging (Hegel; Heidegger), which should be met with distance and criticism. For Popper heuristic is simply a method, to which one turns for help when difficulties arise in the interpretation of texts. Insofar as Gadamer shares this aim with Popper, there is no problem with pointing the similarities between these two thinkers. But thereby nothing is gained for religion. Insofar as Gadamer presumes, that the heuristic alone can bring us to the truth, there are deep-seated and decisive differences between these two thinkers.

In order to remain by a fallibilistic point of view one needs a newly developed historiography of philosophy, which is developed in a critical rationalist framework. In my study of William Whewell I have put forth an example of writing the history of philosophy following the principles of critical rationalism (Wettersten 2005a). In his commentary on this study Agassi placed my approach in the development of critical rationalism (Wettersten 2005a, pp. 315ff.). In my answer to his commentary I have sketched the historiography of philosophy according to the principles of critical rationalism (Wettersten 2005a, pp. 369ff.). It is important in such an historical description not to put into the history too much rationality and/coherence. One should describe the inadequacies of theories when they appear. Theories understood in this way can possibly be useful in the formulation of new problems and new solutions. But they offer no path to wisdom which lies beyond the borders of rationality.

Personal digression: The religious experience of a nonreligious person.

Religious thinkers regularly base their portrayals on the importance of religious experience. These experiences are considered as perhaps the most important aspect of religion. This religious experience is found in the personal relationship to God through prayer, worship services, or membership in a religious community. On my perspective nonreligious individuals also have their “religious” experience—at least those who have grown up in religious families. These are experiences which one as a member of a religious community has before one has rejected the religious belief, but also perhaps similar experiences which nonbelievers have, when, for example, they view the wonder of nature with its unexplainable beauty and endlessness.

In regard to the problematic here discussed is my own “religious” experience insofar relevant as it gives me the only beginning point I can find, when I want to understand religious individuals, who have no interest in rationally explaining their religious feelings, experiences or the meaning of the world which their religion gives them. My experience is insofar relevant as my break with religion was not simply something that happened by the way, as if I had merely decided to shave off a beard, as it appears to be for some individuals. Rather, my ‘religious” experience had to do with the intensity of a Kierkegaard. Instead of strengthening the intensity of my belief, my coming to terms with religion, the ending of my religious belief, ended with painful separation from the religion of my family and the thereby associated religious community.

This break with religion began, when as a thirteen year old I refused to join the church; I could not, as would have been required, say that I believed in the reincarnation of the body. But such doubts, even though they lasted years, did not mean a separation from the religion. As long as one poses the question of belief and views this question as a question of Christian belief, one remains a part of the community. One still tries to be a good Christian, even when this is very hard. This fight belongs to everyday Christian life according to Christian doctrine. This experience is to be sure not specific to Christian communities. As I spoke with an Indian woman, who grew up in a Hindu community, we each had the impression that in our different separations from religion we had very much the same experience.

When I studied at a Christian college all intellectuals there were interested in the problems of Christian belief. Literature such as Sören Kierkegaard, Martin Buber, Paul Tillich, Richard and Reinhold Niebuhr were taken to be unimportant or scarcely important. In my last years there one of my best friends decided to be a preacher. When we spoke about our beliefs we found scarcely any differences. He fought for his belief. I fought to draw the right conclusions from my knowledge and experience. To the later also belonged my experience with members of the religious community: They had all too often sacrificed their intellectual honesty to the benefit of their devoted membership in the religious community.

When I was in this rather modified way a member of the religious community I found it easy to express my differing opinions in a friendly way, even though I knew that these opinions amounted to separation. As the preacher who also was student counselor spoke to a small group of students, I had no problem simply saying that, according to his belief, some will be lost and that I was one of those. I felt less at ease as I noticed that he could hardly hide the tears he shed for my lost soul.

I left the community and started my studies in the philosophy of science at the University of Illinois. I wanted to lead a life in accord with principles of rationality and I hoped I would find a way in the philosophy of science. During the first week I had an interview with a professor I did not know: Agassi. In this interview he asked me about my religious belief and I answered quite clearly that I was no believer. But to my surprise and embarrassment, my whole body shook. That was the first time that I expressed my perspective outside of the religious community. Quite clearly my turning away from the religious community was for me far more serious than merely changing my opinion. But I never again had any problem living as a non-believer.

