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Essays on Linguistic Realism

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Introduction to Essays on Linguistic Realism

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1. Three kinds of linguistics

Linguistics is the scientific study of language. Agreement about the nature of linguistics may not go much further than to a statement of this kind. Not only is the set of methods linguists use to study language multifaceted; even the question what language actually is has received a number of different answers. In his 1981 book *Language and other abstract objects*, philosopher Jerrold J. Katz provided a typology regarding different views of the nature of language. In the history of linguistics, he identified three distinct approaches that he labeled – with reference to the problem of universals exhaustively discussed in antiquity and medieval times – nominalism, conceptualism, and realism. In different times and different frameworks, linguists have assumed that language is a set of physical facts (nominalism), an aspect of the human mind or brain (conceptualism), or an abstract object (realism). In the past 60 years, conceptualism has been the dominant paradigm, particularly the form based on the work of Noam Chomsky (e.g. Chomsky 1959, 1966, 1975, 1986, 2000, 2012). Nominalism is a paradigm that always had a number of defenders during this period. Realism, on the other hand, has received scant attention in linguistics in the period under discussion. For that reason, realism is unfamiliar to many linguists and philosophers and remains profoundly underdeveloped. Katz has devoted a number of books and articles to the topic of Linguistic Realism (Katz 1985, 1990, 1996, 1998, 2004). From the linguistic side, it was particularly Paul M. Postal who defended and further developed this paradigm (e.g., Postal 2003, 2004, 2009, 2012).

According to Katz and Postal (1991), most traditional areas of linguistic research can be subsumed under the realist framework:

> Realist linguistics requires not a *new* field, but merely *a different interpretation of an existing one*. What could remain and what would have to be eliminated require specification, but most of what generative linguistics takes to be syntax, semantics, phonology, etc., could be preserved. (Katz & Postal 1991: 531)

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Because Linguistic Realism is the least explored linguistic paradigm, much discussion regarding the ontology of linguistics remains incomplete. Therefore, this collection of papers is an urgently needed first step towards filling this lacuna. Of course, supporters of Linguistic Realism do not argue in favor of this paradigm because it has not received more attention in the past. Realist linguists are convinced that this paradigm is superior to the other paradigms. Their argumentation can be summarized as follows:

In the empirical world, we can observe that people behave in a certain way that we interpret as using language. If people have the ability to use language, they must have knowledge of language. In other words, knowledge of language is a requirement for using language. This insight was (according to Katz 1981) Chomsky's motivation for replacing the dominant framework of the 1950s (nominalism) with a novel framework (conceptualism). Chomsky provided extensive arguments that convinced many linguists and philosophers that conceptualism was the superior framework. However, Katz and Postal reasoned that if it is the case that people have knowledge of language, then the object known must have a different ontological status than the knowledge of it (e.g. Katz & Postal 1991). What kind of an object language is and how it can be known and be made use of, is what Linguistic Realism is focused on. According to this paradigm, language is an abstract object comparable to the objects of mathematics and logics, for example. Studying language as an abstract object means reconstructing language as an abstract system by giving an explicit model of a particular language.

If language is neither an empirical nor a mental object, this does not mean that linguists should not study any empirical or mental objects. On the contrary, both the use of language and the knowledge of language are important topics in linguistics. These topics are part of a comprehensive study of linguistics. But studying the use people make of linguistic objects is not studying language. It is studying language use (or performance in Chomsky's terms). Similarly, studying the knowledge people have of linguistic objects is not studying language but studying knowledge of language (or competence). Studying language as an abstract object, then, is the core of linguistics; it is here where linguistics is a science of its own, while the study of both language use and knowledge of language are highly interdisciplinary.

2. The chapters of the volume

The chapters of this book offer different perspectives on Linguistic Realism, either supporting this paradigm or taking it as a starting point for developing modified conceptions of linguistics, best characterized as a kind of modified realism. The initial chapters of the book deal with the foundations of linguistics, particularly concerning the ontological status of language and the character of linguistics as a science. Paul M. Postal, one of the most dedicated and explicit proponents of Linguistic Realism in the past 35 years, wrote the first chapter. In *The ontology of natural language*, he reflects on the ontological status of words and sentences, reviewing several approaches to that question since the advent of American Structuralism. Covering both nominalism and conceptualism under the term *naturalistic view*, he claims that such an approach is flawed because it assumes that words and phrases are time/space particulars, which leads to an incoherent conception of linguistics. Linguistic Realism – or in Postal's terms the *Platonist view* – can provide the foundation for a coherent conception of linguistics by taking words and phrases as abstract objects.

In the second chapter, David Pitt asks: *What kind of science is linguistics?* Distinguishing between empirical and formal sciences, he doubts that the ontological nature of the objects a science is dealing with exclusively determines the nature of this science. In other words: Empirical sciences do not study exclusively concrete but also abstract objects. If all sciences have the goal of discovering generalizations, then they deal with abstract objects. That is because the objects of generalizations are types and types are abstract objects. In Pitt's view, the nature of a science is determined by its methodology. This evaluation leads to a plea for the ontological diversity of linguistics.

Robert Levine discusses in *'Biolinguistics': some foundational problems* two notions of the term 'biolinguistics': In one sense – that he regards as scientific credible – this term denotes the inquiry into identifying neurological structures corresponding to the human capacity of language. In another sense, the biological base of language is interpreted literally and mental grammars are regarded as real objects. Generative Linguistics since the early 1980s is based on the second interpretation. Levine argues that the frequently cited progress in the study of visual cognition does not support this kind of biolinguistics. He furthermore reflects critically on the assumed domain specificity of linguistic knowledge, and proposes an alternative (a set-theoretical model) that takes the abstract nature of language for granted.

In the next chapter, *The relevance of realism for language evolution theorizing*, Christina Behme argues that reconsidering the ontological status of natural languages might lead to novel approaches to language evolution puzzles. Contemporary work on language evolution, focused on brain evolution, language acquisition, and communication systems of other primates, has provided a rich body of knowledge. Yet, so far such approaches have been unable to account for some aspects of grammar. Paying closer attention to the distinction between language and knowledge of language, as insisted upon by realists, could move language evolution research beyond the existing impasse.

The subsequent chapters present specific approaches within the paradigm of Linguistic Realism, beginning with two papers that present a rather general perspective. In *Describing linguistic objects in a realist way*, Hans-Heinrich Lieb argues for a *Modified Realism* as a suitable framework for linguistics. Based on the claim that languages and linguistic structures need to be regarded as abstract objects, he adds assumptions about intentionalism and functionalism to the conception. Lieb's specific approach, which he has been developing for 50 years, is *Integrational Linguistics*. By distinguishing grammars from theories of language, Lieb presents the characteristics of his axiomatic conception. He stresses the relevance of formal grammars for informal grammars as well as for comparative grammar writing.

Using a similar approach, Ryan Nefdt reflects on *Languages and other abstract structures*. In his chapter, he proposes that a linguistic paradigm must be able to deal with linguistic creativity and infinity, the ontological status of linguistic objects, and the relation of linguistic structure and competence. He then argues that none of the linguistic paradigms Katz (1981) distinguishes allow in their pure form to handle all these topics. Therefore, he proposes a conception he calls *Mixed Realism*. This hybrid view takes Linguistic Realism as a base but supplements it with assumptions of both nominalism and conceptualism. This leads to a view that linguistics is both an empirical and a formal science.

The next chapters look at a specific part of the grammar of languages and offer models how to analyze these parts in the paradigm of Linguistic Realism. In the first chapter of this section, Martin Neef explores the level of phonology. In *Autonomous Declarative Phonology: A realist approach to the phonology of German,* he argues that it is not language in general that needs to be regarded as an abstract object but a particular language. Therefore, a linguistic theory has to approach individual languages in the first case. With the example of German, he shows how a theory of phonology can be conceived by taking phonology as the study of units that have the potential to distinguish meaning in a specific language system. To this end, he reinterprets and modifies a number of concepts known from generative phonology such as CV-phonology and sonority.

Focusing on morphology, Andreas Nolda's chapter *Explaining linguistic facts in a realist theory of word formation* sketches the *Pattern-and-Restriction Theory* as a morphological approach in the realist framework of *Integrational Linguistics* as conceived by Hans-Heinrich Lieb. Nolda assumes that the word-formation facts to be described are word-formation relations about abstract lexical units in a specific language system. This approach allows dealing with conventionalized lexical units as well as possible lexical units, e.g. potential words. Nolda also provides an axiomatic reconstruction of some pertinent facts of the word-formation component of German from the field of conversion.

In *Cognitive propositions in realist linguistics*, Scott Soames focuses on semantics. He argues that without appeal to prior notions of truth and propositions, intensional truth theories do not provide any information about meaning. Soames proposes that genuine semantic theories need to map sentences to real propositions, the truth conditions of which are derived from their representational properties. In his terms, cognitive propositions are subject of linguistics proper and not of psychology. He concludes that "the primary object of the study of realist semantics is the language, not its causal origin or its realization in particular populations of speakers."

Semantics is also the subject of the next paper, *Languages as complete and distinct systems of reference*. D. Terence Langendoen explores Sapir's assessment that language as 'a complete system of reference' (a Saussure quote) is ontologically comparable with a number system or a set of geometrical axes of coordinates. Stating that Sapir's program concerning the two most fundamental properties of human language has until yet not been fulfilled, Langendoen explores aspects of arithmetic and ancient as well as recent approaches to logic that might contribute to the fulfillment of Sapir's program. He concludes with a presentation of his own recent research on extending first-order logic.

In the final paper of the volume, *The so-called arbitrariness of linguistic signs and Saussure's 'realism'*, Armin Burkhardt addresses the question in what relation linguistic Structuralism stands to Linguistic Realism. With the example of Saussure (conceived as both the linguist from Geneva and the collective authors of the *Cours*) and based on an elaborate discussion of the concepts of arbitrariness and motivation, Burkhardt shows that there are traces of a realist conception of linguistics in Saussure's thinking. In addition, there are also traces of both conceptualism and nominalism, something which is not uncommon for the period of linguistic Structuralism.

3. The history of the book

The book is a collection of papers presented at the two-days workshop *The Foundations of Linguistics: Languages as Abstract Objects* that took place in Braunschweig, Germany, in June 2015. This workshop was organized by the editors of this book with the support of Paul M. Postal who served as a scientific advisor. Though he could not attend the workshop personally, Postal was virtually present and opened the workshop with a video greeting. The workshop was the first scientific event ever that was exclusively devoted to the topic of Linguistic Realism. The different talks presented to the workshop provided both arguments for this position and responded to common criticism. Likewise, the articles of this volume explore linguistic and philosophical aspects of Linguistic Realism and hopefully offer a starting point for future debates.

The workshop was made possible by a grant of the Fritz Thyssen Stiftung (Köln, Germany) to Martin Neef (Az. 30.15.0.057SL). We gratefully acknowledge this support. We also thank the TU Braunschweig for providing the meeting space and the members of the Institute for German Studies for helping with the coordination of the sessions. We further thank the authors of the chapters of this book for their intellectually stimulating contributions as well as their ongoing support and their patience. We also thank the external reviewers who considerably added to the quality of the chapters of this book. A special thanks goes to Lena-Marie Bültemeier for help with the index.

The attentive reader will notice that two of the most prominent sons of Braunschweig, Carl Friedrich Gauß and Richard Dedekind, have found their way into this book on linguistics. Given that they have been mathematicians, it may not seem self-evident that they would have to be mentioned in this book. But given that supporters of Linguistic Realism emphasize a close relationship between mathematics and linguistics, this Braunschweig connection is more than an 'accident': Braunschweig is a 'natural' host for Linguistic Realism.

Halifax, Canada/ Braunschweig, Germany, January 2018
Christina Behme and Martin Neef

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CHAPTER 1

The ontology of natural language

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This chapter discusses natural language ontology, focusing on the nature of sentences. Two contrasting views about such elements are considered. First, the *naturalistic* view takes sentences to be elements of the physical world. There are two variants. One, which regards sentences as utterances, has few if any current advocates. The other, which views sentences as mental/biological things, is currently dominant, defining the position of Noam Chomsky. Second, there is the nonnaturalistic, *Platonist* view advocated intensively by Jerrold J. Katz, which takes sentences to be abstract objects. This view is consistent with the fact that sentences are timeless, locationless entities entering into no causal relations. Only the naturalistic view is inconsistent with actual linguistics, where sentences are uniformly treated in set-theoretical terms.

Keywords: sentence, ontology, Platonist view, biolinguistics, naturalistic imperative

In the award-winning 2002 *The Cambridge Grammar of the English Language*, the editors say the following at the outset:

> This book is a description of the grammar of modern Standard English, providing a detailed account of the principles governing the construction of English words, phrases, clauses, and sentences. (Pullum & Huddleston 2002: 2)

There is nothing ontologically special or unique to English in these remarks. Parallel declarations could be found for a multitude of other languages. What I conclude is that linguists tend to speak rather casually of natural language *words, phrases, clauses, and sentences*. They do not feel obliged to justify the existence of these linguistic entities nor to give an account of their ontology before proceeding to their descriptive or theoretical business. It would have been ludicrous, would it not, for the writers cited above to add that things like phrases and clauses are real things?

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I conclude that essentially no linguist seriously doubts things like words and sentences exist, although Noam Chomsky did at least once in effect deny it. Chapter 11 of my 2004 Oxford volume *Skeptical Linguistic Essays* provides an uncomplimentary analysis of this idiosyncratic, preposterous denial (Postal 2004).

If then the linguistic entities referred to earlier exist, a simple-minded view, in fact mine, is that the ontological question about natural language reduces to the issue of the ontological nature of words, phrases, etc. Since the ontology of all the types of things mentioned is identical, one can focus on sentences for simplicity. So what kinds of things are they? Two distinct classes of answer have existed. The sociologically overwhelmingly dominant one at every period claims that sentences are one aspect of the physical universe. The other, called Platonist or realist, associated in more recent times particularly with the work of the American philosopher Jerrold J. Katz, takes sentences to be abstract objects.

Two different versions of the *naturalistic* view (nominalism and conceptualism according to Katz (1981)) are worth mentioning. Fifty or sixty years ago, many linguists, American structuralists, as well as the British R. M. W. Dixon for example, claimed that sentences were just utterances produced by physical activations of the human vocal apparatus (nominalism). I doubt anyone proposes an utterance-based view of sentences today, and for good reasons. Specifically, each utterance has time/space coordinates, has a temporal beginning and end; sentences have none of these properties. Also there are immensely more sentences than there ever could be utterances. So at best utterances qualify only as tokens of a tiny subset of sentences.

While current linguistic culture does not take sentences to be utterances, it shares with such views an underlying drive, call it the *naturalistic imperative*. This requires that whatever one's specific ontological assumptions about natural language, they must take it to be part of the physical world. The currently dominant view (conceptualism) is that sentences are somehow psychological or biological entities, aspects of the human mind or brain. This is no doubt considered by many a much more sophisticated view than the utterance one. But ontologically speaking, this is true to an almost irrelevantly minor extent. For the mind/brain view suffers from the same flaws as the utterance ontology for sentences. Whatever goes on in mind/brains also is temporally and spatially determined, has a beginning and an end, and is vastly more limited in scope than the collection of sentences. So at best the mind/brain view also conflates sentences with some kind of tokens. Where the utterance view confuses them with external tokens, the currently popular one confuses them with internal tokens.

I cannot explain what motivates the naturalistic imperative. Perhaps there lurks the fear that without it, linguistics would fail to qualify as a *science*. The idea might be that if linguistics is not a naturalistic inquiry, it cannot be intellectually serious, rendering its access to government grants more than uncertain, what a horror. Such a view is tenable at best only by ignoring the nature of logic and mathematics. No one could claim these are not serious domains, but few would try to argue that they could be, still less have been, shown to have a naturalistic basis.

For instance, if logic were a matter of contingent empirical truth, it would be deeply mysterious what features of the physical universe it involves empirical facts *about* and why it has the prescriptive force that it is taken to have. One cannot actually violate physical laws, otherwise they would not be such, but people violate various principles of logic with regrettable regularity.

It would be bizarre at best to see the principle of modus ponens, for example, as something of the same order as the second law of thermodynamics. If though other indubitable domains of reality are non-naturalistic, natural language could also be non-naturalistic and the field which studies it could be concerned, like logic and mathematics, with a domain of non-naturalistic truth? Given this evident possibility, one might expect those most energetically advocating a naturalistic view of natural language to have developed a serious literature justifying the naturalistic view, specifically justifying it against a Platonist view. Perhaps it is just ignorance on my part but I am unaware of any such literature. While I am confident that the vast majority of contemporary linguists accept some version of the naturalistic view, its acceptance seems to me to just represent a largely unquestioned overwhelmingly culturally dominant conformism.

Current instantiations of the naturalistic view, driven, in particular, by the enormous notoriety of the views of Noam Chomsky, imply that sentences are a kind of biological entity, one involving brains or their activities. In several respects, this second variant of the naturalistic view is ontologically *worse* than the utterance view. Despite various faults, the utterance view at least provided a transparently clear notion of the ontology of sentences. But I defy anyone to determine what so-called biolinguistics takes to be their actual biological ontology. I have unsuccessfully searched Chomsky's writings to find a clear statement as to the specifics of that ontology.

I suggest that the failure of biolinguistic views to specify the ontological nature of sentences is not mere theoretical incompleteness. The lack of clear statement about the ontology of sentences in the work of Chomsky, his followers and others taking a biological view of natural language is rather due to the fact that the view makes no sense.

A non-naturalistic or Platonist view of the ontology of natural language sentences denies that sentences are part of the physical world, and takes them instead to be abstract objects, objects with no temporal or spatial properties and not entering into causal relations. It thus takes sentences to have the same ontological type as integers, sets and propositions. Various specific, technical things can be and have been said in favor of a Platonist view of natural language. One is advised to consult various works of Jerrold J. Katz in this regard (Katz 1981, 1990, 1996, 1998, 2004). But to me, the fundamental support for a Platonist view comes from the fact that it is coherent, whereas the popular biolinguistic version of the naturalistic position is not even close to coherent.

The reason for biolinguistic incoherence is simple (cf. also Postal 2009). Internal to the descriptive and theoretical work linguists actually do when they are not discoursing on grand ontological issues, essentially everything said about natural language sentences involves taking them to be abstract objects, specifically, various kinds of set-theoretical objects. But sets do not occur in brains, or anywhere else in the physical universe.

So to say that sentences are brain objects, while describing them as set-theoretical objects is incoherent, an incoherence highlighted in particular by Chomsky's recent decade appeal to the notion *Merge* claimed to be the core of natural language (e.g. Chomsky 1995, 2002, 2012). This is explicitly defined as a set-theoretical operation. Since sets occur neither in time nor in space, taking any set-theoretical object to be brain-based, as is inherent in Chomsky's ontological doctrine, must inevitably lead to contradiction. Another form of the contradiction arises from Chomsky's talk of *Merge* originating in a biological mutation, embodying the confusion of assuming a set-theoretical operation, not existing in time or space, could have a direct causal relation to an organism mutation.

If one adds, as Chomsky always has (Chomsky 2002, 2012), that natural languages have *infinitely* many sentences, a coherent claim if sentences are abstract objects, further contradiction is inevitable. From the view that each of the infinite sentences is a brain object arises the contradictory entailment that a finite brain can incorporate infinitely many objects or actions. A brain would have to be a sort of Hilbert's Hotel for sentences. But Hilbert's Hotel is an infinite abstract object unconstrained by physical limitations, not a physical thing like a brain, and thus its nonfinite property yields no contradiction. Or, if, as one sometimes reads, the claim is instead that the brain incorporates a finite biological coding which has infinitely many *potential* outputs, contradiction is seemingly avoided. But this is intellectual slight of hand since almost all 'potential sentences' forming any infinite set are not even possible biological objects. Think of a sentence which is four hundred trillion words long. Rather, they are at best abstract objects occurring nowhere in space and nowhere even in future time. So this slight of hand does not save the biolinguistic version of a supposed infinite natural language. Rather, it *surreptitiously* abandons a naturalistic position for a Platonist one.

Astoundingly, as pointed out in my *lingbuzz*-posted article *Chomsky's Ontological Admission* (Postal 2012), Chomsky's 2012 book *The Science of Language* essentially *admits* the incoherence of his biolinguistic position, saying, we have to accept things that don't make sense, like sets. This laugher implicitly recognizes the contradiction between claiming natural language reality is biological, while taking sentences to be set-theoretical. Striking is that even in the face of recognized utter contradiction, the naturalistic drive is so strong for Chomsky that he would rather accept incoherence and promulgate it to others than abandon it. And as justification he merely absurdly denigrates those elements, sets, which are the foundation of work in logic, mathematics and even his own linguistics. Apparently, abandoning his naturalistic ontology is unthinkable. However, since neither he nor anyone else knows how to do linguistics on a *non*-set-theoretical basis, he could only embrace incoherence. Where the fact that a position does not make sense normally suffices as argument for abandoning or modifying it to eliminate the corrosive assumptions, Chomsky's a priori commitments lead to a ridiculous demand that others accept his incoherence. One might ask him why, when he received Russell's observation that his set-theoretic ideas yielded contradictions, Frege didn't just reply that one needs to accept things that don't make sense, like sets.

What no doubt especially drives the view that linguistics must be concerned with something mental or biological is a confusion between language and knowledge of language. This confusion underlies the endless talk in current linguistics about language learning and its preconditions in human nature. But one can and should grant the banality that knowledge of language is something mental/biological without accepting the dogma that therefore language itself is. Think of the analogy with logic.

Contrasting with the incoherence of a naturalistic view of natural language, in Platonist terms there is of course no contradiction between the set-theoretical nature of actual linguistics and the basic ontological view that sentences are abstract objects.

So if for some arcane reason one refuses to accept things that don't make sense and stubbornly seeks a coherent ontology of natural language, the bottom line is as follows. If one can't accept identifying sentences with utterances (also a view incompatible with the actual set-theoretical notions linguists appeal to), one must, as far as I can see, accept a Platonist view.

But if you do, I would advise being quiet about it or you might not get a grant.

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CHAPTER 2

What kind of science is linguistics?

David Pitt
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I argue that what determines whether a science is 'formal' or 'empirical' is not the ontological status of its objects of study, but, rather, its methodology. Since all sciences aim at generalizations, and generalizations concern types, if types are abstract (non-spatiotemporal) objects, then all sciences are concerned to discover the nature of certain abstract objects. What distinguishes empirical from formal sciences is how they study such things. If the types of a science have observable instances ('tokens'), then the nature of the types may be determined empirically. If they types have either abstract tokens, or no tokens at all, their nature must be determined by non-empirical methods involving intuition, reasoning and proof. I conclude that the status of (theoretical) linguistics depends on the methodologies of syntax, semantics, phonology, morphology and orthography (and any other subdiscipline that is concerned with the study of the structure of language).

Keywords: empirical, formal, ontology, methodology

1. The nature of formal and empirical sciences

More specifically, the question this chapter addresses is what kind of science is theoretical linguistics – i.e., the study of the syntactic, semantic, phonological, morphological and orthographic *structure* of language? Well (one might ask), what kinds of sciences *are* there? The most fundamental distinction to be made is between sciences that are *empirical* and sciences that are *formal*. Empirical sciences, such as physics, chemistry and biology, are *a posteriori*, essentially involving observation and experimentation. Formal sciences, such as mathematics and logic, are *a priori*, and essentially involve intuition, reasoning and proof in place of observation and experimentation. Both kinds of sciences also feature theory construction as a central component.

Historically, there are three main positions in the foundations of linguistics, which are distinguished by what they take the ontological status of languages to

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be. Linguistic *nominalists* hold that languages are collections of physical objects – marks and sounds. Linguistic *conceptualists* hold that languages are psychological objects, such as mental representations of rules or grammars. Linguistic *realists* hold that languages are abstract (non-spatiotemporal) objects. It is frequently assumed in debates among adherents of these positions that the status of a science is determined by the ontological category of its objects of inquiry,[1] and, hence, that a science is empirical if and only if its objects of inquiry are empirical (i.e., observable, concrete (spatiotemporal) entities and phenomena). Thus, nominalists hold that linguistics is a physical (empirical) science, conceptualists that it is a psychological (probably empirical) science, and realists that it is a formal (non-empirical) science.

Historically, linguistic nominalists (the American structuralists) adopted the extreme physicalism of the logical empiricists (the Vienna Circle), with its attendant view about the status of empirical science. Since on this view only concrete particulars are real, only empirical science is genuine science, and all inquiry into alleged non-physical reality is pseudo-science. Thus, the nominalists held that linguistics, if it is to be a real science, can be concerned only with physical, observable objects and phenomena. Such purported entities as 'ideas' (subjective mental things) and abstracta are not empirically accessible, and, hence, hypotheses about them are not amenable to third-person scientific methods of evaluation. So they can play no role in the constitution or scientific study of language. Marks and sounds, on the other hand, are empirically observable physical objects, and so are fit for genuine scientific study. Hypotheses about them (e.g., concerning their distribution) can be objectively confirmed or disconfirmed. Hence, the nominalists held that languages are just collections of marks and sounds, and that linguistics is the empirical study of them.

It is not clear whether the logical empiricists were more fundamentally motivated by ontological or epistemological concerns. On the one hand, they had a shared aversion to traditional metaphysics and its mysterious non-physical entities. On the other hand, they were impressed with the abject failure of philosophy to make significant progress on its central metaphysical questions, and the contrasting brilliant successes of the empirical sciences. If their motivation was primarily their constitutional distaste for the non-physical, then their empiricism can be seen as a non-foundational corollary. But if their motivation was primarily epistemological, then their physicalism (and nominalism) can be seen as a non-foundational corollary. Linguistic nominalists can thus be understood as arguing

1. Thus, Katz (1996: 282): "the nature of the objects which constitute the subject-matter of a science determines the nature of the science."

either that languages are marks and sounds because linguistics is empirical, or that linguistics is empirical because languages are marks and sounds. In both cases, however, it is clear that the status of the science and the nature of the objects it studies go hand in hand.

Linguistic *conceptualists* hold that languages are mental objects of some kind, and that linguistics is thus branch of psychology. Whether or not this makes linguistics an empirical science depends upon the nature of the relevant mental objects. It is possible to be a Cartesian dualist (i.e., a non-physicalist) about the mental, and hold that the relevant psychological entities and phenomena are not physical and, hence, that linguistics is not an empirical science. In this case, our access to linguistic reality would be entirely through conscious introspection, and linguistics would be a discipline more like traditional Phenomenology than modern psychology. However, linguistic conceptualists have typically maintained that the relevant psychological structures (e.g., the 'language organ') are *brain* structures and, hence, that psychology is, ultimately, a branch of biology. So, for most linguistic conceptualists, linguistics is an empirical science, since psychology is.[2]

In contrast to both the nominalists and the conceptualists, linguistic *realists* hold that languages are abstract (non-spatiotemporal) objects. Sentences are *types* – abstract objects – and as such are in the same ontological category as numbers, sets and propositions.[3] They are not things that can be discovered or studied using empirical methods. Hence, given the assumption that a science is empirical if and only if its objects of inquiry are concrete, according to the realist linguistics must be a formal science, on a par with mathematics and logic, and its methods must be non-empirical.[4]

I think this way of thinking about the foundations and status of linguistics is mistaken. It is not true that the ontological category of the objects constituting the subject-matter of a science determines its nature, or that a science is empirical

2. Since spoken and written expressions (sounds and marks) are not mental objects, conceptualists must hold either that such things are not linguistic at all, or that they are not the fundamental or most important linguistic entities, or that only part of linguistics (the theory of syntax – grammar) is psychological.

3. Abstract sentence *types* are to be distinguished from their concrete *tokens*. For example, the first sentence of this footnote might be written twice on a page – in which case there would, in one sense (the *token* sense) be two sentences on the page. In the *type* sense, however, there would only be one sentence, repeated, on the page.

4. Katz again (1996: 292): "Given that grammatical questions are about types, that is, about abstract objects, they cannot be answered on the basis of causal interactions with natural objects."

if and only if it studies concrete objects. Empirical sciences may, and typically do, have abstract objects as their ultimate objects of inquiry. Moreover, it is not the case that the empirical/formal distinction is exclusive. A science may have both empirical and formal aspects or departments (as, indeed, I will argue, may be the case with linguistics). *All* sciences are, fundamentally, concerned to discover *generalizations*. But the *objects* of generalizations are *types* (or *kinds*), and types (kinds) are abstract objects. Hence, all sciences, whether 'empirical' or 'formal,' have abstracta as their ultimate objects of inquiry.

Physics, for example, is in the business of discovering general truths – *laws* – about such things as particles, fields, forces and processes, as *types* (*kinds*). Physicists want to know what is true of electrons *in general* (electrons as a kind of particles), not some particular electron.[5] Likewise biology (*species, biomes*), chemistry (*acids, enzymes*), astronomy (*spiral galaxies, M-type stars*), zoology (*mammals, insects*), and all of the other empirical, natural sciences. Any theoretical science, whether empirical or formal, seeks systematic accounts of its proprietary kinds, and so is ultimately concerned with discovering the nature of abstract objects of various kinds.[6]

This is, of course, not to say that individual electrons, animals or stars are abstract objects. Nor does it follow that physics, biology and astronomy are subdisciplines of mathematics or logic. Clearly, there are important differences between the natural and the formal sciences. For one thing, natural scientists are not concerned with studying *all possible* kinds of their proprietary sorts. Biologists, for example, are interested in studying *actual*, not merely possible, species (though these latter might be of interest as entailed by general theoretical principles). Mathematicians and logicians, in contrast, are centrally concerned with what is possible, since this forms their (actual) domain of inquiry.

I do not want to deny that there is a real distinction between empirical and formal sciences. But I do want to deny that the distinction depends upon the ontological categories of the objects of their generalizations, since these are one and all abstract objects – *types* or *kinds*.

5. Though of course facts about particular electrons constitute evidence for the theory of electrons as a kind. This point will take center stage shortly.

6. If you do not think types are abstract objects, then read the argument as follows: *even if* physical, biological, etc. types were abstract objects, this would not make physics, etc. formal sciences; hence, it is not the case that a science is empirical if and only if its objects of inquiry are concrete.

2. Methodology vs. ontology

What determines what kind of science a science is, is, rather, its *methodology*. It is *how* its proprietary kinds are discovered and investigated that is important – that is, how we come to know which types are proprietary, and how we determine their nature. Methodology is in turn determined by ontology, but not by the ontological category of the proprietary kinds, the ultimate objects of inquiry, or 'subject-matter' of the science. Rather, it is the category of the *tokens* (instances) of those types that is relevant. Since truths about types are obtained by generalizing upon facts about their tokens,[7] *their* ontological categories determine which methods of inquiry can or must be used. If the proprietary tokens are spatiotemporal objects, empirical methods are appropriate for the discovery and study of the types; if they are abstract objects, formal methods are required.

In physics, facts about individual concrete, observable (albeit indirectly) electrons are evidence for the theory of electrons as a kind. And this is what makes physics an empirical science. We can call this 'Methodological Nominalism,' and contrast it with the 'Ontological Nominalism' characterized above. Ontological Nominalism says that a science is empirical if and only if its objects of inquiry are physical. Methodological Nominalism says that a science is empirical if and only if it studies its proprietary abstract kinds through empirical observation of their physical tokens.

If the tokens are psychological objects, then psychological methods are called for. We can call this 'Methodological Conceptualism,' and contrast it with 'Ontological Conceptualism.' Ontological Conceptualism says that a science is psychological if and only if its objects of inquiry are psychological. If psychological objects in turn are physical (e.g., brain states or structures), then Ontological Conceptualism collapses into Ontological Nominalism, and psychology is the study of a kind of physical (biological) objects. If on the other hand psychological objects are not physical, then Ontological Conceptualism implies that psychology is the study of such non-physical (though non-abstract, since they exist in time) objects. Methodological Conceptualism says that psychology is the science of psychological types, which are abstract, and that the status of psychology, whether it is empirical or introspective, is determined by the nature of the tokens of those types.

7. *This* sample of aluminum conducts electricity, *that* sample of aluminum conducts electricity,…; hence, *aluminum* conducts electricity. Higher-order generalizations go from facts about types to facts about higher-order types: *aluminum* is a metal and conducts electricity, *copper* is a metal and conducts electricity,…; hence, *metals* conduct electricity.

If the syntactic rules of English are represented in the brains of competent speakers of English, then knowledge of those rules could, at least in principle, be gleaned from the study of competent speakers' brains. (This need not involve surgery.) This is completely consistent with holding that sentences, and the rules that generate them, are abstract objects. It simply does not follow (as Katz, Postal and others seem to think) from this kind of conceptualism that sentences themselves are psychological objects. But even if it did, we could still think of sentences as psychological *types*, and, if we wanted to insist that there are infinitely many of them, we could consistently hold that some (most) of these types are untokenable by finite minds.

Perhaps some conceptualists conceive of the 'generation' of sentences by rules that are mentally represented as actual *production* of them. But one need not think this way. One may just as well say that the infinitely many abstract sentence-types of a language are not *generated*, but have structures that can be accurately *described by* recursive rules. It no more follows (*pace* intuitionists and constructivists) that someone or something has to *put them together* than that the range of a recursive function on numbers (i.e., *numbers*) does not exist until the function is applied to them. Recursive rules are not like machines (or people) in a factory assembly line.

If, on the other hand, the tokens are abstract, formal methods are required. We can call this 'Methodological Realism,' and contrast it with 'Ontological Realism,' which says that a science is formal if and only if its objects of inquiry are abstract. Methodological Realism says that formal methods are required only if the types that are the objects of inquiry have abstract tokens (or if they are not *types* at all, but abstract particulars, like numbers). On this way of categorizing disciplines, mathematics and logic turn out to be formal sciences, since the types they study – *prime number, rectangle, proposition, set* – have abstract tokens, while physics, biology and astronomy are empirical, and psychology is either empirical or introspective.[8]

In sum, the sciences study types, and the natures of types are discovered by studying and generalizing from facts about their tokens. What kind of discipline a particular science is, is determined by what kinds of tokens its proprietary types

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8. Geometry is an interesting case. Physical 'rectangles' are not rectangles, since they are three-dimensional, while rectangles are two-dimensional. Yet in geometry we do reason from three-dimensional physical representations to conclusions about abstract two-dimensional particulars. The properties we abstract from the representations, in a process of idealization, are properties of abstract tokens, from whose properties we learn about the abstract types. So, though tokens of two-dimensional geometrical types are themselves abstract objects, we study them (in part – pure definition and reasoning also play a role) by studying concrete tokens of similar three-dimensional types, ignoring the obvious differences.

have. So we cannot infer from the claim that physics and biology use empirical methods that they are not in the business of discovering and describing abstract objects. Nor can we infer from the claim that languages are abstract objects that linguistics is a formal science. It depends on *what kind* of abstract objects languages are, in particular, on what kind – abstract or concrete – of tokens their proprietary types have.

3. Linguistic kinds: Sentences

So the relevant questions to ask about linguistics are What are its proprietary kinds? and What kinds of tokens do those kinds (types) have? To begin, we can say that the linguistic kinds are, at least, *language, languages* and *sentences* (*expressions*). Thus, we can ask questions such as What is language? What is *a* language? What kind of language is English? What are the properties of English sentences in general? and What are the properties of this particular English sentence?

To fix ideas, let us focus on (English) *sentences*. Sentences appear to be *types* – they are repeatable, shareable entities. A sentence tokened (uttered) at one time can be tokened (uttered) again at another time. And a sentence can be tokened more than once at a given time. These are the hallmark properties of types (universals). We count sentences in two ways. There are two correct answers to the question how many sentences there are in the box below:

> The present king of France is bald.
> The present king of France is bald.
> The present king of France is bald.
> The present king of France is bald.

We can say that there is one sentence, and we can say that there are four sentences. Intuitively, however, the correct thing to say is that there is *one* sentence written four times: there are four tokens of one sentence type. Counting by types seems more fundamental. There is only one sentence *The present king of France is bald* in English (just as there is only one letter <e> and one word *the*), and it is written four times in the box. Thus, if types are abstract objects, then so are sentences. And if languages are sets of sentences, then languages are abstract objects too.[9]

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9. In fact, it is *not* the case that the sentence type *The present king of France is bald* is tokened four times in the box above, since there are no phonetic tokens there. So what we must say is

But, again, it does not follow from this that theoretical linguistics is a formal science. In order to determine what kind of science linguistics is, we must consider what sorts of methods are used for studying the tokens of the relevant types. Since written and spoken sentence-tokens are physical objects (the marks and sounds focused on by the nominalists), orthography and phonetics (phonology) will employ physical/empirical methodology to study these aspects of sentence structure, and so will count as empirical subdisciplines of linguistics.[10]

But sentences have syntactic and semantic properties as well. Perhaps we can think of them as bundles of types of various kinds, or complex types. So the nature of syntactic and semantic types would have to be determined before a final verdict on the status of linguistics could be reached.

While the determination of the status of phonetics (phonology) and orthography is straightforward, syntax seems to me to be much more problematic. For, it is not obvious (at least to me) what of kinds of tokens syntactic types (structures) have – or indeed if they are even tokenable types at all. It is plain that they are not intrinsic properties of marks or sounds. For example, the string of marks

(*) John is eager to please the present king of France

has, *qua string of marks* (physical objects), only *geometrical* structure: it is a series of physical tokens of shape-types. Moreover, being a proper noun is not an intrinsic property of the series of shapes 'John'; nor is being the direct object of *please* a property instantiated in the series of shapes 'the present king of France'. So it cannot be that (*) has syntactic properties in the way in which it has orthographic properties – or the way in which an utterance of it would have phonetic properties. The marks and sounds do not have syntactic structure intrinsically; *they* are not tokens of syntactic types. (The existence of *covert* syntactic structure – e.g., the presence of PRO or *trace* in the syntactic structure of a sentence – only strengthens this point.)

If written and spoken sentence tokens do not *instantiate* syntactic structures – that is, if they are not *intrinsically* related to them – then if they *have* them it must be by being *extrinsically* related to them in some way, or to something that does instantiate them. If we suppose, for example, that it is *meanings* that have the structures syntacticians assign to sentences, then written and spoken sentence

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that a *part* of a sentence type is tokened, or (equivalently, I think) that a sentence is *partially* tokened, in the box.

10. I am not clear about the status of morphology, since morphological properties are at least in part *grammatical*, and so might better be classed with syntactic and/or semantic properties. Morphology itself might be a mixed science (even 'generative,' in the sense of the generative semantics of the 1960s and 1970s). Likewise phonology. I leave the application of my argument to these branches of linguistics to the experts.

tokens would have syntactic structure in virtue of having meanings. And since marks and sounds do not instantiate meanings either, the *having* relation between sentences and their syntactic structures would remain extrinsic in this case.

Alternatively, syntactic structures could be structures of mental representations, in which case syntax would be a department of psychology, and hence empirical if psychology is. It does seem unlikely, however, that – even given a language of thought – brain states *literally* have syntactic structure. For example, it does not seem to make sense to talk about neural assemblies, or patterns of activation, being, e.g., in the C-command relation. (But, again, I leave matters to the experts. I am simply concerned to argue that if syntactic types (as opposed to generative rules) are tokened in the mind/brain, then the methodology of syntax is that of psychology (or neuroscience, or whatever).)

Another possibility is that syntactic structures are not tokenable types at all – that they are, rather, abstract particulars (like numbers), which are not *instantiated in* (*tokened by*) *anything*.

This strikes me as very implausible. While the relations between marks and sounds and their meanings and syntactic structures do not seem to be *intrinsic* (that the marks and sounds of English have the syntax and semantics they do is in some sense contingent) the relation between meaning and syntactic structure seems much more intimate. It does not seem possible that, for example, the *proposition* that the present king of France is bald has a structure that is arbitrarily related to the syntactic structure assigned to the sentence that expresses it. Surely *some* syntactic structure is *logical* structure, and logical structure *is* the structure of propositions. It scarcely seems coherent to hold that (e.g.) the syntactic relation *being the direct object of* is *arbitrarily* mapped onto (or from) the logical relation predicated of István and his dogs in the proposition *István walked his dogs*, or that 'the greater of *a* or *b*' is ungrammatical while 'the greater of *a* and *b*' is not, has nothing to do with the logic of the greater-than relation. (This notwithstanding the fact that some grammatical (or stylistic) rules – e.g., against splitting infinitives, ending a sentence with a preposition or beginning a sentence with a conjunction – do not have much to do with *meaning*.)

4. Discovering and investigating meaning structure

Thus, it seems most plausible that syntactic structure – at least *deep* structure (e.g., the kind of structure represented at LF), as opposed to *surface* structure (e.g., arbitrary things like word order) – is kind of *meaning* structure (i.e., a kind of structure that meanings have). If this is the case, then the fate of syntax is intertwined (at least) with the fate of semantics.

One might argue that semantics is methodologically empirical, since its task is to determine what the meanings of words, terms and sentences in a particular language are, and this can be done by asking native speakers what the expressions of their languages mean. Thus, even if languages are (Lewisian) functions from strings of abstract mark- or sound-types to abstract propositions, determining *which* language a given population speaks – i.e., which of those functions is English, or Hungarian – is not a 'top-down' enterprise, requiring a priori non-empirical access to abstracta. We need only determine through interrogation or observation what competent speakers mean when they use expressions of their languages, and theorize about the results.

This strikes me as rather like saying that mathematics is empirical because we can discover mathematical truths by asking mathematicians what they think about numbers. There is a more fundamental issue that is being evaded – *viz*., how it is that one's informants know what they know about the abstract objects in question in the first place? If speakers' reports about which meanings go with which expressions are a source of data for semantics, then the reliability of such data depends upon the reliability of the informants. And this in turn depends upon *their* having access to the meanings of their expressions (as well as to the facts about how those meanings are paired with the expressions of their languages). Presumably they do not accomplish this by having someone ask them what they mean. Presumably, competent speakers have a kind of privileged access to what they mean – *they* know in a way the investigator does not. (Though of course investigators have the same sort of access to what *they* mean by their words.) In the case of mathematics, if we rely on the reports of mathematicians it is because we suppose that mathematicians have access to the facts about numbers. They have their ways. (Which, of course, at least in the nearer reaches of the numerical underworld, are our ways as well.)

The question then becomes how speakers have access to meanings. And this in turn depends upon what meanings *are* – i.e., assuming that they are abstract objects, whether they are tokenable types or not, and, if so, what the ontological status of their tokens is.

Sentence meanings are typically taken to be *propositions*, which are typically taken to be mind- and language-independent abstract objects having truth-conditions essentially. Further, such things are usually held to be such abstract *particulars* (i.e., not types) as n-tuples of objects and properties, functions, or sets of possible worlds. If sentence meanings are understood in this way, then semantics, the study of meanings, is a formal science, since meanings so understood do not have tokens at all (though they are themselves abstract tokens of higher-order abstract types). On this view, our apprehension of meanings is non-empirical. We discover and investigate them through intuition and a priori reasoning (e.g., postulates, proofs, models), much as we discover and investigate numbers.

Some philosophers are very worried about ending up having to say that a science, even mathematics, is about abstract, non-spatiotemporal objects, since such objects would seem to be inaccessible to spatiotemporal scientists. It seems to me that the way out of this problem (Benacerraf's (1973) epistemological problem for Platonism) is to maintain that numbers – and all other abstract particulars – are *theoretical entities*. If we reflect on *why* anyone thinks there are such things as numbers, and *why* anyone thinks numbers are non-spatiotemporal, it should become clear that these things are not *encountered* in perception or thought, and then investigated. (Though, I would argue, *concepts* of numbers *are* encountered in thought.) They are, rather, *postulated* in order to explain certain facts (e.g., that the sentence '2 + 3 = 5' is true), and to account for our intuitions about what such things could be (e.g., that it is *absurd* to suppose that 5 has mass, location, size, charge, credit rating, etc.). Similarly, reasoning from intuitions about the similarities among things, the repeatability of properties and relations, the impossibility of, e.g., *red* becoming *green* (though all red *things* can change their color to green), and so on, leads to the hypothesis that types and universals exist, and are non-spatiotemporal. There may in fact *be* no such things; but it is wrong to suppose that the only reason one can have to believe in their existence is that one has had perceptual or cognitive *contact* with them. Moreover, it is not clear that a hypothesis has to be even eligible for empirical confirmation or falsification in order to be scientifically legitimate. (Some recent theories in physics and cosmology, for example, include empirically untestable hypotheses.)

But if speakers have *direct* access to meanings – i.e., if they are not theoretical entities, but things we do have direct cognitive contact with – then thinking of them as untokenable abstract particulars would entail that they have direct intellectual contact with abstract objects. And this would seem to require a kind of cognitive faculty that few believe humans possess.

On the other hand, if we suppose that linguistic meanings are identical to thought contents,[11] then there is a way out of this problem. For there are good reasons to think that thought contents are directly accessible *introspectively* as a kind of *experience*.[12]

11. This is the default (though by no means uncontested) view in analytic philosophy of mind and language. The thesis that the intentionality of language is inherited from the intentionality of thought is what prompted philosophers to pursue meanings into the mind, and to found the 20th century 'psychosemantics' industry (whose captains were Dretske and Fodor).

12. Soames (2015) defends a similar view of meanings.

5. The phenomenology of meaning

I have argued elsewhere (Pitt 2004) that it is possible for a thinker to know the contents of his occurrent conscious thoughts (i.e., *what* he is occurrently consciously thinking) introspectively and non-inferentially, but that this could only be possible if occurrent conscious thoughts had a *sui generis* kind of phenomenology – what I called a 'cognitive' (or *conceptual* or *propositional*) phenomenology. According to the view I call 'intentional psychologism' (Pitt 2009), there is a phenomenology of occurrent conscious thought that is *proprietary, distinctive* and *individuative*. By 'proprietary' I mean peculiar to thought, and as different from more familiar kinds of phenomenology (visual, auditory, olfactory) as they are from each other. By 'distinctive' I mean that thoughts with different contents have different phenomenologies of the cognitive kind. Conscious thoughts are distinguished one from the other in the same way that visual, auditory and olfactory experiences are – phenomenologically. By 'individuative' I mean that the cognitive phenomenology of a conscious thought is its content – in the way that the phenomenology of a visual or auditory experience is its content. To think that *p* is to token a maximally determinate complex cognitive phenomenal property – i.e., to have a specific kind of cognitive experience. Thoughts are individuated by their contents, which are experiences, and such experiences, qua types, are repeatable and shareable.

This view is further supported by considerations concerning the individuation of conscious states generally (see Pitt 2011). Since conscious experiences *as such* (i.e., *qua* conscious states) are individuated phenomenologically, conscious thoughts, which are not reducible to experiences of more familiar kinds, must have their own, proprietary, distinctive and individuative propositional phenomenologies. The first (epistemological) argument claims that *we* distinguish conscious thoughts one from another, and from all other kinds of conscious states, on the basis of their proprietary phenomenology, while the second (metaphysical) argument claims that *they* are distinguished, one from another and from all other kinds of conscious states, on the same basis.

So, if linguistic meanings are thought contents, the study of meaning is the study of experiences of a certain kind, and the methodological status of semantics depends upon how experience is studied. At present, empirical psychology (experimental psychology, neuroscience, cognitive psychology, et al.) has nothing at all to tell us about how it is (how it *could be*) that brain activity gives rise to consciousness. So we cannot rely on it to tell us anything about the nature or structure of cognitive experience – at least not in the first instance. Insofar as it is useful (and I am *not* saying it is not) in the study of consciousness, empirical psychology depends upon independent access, through subjects' introspective reports, to the phenomena in question. These form the foundation of the study of consciousness and experience. Discovering the 'neural correlates of consciousness,' foundational to the establishment of an empirical theory of consciousness, itself essentially depends upon first-person access to conscious states. Without it there would be nothing to correlate, nothing to explain, nothing to theorize about. So, at least for the present, the study of experience, and of meaning, is (on the present view) fundamentally an introspective enterprise. And the status of introspection itself, as a form of observation, will determine what sort of science semantics is.

Is introspection empirical? Well, it is in the sense that it involves *experiential* access to its objects. Yet at the same time it is not, in the sense that it does not afford *intersubjectively shareable* access to its objects. The latter fact may to some extent be ameliorated by the possibility of shareable *indirect* access, as can be achieved by empirical psychology. But, again, I would argue that such empirical methodology is not foundational in the study of experience, or, in consequence, on the view put forth here, the study of meaning (and perhaps syntax). The primary access we have to semantic data, as to experience in general, is introspective. This need not render semantics 'unscientific' (in the sense that Introspectionist psychology was deemed to be such), or unrigorous. There is careless, sloppy, undisciplined introspection, and there is careful, precise, systematic introspection. The latter is, I would argue, exactly what able semanticists (and perhaps syntacticians) are good at.

6. Linguistics as a mixed science

So far, then, linguistics would seem to be a mixed science, having straightforwardly empirical departments – orthography, phonetics – whose token objects of study are concrete, and a psychological department – semantics – whose token objects of study are a particular kind of conscious experiences. Whether or not this is another empirical part of theoretical linguistics depends upon the epistemic status of introspection.

The outstanding question (for me, anyway) concerns the status of syntax. If (*pace* my arguments above) syntactic structures are literally instantiated by written or spoken sentence tokens, then syntax is a physical science. If they are instantiated by meanings, then syntax is (on the view developed here) a psychological science. If they are abstract particulars not instantiated by anything, then syntax is a formal science.

One need not agree with me about the nature of meaning, the status of the psychology of conscious experience or the place of syntactic structure in order to accept the main point of this paper, which is, to repeat, that the kind of science a science is, is determined not by the ontological status of its ultimate objects of inquiry, but by the methods used to study them. Even if these ultimate objects are abstract, it does not *follow* that the science is formal. What determines the status of a science is not the ontological category of its proprietary *kinds*, but its methodology; and what determines its methodology is the ontological category of its proprietary *tokens*. Since theoretical linguistics is methodologically diverse, it is more than one kind of science.[13]

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13. I have benefitted from discussion of these issues with participants in the workshop on realist linguistics held at the University of Braunschweig, June 2015, especially Geoff Pullum, Christina Behme and Ryan Nefdt.

CHAPTER 3

‘Biolinguistics’

Some foundational problems

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The ‘Biolinguistics’ program seeks to establish specific neuroanatomical models corresponding to the representations and operations characterizing the species-specific language faculty in human beings. Yet after decades of research, no neural structures corresponding to specific linguistic structures, rules, constraints or principles have ever been identified. A key to biolinguistics’ failure is, I suggest, its long-term adherence to two dubious assumptions: (i) a kind of literalism in envisaging the relationship between neural anatomy and linguistic representations, reflecting a seriously misconstrual of Marr’s (1982) tripartite division of cognition, and (ii) a view of such representations as objects fundamentally different from other components of human cognitive capacity. (ii) rests on the premise that phrase markers are the optimal formal representation of natural language sentences, despite major empirical difficulties that syntactic accounts based hierarchical phrase structure face in handling a wide variety of grammatical patterns, including non-canonical coordinations and ellipsis constructions. In contrast, proof-theoretic approaches such as type-logical grammar do not face these difficulties, and their foundational assumptions link language to the higher-order cognitive functions supporting deductive reasoning. This conclusion suggests a promising alternative to the current, essentially result-free ‘Biolinguistic’ paradigm.

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1. Preview: What’s wrong with ‘biolinguistics’

The appearance of scare quotes in my title and in the heading to this introductory section points to a critical distinction maintained throughout the discussion below. There is a perfectly respectable interpretation possible for the term ‘biolinguistics’, which came into common use during the past decade, in which the term

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identifies a domain of inquiry which aspires to identify neurological (and other anatomical) structures corresponding to the human linguistic capacity. This aspirational use of the term seems quite reasonable, in the same way that 'exobiology', denoting the study of extraterrestrial life, is. There are actually no exobiological results – we have yet to detect life anywhere except on Earth; but there is a very well developed body of interdisciplinary studies about the conditions under which life can evolve, where and how in the universe such conditions might be realized, what form it might take, and so on. In the same way, there are, as I argue below, no results that would come under the heading of a biocomputational theory of linguistic cognition that bear comparison with, for example, proposals that would legitimately describable as the biocomputational theory of visual cognition. But, rhetorical posturings aside, no one, regardless of theoretical framework or ontological stance, denies that the human capacity for language rests on a neurological basis. The evidence for this conclusion is overwhelming and has been taken as a given from the time Broca's discoveries became widely known. Several of Brodmann's areas in the neocortex are heavily implicated in speech and language comprehension and production areas. What remains a complete mystery is how specific neuroanatomical structures 'run' native speakers' knowledge of their languages, and, most contentiously, what the relationship is between that knowledge and the formal contents of the various grammar architectures proposed by linguists as the optimal theory of those languages. A field called biolinguistics which carefully attempts to lay out the many issues, lines of investigation, and potential problems with such investigation surely has a claim to scientific credibility, even in the absence of any robustly confirmed proposals. But that isn't the sense of the 'biolinguistics' referred to in the header, whose scare quotes are deliberate.

What I'm referring to is rather the sense of the word as it appears in the following comment by Gillian Ramchand, quoted in Martins and Boeckx (2016: 2).

> There are parts of generative grammar that I do not feel a particularly strong part of. For example, I am not sympathetic to recent trends in Biolinguistics, which to my mind is guilty of extreme Overreach in attempting to connect linguistics to Biology. I think it gives the whole field a bad name. The granularity gap and the terminology gap (to put it in Poeppel's terms) are still too great to sustain the specific kinds of proposals that are being taken seriously in this sub-group.

The kind of idea that Ramchand appears to be objecting to is the proposition, seriously offered by prominent long-time adherents of P&P approaches, that grammars are *literally* instantiated in neural tissue. An extreme example is the statement, made in the course a debate with Dan Everett on the relationship between linguistic capability and human biology, in which Anderson and Lightfoot assert that

> the view that if linguistics were as we claim, syntactic trees should be visible in CAT scans, asserted in Everett (2001), seems to reflect more on the adequacy of current brain imaging techniques than it does on the nature of language.
> (Anderson & Lightfoot 2006: 81)

On the face of it, this observation is analogous to a prediction that with sufficiently powerful telescopes capable of inspecting regions of space subject to enormous curvature due to to some compact massive object, we should be able to visually detect the field equations of general relativity, and has about as much credibility. But views of this sort are sufficiently common that Ramchand identifies them as *trends.* The authors speculate that possibly she is refering to "'biolinguistic' proposals made [by] those who use term as just another name for the kind of linguistic theorizing that has been common practice for decades" (Martins & Boeckx 2016: 2), but it strikes me as far more likely that her remarks were directed at precisely the kind of view expressed by Anderson and Lightfoot's retort to Everett, identified in her statement as a common enough view of this field to be a source of intellectual discomfort with its name. Nonetheless, Martins and Boeckx's suggestion calls attention to an important thread in the development of that view.