When humans have lost their belief and/or their religious community it happens rather often that they feel a gap that man wants to fill with a substitute. As I lost my belief, I felt attracted by authors who expressed this feeling. Thereby, I read Russell's *A Free Man's Worship*, Albert Camus, and Walter Kaufmann's, *The Faith of a Heretic*. Popper's rather emotionally expressed idea, that one could decide for rationality, but that there was no rational basis for this decision belonged at the time to this circle of authors. Many individuals, above all religious individuals, believe that this emotional pain stems from an unsatisfied religious need. But in many cases, including mine, this pain is rather short lived. The literature that lessens this pain lessens or consoles, loses its interest and importance rather quickly.

Is there a task in regard to religion for all critical rationalists today?

Out of the various versions of critical rationalism today the question arises, which problems should critical rationalist place on the agenda? An overview of the various versions of critical rationalism shows how wide the different versions go. Perhaps it can be useful to find common ground between religious and nonreligious thinkers. Can we raise the degrees of rationalist of everybody without pushing the believers out of bounds and without opening the path to irrationality?

Popper tried to portray religion as a private affair; he saw little reason to view religion as an important theme for the study of rationality or the society. This is a mistaken estimation of the situation. Religion is a very important social factor in every society. Its social characteristics are closely related to doctrines and the degree of rationality with which these doctrines are defended and/or corrected.

Bartley's theory partially corrects Popper's approach in that he presented the ideal each individual can practice comprehensive rationality insofar that he or she holds all of his or her views open to criticism. But as an answer to the social and/or personal problems, which religion causes, it is rather unrealistic. Every serious attempt to carry it out as a social strategy must degenerate into Popper's moralizing secular Puritanism.

Agassi's theory of natural but partial rationality, whose degree can be raised, avoids this moralizing politic. We can encourage individuals to make progress and presume that they can improve their rationality. This strategy can achieve only limited results. From my point of view concentrating on institutions rather than individuals offers a more promising alternative. The main task can be improving institutions in ways that bring about progress. Agassi defends Popper's methodological individualism and the all too sharp separation between beliefs and institutions which comes with it. We need new methods in the social sciences in order to examine the rationality of institutions, that is, we have to conduct social inquiries which can determine to what degree institutions allow the appropriate exercise of criticism and how from this point of view they can be improved.[322]

Following Bartley's program Albert concerned himself rather intensively with theology and thereby determined that the standards of rationality proposed by critical rationalists regularly and often in rather extreme ways were violated by defenders of belief. The answer to such criticism is however unfortunately all too often is that they have no intention to put their belief before this court. The carrying out of Albert's tasks is nevertheless somewhat useful because the gap between those who want to critically examine religion and those who want to find a special path for religion is rendered clear. They show where our problems lay and how deep they really are. But these remaining problems cannot be solved with further criticism following Albert's approach, because the participants in such debates all too often talk past each other.

Antiseri's program of using critical rationalism to find a 'free place' for religious life without violating the standards of intellectual honesty illustrates quite clearly this gap. Antiseri finds in Popper's theory rather narrowly drawn borders of rationality. He presumes that the failure of so-called absolute philosophies such as idealism, positivism or Marxism creates a free space for belief. But he creates this free space only by contrasting justified theories with a "calling" from the other side of the borders of rationality.

In the framework of critical rationalism is the corresponding comparison between non-justified theories which are viewed critically and points of view which are protected from criticism, if one, for example, calls them "callings". In contrast, Antiseri finds commonness between his fallibilist points of view and the points of view of many thinkers who have shown the borders of rationality such as the Wittgensteinian language games, Kuhn's paradigms and the specialization as well as the separation of various aspects of science. Although these thinkers show borders of rationality they do not defend the core of critical rationalism, that is, that criticism is important. Popper criticized the theses of these thinkers, who Antiseri quotes in apparent justification of his special path. They defend the "Myth of the Framework", that is, the idea that defenders of differing frameworks cannot engage in rational discussion with each other.

[322] I have put forth a program for carrying out such research in *How Do Institutions Steer Events?*

Antiseri suggests that the discovery, that we cannot justify theories, leads to the result that we must, at least in some fields such as ethics, accept relativism. The idea, that only proven theories are true has in fact played a role in the history of philosophy and of logic. When Popper wrote *Logik der Forschung* he did not want to speak about realism, even though he was a realist. He had taken over from logic the thesis that only proven sentences could be viewed as true, at least when one wanted to hold onto the presumptions of modern logic. Tarski freed him from this barrier in that he demonstrated that there is also in logic true sentences which are not proven (Wettersten 1992a, 186; 196; 216; 224; 2005b).