The story here runs along the following lines: for decades, the standard view of natural language grammars is that they are formal accounts of that capacity – or, more precisely, of the specific instantiation of that capacity in the minds of adult native speakers. On this view, dominant in transformationalist circles since the early 1980s, such speakers have fixed various parameters left open in a pan-human 'language organ', an initial state of the brain which, exposed to linguistic data in infancy and childhood, sets values for these parameters that successive approximate the input data until a stable state, the adult grammar, is achieved. This model of the language acquisition process, in which the initial state consists of a set of general principles with the values of certain parameters undetermined at the outset, and only set in accordance with input data during the linguistic maturation process, has been the dominant paradigm in the field for nearly four decades – more than half of the entire career of generative grammar, supposedly accounting for both the course of language development in individuals and the range of possible variation among natural languages.[1] In earlier work, Chomsky identified

1. Remarkably, however, there is no actual theory of the parameters which play such a crucial role in this 'Principles and Parameters' (P&P) view, nor any concensus on critical questions such as what dependencies hold amongst the set of parameters, or even what parameters there are in the first place. See Newmeyer (2005) for a broad and deep critique of the network of assumptions, and their confrontation with results from typological research.

linguistic cognition in what seem to be metaphorical terms, seemingly hedged to avoid any specific ontological commitment to the nature of this mental ability:

> We may regard the language capacity *virtually* as we *would* a physical organ of the body and can investigate the principles of its organization, functioning and development in the individual and the species.
>
> (Chomsky 1976: 46, emphasis added)

Somewhat later, however, the 'stable state' corresponding to adult grammars is explicitly identified as a having a literal physical being:

> a mentally represented grammar and UG are real objects, *part of the physical world,* where we understand mental states and representations to be physically encoded in some manner. Statements about particular grammars or about UG are true or false statements about steady states attained or the initial state (assumed fixed for the species), each of which is a definite real-world object, *situated in space-time and entering into causal relations.*
>
> (Chomsky 1983: 156–157; emphases added)

Linguistic capability now appears to be regarded, not 'virtually' but literally, as a physical object, with speakers' adult language capacity realized as a specific set of neural structures.

From this point, it is conceptually only a rather short step to the reductio that grammars themselves, the formal expression of that capability, should, with sufficiently sophisticated technology, be identifiable in the neural scans such technology would make available, as in Anderson and Lightfoot (2006: 81). Postal (2009), commenting on the passage from their reply to Everett cited above, observes that it makes the elementary category error conflating tokens (concrete objects) with types (abstractions), but the problem is still more pernicious. A phrase structure tree is simply a graphical representation of a set of nodes under two mutually exclusive partial orderings. The advantage of such a representation is legibility: by assigning the vertical representations to the domination partial ordering, and the horizontal to the linear precedence ordering (itself a convenient graphic encoding of a temporal, not spatial, order), we avoid the delimiter-counting problem which makes labeled bracket notation so hard to parse visually. But there is nothing more intrinsically tree-like than bracket-like about the set-theoretic objects we use these geometric or typographic conventions to illustrate, and there are any number of alternative encodings for these objects, e.g., the attribute-value matrices commonly employed in Head-driven Phrase Structure Grammar, Lexical-Functional Grammar, Functional Unification Grammar and others. Anderson and Lightfoot are apparently under the impression that a sufficient increase in the sophistication of our imaging technology would see the neural enscription of dominance and precedence relations in ANY particular conventional notation. The key problem with the view just cited, then, is the implausibility of the conviction that neural arrays – the kind of thing that brain scans reveal – can literally embody set-theoretic, algebraic or category-theoretic objects and mathematical relations. As I discuss below, neural function can reflect a relatively concrete relation such as amplitude, and this is the basis for the success of the Marr framework for mathematically modeling the visual system. Marr’s position was – as will become evident – far closer to Everett’s than to Anderson and Lightfoot’s.

The first problem with this fundamentalist or, as I’ll call it, *literalist* interpretation of the biolinguistic perspective, as exemplified explicitly in Chomsky (1983) and Anderson and Lightfoot (2006), is thus the kind of category error noted by Katz (1981), Katz and Postal (1991), Postal (2009), Behme (2012), Behme (2013), inter alia, and at least implicit in much of recent and current work adopting this perspective. Advocates of the neurological reality of syntactic representations (including Anderson & Lightfoot 2002 themselves) have, however, often invoked psychophysical models of other cognitive domains to justify this premise of the biolinguistic framework, in particular the watershed research of Marr and his associates in the ‘Visionaries’ group studying the human visual system at MIT’s Computer Science and Artificial Intelligence Laboratory. In § 2 below, I outline the critical contribution of Marr’s work on edge detection – arguably the foundation of his detailed mathematical formalization of visual perception – and suggest that Marr’s actual accomplishment offers no support at all to the claims made in the biolinguistic literature along the lines in Anderson and Lightfoot; on the contrary, he explicitly rejects a view very close to what they themselves assert in the above quotation.

Neurological literalism is not, however, the only questionable aspect of the biolinguistic framework which needs to be critically scrutinized. The passages quoted above from Chomsky, particularly the second one, contain a claim of so-called ‘domain specificity’, implicit in the reference to the anatomical composition of the human body. Invocation of physical organs carries with it a strong implication resting on the dedicated nature of such organs. The cells of the kidney, brain and liver are fundamentally different in structure from each other and from any other kind of cell type; they evolved to subserve a specific range of functions within the organism, and the arrangement of these cells into the tissue configurations each organ comprises is unique, corresponding to the unique work that that organ carries out in maintaining homeostasis in the body. The analogy to linguistic ability seems to be a conclusion drawn from the supposed disconnection between the critical predicates appealed to in generative grammar at various stages (‘command’, ‘c-command’, ‘max-command’, ‘movement rule’, ‘cyclic node’, ‘governing category’, ‘barrier’ etc.), on the one hand, and other cognitive/sensory modalities on the other. Such notions have played a crucial role in syntactic accounts of islandhood patterns and configurational conditions on anaphora (under the misnomer ‘binding theory’). More recently however, both islandhood and conditions on anaphora have been argued – with extremely strong support in especially in the case of the former – to originate in a mix of processing, pragmatic and prosodic factors. These points become critical in the discussion in § 3. Domain specificity is perhaps a more generally held postion, at least publically, than neural literalism, but it has no better a base of empirical support.

Both aspects of the ‘biolinguistic’ speculation thus turn out to be logical consequences of long-held generative assumptions. The ‘literal instantiation’ aspect is in effect the reductio of the position that grammars are themselves the form of human knowledge of grammar. In itself, a grammar could serve as a formal – ultimately mathematical – model of that knowledge, without any particular ontological commitment (apart from one’s view of the reality of mathematical objects), a position completely compatible with a Platonist characterization of grammar. Chomsky has however consistently rejected any position but the naturalist interpretation of NL grammars: the latter constitute the literal form of the ‘implicit knowledge’ native speakers have of their language. And domain specificity, ultimately, is rooted in the same source: if NL grammars are the content of human knowledge of language, and that content is inscribed in neural tissue, then not only does neurological literalism follow straightforwardly, but, to the extent that the content of grammars looks nothing like any other aspect of human cognition, so does domain specificity. But as I argue directly, both positions are quite vulnerable, resting as they do on a variety of a priori arguments whose empirical bases are fragile at best, and are interlinked in a way which augurs poorly for the future of the aspirations of ‘biolinguistics’ (as vs. much recent and current research on the neuroanatomical basis of linguistic ability, some of which is discussed below in § 2 and § 4) to one day become a genuine domain of knowledge.

2. Visual cognition: The role of early edge detection

By far the most empirically successful formal model of a cognitive module arose out of the research paradigm inaugurated by David Marr and his students and colleagues had a transformative effect on vision research in particular, and the standards and practices of the emerging domain of cognitive science and the diverse disciplines it comprised, of a scale at least as great as that of Richard Montague in semantics. The most comprehensive formulation of not just Marr’s specific scientific discoveries, but the methodological architecture in whose terms he reformulated the agenda of research in cognition, is given in Marr (1982). For Marr, even more critical than any specific proposal that he offered, was the decomposition of the scientific investigation of sensory modalities into four separate phenomenological domains, with concommitant strategies for giving an adequate account in each respectively:[2]

- *The physical problem:* Define the cognitive objective as the solving of a physical problem in space and time.
- *The mathematical task:* Model the physical problem as a specific mathematical task to be accomplished, where, ideally, components of the task will prove to correspond to components of the cognitive operation under study.
- *The algorithm design:* Identify plausible candidates for the procedure that implements the mathematical computation.
- *The wetware components:* Hypothesize an explicit neural circuitry which is optimized to instantiate the candidate algorithm(s) under investigation.

Using this division of labor, Marr offered mathematical simulations of the human visual system, and its embodiment in specific neural complexes, that underwrote every aspect of the 'mental organ' trope ever used to defend that characterization of human linguistic knowledge, and, unsurprisingly, for much of the late 1970s through the 1980s, the work of Marr and his colleagues' work was explicitly invoked by Chomsky and his circle as the exemplar for cognitive research, paralleled by generative grammar as a theory of the 'mental organ' for language. But Marr's paradigm is radically antithetical to the central neurological literalist claim, and there is, as I argue below, not a single currently known fact about the neural basis of language which gives even a hint of support to this claim.

2.1 The physical problem and its mathematical formulation

2.1.1 *Generalities*

To provide an explicit theory of how the visual system yields detailed mental scene descriptions based on the stimulation of receptor cells in the retinas, it is necessary to identify the basic operation(s) required to jump-start the process of constructing such scene descriptons. From Marr's perspective, these operations correspond to a parsing problem: interpreting the intake of the field into a collection of combinable primitives with specifically visual content which will be assembled into higher-order representations containing all the information human sight delivers; his answer to this primary question – where does vision start? – was that we perceive *distinct* objects in our visual field; i.e., vision begins with the detection of

2. A very similar breakdown of phenomenological levels is outlined in Soames (1984).

the abrupt discontinuities in spatial properties that we identify as edges. Physical edges will correspond in the visual field to changes in intensity, and so the first task for the visual system, and therefore for any successful mathematical model of that system, is the detection of local discontinuities at all scales while supressing fine-grained information corresponding to noise in the optical signal.

This general objective has two components. In the first component, the raw information about levels of light intensity provided by neural arrays in the retina must be blurred, so that small-scale fluctuations are buried. A standard mathematical routine for carrying out this smudging of the intensity information provided by the retina is the use of CONVOLUTION, a familiar technique from signal processing engineering described below. The second component is the identification of the prominent discontinuities in the blurred image, which will be interpreted visually as the edges of discrete objects in the visual field. The key question is what such edges should correspond to mathematically, and the heart of Marr's solution was the identification of a differential operator which, applied to the array of intensity values representing the blurred visual field, would correctly pick out sets of points in that field corresponding to a consistent discontinuity. We outline in turn these two aspects of Marr's solution.

2.1.2 *Smoothing out the visual field*

The physical channel for the visual field begins with photoreceptor cells in the retina, whose central foveal region comprises the networks of cells providing maximum resolution. While there are on the order of between one and two hundred million receptor cells in the whole retina, the number of foveal cells is much smaller, by a factor of approximately a thousand. Simple photoreceptors (rod and cone cells) are activated by light in the visible range, and in turn pass this information up to successively higher intermediate cell layers. A significant amount of data processing has to occur along the way: the information carried by the outermost level of photoreceptors is in effect nothing but a large matrix of pixels (often referred to as a grey-level array) each of which corresponds to a number representing the intensity of the light falling on that microscopic portion of the retina – a far cry from the detailed scene descriptions involving texture, reflectance properties and relative distance that correspond to our immediate visual experience. In Marr's view, the critical point of departure in translating what happens in our retinas into what happens in our visual awareness had to be an account of the fact that we perceive *distinct* objects in our visual field, i.e., vision begins with the detection of boundaries. The first question, then, is how the brain is able to identify changes in intensity which are prominent enough to be diagnostic for a real boundary in physical space, and which are CONSISTENT over the length of the edge.

This turns out to be a far from trivial task. Like all other physical systems, what Aristotle called the sensorium of sight is a noisy channel. Local fluctuations in the behavior of the biochemical medium through which neural impulses flow seriously obscure the basic information reflecting the location and properties of objects in space. Adjacent pixels in grey-level arrays may differ markedly in values, and the initial challenge for any model of the visual system is to explain how the mind distinguishes random discrepancies from the systemic differences in value that correspond to the boundaries of objects in three-dimensional space. One of Marr's signal contributions was his insistence that to solve this essentially neuropsychological puzzle, it was first necessary to construct a predictively successful mathematical model of this information-processing task – in his view, a correct characterization of a kind of smoothing operation, leveling out local spikes and troughs in the visual signal, translating the initial array into one in which the largest-scale differences remain. It is these surviving discrepancies which would then be the basis for inferences about the presence of edges in the physical environment.

Defining the problem of edge detection as, in part, the blurring out of differences that do not carry relevant information allowed Marr to treat the extraction of edge information via a number of mathematical tools – e.g., so-called Gaussian smoothing – familiar from image processing techniques pioneered in the late 1950s (in particular Leipnik 1960 and unpublished work of Irwin Sobel and Gary Feldman at the Stanford Artificial Intelligence Laboratory). The key idea behind all such blurring technologies is to replace the numbers in the original input – the grey-level array, in the case of visual processing – with a new array of numbers which eliminate all but the largest discrepancies at any given scale of resolution. For visual cognition, these discrepancies are robust clues to the presence of edges, but everything hinges on the details – in particular, exactly what method one uses to modify the raw input data of the grey-level array.

One reliable approach to this problem is based on the premise that a physical edge will correspond in the visual field to something like a step: essentially the same light intensities will be measured over all points in a certain neighborhood up till the edge, where a major change will appear in the intensity value at all points along the edge. Suppose we determine the value of the blurred grey-level array at each point in the visual field by, very roughly speaking, averaging the values of the surrounding points along with the value at the point in question. Since the desideratum for any two adjacent points is to have very close to the same intensity value unless there is a good (physical) reason for them not to, a sensible way to proceed is to *weight* the numbers that go into the average for a given pixel P, so that the contribution of pixels to the average drops off smoothly the further they are from P. For two pixels X_1,X_2 which are immediately adjacent to each other, these 'distant' values will obviously be very nearly the same for X_1 as for X_2, so that unless the difference in value of the two pixel points themselves is large, the modified values at the two points will wind up being quite close. For a variety of reasons, the optimal 'weighting' function which determines the modified grey-level value at any point is the so-called 'bell curve' function, the Gaussian: one multiplies the value at a given pixel-point by the highest value of the Gaussian, while closely neighboring points at the same distance are multiplied by a slightly smaller value, and so on until, at a certain distance, the contributions have dwindled down to essentially negligible values. The revised grey-level value at a given pixel-point is then obtained by adding all the contributions together as calculated from that point. In effect, differences in intensity levels at neighboring points are 'swamped' by the addition of weighted values from the rest of the region, leaving only the major discontinuities that reflect the existence of physical boundaries in a region of space within the visual field.

There are, of course, dangers in doing this kind of smoothing-out. If one levels differences over too large a portion of the visual field, crucial details, corresponding to discernable components of what we see, will be lost. The trick then is to carry out the averaging procedure just sketched at multiple scales, and the mathematical form of the Gaussian function makes it straightforward to do this. A second issue that any realistic model of visual cognition must address is the fact that the actual mathematical operation that matches this weighted average operation holds over all points in the 'space' of the visual field – a space in which any two points may be arbitrarily close to each other. This means that no matter how many neighboring points are included in the average, there are, between any two that *were* included, an infinite number that have been omitted. From this point of view, simply summing the weighting of some subset of points in retinal space is inadequate. But what else is there to do?

The smoothing operation can in fact be elegantly handled by using a special mathematical technique called CONVOLUTION. Imagine that we partition one dimension of the region of space in question into a set of rectangles, one side of which has the length of the weighted intensity which we treat as constant over the width of rectangle, taken to be an extremely small fixed length written Δx. We now sum the areas of all these rectangles, allowing the width of Δx to approach arbitrarily close to zero – a standard technique in integral calculus, allowing us to calculate a number that this sum converges to no matter how small Δx gets (and therefore no matter how many rectangles of smaller and smaller width we pack together). This number is called the *limit* of the infinite sum of infinitesimally narrow rectangles, and corresponds to the weighted value of the blurred image at that point in space, with all points in the space included in the weighting. Finding this limit is carried out by taking the definite integral of the product of the field and the weighting function over the (relevant subportion of) the visual field; again, what is critical is that infinity enters into this operation in two ways: the narrowness of each of the rectangles in the sum, and the number of rectangles which enter into that sum as their width approaches arbitrarily close to zero.

The approach to the smoothing of the visual field by what has come be called convolution with a Gaussian filter, as just described, was the first part of Marr's innovative mathematical model of early visual processing. But the second part of the edge detection task – identifying the signature properties of edges in that blurred signal – now becomes critical. As it happens, it was Marr's choice of probe for edges that allowed him to connect the mathematical model of edge detection to known properties of neuroanatomical function, making the psychophysics of vision arguably the gold standard for cognitive science and the model towards which those who characterize their research as biolinguistics aspire.

2.1.3 *Identifying edges*

To retrieve the discontinuities in the optical signal that the brain identifies as the physical boundaries of objects in space, Marr begins with a proposal to model edges in the via what is called the step function. Suppose we disregard the visual field for a moment and focus simply on the geometric properties of steps in a staircase. In a standard Cartesian frame of reference, where the x axis corresponds to values along a horizontal dimension, the unit step function θ is defined by

$$\theta(\mathrm{x}) = \begin{cases} 0, & x < 0 \\ 1, & x \geq 0 \end{cases} \quad (0)$$

Up to a certain point in space, which we take to be the 0 value of x, the value of the function (standardly written $\theta(x)$) is 0; at that point and thereafter it has the value 1 – like an infinitely long step which appears at one point on an infinitely long floor. This is, of course, an idealization (much like the frictionless surfaces and perfectly spherical object assumed in elementary physics problems), but it does provide a kind of laboratory that allows us to test various proposals for what sorts of mathematical properties sharp discontinuities display that could serve as clues to the presence of edges in the visual signal.

Some terminology and notation are useful at this point. The rate at which a function of some variable changes its value over a vanishingly small change in the value of that variable corresponds to the operation in differential calculus of finding its first derivative. The rate of change of the position of a moving object at a single instant in time is a first derivative, called velocity; the rate of change in the value of a curve in space at a given point is called the *slope* of that curve.

A convenient way to notate the rate of change in some function *f* as some variable *x* changes is $D_x f$. We obtain this derivative straightforwardly by allowing *x* to change by a very small amount Δx and then comparing the ratio of the change in *f* to the change Δx as Δx approaches arbitrarily close to 0. In fact, $D_x f$ is nothing other than the limit of this ratio, in exactly the same sense discussed above in connection with the convolution smoothing operation.

The first striking thing about steps is that their slope – $D_x\theta$, using the notion above, which is standardly written $\delta(x)$ – is zero everywhere except at $x = 0$, where it is infinite. This 'spike' shape is a good model of an isolated impulse, but more useful in the present context is the behavior of this spike itself. It can be shown rigorously, but may also be apparent intuitively, that the slope of the spike – the reapplication of D_x to $\delta(x)$, which would be notated $D_x\delta(x)$, yielding the *second* derivative of the step (which can also be written $D^2{}_{xx}\theta$) – has to become positively infinite, just like the spike itself, at zero. But unlike the step function itself, the slope then has to become negatively infinite as it 'falls down the other side' of the spike, like a seesaw tipping virtually instantaneously from one maximum position to the opposite one. This 180° shift in direction at the edge itself means that the line corresponding to the slope of the spike has zero amplitude at the very point where, as we have been assuming, the rise of the step itelf appears.

It follows, then, that if we take the step to correspond in the visual signal to the physical presence of an edge, then wherever the second derivative of the signal yields a zero value, we can posit the existence of an edge. Combining this method of identifying the zero crossings of the visual field with the method already described of smoothing that field out before the $D^2{}_x$ operator applies to it were the two fundamental contributions Marr and his associates made to the psychophysics of vision.

But what gave Marr's model its special cachet in the history of cognitive science was the fact that the zero-crossing model has a close homologue in the specific neuroelectrical wiring of the early-to-middle visual system. This insight underwrites the special authority that Marr's work has as a touchstone for past and current 'biolinguists', who take his work to be a paradigm example of what they hope to achieve in a different sphere of cognition. Yet those who seek to reproduce Marr's success in the novel domain of natural language need to bear in mind that, by the very nature of his achievement, Marr had adopted a strongly anti-literalist ontology in his understanding of the relationship between the formal theory of visual cognition on the one hand and the neurological embodiment of that theory on the other. To see this crucial point clearly, we need to back up a bit and revisit the components of Marr's mathematical model of edge detection vis-à-vis the way neural cell complexes work in real time.

2.1.4 *From mathematical model to neural architecture*

The first point to note is that Marr's model is based completely on methods from the domain of mathematical analysis which manipulate infinities. The Gaussian smoothing filter described earlier is based, as noted, on use of convolution integrals, where integration requires, in essence, determining the limit on the area of an infinite sum of rectangles, one side of each of which is taken to become infinitesimally small. And the D^2_{xx} operator applied to this convolution integral involves two applications of the differential calculus technique of taking a derivative – the ratio with infinitesimally small denominator described earlier. Since it would make no sense whatever to suppose that there are cell arrays in a finite visual neuroanatomy which are iconic with the operation of taking the limit of an expression in which one variable approaches arbitrarily close to zero, or with an actual infinite sum, the obvious conclusion is that the mathematical model of edge detection Marr proposes cannot be directly instantiated in the wetware of the visual cortex.

Rather than looking for a literal neural instantiation of the mathematical model they had developed, Marr and his associates realized that to link their mathematical account with the biology of vision, they needed to identify the *work* that the separate components of that account were doing and identify neural complexes that would accomplish the same work. So far as the first is concerned, the key is that taking the second derivative of a Gaussian-blurred visual field turns out to be identical to taking the second derivative of the Gaussian operator, and then using that derivative to blur the raw input array. What kind of operator do we get when we take the second derivative of the Gaussian? The result of this operation is something like the original bell curve, but noticeably narrower and steeper, with negative dips on both sides that rise and flatten out to the 0 line. Basically, then, the second Gaussian derivative exaggerates the amplification of a pixel and those closest to it, but–unlike the original Gaussian–adds an inhibitory contribution to the signal via the 'dip' surrounding the central region which tends to suppress the signal from the latter. The result is that local noise over a given part of the raw signal will be levelled out, but a significant difference between the center (the excitatory portion) and the surround (the inhibitory portion) will translate into a dramatic spike in values, and a corresponding dropoff and depression of pixel values within a relatively short distance. At an actual edge, the pixels on one side of the edge will correspond to a line of positive spikes, while immediately on the other side of the edge activation line there will be a parallel 'trough', expressed in negative numbers, that appears (because of a sign reversal built into the mathematics of the convolution).

This scenario might seem problematic for a simple anatomical translation of the Marr-Hildreth scenario, however. A neuron can fire or not fire, so how can it fire in a way that corresponds to negative numbers? What Marr realized is that the effect of the negative portion of the zero-crossing profile can be simulated by neurons firing strongly in some way which is the opposite of their immediate neighbors on the other side of the edge. Since neurons fire on an all-or-nothing basis, there can be no literal translation of the negative portion of the zero-crossing intensity profile. Something other than individual cell response seems called for.

As it happens, the physiology of the retina reflects an elegant solution to this problem. The outermost layer of retinal cells are the ordinary photoreceptors largely comprising rods and cones, but these cells are linked in clusters to a deeper layer of tissue containing what are called ganglion cells, themselves activated under one of two conditions. One possibility is that light falling in the center of a cluster of photoreceptors tends to trigger an electrical impulse in the linked ganglion cell, while light falling on the cell ring surrounding the center serves to inhibit that impulse; these ganglion cells, labeled 'on-center/off-surround', share the ganglion layer with others whose firing pattern is exactly the opposite, appropriately labeled 'off-center/on-surround'. These patterns might seem familiar: both kinds of ganglion cells correspond to the shape of Marr and Hildreth's $D^2{}_{xx}\mathcal{G}$ blurring operator (narrow, high-amplitude effect surrounded by a shallower dip in the opposite direction). The two different center/surround firing patterns are a perfect analogue of positive and negative numbers: if two of them with opposite polarity were somehow wired together and fed the same illumination, their linked actions jointly identify a shift in the direction of the amplitude.

Suppose, Marr reasoned, we measure a line of positive center/surround cells lighting up immediately across from a matching line of equally active negative cells. Such a configuration corresponds perfectly, not to the mathematical *form* of the $D^2{}_{xx}\mathcal{G}$ smoothing operator, but to its functional effect: if the two lines of cells are linked, and high activity levels on both the on-center and off-center feed higher-level neuronal structures, then we have an effective simulation of the characteristic zero-crossing property which is the signature of the mathematics of convolution under a second-order derivative operator. In Marr and Hildreth (1980), the authors suggest that such paired arrays actually do exist in the lateral geniculate nucleus, a major downstream processing structure mediating between the retina and the visual cortex.

2.2 The Marr hierarchy and neurological literalism

There are a couple of striking takeaways from this brief review of Marr's approach to the cognitive problem of visual image formation. The more obvious one is the context it provides for Marr's own expression of his attitude towards views such as Anderson and Lightfoot's:

> To say that early visual representations are retinocentric does not literally imply that a Cartesian coordinate system, marked out in minutes of arc, is somehow laid out across the striate cortex, and that whenever some line or edge is noticed it is somehow associated with its particular *x*- and *y*- coordinates, whose values are somehow precisely carried around by the neural machinery. This process would be one way of making the representations, to be sure, *but no one would seriously propose it for human vision.* (Marr 1982: 42, emphasis added)

This is as explicit and emphatic a rejection of Anderson and Lightfoot's neurological literalism as one can imagine. It is probably evident, but perhaps worth stressing, that if indeed there is no explicit literal representation of a Cartesian coordinate grid in neural tissue, then there likewise cannot be any literal embodiment of mathematical structures and operations stated on those coordinates, such as differential operators or location-dependent Gaussian functions, in cortical sulci. Rather, the mathematics identifies what are, in effect, the hoops that must be jumped through by *any* organism or machine identifying features of the physical world by information carried in wave trains of reflected light. Precisely *how* those hoops are jumped through for any particular species or device is an open empirical question, but for Marr the crucial work, in the case of our species at least, and the order in which it was to be pursued, comprised the following:

- *The physical problem:* identify extended lines of discontinuity in space corresponding to edges and borders.
- *The mathematical task*: determine the step functions latent in the visual field via the zero-crossings of the Laplacian/Gaussian-convolved raw intensity array.
- *The algorithm design:* evaluate the zero-crossings via neighboring high (absolute-)valued difference-of-Gaussians opposed for positive and negative values respectively.[3]
- *The wetware components:* simulate the difference-of-Gaussians approximation with paired rows of on-center/off-surround and off-center/on-surround ganglion and geniculate cells.

3. The difference of two Gaussians provides a robust approximation of the smoothing transformation with a second order differential operator outlined above. No derivative need actually be taken; rather, the raw image is smoothed by two simple Gaussian 'bell-curve' operators of different sizes, and then one of the two 'blurred' images is subtracted from the other. This algorithm can be done very quickly and, if the sizes chosen are correct, the result is an extremely close simulation of the action of the $D^2_{xx}\mathcal{G}$ operator on the image array.

One might reply to all this, yes, well, whatever, let's just get on with it. If biocognition – what Dana Ballard has called natural computation – is a matter of completing the analogues of Marr's four steps in whatever domain and whatever species we're interested in, fine, we won't waste time expecting theoretical representations of whatever the top-level task consists of to be directly inscribed in cytoplasm and tissue complexes and so on. We'll just go ahead and look for evidence of how the brain carries out simulations comparable in the domain of grammar to what Marr did with edge detection. This would be a considerable improvement over the Anderson-Lightfoot view of things, but it would neglect a deeper point than just the fact that probably our best-understood domain of explict cognitive modeling is totally incompatible with the literalism they advocate. The great likelihood is that the neural mechanisms which simulate the formal components of the correct theory of grammatical representation – whatever these are – will turn out to be far more difficult to identify than those involving vision. My basis for this somewhat pessimistic view is the difference between (i) the relationship between the computational problem to be solved and the organization of the neural processing stream in Marr's theory of visual perception, on the one hand, and (ii) that same relationship in the domain of natural language, on the other.

Marr's great insight was in the first place the recognition that the visual problem of identifying edges in the world involved a correspondences between spatial and neurological *amplitudes,* and in the second the identification of a candidate for the former which could receive a natural expression in the structure and function of particular neural complexes. Neurons specialize in expressing amplitudes; it is, in effect, what they do, via the action potentials they can propagate and the number of cells participating in that activity. There is thus a very direct relationship possible between the amplitude of signals in the visual array carried by reflected light, and the amplitude of neural activity linked (perhaps via intermediate steps) to the action of (combinations of) foveal receptors in the retina. The trick is to identify correctly just which spatial amplitudes need to be calculated and which neural mechanisms have a relation of homology to that amplitude which is predictively successful vis-à-vis the observed quantitative behavior of the visual system.

But matters are entirely different when it comes to natural languages. Unlike edges and other varieties of spatial discontinuity there is no external object at all which determine the form of the computations native to the cognitive system in question. Whereas such discontinuities are universally accepted amongst vision research scientists as a core component of how the 'sensorium of sight' operates, a consenus on what it is that the supposed analogous system for language comprises awaits us only in the far future, if ever. Grammars have been variously assumed to be conditions on phrase-structure configurations, with or without structure-to-structure mapping relations; relational-arc networks; proof-theoretic calculi defined on syntactic types with prosodic and semantic term labels; dependency graphs, and any number of other formal objects, with significant ontological differences amongst them. The mathematical interpretation of phrase markers, as discussed earlier, takes them to be partially ordered sets defined under relations of precedence and inclusion; the proofs of categorial grammar, in contrast, are inferences typically made in a some variant of a calculus of types parallel in critical respects to the implicational fragment of some substructural logic, etc. It is fair to say that even the most basic questions that a 'biolinguistics' aspiring to the scientific status of cognitive theories of vision needs to answer are, at the moment, essentially unanswered, and likely to remain so for generations.

Even more problematic, however, is the fact that the relationship between the kinds of formal objects presupposed in the framework in which virtually all self-described 'biolinguistic' research is carried out, on the one hand, and the capabilities of neurons and neural complexes on the other, is altogether obscure. The situation could not be further from the theory of visual perception, where, over the course of the past half century, different approaches have lived and died on the basis of whether or not a predictively successful connection could be established between characteristics of the object on the one hand and the functionality of the complete visual cortex on the other. Marr's computational approach, based on largely bottom-up edge detection as per the preceding discussion, played a major role in displacing the prior state-of-the-art framework, based on Fourier decomposition in separate channels, precisely because, as Westheimer notes,

> The most direct physical implementation of a spatial-frequency analysis would be by way of Fourier combs, i.e. a series of detecting templates that have a spatially sinusoidal acceptance function and whose output is proportional to the image's content at their spatial frequency. For completeness they would have to be duplicated with 90° phase shift. *The eye's optics can certainly be treated in such a manner, but the anatomy of the retina is not in accord.* Processing here is strictly local, transduction is compartmentalized in the structural elements, rods and cones, and the generation of neural signals is confined within small neighborhoods. (Westheimer 2001: 538; emphasis added)

He explains that, by the end of the 1970s, "the realization set in that […] the distance between [Fourier spectral] theory on the one side, and the structure and wider actuality of visual functioning on the other, had been widening rather than narrowing" (Westheimer 2001: 538). Note also the empirical problem with Fourier spectra in terms of texture identification described in Julesz and Caelli (1979). Edge detection via differential operators applied to step functions superceded the Fourier decomposition approach precisely because a significant correlation could be made between neural function on the physiological end and mathematical properties of the signal on the physical end, via the relationship between amplitudes noted above. In contrast, it is not in the least clear what neural function could correspond to the syntactic hierarchical structural relations and derivational changes in those structural representations assumed by essentially the entire 'biolinguistic' research community – and so far as I am aware, no concrete proposals for a plausible formal operation (corresponding to the Marr-Hildreth convolution of $D^2{}_{xx}\mathcal{G}$ with the intensity array in the retina's visual field) directly implicating some neuroanatomical function (e.g. the role of retinal ganglion and lateral geniculate X-cells mirroring the firing patterns of antagonistic blocks of foveal photoreceptors) has ever been proposed in the 'biolinguistic' literature. Without such a correspondence, there cannot possibly be anything like a biology of linguistic cognition remotely comparable in any sense to the genuine biology of visual cognition that Marr's research program inaugurated, and it is therefore unremarkable, as Postal has observed, that "in total contrast to actual biological science, in four decades [Chomsky] has not specified a single physical property of any linguistic object". (Postal 2009: 256).

The bottom line, then, so far as the neurological literalism advocated by Anderson and Lightfoot is concerned, is that there is not even a glimmer on the horizon of anything suggestive of neural structures whose function corresponds to syntactic hierarchic representations, let alone relevant constraints such as c-command (or indeed any other command relation), filler/gap connectivity via multiple Ā movement, Late Merge or anything else in the P&P conceptual toolkit. This is the picture which emerges from the work of scientists who actually *do* work on the neurobiological basis of linguistic ability, as summarized in, e.g., Embick and Poeppel (2015: 358–359):

> [A]lthough cognitive theories and neurobiological theories [of natural language] are advancing in their own terms, there are few (if any) substantive *linking hypotheses* connecting these domains [...] There are two major problems that can be identified when we try to link C[omputational-]R[epresentational] and N[euro] B[iological] theories [...] The first is that CR and NB theories have different types of primitives, i.e., distinct ontologies, making any attempts at directly linking the two domains prima facie problematic, *if not outright incoherent* [...] The problem is that one cannot simply 'draw lines' between the categories provided by each domain and expect such an attempt at 'alignment' to withstand any serious scrutiny. For example, the claim that the object MORPHEME in the CR-theory corresponds to the object NEURON in the NB theory is a non-starter; it is not even wrong.

This summary speaks, I think, as directly as one could wish to the plausibility of Anderson and Lightfoot's expectation that one day an improvement in neural scanning technology will reveal the presences of (presumably binary-branching) trees somewhere in the visual cortex. The uncomfortable truth, rather, seems to be that, as Embick and Poeppel put it, "there is at present no clear idea of how the brain represents and computes *any* of the computations that are part of language" (Embick & Poeppel 2015: 360).

The picture drawn by linguistic neuroscientists thus contrasts markedly with the detailed specification of comparable conceptual basics in vision research in the Marr paradigm. In this sense, 'biolinguistics' appears to be a field conspicuous mostly for having, not only no actual results to speak of, but nothing remotely close to well-defined, testable hypotheses about what kind of biological structures and mechanism to look for as embodiments of the grammatical structures its practitioners for the most part assume. If the term has any utility at all, it presumably exists, as I suggested at the outset, as an aspiration – a hope that one day a detailed connection between linguistic cognition and the detailed cortical anatomy of our species will be established that it makes sense to speak of in the same breath as the state of knowledge achieved in the realm of visual cognition forty years ago.

3. Domain specificity and natural language

While neurological literalism might seem eccentric or bizarre even to committed adherents to one or another research program identified by its practitioners as biolinguistics (cf. Ramchand's comment and Martins and Boeckx's interesting article cited earlier), it is probably fair to say that the doctrine of domain specificity is far less likely to raise eyebrows – despite the fact that, as pointed out in Everett's excellent critical review of Anderson and Lightfoot (2002) (Everett 2005) and, still more forcefully in his evisceration of their 2006 reply in the same issue (Anderson & Lightfoot 2006, Everett 2006), there is no empirical evidence which supports this specificity over various competing alternatives. Below I sketch some research by people who are actual practicing neurocognitive scientists, published in premier journals of cortical neuroanatomy and its cognitive function, that points in quite the opposite direction. But in the following section, I present an argument that, on 'biolinguistic' assumptions – which, echoing Soames' (1984) framing of the confusion, conflate the mathematical characterization of the object of inquiry with the physical realization of tacit knowledge of that object – this specificity does *not* hold in linguistic cognition.

3.1 What computation do sets of NL sentences represent?

In addressing the general question of domain specificity, it is instructive to start from Marr's 'top level' question: what computation(s) does a theory of natural language structure require? Natural language sentences constitute pairings of form and meanings; entailing that this pairing must be the yield of the correct theory of NL. The predominant formulations of this theory have posited recursively defined combinatoric units largely based on the narrow criteria of extraction (only constituents can undergo unbounded displacement) and proform replacement (only constituents can serve as antecedents to pro-forms). But there is strong evidence from a wide range of linguistic phenomena that there are combinatory units which must be recognized in order to achieve a credible syntax/semantics interface, but which are neither extractable nor replaceable. On that basis, a strong case can be made that what is essentially the default assumption about syntactic representations – hierarchical organization of words into successively larger combinatory units whose structural boundaries are preserved in those representations – is not the optimal theory of syntactic structure. A few examples will illustrate the empirical basis for this scepticism about phrase markers as models of sentences.

Consider the following examples:

(1) **Chris nominated, and Terry voted for,** Robin and Leslie (respectively).
≠ Chris nominated Robin and Leslie (respectively), and Terry voted for Robin and Leslie (respectively).

(2) **Robin was singing, and Leslie was whistling,** the same tune.
≠ Robin was singing the same tune, and Leslie was whistling the same tune.

(3) **Robin gave the Sierra Club, and his sister pledged to the NRDC,** a total of $100,000.00.
≠ Robin gave the Sierra Club a total of $100,000.00, and his sister pledged to the NRDC a total of $100,000.00.

Such data, exemplifying what has been called Right Node Raising, exhibit systematic scope anomalies: the semantic operators corresponding to *respectively, same/different/equal/identical* etc., and summative predicates such as *an average/total,* do not distribute over their associated coordinations, but rather scope over something like the denotations of the conjoined sequences. The problem of course is that the conjoined sequences themselves are not phrase structure constituents, and hence should not be coordinable in the first place. A variety of complex operations has been proposed in a variety of phrase-structure-based approaches, all of which involve highly problematic assumptions and intricate stipulations whose objective is to somehow or other represent the coordinated material in these examples as a genuine PS constituent whose 'visible' form has been truncated by one or another means (for detailed discussion, see Levine 2011, Kubota & Levine 2014 and Kubota & Levine 2015). But in not a single case does the pattern in question fall out as the null hypothesis of *any* phrase-structure-based syntactic framework.

Gapping and ellipsis phenomena exhibit the same intractability so far as the syntax/semantic interface is concerned. We have, for example, the following kinds of data (where the material missing in the second clause is notated in boldface in the first clause, and where small caps indicate contrastive emphasis):

(4) a. John **bought** a CHESS set, and Mary Ø a BOOK.
b. **I gave Mary a book,** and Sue, Ø a CHESS set. [ambiguous].
c. John **decided to go with** WINE, and Leslie, Ø BEER.
d. John **continued to try to subscribe** to a NEWSmagazine, and Mary, Ø to a POLITICAL journal.

(5) a. You can't **take the lining out of** that coat. You can Ø this one.
b. I didn't **try to subscribe to as many wine clubs** as I did POLITICAL journals!
c. I would **bet a friend** more DOLLARS that the sun would rise in the WEST tomorrow than I would EUROS that the Earth was FLAT.

In all of these cases, what is missing, and needs to have its interpretation supplied to the remnants in the second conjunct (4) or clause (5), is not a constituent, and what actually appears in these contexts respectively does not, itself, correspond to an acceptable utterance on its own. Again, current PS-based accounts prove to be analytically unsatisfactory and empirically inadequate (see Kubota & Levine 2016a and Kubota & Levine 2017a for details). The failures of PS-based approaches across this rather vast expanse of phenomena strongly suggest that the criteria adopted in such approaches for the identification of combinatorial units are flawed at the foundations. A different kind of formal basis for syntactic representation is needed.

In general terms, the most obvious such basis is some version of Categorial Grammar (CG). As we show directly, in a particular type-logical form of CG, sequences such as *gave Mary, try to subscribe to* and *bet a friend* are all derivable as structural units, are automatically assigned a category, and at the same time are provided with both prosodic and semantic labels (which can be thought of as interpretations) leading to full compositionality in both domains.

In the past, there was much more of a consensus in theoretical linguistics that phrase structure configurations were the indispensible basis of syntactic representations. Islandhood phenomena, first extensively studied in Ross (1967), were regarded as explicable solely on the basis of the geometry of such representations, and deriving these effects from ever more abstract characterizations of syntactic structures, which arguably reached its peak in the *Barriers* era following the appearance of Chomsky (1986), was probably the main theoretical initiative in syntactic research. But more recent work (e.g., Kluender 1998, Hofmeister & Sag 2010, Hofmeister & Sag 2012, Hofmeister & Sag 2013a, Hofmeister & Sag 2013b, Kehler 2002, Kandybowicz 2006, Kandybowicz 2009, Kubota & Lee 2015) has cast very strong doubt on structural sources for patterns of islandhood; see Newmeyer (2016) for a useful overview of the issues. Similarly, anaphora and related phenomena that were included under what in earlier versions of the Principles and Parameters framework was called the 'Binding Theory' were assumed to require reference to syntactic domains defined configurationally, as in Chomsky (1981), but more recent work, both within the P&P framework and outside it, no longer takes configurational relations in phrase structure trees to be critical to the definition of anaphoric possibilities (see, e.g., Pollard & Sag 1992, Pollard & Sag 1994, Jacobson 2007, Safir 2004, Nediger 2015). Moreover, the assumption of phrase structure creates, as in the case of the Gapping data presented above, and many other syntactic phenomena, a number of seemingly intractable difficulties for the syntax-semantics interface that have led to various attempts among phrase structure researchers themselves, to relax the requirement that syntactic combinatorics make reference to strictly phrase structural objects; see, e.g., McCawley (1982), Citko (2000), de Vries (2009), etc. As the apparently overwhelming evidence for configurational structure has faded and the difficulties created by the assumption of such structure as a condition on syntactic representation have become more evident, alternative approaches have become increasingly attractive. In the following discussion, based on the analysis detailed in Kubota and Levine (2016a), I lay out the basic principles of one such alternative, show how it can straightforwardly handle the syntactic/semantic difficulties posed by noncanonical varieties of coordination, and draw some conclusions based on the formalism of this approach for the notion of domain specificity in linguistic cognition.

3.2 Rules in Hybrid Type-Logical Categorial Grammar (HTLCG)

HTLCG belongs to a family of categorial grammars which can be characterized as Curryesque type logics, deductive systems isomorphic to the implicational fragment of intuitionistic linear logic. 'Curryesque' frameworks are so described on the basis of work by the logician Haskell Curry, who argued that prosodic and structural information in linguistic expressions – what he distinguished as PHENOGRAMMAR and TECTOGRAMMAR respectively – though linked, need to be kept separate and each equipped with its own proper combinatorics. This key idea was implemented in Oehrle (1994), and is reflected in the tripartite signs which appear in the proofs below; thus, the sign for *give* would be GIVE; **give;** ((NP\VP)/NP)/NP, where GIVE is an abbreviation for a complete phonological specification, **give** a representation of the corresponding model-theoretic function typed ⟨e, ⟨e, ⟨e, *t*⟩⟩⟩, and the syntactic type specifies the combinatoric possibilities available to the signs bearing it and the direction in which those possibilities are realized, in accordance with the inference rules given in (6):

(6)

Connective	**Introduction**	**Elimination**
/	$\dfrac{\begin{array}{c}[\varphi; x; A]^n\\ \vdots\quad\vdots\\ b \circ \varphi; F; B\end{array}}{b; \lambda x.F; B/A}/\,\mathrm{I}^n$	$\dfrac{a; F; A/B \quad b; G; B}{a \circ b; F(G); A}/\mathrm{E}$
\	$\dfrac{\begin{array}{c}[\varphi; x; A]^n\\ \vdots\quad\vdots\\ \varphi \circ b; F; B\end{array}}{b; \lambda x.F; A\backslash B}\backslash\mathrm{I}^n$	$\dfrac{b; G; B \quad a; F; B\backslash A}{b \circ a; F(G); A}\backslash\mathrm{E}$
|	$\dfrac{\begin{array}{c}[\varphi; x; A]^n\\ \vdots\quad\vdots\\ b; F; B\end{array}}{\lambda\varphi[b]; \lambda x.F; B\upharpoonleft A}\vert\mathrm{I}^n$	$\dfrac{a; F; A\upharpoonleft B \quad b; G; B}{a(b); F(G); A}\vert\mathrm{E}$

(where the ○ operator denotes linear concatenation). The elimination rules are nothing other than the type-logical homologues of modus ponens in intuitionistic propositional logic. The two directional slashes are type constructors which define a category, i.e., a class of terms, which will yield a term of the type on the ‘numerator’ side of the slash when combined with a term matching the ‘denominator’ side, on that side: a *B/A* term concatenated with a following *A* term constitutes a *B* term, and similarly for an *A\B* term and a preceding *A* term. The $B\upharpoonleft A$ type works the same way, except that the connective $\upharpoonleft$ is not directional. In effect, a $B\upharpoonleft A$ term lacks an *A* term somewhere within it, and has a functional prosody and semantics which, applied respectively to the prosody and semantics of such a term, returns a sign of type *B*, whose prosody is derived by applying the functional prosody of $B\upharpoonleft A$ to the prosody of the *B* term, and the semantics of $B\upharpoonleft A$ to the semantics of the *B* term. All three rules are type-logical correspondents of the Implication Elimination rule found in the Natural Deduction construction of standard logic in Prawitz format.

The dual of Implication Elimination, Implication Introduction, is likewise realized in the inference system in (6). The three Introduction rules given there are slightly less obvious intuitively, but in all cases the basic idea is that if by hypothesizing a type *A* (corresponding to a prosodic variable φ and a semantic variable *x*) we are able to derive a sign of type *B*, then by withdrawing that hypothesis we are left with a sign which *would* be of type *B* if it were to combine with a sign of type *A*. That is, if some prosodic material corresponding to φ were to appear where φ appears, and some semantic expression of the appropriate type were to appear in *B*’s semantic term where *x* appears, then we would have the prosodic and semantic expressions of a type *B* sign. In other words, we would have a function from the prosody and semantics of an *A* term to the prosody and semantics of a *B* terms. This can be exactly modeled by taking the result of the withdrawn hypothesis to be λ-expressions abstracting on φ in the prosodic sector and on *x* in the semantic sector.

In broad terms, the rule system in (6) is a synthesis of the strictly directional syntactic calculus given in Lambek (1958) with the exclusively non-directional system developed in slightly different formulations in de Groote (2001) and Muskens (2003). The two calculi, previously regarded as mutually exclusive, jointly yield a wide variety of empirically robust results involving the interaction of, inter alia, coordination, ellipsis, and comparative constructions with scopal phenomena including generalized quantifiers, 'respectively' interpretations, symmetrical and summative predicates and much else (for details, see Kubota 2015, Kubota & Levine 2016a, Kubota & Levine 2016b, Kubota & Levine 2017a and Kubota & Levine 2017b).[4] The interplay between directional and nondirectional inference rules allows us to saturate valence requirements, including medial arguments, with variables, and subsequently withdraw the hypotheses corresponding to these variables, allowing us to control the relationship between form and meaning over all positions in the word string. The phenomenon of Gapping provides a useful showcase for the descriptive power of this purely logic-based approach to the syntax-semantic interface.

3.3 Gapping

Gapping is a particularly mysterious instance of apparent nonconstituent coordination in which even the symmetry between the coordinated fragments apparent in Right Node Raising and Dependent Cluster Coordination is missing:

(7) a. Robin **speaks** French, and Leslie, Ø German.
  b. Robin **wants to speak** French, and Leslie, Ø German.
  c. To Robin **Chris gave** the book, and to Leslie, Ø the magazine.

We have in each case an ostensible conjunction of a full clause with a series of phrases that appear to be remnants of a sentence from which material (possibly a nonconstituent itself) corresponding to a semantic predicate has been omitted, and whose semantics is applied to the remnant phrases in a way parallel to its application in the full conjunct on the left. There are various ways, many of them conspicuously stipulative, in which the facts in (7) can be accounted for, but any general account of Gapping also needs to be able to account for the unusual scope anomaly observed in Siegel (1984) and Oehrle (1987), exhibited in (8):

4. The formal soundness of HTLCG has been confirmed in Moot's demonstration that its proof theory can be embedded in first order linear logic; see Moot (to appear).

(8) a. Mrs. J **can’t live** in LA and Mr. J Ø in Boston. $(=\neg\Diamond[\varphi \wedge \psi])$
b. Kim **didn’t play** bingo or Sandy Ø sit at home all evening. $(=\neg\Diamond[\varphi \wedge \psi])$

The problem is that the scope of the modal operators attached to the semantics of certain auxiliaries ordinarily does not reach beyond the confines of the minimal clause in which the auxiliaries in question appear. There have been attempts to reconcile this fact with the interpretations in question, e.g. Johnson (2000), Johnson (2009), Toosarvandani (2013), but these encounter major empirical challenges that strongly suggest the analyses offered are on the wrong track (see Kubota & Levine 2014, Kubota & Levine 2015 for detailed critiques of a range of approaches based on phrase-structural syntactic architectures, both derivational and nonderivational). The key to the solution is the recognition, following Oehrle (1987), that Gapping is in fact an instance of like-category coordination in which a single functor, corresponding in the case of (7a) to *Robin__French and Mary __ German,* is constructible as the conjunction of two clauses with a missing verb whose prosody is β-converted into only the first conjunct, but whose semantics is distributed over both. The kind of hypothetical reasoning used to obtain data such as (7) turns out to yield the (8) examples with no extra machinery at all.

The proof for standard Gapping is laid out in stages below. We apply the Lambek component of the system to derive a string of type S, based on a variable of type VP/NP, i.e., a normal transitive verb. The first step saturates both of the NP arguments in this variable’s type description.

(9)
$$\dfrac{\dfrac{\text{john; }\mathbf{j}\text{; NP} \qquad \dfrac{[\varphi_1; P;\text{VP/NP}]^1 \qquad \text{mary; }\mathbf{m}\text{; NP}}{\varphi_1 \circ \text{mary; } P\,(\mathbf{l})\text{; VP}}\,/\text{E}}{\text{john} \circ \varphi_1 \circ \text{Mary; } P\,(\mathbf{m})(\mathbf{j})\text{; S}}\,\backslash\text{E}}{\lambda\varphi_1.\text{john} \circ \varphi_1 \circ \text{mary; } \lambda P.P\,(\mathbf{m})(\mathbf{j})\text{; S}\upharpoonright(\text{VP/NP})}\,\upharpoonright\text{I}^1$$

We now carry out a parallel proof with *Bill* as subject and *Sue* as object:

(10) $\lambda\varphi_1.\text{bill} \circ \varphi_1 \circ \text{sue}; \lambda Q.Q(\text{s})(\text{b}); \text{S}\upharpoonright\text{TV}$

Finally, we introduce into the proof the special conjunction operator specific to Gapping displayed in (11):

(11) $\lambda\sigma_2\,\lambda\sigma_1\,\lambda\varphi_0.\sigma_1\,(\varphi_0) \circ \text{and} \circ \sigma_2\,(\epsilon); \lambda W \lambda V. V \sqcap W; (\text{S}\upharpoonright\text{TV})\upharpoonright(\text{S}\upharpoonright\text{TV})\upharpoonright(\text{S}\upharpoonright\text{TV})$

where $\sqcap$ denotes the standard generalized conjunction operator introduced in Partee and Rooth (1983), where for propositions ϕ, ψ, $\phi \sqcap \psi = \phi \wedge \psi$ and for functors f, g, $f \sqcap g = \lambda\alpha.f(\alpha) \sqcap g(\alpha)$. The operator in (11) differs from ‘normal’ *and* only in its phonological action: it builds an empty string argument $\boldsymbol{\epsilon}$ into the prosodic functor, as per (12):

(12)

$\vdots \quad \vdots$
$\lambda\varphi_1.$john $\circ\ \varphi_1 \circ$ mary; $\lambda P.P\ (m)(\mathbf{j})$; S↾TV

$\lambda\sigma_2\lambda\sigma_1\lambda\varphi_0.\ \sigma_1(\varphi_0) \circ$ and $\circ\ \sigma_2(\epsilon)$; $\lambda W\lambda V.V \sqcap W$; (S↾TV)↾(S↾TV)↾(S↾TV)

$\vdots \quad \vdots$
$\lambda\varphi_1.$bill $\circ\ \varphi_1 \circ$ sue; $\lambda P.P\ (\mathbf{s})(\mathbf{b})$; S↾TV

———

$\lambda\sigma_1\lambda\varphi_0.\sigma_1(\varphi_0) \circ$ and $\circ$ bill $\circ\ \epsilon \circ$ sue; $\lambda V.V \sqcap \lambda P.P\ (\mathbf{s})(\mathbf{b})$; (S↾TV)↾(S↾TV)

———

met; **meet;** TV

$\lambda\varphi_0$ [john $\circ\ \varphi_0 \circ$ mary $\circ$ and $\circ$ bill $\circ\ \epsilon \circ$ sue]; $\lambda P.P\ (\mathbf{m})(\mathbf{j}) \sqcap \lambda Q.Q\ (\mathbf{s})(\mathbf{m})$; S↾TV

———

john $\circ$ met $\circ$ mary $\circ$ and $\circ$ bill $\circ\ \epsilon \circ$ sue; **meet**(**m**)(j) ∧ **meet**(**s**)(**b**); S

This account extends with no further technical additions to the other examples in (7), and more complex cases still, e.g.,

(13) John solved a certain difficult crossword puzzle on Monday and Mary on Tuesday, but I don't remember what paper it was in.