After Antiseri drew such narrow boundaries for rationality, he tried in Vattimo, Gadamer and in religion other paths to belief. The argument, that the application of heuristic methods can show that the traditional religious language is meaningful can hardly satisfy anyone who demands that all points of view should be subject to the same standards of proof. The issue is not whether a religious or a Christian message can be interpreted in a meaningful way, but rather whether it is true or false.

Antiseri speaks as if the weak rationality of Vattimo corresponds to that of Popper. But both theories have nearly nothing to do with each other. Popper defends a realistic theory: We use criticism in order to get closer to the truth. Vattimo dispenses with truth in Popper's sense. Antiseri's development of his theory with discussions of the philosophers Heidegger, Gadamer and Vattimo uses a language which I cannot follow. I see in his research a concentrated attack on the application of rationality to solve our problems. And in the application of the methods of rationality I see the only possibility to maintain an open, that is, a civilized society.

Religious communities: Can one raise the degree of rationality?

When one presumes that there is a deep divide between believers and nonbelievers, it can still make sense to ask if there is common ground and indeed common attitudes toward religion. One finds different problems even without different points of view by questions of the role of religion in modern society. I am by no means sure whether one can formulate these problems in such a way that believers as well as nonbelievers would accept them as appropriate and important. But we can try. This task presumes that we use rational practice to build bridges between believers and nonbelievers. When religious thinkers such as Antiseri presume that rationality consists of criticism and that an open society should be defended and developed, one has an opening for bringing sides together.

The success or failure of such an attempt depends on whether one can find sufficient agreement on how rational practice is conducted and where it should be conducted.

Nonbelievers do not take much interest in theology. For them it is boring and abstruse. But their contact with religious individuals in society cannot be avoided. Religious practices and communities are important for the shaping of every society. Especially in social areas religious communities do much that is generally viewed as valuable. But therein lays potential for intolerance, violence and war, which one does not have to emphasize today.[323] The only way to stave off this danger is to integrate religious communities as deeply as possible into the open society. This means that, in regard to their own members and in regard to nonbelievers, religious communities need to observe the rules of the open society.

Without critical rationalism religious communities are unable to meet these standards. Each religion is caught in its own doctrines, which for most vary from those of other religions. None of these doctrines can be used to negotiate between religions. The application of rationality as criticism is also needed in religious affairs. Religions can be viewed as integrated in open societies only insofar as they use criticism in religious affairs.

In order to promote this aim social scientific studies of religious communities and their effects on institutional standard of criticism are needed. These studies demand changes in Popper's methodological individualism. They do not concern explanations of social events as the consequences of individuals acting to achieve their own goals. They concern descriptions of institutional rules, the consequences they have, and the identification of possible reforms; they have as consequences critical perspectives on institutions, their rules and the ideas found held in societies.[324] In order to

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[323] Rationality when viewed as justification can also do that, as J.L.Talmon has so impressively shown (Talmon 1961; 1963; 1980). But Talmon bases his perspective on the tu quoque argument and views rationality as an alternative to religion. He did not reject violence in the defense of religion (Talmon 1980; 1987). An understanding of rationality as critical allows one to avoid this possibility.

[324] That methodological individualism is not capable of mastering these problems has been noticed by a number of social scientists. Scott Atran and Jeremy Ginges have, for example, developed an alternative which should take account of the effect of religious belief. In my opinion, however, they lessen the importance of rational action too much. An individualist alternative which sees rational actions as

further the integration of religious communities in open societies the development of political strategies which protect the freedom of religion so long as religions adhere to the rules of open societies.[325]

From my portrayal of alternatives and my own personal experience I have come to the conclusion that the most pressing problem in regard to religion today lays in finding a path and means to integrate religious communities so deeply as possible in open societies. Only in this way can we more or less reconcile, on the one hand, the unavoidable needs of many people to live in religious communities with, on the other hand, the modern needs for openness, for critical perspectives on institutions and freedom for believers as well as nonbelievers.

In order to carry out this task we need changes in the various versions of critical rationalism which have been developed. Popper's program cannot come adequately to terms with this problem, because he separates personal religious belief from religious communities. The communities are the core of religiosity and are not possible without common beliefs. Individual members of such communities do not seek as members to realize their own personal aims on the basis of the logic of the situation. They try to find solutions to problems which are formulated by the religious communities. One has to observe the communities as institutions which determine the problems as well as the approaches to solving them which they propose (Wettersten 2006a).

critical problem solving in imposed institutional contexts is possible (Wettersten 2006a; 2010; forthcoming).

[325] Agassi's Liberal Nationalism for Israel is an attempt of this kind (Agassi 1999).

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