Such cases fall out straightforwardly from the Gapping analysis given and the treatment of generalized quantifiers in HTLCG. But there are still more complex scoping phenomena that need to be acccounted for, e.g., those in (14):

(14) a. John can't eat steak and Mary (just) eat pizza! ¬◊ > Λ
b. Mrs. J can't live in LA and Mr. J in Boston. ¬◊ > Λ
c. Sue wouldn't play bingo or Bill sit at home all evening. ¬ > V

In a nutshell, such examples are instances of a higher-order version of auxiliaries (for which ample independent evidence exists) being gapped, so that the auxiliary is outside the coordinate structure, but prosodically 'lowers'. Modal wide scope then follows immediately with nothing further required. To begin with, consider

(15) Someone must have seen John. □ > ∃

A lower-order entry for modal *must,* along the line of

(16) must; *λPλx.□P(x);* VP/VP

will not do here. True, we can obtain a reading for (15) with a wide-scoping indefinite:

(17)

seen; **see**; VP/NP   john; **j**; NP

———

seen $\circ$ john; **see**(**j**); VP   have; $\lambda R\lambda z.$**PERF** $R(z)$; VP/VP

———

have $\circ$ seen $\circ$ john; $\lambda z.$**PERF see**(**j**)(z); VP   must; λPλy.□P(y); VP/VP

———

must $\circ$ have $\circ$ seen $\circ$ john; λy.□**PERF see**(**j**)(y); VP   λσ1.σ1(someone); ∃; S↾(S↾NP)

———

someone $\circ$ must $\circ$ have $\circ$ seen $\circ$ john; ∃(λy. □**PERF see**(**j**)(y)); S

The interpretation here is that there is one particular person such that it is necessary that, in all possible worlds, that person has seen John. But this is not the sole reading. What we want is the interpretation in which what is necessary is that John was seen, period, though the speaker need have no idea by whom (more technically, in every possible world, someone saw John, but that individual isn’t necessarily the same one in any two or more possible worlds). To achieve the latter reading, in which the modal necessity operator outscopes the existential, we need the higher order version of *must* given in (18):

(18) $\lambda\sigma.\sigma(\text{must});\lambda\mathcal{F}.\Box\mathcal{F}(\text{id}_{\langle e,t\rangle});S\upharpoonright(S\upharpoonright(VP/VP))$
(where $\text{id}_{\langle e,t\rangle}=\lambda P_{\langle e,t\rangle}.P)$

It might seem profligate to have two separate entries for each modal, one for its narrow scope and one for its wide scope properties. A parallel duplication of quantifiers to obtain its wide-vs. narrow scope behavior, for example, is unnecessary in any formalism I know of. And in fact, such duplication is also unnecessary in the case of the modal; the HTLCG calculus derives the standard lower-order version of *must* as a theorem of the sign in (18) (see Kubota & Levine 2016a: 150 for a formal proof). Given this scopal operator, we can straightforwardly deduce the wide-scope interpretation of modal necessity in (15):

(19)

$$\dfrac{\begin{array}{l}\lambda\sigma.\sigma(\text{must});\\ \lambda\mathcal{F}.\Box\mathcal{F}(\text{id}_{\langle e,t\rangle});\\ S\upharpoonright(S\upharpoonright(VP/VP))\end{array} \quad \dfrac{\dfrac{\begin{array}{l}\lambda\sigma.\sigma(\text{someone});\\ \exists(\mathbf{person});\\ S\upharpoonright(S\upharpoonright NP)\end{array} \quad \dfrac{\dfrac{\begin{bmatrix}\varphi_2;\\ z;\\ NP\end{bmatrix}^2 \quad \dfrac{\begin{bmatrix}\varphi_1;\\ f;\\ VP/VP\end{bmatrix}^2 \quad \begin{array}{l}\text{have}\circ\text{seen}\circ\text{John};\\ \lambda v.\mathbf{perf}(\mathbf{see}(\mathbf{j})(v));\\ VP\end{array}}{\begin{array}{l}\varphi_1\circ\text{have}\circ\text{seen}\circ\text{John};\\ f(\lambda v.\mathbf{perf}(\mathbf{see}(\mathbf{j})(v)));\\ VP\end{array}}/E}{\begin{array}{l}\varphi_2\circ\varphi_1\circ\text{have}\circ\text{seen}\circ\text{John};\\ f(\lambda v.\mathbf{perf}(\mathbf{see}(\mathbf{j})(v)))(z);\ S\end{array}}\backslash E}{\begin{array}{l}\lambda\varphi_2.\varphi_2\circ\varphi_1\circ\text{have}\circ\text{seen}\circ\text{John};\\ \lambda z.f(\lambda v.\mathbf{perf}(\mathbf{see}(\mathbf{j})(v)))(z);\\ S\upharpoonright NP\end{array}}\upharpoonright I^2}{\begin{array}{l}\text{someone}\circ\varphi_1\circ\text{have}\circ\text{seen}\circ\text{John};\\ \exists(\mathbf{person})(\lambda z.f(\lambda v.\mathbf{perf}(\mathbf{see}(\mathbf{j})(v)))(z));\ S\end{array}}\upharpoonright E}{\begin{array}{l}\lambda\varphi_1.\text{someone}\circ\varphi_1\circ\text{have}\circ\text{seen}\circ\text{John};\\ \lambda f.\ \exists(\mathbf{person})(\lambda z.f(\lambda v.\mathbf{perf}(\mathbf{see}(\mathbf{j})(v)))(z));\ S\upharpoonright(VP/VP)\end{array}}\upharpoonright I^1}{\begin{array}{l}\text{someone}\circ\text{must}\circ\text{have}\circ\text{seen}\circ\text{John};\\ \Box\,\exists(\mathbf{person})(\lambda z.\mathbf{perf}(\mathbf{see}(\mathbf{j})(z)));\ S\end{array}}\upharpoonright E$$

It turns out, however, that this independently motivated higher-order modal operator directly yields the interpretations in (14). The storyline of the proof in (20) is essentially the same as that by which interpretations of ordinary generalized quantification is obtained: a variable corresponding to a lower-order term is composed into a proof by the Lambek component of the inference rule; this variable is then bound by abstraction via nondirectional implication introduction, and the resulting λ-term is provided as an argument to the higher-order operator.

(20)

$$
\dfrac{
 \dfrac{
  \dfrac{
   \dfrac{
    \begin{array}{l}\text{john};\\ \mathbf{j};\ NP\end{array}
    \quad
    \dfrac{\begin{array}{l}\varphi_1;\\ f;\ VP/VP\end{array} \quad \begin{array}{l}\text{eat}\circ\text{steak};\\ \text{eat(s)};\\ VP\end{array}}{\begin{array}{l}\varphi_1\circ\text{eat}\circ\text{steak};\\ f(\text{eat(s)});\\ VP\end{array}}
   }{\begin{array}{l}\text{john}\circ\varphi_1\circ\text{eat}\circ\text{steak};\\ f(\mathbf{eat}(\mathbf{s}))(\mathbf{j});\\ S\end{array}}
  }{\begin{array}{l}\lambda\varphi_1.\text{john}\circ\varphi_1\circ\text{eat}\circ\text{steak};\\ \lambda f.f(\mathbf{eat}(\mathbf{s}))(\mathbf{j});\\ S{\upharpoonright}(VP/VP)\end{array}}
  \quad
  \dfrac{
   \begin{array}{l}\lambda\sigma_2\,\lambda\sigma_1\lambda\varphi_0.\\ \sigma_1(\varphi_0)\circ\text{and}\circ\sigma_2(\epsilon);\\ \lambda\mathcal{F}_2\,\lambda\mathcal{F}_1.\mathcal{F}_1\sqcap\mathcal{F}_2;\\ (S{\upharpoonright}X){\upharpoonright}(S{\upharpoonright}X){\upharpoonright}(S{\upharpoonright}X)\end{array}
   \quad
   \begin{array}{l}\qquad\vdots\quad\vdots\\ \lambda\varphi_2.\text{mary}\circ\varphi_2\circ\\ \text{eat}\circ\text{pizza};\\ \lambda g.g(\text{eat(p)})(\text{m});\\ S{\upharpoonright}(VP/VP)\end{array}
  }{\begin{array}{l}\lambda\sigma_1\lambda\varphi_0.\sigma_1(\varphi_0)\ \text{o and}\circ\text{mary}\circ\boldsymbol{\epsilon}\circ\text{eat}\circ\text{pizza};\\ \lambda\mathcal{F}_1.\mathcal{F}_1\sqcap\lambda g.g(\mathbf{eat}(\mathbf{p}))(\mathbf{m});\\ (S{\upharpoonright}(VP/VP)){\upharpoonright}(S{\upharpoonright}(VP/VP))\end{array}}
 }{\begin{array}{l}\lambda\varphi_0.\text{john}\circ\varphi_0\circ\text{eat}\circ\text{steak}\circ\text{and}\circ\text{mary}\circ\boldsymbol{\epsilon}\circ\text{eat}\circ\text{pizza};\\ \lambda f.f(\mathbf{eat}(\mathbf{s}))(\mathbf{j})\sqcap\lambda g.g(\mathbf{eat}(\mathbf{p}))(\mathbf{m});\\ S{\upharpoonright}(VP/VP)\end{array}}
 \quad
 \begin{array}{l}\lambda\sigma.\sigma(\text{can't});\\ \lambda\mathcal{F}.\neg\Diamond\mathcal{F}(\text{id}_{et});\\ S{\upharpoonright}(S{\upharpoonright}(VP/VP))\end{array}
}{\begin{array}{l}\text{john}\circ\text{can't}\circ\text{eat}\circ\text{steak}\circ\text{and}\circ\text{mary}\circ\boldsymbol{\epsilon}\circ\text{eat}\circ\text{pizza};\\ \neg\Diamond[\mathbf{eat}(\mathbf{s})(\mathbf{j})\wedge\mathbf{eat}(\mathbf{p})(\mathbf{m})];\\ S\end{array}}
$$

The wide-scope interpretation of the modal in (14a) is thus accounted for, requiring only the higher-order modal and the gapping operator, both of which are needed for reasons quite independent of the Oehrle-Siegel phenomenon. And as shown in Kubota and Levine (2016a), all of the other Oehrle-Siegel anomalous scoping cases in Gapping fall out as well, on the same basis. As noted above, the HTLCG framework makes available elegant and comprehensive accounts of the syntax/semantics interface for Gapping phenomena, the interaction of scopal operators such as generalized quantifiers with ‘nonconstituent’ coordination (Kubota & Levine 2015, Kubota & Levine 2016a), *respectively* readings, symmetrical predicates and summative predicates, their interaction with each other and with ‘nonconstituent’ coordinations (Kubota & Levine 2016b; cf. (21a)), and pseudogapping and its interactions with, e.g., Gapping (Kubota & Levine 2017a; cf. (21b)).

(21) a. John offered, and Mary gave, the same advice to Bill and Anne (respectively) on the same day.
 b. John can eat more pizza than Bill can sushi or Mary fish & chips.

Given the success of HTLCG providing completely explicit accounts of this rather large range of notoriously problematic empirical phenomena on the basis of a simple formal calculus without any appeal to extra stipulations, inexplicit or even unformulated principles, or violations of basic compositionality, I think it can fairly claim some credibility as a candidate 'optimal theory' of NL. The critical point of this whole section, however, is that this broad and deep coverage has been achieved on the basis of a combinatoric system consisting of nothing more than a type-logical homology with the implicational fragment of intuitionistic propositional logic. A careful inspection of the preceding proofs will make clear that every proof step in every proof belongs to either the set of introduction rules or the set of elimination rules for the connectives instantiating hypothetical reasoning and modus ponens respectively in the hybrid proof theory that serves as the platform for HTLCG – a fact with significant consequences for 'biolinguistic' claims of domain specificity.

4. Implication is not domain specific

Suppose then that the combinatorics of the syntax/semantics interface in natural language takes the form of a fairly literal version of the inference rules in some standard logic. The most important takeaway from this possibility is, as I now argue, that there is no secure basis for any claim of biologically determined domain specificity in determining the human capacity for language. To establish this point, I first consider the relationship between the cognitive basis of ordinary reasoning, on the one hand, and the formal rules of logical calculi on the other.

The psychologist John Macnamara has some useful comments on this question. He observes (Macnamara 1986: 31–34) that

> The main task for the psychologist who is studying human reasoning is to account for both our ability to reason validly and our intuitions about logical validity. The set of valid inferences is infinite […] it follows that we must have access to a set of rules that can be combined in various ways to yield an infinite set of inferences. Thus the foundations of the logics at which logicians aim, the ideal logic(s), must be psychologically real *in the sense of being instantiated in some form in the mind.* Further the best logics found in logic books today provide the best available guides to logical competence. *It does not follow, of course, even if a logic characterizes an aspect of logical competence, that the competence is instantiated in any of its currently available forms, say, axiomatic or natural deduction.*
>
> Basic logical competence […] include[s] the logical resources that are deployed in natural-language sentences. These include some set of sentential connectives […] [whose] logic […] in its simplest aspects may be given to us in a form that is close to natural deduction rules.

(Emphases added). The point is that sets of logical inference rules in explicit deductive calculi are mathematical systems which can usefully *model* psychological constraints on acceptable sequences of thoughts in the course of everyday reasoning. These systems are in effect distillations of that reasoning which capture their invariant properties over all possible content. The psychology of reasoning is an ontologically quite different creature from the rigorous proof theories which constitute that distillation, but in effect simulates the reasoning steps imposed by these proof theories – particularly in their natural deductive formulation.

Rips (1994) takes this programmatic view of the relationship between logical cognition and its mathematical formalization as his point of departure, setting forth a detailed model of reasoning, with extensive experimental support, built around a core of natural deduction rules and elementary theorems of those rules called PSYCOP (*psychology of proof*). At the heart of his proposal is the premise that

> a person faced with a task involving deduction attempts to carry it out through a series of steps that take him or her from an initial description of the problem to its solution. These intermediate steps are licensed by mental inference rules, *such as modus ponens,* whose output people find intuitively obvious.
>
> (Rips 1994: x., emphasis added)

Rips argues that a psychological instantiation of natural deduction inference rules and elementary entailments from those rules undergirds deductive reasoning generally, and that "deduction must be a component of many other psychological processes on any reasonable account of their operation" (Rips 1994: 12), citing comprehension, planning and several other higher-order cognitive activities. He further observes that the reasoning process summarized in the rules for the introduction and elimination of the connectives in standard logics must be built into the neural basis of thinking, pointing out that without these fundamental reasoning steps at the outset, there is no way that learning could take place, because these reasoning steps are crucial (and prior) to any conclusion that could be drawn from experience.

Suppose then that it is some transduction of standard deductive calculi into type-logical systems in which (some subportion of) the same rules of inference are faithfully maintained which constitutes the combinatoric core of natural language capability – as we would certainly conclude if some type-logical grammar such as HTLCG (or one of the several alternative logic-based versions of Categorial Grammar) proves to be a successful competitor in the marketplace of grammatical frameworks. Suppose further that the reasoning methods embodied in those calculi are also employed across the spectrum of higher-order cognitive activities, as much of Rips' survey of experimental evidence argues for. Then the crucial premise of domain specificity for linguistic knowledge, and the idea that 'biolinguistics' has as its object a circumscribed set of neural structures dedicated to a cognitively unique system of functions, become radically untenable.

This conclusion receives some support from recent work on the neurobiology of language carried out by actual neurobiologists – what one might call real biolinguistics, without the need for scare quotes. Particularly interesting are the results of the experimental study reported in Reverberi et al. (2007), which investigated what the authors call 'elementary inferences' – simple deductions based only on implication and disjunction elimination. Using functional magnetic resonance imaging technology to identify centers of brain activation during subjects' execution of the inference tasks presented during the experiments, they were able to demonstrate that simple deductions were associated most strongly with a specific complex of neural structures:

> Our results implicate the left inferior frontal gyrus (mainly B[rodmann's]A[rea] 44, left precentral gyrus (BA 6) and left parietal cortex (BA 40)) […] Based on this evidence, we argue that the frontoparietal network identified represents the neural basis of the generation of conclusion in elementary deductive problems. (Reverberi et al. 2007: 758)

BA 44 is in fact none other than the celebrated Broca's area, while BA 40 is a component of what Ardila et. al (2016) identify as part of a peripheral zone in an "extended Wernicke's area", contrasting with the "Broca's complex" they identify as "a complex frontal subcortical circuit involved in language production and grammar" (Ardila et al. 2016: 120). The rather striking fact which emerges from Reverberi et al.'s results is that the core area involved in the most basic kind of deduction – modus ponens, whose dual is hypothetical reasoning – is located in what is currently regarded as the neural center of grammatical cognition. These results are highly suggestive in terms of my proposal that grammatical combinatorics are nothing other than a version of logical inference restricted to the implicational fragment of standard logics.

It might be objected that, since the nature of the deductions that Reverberi et al.'s experimental subjects were asked to carry were framed as verbal problems, and since BA 44 is perhaps the key area for the grammatical processing of oral language, the high level of activity measured in this region of the cortex is unsurprising. But this objection would be misguided, for, as reported in Monti et al. (2009), a set of laboratory trials revealed that verbally framed logical deductions of a relatively high degree of complexity, corresponding in difficulty to the De Morgan laws "did not recruit [regions typically reported for linguistic processing] but rather a network of regions highly similar to that reported in previous studies of deduction with sentential connectives and quantifiers" (Monti et al. 2009: 12555), emphasizing later in their paper that "inference involving sentential connectives relies on a circuit that is largely independent of areas recruited by semantic and syntactic processes specific to natural language". Monti et al. note the apparent discrepancy between their results and Reverberi et al.'s, suggesting that it is the very simplicity of the deductive requirements in the latter's study which leads to these divergent patterns; as they note, "[t]he more challenging deductions figuring in the present experiment provoke extended and vigorous reasoning, not to be expected from elementary schemata like modus ponens" (Monti et al. 2009: 12557).[5]

Further evidence against domain specificity from real biolinguistics comes from the work of Tettamanti et al. (2009), who observed that Brodmann's area 44 is implicated in syntactic computations in a separate cognitive module, the visuo-spatial domain. Specifically, Tettamanti et al. found that the processing of what they call N[on]R[igid]S[equential][D]ependencies – essentially, relationships which are defined by arbitrary iterations of co-constituency definitions – are in all cases associated with Brodmann's area 44. They found that

> non-rigidly organized stimuli in both the language and the visuo-spatial domain are processed by a common bilateral fronto-parietal network, with an essential contribution of the left I[nferior]F[rontal]G[yrus]. In other words, in the presence of non-rigid dependencies, the processing of spatial information also depends on left hemispheric recruitment, in a qualitatively similar way to the processing of linguistic information […] More specifically, it was recently shown that the left IFG is crucially involved in the executive control of hierarchically organized action sequences. (Tettamanti et al. 2009: 836.)

5. It is worth noting that once one gets very far away from the kinds of elementary reasoning that Reverberi et al. based their test suite on, the actually reasoning steps that people use to determine whether a given inference is valid almost certainly look very little like the tidy proofs that count as formally correct, notwithstanding the appellation 'natural deduction'. It is unlikely that an ordinary reasoner, confronted by the premises 'Either John was out late or Mary was home early' and 'John wasn't out late', concludes that Mary must have been home early by appeal to the rule of Disjunction Elimination and a subproof of the radically counterintuitive ex falso quodlibet conclusion that $p \vdash \neg p \supset q$, a crucial part of arriving at the immediately obvious inference. It is quite plausible, in fact, that what Monti et al. observed was the operation not of literal logical inference in the deduction of complex theorems such as the De Morgan laws, but rather the kind of ordinary reasoning in which each step involves the chunking together of several basic steps into what we might think of as macros. This aspect of the reasoning process is still largely unknown, but it would not be at all surprising to find that it does not involve the same circuitry as that simple elementary applications of the rules for implication that Reverberi et al. found linked to the Brodmann's language areas connected with grammatical knowledge, and which I have suggested above represent a serious candidate for the formal representation of the combinatorics determining the syntax-semantics interface. It seems quite possible, in fact, that ordinary reasoners do not approach the *John/Mary* example by some simulation of a proof of the Disjunctive Syllogism, but rather access something much more like the truth table for v, which immediately guarantees that if one disjunct is false, the disjunction can only be true if the other disjunct is. That is, the semantics of *or* may in effect prepackage the Disjunctive Syllogism without any deductive reasoning being involved at all.

They conclude that "the human brain has some distinctive traits by which it is capable of encoding NRSD across diverse higher cognitive functions" (Tettamanti et al. 2009: 836).[6] Once again, then, we find that research on the biology of language which is informed by actual knowledge of biology yields results that severely challenge the 'encapsulated modularity' view of human linguistic cognition asserted in, e.g., Fodor (1983).

5. Summary and conclusions

The preceding discussion has I think shown that there is a crushingly heavy burden of proof on advocates of neuroanatomical literalism as a premise of the 'biolinguistic' program: the infinities in the theory of limits alone make the visual system inherently uninstantiable in literal fashion in the brain, on the assumption that Marr's analysis (or something like it, e.g, a neurological expression of the Canny edge-detection model, which takes the gradient of a smooth-filtered visual array as its core computation) is correct. And there is no more reason to believe that the discrete infinities that so much has been made of in previous discussion can be any more easily be embodied in 1-to-1 fashion in the fine-grained anatomy of the cortex.

Even Chomsky himself appears to have finally accepted this conclusion, judging by his remarks a few years ago in *The Science of Language*:

> In the work that I've done since *The Logical Structure of Linguistic Theory* – which just assumes set theory – I would think that in a biolinguistic framework you have to explain what that means. *We don't have sets in our heads. So you have to know that when we develop a theory about our thinking, about our computation, internal processing and so on in terms of sets, that its going to have to be translated into some terms that are neurologically realizable.* (Chomsky 2012: 91, emphasis added)

6. Tettramanti et al. also make the extremely interesting point that

> the fact that […] some nonhuman species can be taught simple NRSD is consistent with the view that language emerged in the course of evolution by drawing on set of cognitive and computational capabilities that, at least in rudimentary form, are shared across higher vertebrates. (Tettamanti et al. 2009: 837)

– a flat contradiction to Chomsky's biologically bizarre saltation hypothesis whereby the ability to iterate the set-building operations underlying natural language capabilities was the result of a single genetic mutation in a single individual in Homo sapiens' ancestral chain. For a penetrating critique of the scientific status of these speculations, see Behme (2014).

Apparently, then, Chomsky himself now disagrees with Anderson, Lightfoot and other neural literalists about just what we might expect to see as cortical imaging techniques continue to improve.

But even if this critical point is recognized, what remains is a formidable obstacle that does not seem to have been acknowledged even in work such as Martins and Boeckx (2016), which reveals at least some awareness of the huge gap between what has been done and what would need to be done to justify identifying biolinguistics as an actual science based on a range of robust results. The critical problem, again, is the fact in order to bridge this gap, researchers have to agree on just what it is that their best computational-level models – the abstract objects that are presumably realized in neural wetware – are models *of*.

The crucial contrast is with the detailed mathematical model of early visual processing discussed in § 2 above, which Marr and his associates were able to work out based on the key insight that the inital step in visual scene construction as a cognitive activity was the recognition of spatial discontinuities, and the possibility of treating the latter in terms of step functions, using the tools of signal processing technology to define a background against which these discontinuities would be recoverable using the methods outlined earlier. The empirical gravitas of that enterprise was underwritten by specific predictions about the information content of grey-level arrays subject to the convolution and zero-crossing calculations detailed in the Marr-Eldreth theory of edge detection and its neuroanatomical expression; likewise for higher order operations proposed by others in the Visionaries group of researchers at MIT in the early 1980s and after.

In contrast, the 'biolinguistics' community has yet to achieve a secure identification of *any* computational task in the domain of language comparable to the Marr group's edge detection hypotheses. Marr's breakthrough was in correctly identifying, at the first level of approximation, the crucial 'object of computation' for visual cognition. What is the corresponding object in the domain of linguistic cognition? Right now, we cannot talk about anything finer-grained than Brodmann's areas; to identify specific neural structures will require an insight fully comparable to Marr's forty years ago, and quite possibly still deeper. And despite all of the aspirational enthusiasm – and outright boasting – in the 'biolinguistic' literature, it seems very unlikely that anyone in that research community would claim that we are on the verge of such a breakthrough, let alone that we have already achieved anything remotely like it.

This is where the issue of domain specificity becomes crucial. While the implementation of at least certain components of visual computation does appear to be a highly specific, dedicated system unconnected with other sensory modalities, the authentically biolinguistic work cited above – carried out, it should be stressed, by actual experts in the biology of the human nervous system – has instead provided very suggestive evidence of domain generality, with the real-time implementations of basic deductive reasoning, sentence processing and – as per the research reported in Tettamanti et al. (2009) – visual/spatial awareness, all sharing the same circuitry, in Brodmann Area 44 and other neurological domains.

In view of these severe obstacles to the prospect of identifying distinctively neuro*linguistic* structures in the human neocortex, it seems appropriate to conclude with some observations by a prominent proponent of the 'biolinguistic' perspective that seem applicable to where things stand, and are likely to continue to stand, into at least the foreseeable future:

> The reasons why economics is unlikely to reduce to physics are paralleled by those which suggest that psychology is unlikely to reduce to neurology. *There are no firm data for anything but the grossest correspondence between types of psychological states and types of neurological states, and it is entirely possible that the nervous system of higher organisms characteristically achieves a given psychological end by a wide variety of neurological means.* It is also possible that given neurological structures subserve many different psychological functions at different times […] In either event, the attempt to pair neurological structures which psychological functions could expect only limited success. (Fodor 1975: 17, emphasis added)

All that is needed to capture the current state of 'biolinguistics' is to replace 'psychology' in the above passage with 'linguistics' – and, after all, Chomsky has for by far the better part of his career, identified linguistics as indeed a branch of 'theoretical psychology'. In the more than forty years since Fodor's comment appeared, the substantive situation with respect to 'biolinguistics' has not changed in the slightest, as Embick and Poeppel's state of the art overview cited earlier attests – notwithstanding the 'triumphalistic rhetoric' alluded to in a passage from Newmeyer (2003: 586) devoted to Chomsky's characterizations of minimalism, but also applicable to the tone of the neural literalism that Ramchand found so objectionable. And there is nothing on the horizon to lead us to expect that situation to change.

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CHAPTER 4

The relevance of realism for language evolution theorizing

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It may appear counterintuitive to suggest a connection between language evolution and linguistic realism. Only biological objects evolve but linguistic realism holds that natural languages are abstract objects. However, given the fact that currently no approach to language evolution can account satisfactorily for all aspects of language, I suggest that reconsidering the ontological status of natural languages might lead to novel approaches to language evolution puzzles. Most contemporary work on language evolution assumes without argument that natural languages are either biological entities or produced by biological organs (human brains), and focuses on brain evolution, language acquisition, and communication systems of other primates. Yet, so far such approaches have been unable to account for some aspects of grammar. Furthermore, to date little is known about the bio-physiological implementation of natural languages. I suggest that the debate could profit from paying closer attention to the ontological status of language and the exact relationship between language and biology. Finally, I discuss the kinds of evidence used in linguistic research and demonstrate that, contra to widespread belief, the linguistic Platonist is neither relying on inferior evidence nor ruling out evidence that is clearly relevant to linguistic research.

Keywords: realism, language evolution, innatism, animal communication, linguistic evidence

1. Introduction

Noam Chomsky is often credited with reawakening language evolution research from the 'sleeping beauty' existence it had led for more than a century. Decades ago he proposed that the "idea of regarding the growth of language as analogous to the development of a bodily organ is […] quite natural and plausible" (Chomsky 1975: 11) and that "the language faculty may be regarded as fixed

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function, characteristic of the species, one component of the human mind" (Chomsky 1977: 63). These proposals came at a time when Chomsky's generative grammar had "become the conventional wisdom [in linguistics]" (Searle 1973: 8) and his influence had extended into psychology, philosophy, anthropology, and other fields. Furthermore, ontological naturalism (the belief that only concrete physical objects exist) had become the widely accepted default position. Therefore, many non-linguists readily accepted the assumption that language is a biological organ, and attempted to address the question of how such an organ might have evolved.

As a result, of this development, today exists a thriving language evolution research community that includes experts from anthropology, archaeology, cognitive neuroscience, computer science, genetics, linguistics, neurophysiology, philosophy, physics, primatology, and psychology. These experts have generated a sizeable body of work. However, in spite of impressive accomplishments there remain significant gaps in our understanding of language evolution. A few years ago, one leading language evolution researcher remarked: "many different scholars have reached valid insights about human language […] but no one scholar or discipline has yet achieved an adequately comprehensive overview of this complex system" (Fitch 2010: 2).

In addition to the problems common to all evolutionary theorizing there is also no generally accepted consensus about the 'object' of evolution: language. While virtually everyone assumes something in our biology accounts for our ability to use language, the exact nature of the 'language organ' or "'anguage instinct' remains a matter of controversy, and many questions about the ontological status of language and the exact relationship between language and biology still await satisfactory answers. Unfortunately, many researchers who focus on formal models of syntax and/ or semantics seem to have little interest in the language evolution debates. Yet, their input is needed to address the question whether the same mechanisms that account for animal cognition and early stages of language acquisition can eventually account for all features of natural language. Furthermore, there is disagreement about whether the primary function of language is communication. Some have argued that communicative language derives from non-communicative activities such as play, or theory of mind. Another aspect of cognition that may be critical, and have identifiable precursors, is mental time travel. Some researchers propose that a critical feature of language is displacement, and that language evolved primarily to communicate about the non-present. Finally, even though there exists an impressive body of language evolution literature, little of this work seems to have an impact on the work on 'general' evolution. For example, a Web-search for 'syntax' generated only hits for research on birdsong in the journal *Evolution* and for research on birdsong and some book-reviews in *Evolution and Human Behaviour*.

2. Some highlights from current language evolution research

Given the interdisciplinary nature of language evolution research, single author attempts to provide a comprehensive overview (e.g., Fitch 2010) or even a complete account for the transition from pre-linguistic ancestors to modern language users (e.g. Arbib 2005; Deacon 2009; Tomasello 2009; Lieberman 2013) remain rare, and these authors seldom discuss ontological issues. Comprehensive work that focuses predominantly on the evolution of the structure of language (e.g. Hurford 2007, 2011) is even rarer. The vast majority of language evolution researchers focus on what seems one small piece of a much larger puzzle. Given how many missing pieces remain, such work is certainly needed. But often it is not clear how this specific work relates to other work on language evolution and whether the techniques used to generate some specific finding could be extended to other domains. It would be impossible to give an even cursory overview of recent findings here. Consequently, only a few representative examples can be discussed below.

2.1 Cognitive capacities in non-human animals

Many evolutionists prefer gradualist accounts and tend to focus on cognitive capacities of non-human species. Much attention has been devoted to the study of the mechanisms underlying the communication of our closest relatives, non-human primates.[1] Researchers attempt to establish which aspects of human language are within the reach of non-human primates' mental abilities and which are not. It has been known for some time that vervet monkeys (*Chlorocebus pygerythrus*) produce predator specific alarm calls for leopards, martial eagles, and pythons. Different alarm calls seem to evoke different responses. The leopard alarm calls prompt the monkeys to climb into trees, the eagle alarm calls result in monkeys hiding in nearby bushes, and the python alarm calls prompt the monkeys to cautiously survey the ground. It could be argued that, like the words of a language, these alarm calls convey specific information about

1. Research that has been conducted on attempts to teach human language to non-human primates is not discussed here. Even though this work has generated much publicity and many interesting results (e.g. Savage-Rumbaugh & Lewin 1994), it remains controversial and can be challenged on methodological grounds. The general consensus seems to be that it is unlikely that, in spite of their undeniable cognitive sophistication, non-human primates can be taught a communication system that rivals the full complexity of human language. Furthermore, to gain a genuine understanding of the cognitive abilities of non-human primate, it seems more appropriate to study these animals in the environment they have adapted to.

approaching predators to other members of the group. Yet, it was assumed that the calls where instinctual, and did not involve vocal learning. Further calls seemed always to be a direct response to a visual stimulus (predator) and not intended to convey benefits to other group members (Seyfarth et al. 1980). These findings suggested that alarm calls are fundamentally different from the words of a human language.

Recent work on other species has shown closer similarities to human language. For example, 'contest hoots' are acoustically complex vocalizations produced by adult and subadult male bonobos (*Pan paniscus*). These calls are often directed at specific individuals and regularly combined with gestures and other body signals. They provoke a social reaction in the targeted individual and may function to assert social status. The intentional use of multimodal sequences to initiate social interactions with important group members could indicate more cognitive complexity than previously attributed to bonobos. (Genty et al. 2013).

Further corroboration for the intentionality of primate vocal communication comes from research on chimpanzees (*Pan troglodytes*). In an experiment designed to test for higher order intentionality (one animal's belief about what another animal likely knows), researchers presented wild chimpanzees with a python model and found that alarm calls depended on the presumed knowledge of receivers. The researchers argue that "alarm calls were: (i) socially directed and given to the arrival of friends, (ii) associated with visual monitoring of the audience and gaze alternations, and (iii) goal directed, as calling only stopped when recipients were safe from the predator" (Schel et al. 2013: 1). Taken together these findings suggest that cognitive capacities required for human language use (the understanding of both one's own and others' mental states and a desire to modify another's mental states) are present in some non-human primates.

These and similar findings might suggest that the cognitive gulf between humans and other primates may not be as wide as previously assumed. However, the differences between non-human primate communication systems and human language are undeniable and very little is known to date about how the former could have evolved into the latter. In this context it has been pointed out that, unlike humans, non-human primates are not able to produce a wide variety of distinct sounds and thus lack one seemingly essential prerequisite for spoken language (for overviews see Fitch 2010; Lieberman 2013). On the other hand, songbirds are able to produce a wide variety of distinct sounds, and it has been shown that they are capable of complex vocal learning, an ability needed for language. For example, European starlings (*Sturnus vulgaris*) can be trained to recognize acoustic patterns defined by a recursive, self-embedding, context-free grammar. They are able to recognize new patterns defined by the grammar and reliably exclude ungrammatical patterns[2] (Gentner et al. 2006). Recent work on zebra finches has shown that these songbirds can learn to recognize affixations. Affixes have grammatical function in language and recognizing them is one of the many abilities needed in language acquisition. Finches learn to recognize different affix-patterns and show preference for prefixes (Chen et al. 2014). The claim is not that finches recognize affixes as affixes, far less that they are aware of grammatical function. Rather, they are capable of very fine-tuned discrimination, an ability considered crucial for human language production and comprehension. Extensive work on African grey parrots (*Psittacus erithacus*) has shown that these birds can acquire a large vocabulary, learn to differentiate meaning and rudimentary syntax, and can engage in a simple conversation with a human trainer (Pepperberg 2008). In addition to vocal ability some corvids also show impressive social intelligence, suggesting that these birds have complex cognitive abilities. For example, Scrub jays (*Aphelocoma coerulescens*) remember not only numerous sites where they cached food but also where conspecifics have cached. They pilfer those sites when given the opportunity. It was shown that jays with prior experience of pilfering another bird's caches subsequently re-cached food in new cache sites when they had been observed by other birds caching. This suggests that jays relate information about their previous experience as a pilferer to the possibility of future stealing by another bird, and modify their caching strategy accordingly (Emery & Clayton 2001). Similarly, complex abilities have also been confirmed in ravens (*Corvus corax*) (Bugnyar & Heinrich 2005), and New Caledonian crows (*Corvus moneduloides*) (Taylor et al. 2012).

A wide variety of work on non-human animals suggests that several species possess rich cognitive resources and the ability to interpret and, to a lesser degree, produce meaningful acoustic signals. Yet, no other species has a communication system rivaling human language. Possibly one reason for this disparity is that only some of the cognitive and physiological resources needed for language are accessible to members of other species while only humans have access to the full suite of resources. Or it could be the case that non-human animals are unable to overcome specific barriers to achieving a linguistic communication system. "How humans overcame (or side-stepped) these limitations [remains] a central question for theories of language evolution" (Fitch 2010: 202).

2. This work has been criticized (e.g. Corballis 2007; Fitch & Friederici 2012). Here it is only relevant that starlings were able to *hear* the differences between 'grammatical' and 'non grammatical' sound sequences. Whether, additionally, they cognize recursive structures *as* recursive structures is irrelevant.

2.2 How close to language is animal communication?

In spite of the rapidly growing body of work on animal communication, many linguists remain sceptical whether this research has the potential to shed light on the evolution of the structural complexity of human language. Granted that cognitive abilities confirmed in non-human species may play a crucial role in the transition to language they would need to be supplemented with mechanisms that can account for the full complexity of human languages. And, some linguists argue, precisely this full complexity is in need of an evolutionary account.

Possibly in response to those concerns, some language evolution researchers are emphasizing not merely the cognitive similarities between humans and non-human species, but claim that there are (strong) structural analogies between animal communication systems and human language. It has been proposed that the communication systems of a wide variety of species have either some or all of the properties attributed to human languages: phonology, morphology, syntax, semantics, and pragmatics. Yet, some researchers seem to use those terms rather loosely or metaphorically.

The term 'syntax' is especially subject to widely divergent uses. On the one hand, one can say that any system consisting of elements that are or can be combined in a regular way, has a syntax. From this perspective chemical compounds, works of architecture, musical compositions, etc. have a syntax. On the other hand, in linguistics, syntax usually refers to a rule system for combining the elements (morphemes, words, phrases, sentences etc.) of a language into well-formed, meaningful units. Here syntax "is more than just putting things next to each other [it allows the] hierarchical embedding of constructions within each other" (Hurford 2014: 128–129). One might hope that in the language evolution literature 'syntax' would be used in the latter sense. This is not always the always case. Syntax has been attributed to the vocalizations of a wide variety of species. For example, when reporting on the "syntax in communication sounds emitted by mustached bats" (Kanwal et al. 1994: 129), these authors use 'syntax' to refer to sound-combinations of 33 different types of syllables that can be further classified as simple syllables, composites, and sub-syllables. They do not claim that these syllables confer meaning. Presumably, quite a few linguists would question whether 'syntax' is used appropriately in this case.

Syntax has also been attributed to whale song: "There is a strong structural constraint, or syntax, in the generation of the songs (of humpback whales (*Megaptera novaeangliae*) CB)" (Suzuki et al. 2006: 1849). Here the term syntax refers to the structuring of sequences of vocalizations (songs). Songs are sequences of themes, which consists of phrases of several sound units. It is important to remember that the description of whale songs is based on acoustic features humans (or more recently automated classifiers) detected. It cannot be assumed that these sound-units and their relationship to each other provide the kind of information to whales that syntax of language provides to humans. Yet, this distinction is not always drawn in the language evolution literature. For example, one reads: "In delphinids, we find […] *an understanding of syntactical structure* in artificial communication systems and the ability to use learned signals referentially" (Janik 2014: 63, emphasis added). Claims like these minimize the differences between whale-song and human-language syntax.

The situation is similar for the voluminous literature on bird song and vocalizations of non-human primates. At times, the claims seem to suggest rather advanced cognitive abilities: "White-crowned sparrows […] can copy a song from another species with quite different phonology and syntax" (ten Cate 2014: 159). However, the author later defines syntax as "the use of rules that guide the arrangement of vocal items" (ten Cate 2014: 161). He leaves it open whether the birds are actually producing songs that follow those rules or if they produce songs that merely conform to rules superimposed by a human observer. When reporting work on non-human primates, researchers also emphasize similarities to language. Recently it has been suggested that alarm calls of male Campbell's monkeys (*Cercopithecus campbelli*) "can and should be studied as formal languages with a sound system, a lexicon, a morphology, a syntax, a semantics, and a pragmatics" (Schlenker et al. 2014: 441). The authors caution that this proposal "does not imply that [the calls] share non-trivial properties with human language, nor that they share an evolutionary origin with it" (Schlenker et al. 2014: 441). Nevertheless, the terminology they use has the potential to mislead. Others drive the analogies even further. Reporting on one of the early attempts to teach ASL to chimpanzees (*Pan troglodytes*) one author writes: "Washoe was also able to place words in the correct order to make a well-formed sentence […] This demonstrates that Washoe had acquired, at the very least, a rudimentary understanding of syntax" (Evans 2014: 42). Presumably, here 'syntax' refers to the syntax of a human language. The same author also reports the research on the Campbell's monkeys' calls and claims that "the combinatorial power of human syntax can be found, albeit in vastly reduced form, in another species" (Evans 2014: 53).

The term 'syntax' is used to apply to wide variety of phenomena from acoustic properties of sound systems to the phonological and morphosyntactic properties of human language and the transitions between the different applications are fluid. Furthermore, some authors use 'syntax' merely to describe animal communication systems while others imply and/or suggest that syntax actually plays a role in the communication of non-human species. In the latter case, it also makes sense to speculate about the evolution of syntax. Given the multidisciplinary nature of language evolution research it cannot be ruled out that the inconsistent use of terms like ‘syntax’ leads to misinterpretation. This potential problem can be minimized if researchers clearly situate their findings in relation to human language, explicitly state how syntax in non-human communications systems differs from syntax in human language, and how evolutionary transitions from one system to another could be modelled.

3. Ontological considerations

Questions about the ontological status of language are seldom raised in language evolution debates. Even though one occasionally finds remarks emphasizing that language is not a ‘monolithic whole’ but “a complex system made up of several independent subsystems” (Fitch 2010: 17) and that the “unspecified use of […] the word ‘language’ […] is probably best avoided” (Fitch 2010: 24), few language evolution researchers provide a definition of ‘language’ or specify which aspect of language their work deals with. Newcomers to the field must find it perplexing that one can discuss the evolution of X (language) without ever specifying what X is.

Historically, there might seem some justification for the ‘everyone knows what we’re talking about’ attitude. Many of those who first reawakened language evolution theorizing (e.g. Bickerton 1981; Pinker & Bloom 1990; Pinker 1994; Hurford 1989) shared the Chomskyan commitment to a domain specific innate language organ (or language instinct). And early opposition was also directed squarely at this conception of language. However, over the decades, researchers representing a multitude of competing, and only partially overlapping, frameworks have joined the debate. Furthermore, Chomsky “has overturned and replaced his own established systems with startling frequency” (Smith 1999: 1). As a result of this work the once dominating view, that humans have a highly complex domain-specific language organ, is now rejected by the majority of language evolution researchers. Some still defend a fairly strong linguistic nativism (e.g. Jackendoff & Pinker 2005), while others consider language as embedded in and arising from general-purpose cognitive capacities (e.g. Deacon 1997; Tomasello 2009; Lieberman 2013). Still others propose to eliminate the genetic component altogether and consider language to be a ‘cultural tool’ (Everett 2012).

As a result of these changing commitments the language evolution debate is often complicated by terminological ambiguities. Individual researchers rarely define ‘language’ before presenting their findings. Even though many researchers now reject the Chomskyan paradigm, the ontological conflations introduced by him continue to affect the debates. One regularly finds statements like

> The progressive evolution of the biological capacity to learn and use highly complex language is unlikely to be explained primarily by any subsistence or technological advantages that language offers. Rather, language probably served social purposes. In particular, two relationships could have driven selection in favor of increasingly complex language. (Burling 1986: 1)

Burling speaks of (i) a biological *capacity to learn and use language* and (ii) selection in favor of *increasingly complex language*, and throughout the text he uses (i) and (ii) interchangeably without giving any argument that this equivocation is justified.

While many researchers seem to reject the suggestion that the language faculty literally generates linguistic expressions, few seem to contemplate the ontological status of linguistic expressions. If these expressions are not biochemical compounds produced by an innate language organ (like hormones are produced by endocrine glands), then what *is* their ontological status? Raising this question prompts most language evolution researchers to emphasize that they are not (predominately or at all) interested in diachronic language change but in the evolution of the cognitive structures that allow humans to learn and use language. But, even someone who is 'only' interested in studying the evolution of the cognitive structures involved in sentence production and comprehension should be concerned about what kinds of things sentences are, where they stand in the universe of objects, and how they relate to cognitive structures. Attempting to study the evolution of the cognitive structures involved in the production and comprehension of linguistic expressions without having any understanding of the nature of these expressions seems akin to studying the evolution of endocrine glands without knowing what kind of things hormones are.

Possibly the philosophy of language can shed some light on these ontological issues. Philosophers of language distinguish between types and tokens. The distinction can be illustrated as follows:

> ES IST DER GEIST DER SICH DEN KÖRPER BAUT: [S]uch is the nine word inscription on a Harvard museum. The count is nine because we count der both times; we are counting concrete physical objects, nine in a row. When on the other hand statistics are compiled regarding students' vocabularies, a firm line is drawn at repetitions; no cheating. Such are two contrasting senses in which we use the word *word*. A word in the second sense is not a physical object, not a dribble of ink or an incision in granite, but an abstract object. In the second sense of the word *word* it is not two words *der* that turn up in the inscription, but one word *der* that gets inscribed twice. Words in the first sense have come to be called tokens; words in the second sense are called types. (Quine 1987: 216–217)

What is true for words also applies to combinations of words: phrases, sentences, in short to all linguistic expressions. One can (and should) ask whether the sentence 'Es ist der Geist der sich den Körper baut' is the same as or different from the sentence in Quine's quote. Using lower case instead of all capital letters could make the two sentences appear to be different. But, for a speaker of German, the meaning of the words and the grammatical structure of the sentences are exactly the same. One can either (1) treat all sentences as tokens and insist that some tokens have the same meaning. Or one can (2) treat sentences as abstract types. Both choices have important implications. Adopting (1) seems attractive because as tokens all linguistic expressions would be concrete physical objects and would be directly accessible to the methods of natural science. Human brains could be understood literally as token manipulating devices. However, without referring to types, it is unclear what renders two specific tokens tokens of the same sentence. The physical properties of tokens (spoken, written, or merely thought about sentences) vary widely across a range of dimensions. Based on physical properties alone it seems impossible to identify reliably all and only the tokens of *the same* sentence. Adopting (2) offers a simple answer to this problem: All tokens of a type are tokens of the same sentence. However, if sentence tokens are 'the same' by virtue of abstract objects (types), then one has to address the question how human brains can obtain knowledge of such abstract objects.

4. Does linguistic evidence rule out linguistic realism?

During the Chomskyan era realism about abstract types (also called *rational realism* (e.g. Katz 1998) or *linguistic Platonism* (e.g. Katz 1981; Katz & Postal 1991)) has been ruled out virtually without argument. Two reasons make such dismissal seemingly compelling. First, there are widely shared concerns about the implications for kind of evidence for linguistic research. Second, realism about types raises seemingly insurmountable epistemological problems: if abstract types do not causally interact with physical objects, how can we know about them? I address both concerns below.

Generally speaking, scientists rely on two kinds of evidence: direct and indirect. For the linguist direct evidence "reflects the state of grammatical structure without intermediate agency or influence [indirect evidence] reflects the grammatical structure through a causal chain that terminates in behavioural and neurophysiological phenomena" (Katz 1981: 72). When both kinds are available direct evidence is preferable because it reveals grammatical structure directly and can validate indirect evidence: "indirect evidence depends on direct evidence for its legitimization as a relevant source of facts" (Katz 1981: 71).

For linguistic Platonists direct evidence is unavailable because they cannot directly interact with Platonic objects. For linguistic naturalists both direct and indirect evidence is available and often direct evidence is equated with empirical evidence. Researchers are usually commended for gathering empirical evidence and the quality of empirical evidence is seldom subjected to close scrutiny. Especially philosophers seem mainly to be concerned that one might overlook any empirical evidence that could be relevant (e.g. Fodor 1985; Stainton 2006; Iten, Stainton & Wearing 2007; Ludlow 2011).

However, a closer look at the kind of evidence investigated reveals that for the naturalist in most cases indirect evidence is the only evidence available. If one adopts the (so far unproven) Chomskyan framework, direct linguistic evidence can only come from brain research. If the Chomskyan framework is correct then eventually it might be possible to locate the interacting neural cluster mechanisms that contribute to the dynamic patterns in the brain's cortex that correspond to speech perception and production. Currently however, researchers have only very tentative knowledge about the details and "must acknowledge that in the present research situation the precise details of the modules' organization are not yet known" (Schnelle 2010: 201). It is known that several areas in the brain are implicated in language processing and production. But current technology does not allow direct access to the brain structures that are involved in language related tasks. The available indirect evidence merely shows that an area of the brain is more active during a specific task but not how this task is performed there.

> The problem is that the measures don't track the neural transmission of information directly. Rather, they detect only concomitants to neural activity, namely electrical activity and increased blood flow, that result from increased brain activation. At best they tell us about the site of storage or retrieval operations needed in language processing. (Clark 2009: 359)

Given these limitations, evidence from brain research currently needs to be supplemented with other linguistic evidence to provide any useful information. Empirical evidence from the performance or E-language level is more readily available and used extensively. By now there exist for a variety of languages complex collections of utterances made by actual speakers (e.g., Brown Corpus, LOB Corpus, CHILDES). Further, researchers have gathered a wealth of data from a wide variety of linguistic experiments and computational modeling of language acquisition and other aspects of performance.

Of course, in many cases researchers' theoretical commitments will determine which kind of evidence they consider acceptable. Orthodox Chomskyans will accept introspective evidence elicited from competent native speakers (e.g., Chomsky 1975; Smith 1999; McGilvray 2006, 2009) but might reject evidence from computational modeling (e.g., Chomsky 2009, 2012; McGilvray 2009, 2012). Other Chomskyans allow a wider variety of evidence but remain skeptical towards data gathered from random samples of naturally occurring utterances. Common complaints are that these utterances are defective, and that the procedures for obtaining them are not standardized. Finally, researchers working outside of the Chomskyan framework (e.g., Tomasello 2003; MacWhinney 2004; Reali & Christiansen 2005) rely mostly on corpus analysis and are skeptical of what they call the artificiality of eliciting native speaker intuitions. This widespread disagreement about the value of different kinds of empirical evidence makes it difficult to compare work done by researchers with different theoretical commitments.

Additional problems with empirical evidence are familiar from scientific work in general. Given that the evidence any researcher can actually consider is only a small fraction of all available phenomena the problem of representative sampling arises. The interpretation of data is always to some degree affected by theoretical commitments. Some crucial evidence may not be available and pragmatic considerations may further limit the evidence that actually will be considered. Finally, given the recent challenges to language universals (e.g., Evans 2005; Evans & Levinson 2009; Everett 2005) it is no longer possible to assume that research intended to generate hypotheses that apply to all human languages can be limited to one language or even a small representative sample of existing languages as occasionally proposed (e.g., Chomsky 1980, 1986). The resulting need for broadly cross-linguistic comparative work adds to the complexity of the task.

Having shown that naturalists must rely in many cases on indirect evidence I turn now to the evidence available to the rational realist. Given that abstract objects (such as sentence types) are not accessible to the same kind of empirical investigation as concrete objects (such as brain circuits or sentence tokens) the realist relies on indirect evidence. For many realists the main source of evidence is the intuitions of competent speakers. In the literature there seems to be widespread confusion about linguistic intuitions. They are considered to be unscientific because they introduce some kind of mysticism (e.g., Fodor 1985), rely on inexplicable 'gut feelings', cannot be checked objectively, and, allegedly, give a license to stipulation (Fodor 1985; Stainton 2006; Iten, Stainton & Wearing 2007). However, Katz used the term intuition in a distinctly different way:

> In the formal sciences, it is common to refer to seeing that something is the case as intuition and to take immediate apprehension as a source of basic mathematical knowledge. [Intuition is] an immediate, i.e. noninferential, purely rational apprehension of the structure of an abstract object, that is, an apprehension that involves absolutely no connection to anything concret.
>
> (Katz 1998: 43–44, emphasis added)

From Katz's remark it becomes clear that (i) as in mathematics, only basic linguistic facts are intuitively clear, and (ii) like the senses, intuitions are neither infallible nor incorrigible. This means that, just as in empirical sciences, problems can arise when one draws inferences from intuitions. When one clearly sees a bent stick in the water and infers from this sense perception that the stick is bent one can be mistaken. Similarly, when one clearly intuits that all even numbers are divisible by two and infers there can be no even prime one is mistaken. In both cases, it is possible to correct the mistake. Finally, as in sense perception in intuitive perception one relies on objective facts (about perceptible or abstract objects respectively) not on stipulation.

While it is true that the work of rationalist realists depends on unobservable abstract objects this work itself can be observed. Linguists working under this framework do not postulate from the armchair but collect and analyze data and construct, test, and, if necessary, reject hypotheses just like naturalists. They are quick to point out that many subtle syntactic and semantic differences only show up when one tries to study grammar in detail. This detailed study not infrequently provides challenges to previously held convictions. For example, it is widely assumed that pronominal forms must agree with their antecedents in person, number, and gender (e.g., Curme 1931; Heim 2008) as in (1):

(1) Jack praised himself/*herself/*yourself/*myself/*themselves…
However, recently linguists have become aware of numerous exceptions such as:

(2) Your Majesty should praise yourself/herself.

(3) Every one of us thinks we/they are a genius (Collins & Postal 2012: 5).

This example shows that the process of data analysis and theory adjustment is essentially the same for naturalists and rational realists. Rational realists do not stipulate but expand the data-base (e.g., consider more sentences in more contexts, vary lexical items, test stability over time) and/or adjust the grammatical theory.

Having shown that neither naturalists nor rational realists have easy access to direct linguistic evidence and that the available empirical evidence is not superior to the evidence used by the realist I turn now to one final concern. Many linguists prefer naturalism because rational realism seems ‘epistemologically hopeless’ to them. Given that abstract objects do not causally interact with concrete objects the concern is that, even in a hypothetical ‘completed science’ of the distant future, we could never know whether a theory of a rational realist is correct. However, this concern is only valid if one requires that justification for realist theories has to be based on the same type of justification that is required for naturalist theories. Yet, such a requirement burdens the rational realist with an impossible task and ignores the fact that in the established sciences different types of direct and indirect evidence are available. No one would deny the validity of geometric theorems because in the physical world no perfectly straight lines exist. Taken together, these arguments suggest that, at least currently, the evidence available to naturalists is not superior to that used by rational realists. Given that rational realism insists on clear distinction between 'languages' (abstract objects and studied by linguists) and 'knowledge of language' (implemented in human brains and studied by natural scientists), it could offer language evolution researchers an attractive alternative to Chomskyan naturalism. Accepting this distinction would allow language evolutionists to refocus their attention fully on the cognitive capacities that *are* in need of an evolutionary explanation.

5. Conclusions

None of the arguments considered here confirm that rationalist realism is the correct ontological framework. But they suggest that realism should not be ruled out *a priori*. Regardless of one's own ontological commitment, researchers ought to avoid the conflation of incompatible ontologies that has plagued Chomsky's biolinguistics for decades.[3] Chomsky's ontology is internally incoherent because Chomsky assumes language to be both: (i) part of the human brain, and (ii) based on set-theoretic objects currently taken to be generated by the operation Merge (e.g. "a generative grammar as being based on an operation of Merge that forms sets" (Chomsky 2012: 91)). Clearly (i) and (ii) cannot apply simultaneously to the same object (e.g. an I-language). Brains are finite, physical objects. They exist in time and space, are destructible, and enter into causal relations. Sets are abstract objects. They do not exist in time and space, cannot be destroyed and do not enter into causal relations. Therefore, Chomsky's ontology, which attributes these contradictory properties to the same object (I-language), is internally incoherent (for detailed criticism of this view see Katz & Postal 1991; Katz 1996; Postal 2009). Collins (2009) and Watumull (2014) unsuccessfully attempted to reconcile these mutually incompatible ontologies (for critical analysis see Behme 2015), and Chomsky admitted recently that his theories force him to "accept things that we know don't make any sense, and hope that some day somebody will make some sense out of them – like sets" (Chomsky 2012: 91). This issue is relevant to language evolution theorizing because Chomsky's incoherent ontology generates many of the problems language evolutionists are accused of failing to solve. Generativist accounts of language are based a set-theoretic ontology while psychological

3. Detailed arguments showing how internally incoherent Chomsky's ontology is can be found in Katz & Postal (1991), Postal (2009), and Behme (2015).

accounts of brain processes and cognitive structures are only equipped to handle concrete objects. Therefore, it is unsurprising that a purely biological account of human language cannot address some of the linguistic problems that require one to treat languages as abstract objects. Focusing more on ontological issues will allow language evolutionists to side-step many of the futile debates some Chomskyans continue to force upon them (e.g., Berwick et al. 2011).

Language evolution research has generated many important insights. Especially, work showing that the cognitive gulf between non-human animals and humans is narrower than previously assumed raises hope that an account of the beginnings of language evolution is within our reach. Work on early language acquisition and computational modelling (not discussed in this paper) supplements animal research and provides further information about the likely 'starting conditions' of our journey towards language. Nevertheless, there remain significant gaps in our understanding of language evolution and no model that could address the evolution of the full spectrum of human language is on the horizon. It will be beneficial for future research to clarify exactly what specific aspect of language is addressed and to pay closer attention to the ontological issues discussed here.[4]

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CHAPTER 5

Describing linguistic objects in a realist way

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The essay is divided into four Parts A to D: Part A (Sections 1 to 3), Topic and background; B. Grammatical description (Sections 4 to 6); C. Grammars and theories of language: motivating axiomatization (Sections 7 to 9); and D. Grammars as axiomatic theories (Sections 10 and 11). The essay characterizes grammatical description, both informal and formal, from a realist point of view as the description of abstract objects, not to be confused with the concrete data to which the description must be ultimately related. The importance of theories of natural human languages for grammars is emphasized, also in view of comparative grammar writing, and is demonstrated by the detailed analysis of a grammatical statement taken from an informal grammar. There is a discussion of adequacy problems that arise in current frameworks for formal grammars due to an absence of theories of language from such frameworks. A format for axiomatic grammars is outlined by which an axiomatic grammar 'presupposes' a theory of language, in a technical sense. The view of grammars is non-reductionist; concepts of theory integration are characterized that allow us to integrate grammars with linguistic and non-linguistic theories. The conception of linguistics itself is non-reductionist, too, through applying a concept of inter-discipline that relates linguistics to other disciplines.

Keywords: Modified Realism, grammars and theories of language, 'comparative concepts', grammars as axiomatic theories

A. Topic and background of the essay

1. Topic

This is an essay on basic theoretical questions raised by grammatical description, both the description of individual languages and descriptions of a comparative type. While going back to the basics, the essay pays special attention to problems of current interest, which partly determines the choice of subtopics.

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A major point made in the essay concerns the problem of general vs. language-specific grammatical terms, and it is this: informal grammars of a language tend to presuppose, at least implicitly, a general theory of language, however incomplete, and they make use of the grammatical terms of the general theory in describing language-specific grammatical features of the object language. This is not easily reproduced, if reproduced at all, by most formal grammars. A general theory of language (not to be confused with a version of Universal Grammar in a Chomskyan sense) is also used in language comparison. Only very rarely has this been recognized in the debates on grammatical terms that have recently erupted in typology.

A second major point is an argument that formal grammars conceived as applied axiomatic theories are closer to informal ones than formal grammars construed as generative systems, and may be superior to such grammars.

A third major point is a solution proposed for the problem of combining grammars with other theories, both linguistic and non-linguistic.

A fourth major point is a demonstration that grammars conceived in agreement with points one to three are naturally germane to a realist conception of linguistics.

It is in the context of these points that Modified Realism is characterized as background: as one possibility for integrating grammatical description into a larger realist framework. 'Modified' refers to features of the conception that allow grammars and other linguistic theories to be construed as empirical. Modified Realism, if not by this name, was worked out over a number of decades; therefore, describing it will take some space even though it will only be characterized here, not justified. However, one does not have to subscribe to Modified Realism to understand and appreciate the major points of the essay.

2. Modified Realism

2.1 The position

In any field of studies, there are periods when interest, at least with some researchers, turns to the field rather than simply to its domain. In my case, it was in the seventies and eighties of the last century that linguistics itself – in addition to its objects, language or languages – was one of my major concerns. Having this as a key interest resulted from a deep dissatisfaction with the way linguistics was conceived, and in most cases practised, at the time. Over the years, I developed a different conception of linguistics, at odds with the mainstream, doing work that eventually culminated in a number of publications: two essays (Lieb 1987, 1992a) and parts of two books (Lieb 1983, 1993).

Predictably, my proposals for linguistics went unheeded. In 1992, I decided to concentrate on continuing my work *in* linguistics rather than *on* linguistics (documented now in Lieb *ed*. 2017), using the conception of linguistics that I had developed, ever hopeful that the winds of change might eventually disperse what I considered to be dense metalinguistic fog. – So what is it that I was proposing? Briefly, it as as follows:

> I am advocating, then, *combining constructive realism, non-cognitivist mentalism (intentionalism) and weaker functionalism into a unified position that may rightly be called a New Structuralism*. (Lieb 1992a: 68)

Before explaining this, just a word on the term 'New Structuralism'. This proved an unfortunate choice from a political point of view. I now prefer 'Modified Realism' for my own orientation, a term that would have been just as adequate. Structuralism has been making a return in the guise of Construction Grammar, and I see no reason for taking back my arguments for a 'New Structuralism' made in Lieb (*ed*.) (1992). Still, the term 'structuralism' has not managed to lose its negative connotations in many quarters, and 'Modified Realism' is more in line with the terminology used by others in related work, as represented, in particular, by the essays in the present volume. What I am outlining, then, is Modified Realism, a framework *for* linguistics that may be filled in different ways. My own approach, 'Integrational Linguistics', is only one way of filling it; the approach provides orientation and some background for the present essay but is not its topic.

2.2 Three components of Modified Realism

The three major components of Modified Realism may be characterized using quotations from my 1992 essay (Lieb 1992a). The formulations continue to be relevant, not only for historical reasons; I continue to subscribe to them.

Constructive realism
Denoting the approach as 'realism' is due to adoption of the ***'Linguistic Object Claim'***:

> For a proper treatment of linguistic intentionality it is necessary to postulate at least the following entities as abstract, extra-mental objects studied in linguistics:
> a. *linguistic structures*, i.e.
>   i. potential phonetic-syntactic forms that may be forms of speech events,
>   ii. certain proper 'parts' of such forms,
>   iii. structures of proper 'parts' of such forms (this is to cover morphology);
> b. *linguistic meanings*, i.e. meanings of linguistic structures that may be, or may underlie, meanings of speech events;

c. idiolects (in a defensible sense of the term) that are more abstract than sets of speech events; and idiolect systems that are more abstract than idiolects and determine idiolects, linguistic structures, and linguistic meanings;
d. languages that are abstractions from idiolects; and systems of languages that are abstractions from idiolect systems. (Lieb 1992a: 43)

'Constructive' refers to the fact that a 'constructive' hierarchy of ontological levels is assumed on whose higher levels abstract, extra-mental objects are typically placed, a hierarchy whose lowest level includes concrete objects in a traditional sense:

> A hierarchy of ontological levels is *constructive* in the following weak sense: entities of higher levels are *ontological constructs* based on lower-level entities. (Lieb 1992a: 46)

Non-cognitivist mentalism (intentionalism)
The Linguistic Object Claim refers to 'a proper treatment of linguistic intentionality':

> The objects of linguistics are extra-mental but they are still dependant on the contents of mental states and events. Thus, I cannot accept without qualification the Katzian view of abstract objects by which their existence is "independent of mind and matter" (Katz 1981: 12); nor do I subscribe to its epistemological consequence, postulation of a separate faculty of 'intuition' by which abstract objects are known. (Lieb 1992a: 67)

The connection with mental states and events is due to adoption of the 'Hypothesis of linguistics as an intentional discipline', or the ***'Intentionality Hypothesis'*** (1992a: 61–62):

> Something is an object of linguistics only if it is needed for describing the content of intentional mental states or events that are connected – in a non-contingent way – with
> i. speaking,
> ii. understanding speech, or
> iii. judging speech from a communicative point of view. (Lieb 1992a: 61–62)

Weaker functionalism
Following one tradition in linguistics, I also adopt ***functionalism***, as follows:

> The position I am advocating may […] be construed as *weaker functionalism*, a weak form of functionalism that adopts a functional view of speech and does not exclude a functional view of elements of linguistic systems but does not commit itself to such a view. (Lieb 1992a: 68)

3. Discussion

3.1 Explanations. A reason for realism

The Linguistic Object Claim and the Intentionality Hypothesis are heuristic hypotheses in the sense of being guidelines for conceptual development.

The terms 'abstract' and 'extra-mental' are defined in Lieb (1992a: 45, 49): 'abstract' as 'non-concrete', relativized to a 'hierarchy of ontological levels' whose zero-level has different sorts of individuals, at least objects in space and time as one sort and events in space and time as another. Mental states and events may form a third sort, unless reduced, on a materialist position, to objects or events in space and time. 'Concrete' means: belonging to *specific sorts* on the zero-level. (Entities such as numbers, when allowed on the zero-level, must of course be excluded as concrete.)

This notion of 'abstract' as 'non-concrete' differs from a Katzian conception by being relativized to a hierarchy of ontological levels where the entities on a higher level are 'constructs' from entities of lower levels, in a purely ontological sense that does not imply anybody 'constructing' them in a literal sense (which is rightly rejected for abstract objects, once again, in Postal (2012: 29): "the notion of *constructing them* makes no sense" – notwithstanding a sense in which *theories* may be said to be 'constructed').

'Extra-mental' means, roughly: not an actual part of a given mental state or event of a given person. Mental states and events have a content; the content is not considered an actual *part* of the states or events.

The Linguistic Object Claim requires a realist stance for a reason: achieving a proper treatment of linguistic intentionality.

This reason is different from a well-known argument used by Katz and others in support of realism: claiming that any knowledge in the formal sciences (mathematics, logic, and linguistics, following Katz) requires an object of which it is the knowledge, an object that must be abstract. While I see value in this claim, I do not rely on it, due to ambiguities of English *know* and *knowledge* (brought out in German by *können* vs. *kennen* as two possible translations of *know*, which is discussed in relation to languages in Lieb 1987). Instead, I adopt the Intentionality Hypothesis.

What is behind this hypothesis is of course Searle's conception of intentionality (Searle 1983 remains a classic text), in a somewhat extended form and applied to both acting and perceiving.

Very roughly, Modified Realism blends Katz with Searle, adding a functional perspective. It is largely this blending of Katz and Searle and its consequences that requires 'Modified' in 'Modified Realism'. The qualifier brings out the fact that my position differs from the approaches followed by strictly Katzian realists, such as Neef (2014) in arguing for his *Sprachsystemlinguistik*.

3.2 Objects and data

There is a major advantage of blending Katz with Searle: linguistics continues to be construed as an empirical science. Suppose that we consider linguistics in general and grammar writing in particular as non-empirical (the position taken for grammar writing in Neef 2014, again on a strictly Katzian approach). In my view, this is contradicted by too basic a fact about linguistics and actual grammar writing to be truly tenable: all branches of linguistics have data in a sense in which pure mathematics does not ('usage-based mathematics' would be nonsensical, 'usage-based grammar writing' is not). Presupposing Modified Realism, the ***data*** of linguistics are determined by adopting two claims concerning the ***objects*** of linguistics: the Linguistic Object Claim and the Intentionality Hypothesis. Use of linguistic corpora as collections of data is easily covered. Studying the influence of language use on language structure is rightly re-emphasized in linguistics after a period of relative neglect. It would be a mistake though to restrict linguistics to 'usage-based linguistics' narrowly understood, prescribing actual language use as the only source of data, confusing data structures with the objects of linguistics, and generally downplaying the unabated need for conceptual work in most linguistic fields.

Due to the Intentionality Hypothesis, linguistic data may originate from three sources: 'speaking' (in a general sense that is not restricted to orality), 'understanding speech', and 'judging speech from a communicative point of view'. How to use data in the construction of an empirical theory is a question of methodology; in particular, prescribing a bottom-up way of linguistic theory building with data corpora as a basis (advocated in Müller 2016: Chapter 22) would be just as restrictive as any other prescription of method that has occurred in the history of linguistics, and may be no better.

The amount of available linguistic data from the first two sources – 'speaking' and 'understanding speech' – has currently increased to an extent that was previously unimaginable, which makes the development of adequate methods an urgent task indeed (see, for example, Schäfer & Bildhauer 2013; or Levshina 2015). Data from the third source – 'judging speech' – should still not be neglected but must be used in the right way (cf. Schütze 2016 [1996], including references to relevant literature since 1996 in the 'Preface 2016').

Naturally, potential data must be evaluated; hardly ever will a worthwhile linguistic theory automatically result from simply applying methods to data, however large a corpus. True enough, some results in grammar writing may be obtained even by automated data analysis, and there is an entire line of research in this direction (for references, see Duchier & Parmentier 2015: Section 1.2). Also, progress in machine learning research and its applications has been impressive; given nearly unlimited computing power, the application of statistical methods to giant corpora and current progress in the use of neural networks have been decisive factors in obtaining ever more viable imitations of human language use and understanding by machines, on which actual products are increasingly based. This leaves the distinction between the objects and the data of linguistic theories unaffected, though; nor does it diminish the relevance of intentionality when human speakers are involved.

Generally, the objects of a scientific field must be carefully distinguished from its data. Modified Realism construes the typical objects of linguistics as abstract and extra-mental. Due to the constructional ontology adopted in Modified Realism, the abstract objects are ontological constructs ultimately based on objects and events in space and time – excepting cases when abstract objects by themselves form a separate sort of zero-level individuals (allowed for numbers, or, in semantics, for states-of-affairs). Mental states and events do not qualify as objects of linguistics; it is only their content – not considered an actual *part* of the states or events – that does. (In the case of meanings, *content-based properties* of mental states and events are also allowed as linguistic objects if the framework of Integrational Semantics is adopted, as in Lieb 1983: Chapter 13.) On a materialist position, mental states and events are spatiotemporal and concrete; they may therefore be data of linguistics, or sources of data, but not objects. Distinguishing objects from data, we escape the objections raised by Katz (2000 [1998]: Section 1.1.3) against 'naturalized realism'; appearances to the contrary, Modified Realism is not an example of this.

In a somewhat different context, Postal's decade-long criticism of Chomsky's reduction of linguistics through psychology to biology (see Postal 2012 for a recent summary) can also be understood in terms of objects vs. data: as a demonstration that the abstract objects of linguistics are wrongly identified by Chomsky with certain sources of concrete data.

In summary, data are not objects, method is not theory, theory not method, and reference to method alone is inadequate to define a discipline. Linguistics, in particular, is as much theory-driven as it is data-driven – I mean theory, not speculation without data.

3.3 Linguistic inter-disciplines

The relationship between linguistics and other disciplines is partly accounted for in Modified Realism by the notion of *inter-discipline*:

> For example, it may be proposed that *psycholinguistics* is an *interdiscipline of linguistics and psychology*: a shared branch of both, which are neither branches of a single other discipline nor branches of each other. (Lieb 1992a: 63)

('Inter-discipline' is understood as indicated after the colon; for details, see Lieb 1992a: Section 6.3.)

The notion of inter-discipline allows us to reconstruct the relationship between linguistics and certain non-linguistic disciplines without the reductionism that characterizes Chomskyan generative grammar. In particular, *biolinguistics* may be construed as an inter-discipline of linguistics and biology, as a shared branch of both, neither of which is a branch of the other (self-understood in the case of biology and linguistics); nor are linguistics and biology different branches of a single third discipline.

We are not free to construct the relationship between different disciplines as we like. Ultimately, it is the fruitfulness of one construal over another that is the decisive criterion. In this respect, linguistics as a branch of biology fares poorly, both practically: the vast majority of descriptive statements in linguistics as found in informal grammars are simply not of a biological type, and theoretically: whatever one thinks of Chomsky's reduction of linguistics through psychology to biology, the very fact that it has been seriously contested now for more than half a century is sufficient proof that this version at least is of doubtful value. In contradistinction, biolinguistics as an inter-discipline allows us to pursue the interrelations between linguistic and biological phenomena without confusing the two.

Questions concerning the relationship between two disciplines belong to the philosophy of science. Answering them badly may have a distorting effect on actual research in one field or both; linguistics, I claim, has been a sad example.

3.4 Placing Modified Realism

The position of Modified Realism was presented in detail, justified, and confronted with the mainstream alternatives of the time in Lieb (1987), (1992a). *How, then, does Modified Realism fit in with subsequent developments in linguistics and in the theory of linguistics?* A partial answer was provided in Sections 3.1 to 3.3. A number of remarks will be added here that are true of the Integrational Linguistic version of Modified Realism but are not restricted to it.

For the vast amount of work done in the English speaking countries, the question may be answered, in a general way, by referring to the survey article by Scholz, Pelletier & Pullum (2011/2015), *The Philosophy of Linguistics*. In this article, 'three approaches to linguistic theorizing' are distinguished: Externalism, Emergentism, and Essentialism. Very roughly, Modified Realism is close to Externalism by its conception of the objects and data of linguistics. It shares features with Emergentism due to the use of intentionality and adoption of a communication framework for linguistics. By putting an emphasis on the development of a general theory of language, Modified Realism pursues an aim comparable to the universalistic aims of Essentialism.

The communicative outlook of Modified Realism is emphasized by the fact that in the Integrational version, the general theory of language has been embedded in a theory of communication (only partly developed) from the very beginning (Lieb 1968, 1970). This does not mean that the topic of 'language and thought' cannot be treated.

On the contrary, *lexical meanings* are construed in the Integrational version as concepts in a psychological sense, as properties of conceptions and perceptions conceived as individual mental states or events. *Sentence meanings* are taken to be relations between potential utterances and potential speakers; this is also true of referential meanings and propositions as components of sentence meanings. There are at least three plausible ways of construing the relationship between sentence meanings (analogously, utterance meanings) and *thought content* (not yet discussed in the Integrational framework): (i) Thought content is extra-mental and consists of, or is based on, components of sentence meanings. (ii) As before, but with a construal as mental. (iii) There is no direct connection, both sentence meanings and thought content should be treated independently of each other. I tentatively adopt (i), rejecting a model-theoretic, truth-functional semantics for dealing with sentence meanings, utterance meanings, and thought content, similarly rejected by Pitt, e.g. (2009), and Soames, e.g., (2013). (See Lieb 1983: Parts E and F, and Lieb 1992b, for the semantic framework.)

The potential importance of Modified Realism has been increasing rather than decreasing, as appears from the discussion of linguistic realism in Neef (2014). Neef's paper is a reaction to Sternefeld & Richter (2012), a review article of Müller (2010), revised as Müller (2013) and available now in English as Müller (2016). Müller provides an excellent, detailed characterization and comparison of current models of generative grammars in a broad sense, also discussing the Construction Grammar approach. The review article by Sternefeld & Richter (2012) is highly critical of the state of the art. Outlining the realist tradition in linguistics, Neef (2014) argues for linguistic realism as an alternative.

How to describe linguistic objects in a realist way: this is the leading question of the present essay. Presupposing the framework of Modified Realism as background, I discuss a restricted version of the question, still quite general: *how to describe languages and their varieties by grammars in a realist way*. I begin by discussing a number of more general points in Part B, which will also serve to place my own proposals in relation to other descriptive formats.

B. Grammatical description

4. Preliminaries. Informal grammars

4.1 Some basic distinctions

I begin by drawing a threefold distinction between:

(1) a. theories of language, which deal with natural human languages in general,
b. theories of grammars, which deal with grammars as texts in general, or with specific types of such grammars,
c. grammars as texts, which are theories that deal with individual idiolects, language varieties, or languages, or deal with set theoretic constructs of such entities.

'Theory' is a highly ambiguous term. I am using '(a) theory' in (1) in a strictly 'declarative' sense in which a theory satisfies the following minimal requirement: it contains a set of sentences that are understood as assertions and are as such related to an entity that is intended as the object of the theory. This is sufficient to rule out grammars as theories when a grammar is construed simply as a system of rules for generating expressions; on such a conception, grammars do not contain suitable sentences.

The object of a theory of language in (1a) is taken to be, very roughly, the set of natural human languages. Theories of language in this sense must be sharply distinguished from 'Universal Grammar' as understood in generative linguistics, where natural human languages in a traditional sense have been downright rejected as objects of linguistic interest (see Section 5.2, below, for Chomsky). 'Universal Grammar', in any version, is not a theory of language (of natural human languages) even if allowed as a theory. For serious work on developing a general theory of language, Lieb (ed.) (2017) may now be compared: the Proceedings of a research group active over eleven years, documented on close to 2000 pages.

It is traditional to assume in linguistics that grammars are to deal with languages (1c). It would seem then that a theory of grammars (1b) must presuppose a theory of language (1a) if the theory of grammars is to clarify the relationship between grammars and their objects. However, no currently discussed framework for formal or semi-formal grammars conceives theories of grammars in this way. Trying to develop a theory of grammars without presupposing a theory of language that deals with languages independently of grammars does create serious problems, as will appear from Sections 5 and 6, below.

Generally, I will argue for the following ***position***. A theory of grammars must be able to specify the relationship that is to hold between a grammar and its object; insofar it depends on a theory of language to which it must have access.

An individual grammar as determined by a theory of grammars must have access to the theory of language that is made available by the theory of grammars. Such access is necessary if an individual grammar is to describe its object in a sufficiently general way. Theories of language turn out to be basic; if no independent theory of language is available, there is no way of comparing languages directly in comparative work; instead, only grammars – formulated texts – can be compared, whose relationship to natural human languages remains unclear.

Grammars can be roughly classified as ***informal, semi-formal***, or ***formal***, depending on the degree to which the grammar format and the language in which a grammar is written are explicitly specified. Informal grammars may well make up the vast majority of grammars ever written, but no corresponding theory to deal with them appears to be available. Semi-formal grammars are formulated in a regimented form of a natural language whose expressions can be construed as readings of expressions of a formal language, and formal grammars are formulated directly in a formal language.

4.2 Grammars as texts

I will proceed on a conception of grammars whose essential assumptions – informally characterized – include:

(2) A grammar of a linguistic means of communication – i.e., of an idiolect – is a text that determines – either completely or in part – a ***system of*** the means of communication: a system such that something is a normal utterance by a speaker using the means of communication only if the utterance agrees with the system.

(3) A grammar of a set of idiolects is a text that determines, or identifies, a ***system for*** the set of idiolects: a system construed, in the simplest case, as a set of component-specifying properties of idiolect systems such that for any system of any idiolect in the set, the system has each one of the properties.

(4) The object of a grammar is a pair consisting of ***either***
   i. an idiolect and a system of the idiolect, ***or***
   ii. a language construed as a set of idiolects (where a language is either a complete historical language or one of its periods – a certain subset) plus a system for the language, ***or***
   iii. a variety of a language (also construed as a subset) plus a system for the variety (stages of languages or varieties count as varieties of a language).

(5) If a formal or semi-formal grammar has the same intended coverage as a part of an informal one, then any grammatical statement made in this part and any of its logical consequences or presuppositions can, *in principle*, have a semantic analogue in the formal or semi-formal grammar.

These assumptions can be modified to cover more complex grammars such as comparative ones, along the lines of Lieb (1993: Chapters 20 and 21).

The assumptions still allow for a number of different interpretations. Arguably, even mainstream generative grammars are covered by (2) and (3) if the restriction to idiolects is lifted and a 'non-declarative' conception of grammars is permitted; and model-theoretic, constraint-based grammars as characterized by Pullum (2013) or exemplified by HPSG grammars (compare Müller 2016) may be covered by (2), (3), and (4) if idiolects are disregarded and (4) is allowed to apply informally: such grammars are 'declarative', have a statement component. Grammars using a Construction Grammar format appear to be similarly 'declarative', but they are more difficult to judge due to a high degree of informality that is typical of their formulation. (For more on the few formal Construction Grammar frameworks, see below, Section 5.1.)

(5) represents an ***adequacy condition*** for semi-formal and formal grammars, a minimal requirement needed to counter a basic objection of descriptive irrelevance frequently raised against formal accounts (outside a specialized technological context) – why bother with a formal theory in describing a language if it is weaker than a corresponding informal one? One could, of course, simply disregard the objection, as it is frequently done, but not paying attention to the irrelevance claim does not do justice to the fact that it is informal grammars that have been basic to language description and should therefore provide a yardstick for formal ones (which, in turn, may be used for elucidating properties of the informal ones).

Idiolects are taken into consideration in (1) to (5). This is motivated by an attempt to have grammars account for language variability – both within and between languages – right from the start, as in Lieb (1993), rather than abstract away from variability as something that is either unimportant or can be treated later. Some explanations concerning idiolects are in order.

4.3 Remarks on idiolects and idiolect systems

Idiolects (in a defensible sense of the term, cf. Lieb 1993: Chapter 6) are construed as sets that consist of sentences and are determined by idiolect systems; sentences are here understood as pairs consisting of a form component (not necessarily of a standard form) and a sentence meaning that is a meaning of the form component. In the case of a spoken idiolect, the form component is narrowly phonetic but still abstract, in contrast to the concrete sound event in an utterance – a pair of a sound event and an utterance meaning – that realizes the sentence. The sentence meaning is 'contained' in, but is mostly different from, the utterance meaning.

Idiolect systems are set-theoretic entities – certain n-tuples – constructed in a way that relates them to concrete entities in space and time; the details must be made precise in a theory of language (compare, for example, Lieb 1983: Chapter 2).

Determination of an idiolect by a system neither implies that the idiolect is finite nor that it is not. Membership in the idiolect is determined (completely or partly) by system-defined properties of form-meaning pairs that are on the phonetic level if the idiolect is oral. 'Ill-formed pairs' may be analysed and related to pairs in the idiolect by referring to system-defined properties that a pair does *not* have.

Communication by means of idiolects does not require that two speakers must have a shared idiolect or idiolect system. It is sufficient that each is in a position to form correct hypotheses on what the other person's idiolect or idiolect system is; for this, each speaker may fall back on his or her own idiolect if both are using idiolects that belong to the same language. (Such processes are, of course, mostly unconscious.)

I will concentrate on grammars of languages and their varieties; grammars of individual idiolects will not be considered. How, then, does a grammar describe its object? Informal grammars, especially their statements and terms, will provide a yardstick for the adequacy of formal ones, due to condition (5), above. Consider, then, a typical statement from an informal grammar and the terms that are used in it.

4.4 Sample statement from an informal grammar

The following sentence is from an informal grammar of English (Leech & Svartvik 1975: 207):

(6) Unlike many other languages, English requires an article with singular count nouns as complements […].

Reference is, among others, to sentences like 'Jack wanted to be a scientist', not: 'to be scientist'; as opposed to German 'Jack wollte Wissenschaftler werden', where there is no article. Sentence (6) was already analysed in Lieb (1993: Section 20.4); it will here be considered from a more general point of view.

In order to understand (6), we must be competent in a certain part of English that is used in describing its grammatical system. Suppose we are.

Obviously, the following ***double claim*** is being made by means of (6): it is true of English that it makes the requirement specified in (6), and it is true of many other languages that they do not. (Example (6) is sufficient to show that informal grammatical statements cannot be denied truth-values, in some sense.)

There is an obvious logical implication of (6):

(7) English is a language.

A sentence of this type may well be considered the most basic sentence either stated or implied in an informal grammar. – Consider the ***terms*** that occur in (6) and (7).

4.5 Terms in an informal grammar: Language specific and language overarching

There is just a single term in (6) and (7) whose application is restricted to English, and that is 'English' itself, the ***language name***.

The substantival term 'English' as used by Leech & Svartvik (1975) denotes a single entity that has varieties, as in 'Varieties of English' (title of Part One, Leech & Svartvik (1975: 21)). Varieties of all types are allowed but a restriction is made to two *national* varieties, British and American English; see Leech & Svartvik (1975: 21–32).

Recently, the plural 'Englishes' has come into use, mainly as a convenient abbreviation for 'varieties of English'; both expressions tend to be employed alongside of each other as, for example, in Kirkpatrick (2010). This means that the term 'English' itself is introduced in a second, additional sense, not yet found in the Leech and Svartvik grammar.

An analogous ambiguity is created for the term 'language' by Leech & Svartvik in the following quote (1975: 21): "the English language is, in a sense, not a single language, but many languages, each of which belongs to a particular geographical area or to a particular kind of situation". 'Language' is used here in a *first* sense in 'the English language' (with adjectival 'English'), which is synonymous with 'English' as a substantival term, and in 'a single language'. 'Language' is then used in a *second* sense in 'many languages', where the term applies to the *varieties* of languages in the first sense. Neither meaning is explained but 'language' in the first sense appears to mean, roughly, 'natural human language', in an informal sense. Let us call 'language' in this sense the ***domain name***. It is used in (6) and (7).

All other terms in (6) equally apply when speaking about English and about languages other than English; even more importantly, apply in the same sense in either case. This is trivially true of 'requires', which does not refer to any linguistic entities; it is a ***logical, non-linguistic term***, disregarded for now.

Grammatical terms like 'article' as employed in (6) must apply in relation to arbitrary languages: if 'article' could be used only when speaking about English, the comparison in (6) ('unlike many other languages') would not make sense; (6) would be meaningless. Assuming the distinction made in Huddleston's & Pullum's monumental descriptive grammar of modern Standard English between a level of 'general definitions' and a level of 'language-particular definitions'

(2002: 31–33), 'article' in (6) would have to be associated with the level of general definitions.

It may be argued that a term like 'article' in (6) applies meaningfully only with respect to languages 'that have articles', i.e., applicability presupposes or implies ***existence***. This cannot be true. For example, a statement such as 'There are no articles in Russian' would no longer be meaningful; most would agree it is not only meaningful but true. (Admittedly, to be useful such terms should not apply vacuously in relation to *every* language.)

Typically, the language name, and terms that depend on it, are the only ***language specific terms*** of an informal grammar, while all other linguistic terms are ***language overarching***: not restricted in applicability to the given language, and possibly applying in relation to arbitrary languages. Their use does not imply existence claims.

The reference of terms like 'English' in (6) and (7) should appear from a grammar of English. Terms that apply in relation to arbitrary languages must be assigned, for this very reason, to a ***presupposed theory of language***, i.e., of arbitrary languages, a theory that is usually left unspecified in an informal grammar of a language. Such a grammar may contain explanations of the grammatical terms it uses but from a systematic point of view, the explanations are *on* the grammar and do not form *part* of it.

4.6 Grammatical terms in linguistic theories: Descriptive and comparative linguistics

There are two areas where the problem of the language specific vs. the language overarching has come to the forefront in relation to grammatical terms: comparative linguistics and, more recently, the theory of formal grammars. I will deal with the problem in the second area later in the present essay but include some discussion here on the problem as it appears in comparative linguistics.

Most prominent in this area has been the Haspelmath / Newmeyer debate in *Language* 86:3 (2010), where grammatical terms of individual grammars are directly involved. Haspelmath opposes 'descriptive categories' to 'comparative concepts', summarizing his position as follows:

> In this discussion note I have shown how crosslinguistic comparison is possible if one adopts the position of categorial particularism, that is, that grammatical categories cannot be equated across languages. Each language has its own categories because the criteria by which the categories are defined (or recognized by learners) are themselves language-particular. A language-particular category set up by a linguist to account for observed speaker behavior is called a descriptive category. Comparative linguists create comparative concepts against which the descriptive categories of particular languages can be matched. These comparative

> concepts must be universally applicable; that is, they must be based exclusively on more primitive universally applicable concepts: universal conceptual-semantic concepts, general formal concepts, and other comparative concepts (or on extralinguistic situations). This approach has been widely practiced by comparative linguists working in the Greenbergian tradition. (Haspelmath 2010a: 681)

The position taken here has been influential in typology but has not gone unchallenged (it is downright rejected in Lehmann *forthc.*); its impact on frameworks for formal grammars appears to have been minor (see, in this respect, Müller 2016: Chapter 22). The follow-up discussion in typology ([Lingtyp] 2016a and 2016b) is now represented by the contributions in [Plank (ed.)] (2016), the 'Discussion' part of *Linguistic Typology* 20 (2). Most of my critical evaluation of Haspelmath (2010a) also applies to the contributions in *Linguistic Typology* and to the material from which they originate, with some qualifications: Croft (2016) appears to be envisaging, however informally, 'descriptive categories' ('constructions in a language', or 'strategies') as based on 'comparative concepts' ('constructions in general'); similarly, Beck (2016) for certain 'comparative concepts' "that can, and do, provide useful labels for language-particular descriptions" (Beck 2016: 396); and Lander & Arkadiev (2016: 412–413) question the claim that 'aprioristic' features can be avoided in typological descriptions, which may be construed as an argument for presupposing a theory of language. – I am going to stay with Haspelmath (2010a), (2010b), which has set the frame for the discussion and has not been superseded by later work from Haspelmath himself, such as Haspelmath (2015), (2016a), (2016b).

There is an ambiguity in Haspelmath's use of 'comparative concept' and 'descriptive category' that I am going to resolve: between terms, their meanings, and their denotata. For 'descriptive category', compare Haspelmath (2010a: 674): "descriptive categories are akin to proper names in that they refer to unique entities", next to many passages by which categories can only be *denotata* of terms, as in the above quote: 'each language has its own categories'. Similarly, Huddleston & Pullum (2002: 31), in referring to terms like 'noun', speak of "defining or explaining such *terms*" at a general and a language-specific level, but go on to propose "general definitions […] for the *categories* discussed above" (Huddleston & Pullum 2002: 32, emphasis mine, H. L.). In Haspelmath (2017a), 'comparative concept' appears to be used for the meaning of terms rather than the terms themselves.

The ambiguity is non-trivial: when the possibility of 'cross-linguistic categories' is discussed in the Haspelmath / Newmeyer debate, no clear distinction is drawn between the *definition* or *determination* of a category name used in a grammar and the *identification* of its denotatum (the category) by non-definitional criteria. Generally, there is a lack of logical explicitness in the Haspelmath / Newmeyer debate, just as in the debate in *Linguistic Typology* and the [Lingtyp] discussion. This makes them somewhat inconclusive.

Returning to the above summary, it is obvious from Haspelmath's explicit characterization of 'comparative concepts' that such concepts – understood as terms – are easily construed as language-overarching terms (typically, constants) belonging to a general theory of language, a theory that may be presupposed in cross-linguistic studies, in particular, in studies of a typological kind: comparative concepts are to be (i) 'created by the comparative linguist', and are to be (ii) 'universally applicable'.

This is compatible with the following view. In doing comparative work, the comparative linguist may have to formulate ('create') a theory, however incomplete, that is to apply to languages in general by providing 'concepts' (interpreted terms) that are 'universally applicable' and are suitable for the comparative work on hand; without such a theory, the 'concepts' are dangling in the air. The role of theories of language is not recognized by Haspelmath, who appears to wrongly associate general theories of language with versions of Universal Grammar in a Chomskyan sense (compare, in this respect, Haspelmath 2015).

The 2010 debate between Haspelmath and Newmeyer (resumed in a more general way, without involving Newmeyer, in Haspelmath 2015) is largely on the following question: May the *denotata* of 'comparative concepts' – understood as terms – be construed as 'cross-linguistic categories' (understood as linguistic objects) of which the 'descriptive categories' (understood as linguistic objects) of individual languages are 'instantiations'? Haspelmath's answer is decidedly in the negative, in particular, 'no taxonomic relationship' between comparative concepts and descriptive categories – each understood as linguistic objects – is allowed (Haspelmath 2010a: 680). I tend to agree with Haspelmath against Newmeyer (2010) in this respect for a reason that will appear later (Section 6.4, below).

Haspelmath further claims (2010a: 666): "Comparative concepts […] are a sort of metacategory that is irrelevant to language learning or language description / linguistic analysis." This implies, among other things, that comparative concepts understood as terms are *formally different* – in a non-trivial, semantically relevant way – from terms for descriptive categories: from terms that occur as category names in the grammars of individual languages – unless ambiguity is allowed, which is hardly an attractive step. There would be ambiguity of terms like 'article' if these were to be defined twice, at a 'general' and a 'language-particular level', as suggested in Huddleston & Pullum (2002: 31–33), and quasi-ambiguity in case typographical means, such as capitalization, are used to create a formal difference. Haspelmath is aware of the terminological problems (2010a: 673–675), but does not propose a definite solution. Croft (2016: 388–391) formulates five "rules of thumb for labelling language-specific and construction-specific categories" that "linguists implicitly follow" (Croft 2016: 387), but falling back on rules of thumb identifies a problem rather than a solution.

Haspelmath's claim on comparative concepts has a much stronger implication still than requiring a non-trivial difference between comparative concepts and descriptive categories, each understood as terms: the claim implies that the former are simply *irrelevant* with respect to the latter.

This, however, is wrong: 'descriptive categories' as terms for language-specific categories can formally differ in a non-trivial way from 'comparative concepts' as terms and may still be *obtained* from such concepts, as I am going to demonstrate (Sections 6.5, 6.6, 9.4) – a single theory of language presupposed in both comparative studies and grammars of individual languages may supply 'comparative concepts' (terms) to the former that also underlie 'descriptive categories' (terms) in the latter, in a logical sense that is more precise than the unexplained notion of 'matching' suggested by Haspelmath in his summary. In relation to 'typology and language-particular analysis', Haspelmath (2010a: 681) claims that: "Both need each other's research results, but both work with theoretical tools that the other can largely disregard." Such disregard turns out to be an unfortunate error – an error that Haspelmath is unwilling or unable to correct, as demonstrated by his reaction to Lieb (2017) (see below, Section 9.4).

So far, we have been dealing mainly with informal grammars, to which formal and semi-formal ones are related by the adequacy condition laid down in (5), above. This condition gives rise to serious problems that concern 'foundational, methodological and architectural issues in grammar and linguistics' (formulated, in 2016, as one of the 'themes of interest' for the Twenty-first Conference on Formal Grammar, see [FG-2016] – obviously a topic of continuing interest for formal grammar writing even after twenty conferences).

5. Formal grammars: Problems for grammar adequacy

5.1 Types of formal grammars. The requirement of semantic analogues

By an informal distinction, most if not all standard formal grammars – formal grammars as usually discussed – can be classified as either generative-enumerative or as model-theoretic, constraint-based; compare Müller (2016: 489) for the distinction as covering existing frameworks. Concerning Construction Grammar approaches, Müller states (2015: 38):

> The basic notions and key concepts are hardly ever made explicit with the exception of Sign-Based Construction Grammar (Sag 2010, 2012), which is an HPSG variant, Embodied Construction Grammar (Bergen and Chang 2005),

> which uses feature value matrices and is equivalent to HPSG (see Müller 2010a, Chapter 9.6, for a discussion of both theories), and Fluid Construction Grammar (Steels 2011). (Müller 2015: 38)[1]

Including the Fluid Construction Grammar approach, construction grammars that qualify as formal can apparently be assumed to be variants of or equivalent to HPSG grammars, which in turn are model-theoretic, constraint-based and are therefore covered by the informal distinction of standard formal grammar types. (More cautious is the evaluation, especially of Fluid Construction Grammar, in Müller 2017, but still without recognizing a fundamental difference.) There will be no further discussion here of formal Construction Grammars. I leave it also undecided if the distinction between generative-enumerative and model-theoretic, constraint-based grammars covers standard formal grammars of arbitrary type; in this respect, the following discussion is incomplete.

Suppose that the ***adequacy condition (5)*** for formal and semi-formal grammars is adopted (as it should be): they must then allow for semantic analogues of the grammatical statements in informal grammars. Satisfying this condition obviously requires that suitable terms are available in a formal or semi-formal grammar, terms that can function in the same way as terms of the various types distinguished in Section 4.5 for informal grammars. I will argue that this ***requirement of suitable terms*** is currently not met in formal grammars; worse, that it cannot be satisfied without a change in the grammar writing outlook. For simplicity's sake, I restrict myself to grammars of languages, but variety and idiolect grammars could be included. I continue to consider Leech and Svartvik (1975) as my informal grammar, using (6) and (7) for examples.

5.2 Problems with language names and domain names

It was claimed above that the most basic sentence of an informal grammar of a language may well be a sentence stating that we are indeed dealing with a language (a natural human language), a sentence like:

(7) English is a language.

In such a sentence, a language name ('English') is combined with the domain name ('language'). No such sentence is possible in a formal or semi-formal grammar of either of the two major types for a simple reason: such grammars do not contain

1. (Müller 2010a = Müller 2010, references taken over from Müller; for details, see Müller 2016: Chapter 10).

a *language name* nor a *domain name* among their terms (for a qualification, see Section 6.2, below).

Suppose that G is a generative-enumerative grammar informally intended for English. Obviously, there is no term of the grammar that corresponds to 'English' as traditionally understood in linguistics, as a natural human language, and there is no term that would correspond to 'language' as used for natural human languages. It is only by a statement *on* the grammar, not *of* the grammar (supposing that the grammar *does* contain statements) that we could relate G to an entity justifiably called English as a natural human language. Indeed, such entities were pointedly rejected as early as in Chomsky (1982):

> And of course, as every linguist knows, the common sense notion of language is hopeless. Nobody even tries to make any sense out of that. So the question is, is there any sense of 'language' that is worth saving? It is far from obvious that there is. (Chomsky 1982: 107)

This has recently been echoed by Pullum in his discussion of model-theoretic, constraint-based grammars:

> It seems to me that the notion of 'a language' should not be regarded as scientifically reconstructable at all. [...] Human languages are no more scientifically definable than human cultures, ethnic groups or cities. [...] in scientific terms there is no such object as 'Japanese'. Pullum (2013: 504)

Informal use of 'English', 'Japanese' or '(a) language' is not excluded, but such terms are to be outside grammars: since grammars should not deal with objects that are not 'scientifically definable', we do not require terms in a grammar to refer to them.

In summary, given a formal or semi-formal grammar written in a currently adopted format, there are no statements in the grammar that are semantic analogues of basic statements of informal grammars such as (7), if only for lack of a language name and a domain name among the terms of the formal or semi-formal grammar. Lack of *language names* and *domain names* as terms of standard formal or semi-formal grammars is sufficient for the adequacy condition (5) to be violated by such grammars.

Problems that are partly due to the lack of a language name and a domain name also appear when we turn to the *grammatical terms* of a formal or semi-formal grammar.

5.3 Problems with grammatical terms

Suppose that G, a formal or semi-formal grammar informally intended as a grammar of English as a natural human language, does not contain a term for referring to English but still contains the grammatical term 'article'. This term can be understood as a predicate in a formal 'description language' in which the grammar is written. Since there is no term for English in the grammar, 'article' alone must be sufficient to refer to just the articles of English, i.e., must be construed as a *one-place* predicate that can be uniquely related by the grammar's interpretation to a suitable property or set of 'words' that are, as a matter of fact, the articles of English; similarly, terms like 'subject' for 'grammatical relations' would have to be uniquely related to English.

This approach – grammatical terms as, typically, one-place predicates of a grammar that is related to an intended language by informal, grammar-external statements – is exactly the approach followed in Clément et al. (2015: esp. 93, 95), exemplifying a typical feature of model-theoretic, constraint-based grammars intended for individual languages. ('Semantic' attributes "can be represented with two-place predicates" (Lichte & Petitjean 2015: 204), but this is hardly relevant in the present context, and such predicates do not provide a place for languages anyway. For further discussion, see Sections 6.2 and 6.3, below.)

On such a conception, the grammatical terms of a formal or semi-formal grammar differ from the grammatical terms of an informal one in an important respect: the latter are, and the former are not, language overarching. For this reason, there can be no semantic analogue in a formal or semi-formal grammar to many grammatical statements of a corresponding informal one, e.g., no analogue in our hypothetical grammar G to the statement from the informal grammar of Leech & Svartvik (1975):

(6) Unlike many other languages, English requires an article with singular count nouns as complements […].

'Article' in G would be a one-place predicate meant to refer to a certain property or set of English words, and of English words only, in contrast to 'article' in (6) which applies in a single sense to words in many or all languages.

The adequacy condition (5) for formal and semi-formal grammars is violated again, due to a lack of language-overarching grammatical terms.

There is a serious *follow-up problem*: comparative research on different languages becomes difficult (see Section 4.6, above). For example, the term 'article' would refer to different properties, or sets, in a grammar intended for English and a grammar intended for German; using the same term in both cases would have no semantic justification. We could still compare the two grammars rather than directly compare the two languages. However, the relevance of the comparison would entirely depend on grammar-external statements relating the grammars to English and German as 'natural languages' in a sense typically left unspecified.

The problem for comparative work has come to the forefront not only in comparative linguistics (also, in cross-linguistic work in Construction Grammar as represented by Hilpert and Östman 2016, compare, in particular, Wasserscheidt 2016), but also in grammar engineering. Duchier and Parmentier (2015: 10) mention "multilingual and cross-framework grammar engineering" as one of the current challenges in this area, referring, among others, to the 'MetaGrammar' approach of Crabbé et al. (2013). The problem has been implicit in large comparative-grammar enterprises such as Müller's CoreGram project, where it has been addressed only recently (Müller 2015: 44, 2016: Chapter 22). (My criticism of such projects, concerning certain basic points of theory, is not to detract from their general value; it might even lead to improvements.)

Generally, what are the *solutions* that could be considered for the problems discussed in Sections 5.2 and 5.3: the lack of language names and domain names and of language-overarching grammatical terms in formal and semi-formal grammars, causing a violation of a basic adequacy condition for such grammars?

6. How to solve the problems

6.1 First Solution: The Irrelevance Conception

A first, radical solution consists in denying that natural human languages in a traditional sense have a place in linguistics as a scientific discipline. This would allow us to eliminate domain names ('language') and language names ('English') from formal and semi-formal grammars as scientific theories; they might remain as expressions that may be used informally. Naturally, grammatical terms in such grammars need no longer be language overarching if there are no languages of which grammars could be theories. This solution is suggested by Chomsky, and proposed by Pullum, in the passages quoted above (Section 5.2); it may be called *the Irrelevance Conception of Domain Names and Language Names*.

The solution might appear justified if domain names and language names could not be made precise. However, Lieb was Chomsky's nobody, and Chomsky's judgment ('hopeless') as well as Pullum's decree ('should not be regarded as scientifically reconstructable at all') are unwarranted for *domain names* like '(a) language' in view of the work done, already decades ago, in Lieb (1969), (1970: Chapter 17), and subsequently in Lieb (1983: Part A) and (1993: Parts II and III). Given the explication of '(a) language' proposed by Lieb, *specific language names* could in principle be defined by individual or set-theoretic descriptions, applying the criteria for languages in general to the specific case. *However, a language name may also be left as a primitive in a grammar that contains 'determination sentences' for the language*, the position taken below, Sections 10.3 and 10.4.

In summary, Chomsky's and Pullum's solution should be rejected as unwarranted and contrary to linguistic practice. If human languages are not 'scientifically definable', therefore, do not require technical terms – which may well be words of a natural language – for naming them (Pullum), then endeavours such as Ethnologue (Lewis, Simons & Fennig (eds.) 2016) or, more recently, Glottolog (Hammarström, Forkel, Haspelmath & Bank 2016), intended to provide a catalogue of the world's languages, language families and dialects, are meaningless; obviously, they are not – whatever the details. (For the Glottolog principles used to determine English names for arbitrary languages, especially lesser known ones, see Haspelmath (2017b)).

6.2 Second Solution: The Language-Feature Conception

A second solution is considered by Müller (2015: 44) – as a thought experiment that he would not subscribe to himself (personal communication). However, the relevant passages reappear, virtually identical, in Müller (2016: 661–662):

> Instead of using the category *Persian Noun* one could assign objects of the respective class to the class *noun* and add a feature LANGUAGE with the value *persian*. […] Of course, no theoretical linguist would introduce the LANGUAGE feature to differentiate between Persian and English nouns, but nouns in the respective languages have other features that make them differ. So, the part-of-speech classification as noun is a generalization over nouns in various languages and the categories *Persian Noun* and *English Noun* are feature bundles that contain further, language-specific information. Müller (2015: 44, fn. 2)

This may be called *the Language Feature Conception of Domain Names, Language Names, and Grammatical Terms*. Not taking this proposal seriously simply means that the problems we are discussing and to which Müller is reacting in the version brought up by Haspelmath and others (see above, Section 4.6) are left by him without a solution.

Indeed, the proposed solution will hardly work, but it is worth considering the reasons; some basic questions are involved.

Taking the quoted passage literally, we must distinguish between a category as a 'bundle' (?) of features (feature values?) and 'respective classes'. Let us de-italicize expressions to obtain names for the 'respective classes'. The quote then implies *an interpretation of an HPSG grammar that is intended for Persian* – similarly, for English – such that:

$$\text{Persian Noun} = \{x \mid x \in \text{Noun} \,\&\, \textsc{language}(x) = \textit{persian}\}.$$

Here, the following is meant to hold:

i. x is an expression of some language (any language allowed?).
ii. Noun is a set of expressions x of arbitrary (?) languages that is 'a generalization over nouns in various languages' (meaning what?).

iii. LANGUAGE is a function whose arguments are (any?) expressions x and whose values are the natural human languages ('natural human language' is justified by the context of the quote); ***alternatively***:
iv. LANGUAGE is a function whose arguments are (any?) expressions x and whose values are the properties that each consist in being an x related to a *specific* natural human language.
v. *persian* = the Persian language; ***alternatively***:
vi. *persian* = the property of being related to the Persian language.

Obtaining the required interpretation of the grammar is quite problematic, though; the "simple trick" (Müller 2015: 44, fn. 2) of introducing a LANGUAGE feature to overcome the restriction of HPSG grammars to single languages runs afoul of serious problems. Even leaving aside points that are not clear in (i) to (vi) – are (iii) and (v) intended, or rather (iv) and (vi)? – the following problems remain.

Problem 1. The term 'LANGUAGE' denotes a function; for this reason alone, it is not a domain name: is not an explicatum of 'natural human language' in an informal sense; such names must be predicates, preferably one-place.

Problem 2. There are no formal interpretations of HPSG grammars known to me that would allow for a grammar with an interpretation such that 'Persian Noun' can be understood as above and assuming (i) to (vi); such interpretations have no access to natural human languages as independently given.

If these problems cannot be solved, the Language-Feature Conception cannot be adopted.

6.3 Rejecting the Language-Feature Conception

Problem 1 may appear to raise a minor formal point. However, trying to get around it, if possible at all, requires changes in current HPSG frameworks, as appears from proposals (not to be discussed here) that were made to me by Frank Richter (personal communication). In any case Problem 2 remains.

The most careful attempt to interpret HPSG grammars is Part I of Richter (2004). Richter, obviously recognizing the problem, suggests that (2004: 49): "A natural human language can be construed as an interpretation I of the signature Σ of a grammar ⟨Σ, Θ⟩ such that I is a model of ⟨Σ, Θ⟩." However, an interpretation I is a quadruple that indirectly involves symbols from the alphabet of the grammar, which renders the natural human language dependent on the linguist's grammar; this disqualifies the proposal as an explication of 'natural human language': such languages, whatever they are, exist independently of anybody's description. Richter (personal communication) points out that a language for which an HPSG grammar is intended should be identified with a so-called 'exhaustive model'; this might be modified so that symbols from the grammar's alphabet would no longer figure in the natural human language. The suggested modification may or may not work; this can be judged only after it has been formally elaborated.

There is a more basic objection: it is questionable to begin with to try and identify natural languages with models of grammars, for two reasons:

i. For any given model-theoretic grammar that is intended for a specific natural human language, there will always be an indefinite number of models. It is hard if not impossible to identify a specific model as being the intended language.
ii. Even disregarding (i), the models are *models of grammars*. If natural human languages must be characterized as models of grammars ('exhaustive' or not), then a general characterization of natural human languages still depends on a general characterization of grammars; in this sense, the linguist's grammars rather than the languages are primary, a dubious consequence.

Because of (i) (recognized as a problem in Richter 2007), a language name like 'Persian' construed as the name of a model of a grammar has no definite reference, and the grammatical term 'Noun' in a specific grammar cannot be understood as denoting 'a generalization over nouns in various languages', due to the fact that languages cannot be uniquely specified.

But suppose that modifying 'exhaustive models' works, and we somehow get around (i). This still is no help in confronting (ii).

Generally, an HPSG framework does not provide for natural human languages as objects that have existence and can be studied independently of grammars. Müller blurs the problem, especially in Müller (2016: Chapter 22), a late addition to earlier versions of the book: in talking there about grammars and languages, he falls back, for all practical purposes, on the position taken by Pullum (above, Section 4.2) – that is, adopts the First Solution, the Irrelevance Conception of Domain Names and Language Names – while informally but systematically making use of traditional classifications of natural human languages into language families and transferring grammar notions, in particular the notion of grammatical constraint, from grammars to languages, without further justification.

What Müller is comparing in his CoreGram project (and what is compared in other similar projects) is grammars of the linguist, not natural languages in a grammar-independent sense. The grammars are checked, in particular, for shared constraints or principles. True, having grammars that share a certain feature gives rise to the class of languages for which there are such grammars – provided we have languages to begin with!

What is missing here – as in other formal-grammar frameworks but also in comparative linguistics if construed along the lines of Haspelmath (2010a) – is a general theory of language: of natural human languages that are independent of grammars; a theory of language that supplies, among other things, constants that are the basis for grammatical terms in the grammars of different languages and can be used in each grammar in the same sense.

The only author known to me (other than myself) who has been explicitly emphasizing the importance of theories of language in this context is Lehmann (forthc.: Section 2.4, on 'interlingual grammatical categories'):

> A theory of language necessarily comprises the variation intrinsic and essential to its object, viz. languages; and the description of one of these is the more scientific the more it is based on such a theory. A description of some language which only uses its own concepts would be a contribution to no legitimate scientific activity.

However, in discussions relating to the position taken, once again, in Lieb (2017), a number of workshop participants (Kasper Boye, Zygmunt Frajzyngier, Paolo Ramat) emphasized the importance of theories of language to typological work in a very similar way.

Given a theory of language, there are still two different possibilities of dealing with grammatical terms, leading to our Third and Fourth Solutions, which are restricted to the problem of grammatical terms.

6.4 Third Solution: Cover-all Conceptions of Grammatical Terms

It may now be proposed to solve the problem of language-overarching grammatical terms in a grammar as follows.

Terms like 'article' are considered to be constants of a theory of language that may be presupposed in grammars. The constants are treated as non-relational, one-place predicates, each interpreted in the theory of language to denote the set of all linguistic entities of a certain kind – such as the set of all articles – in *arbitrary languages*, in *arbitrary language systems*, or in *arbitrary idiolect systems*. The terms – one-place predicates – are then used in their general sense as grammatical terms in any grammar that presupposes the theory of language; this makes the terms semantically comparable wherever they appear. Terms like 'subject' for 'grammatical relations' are also taken over from a theory of language, where they occur with a general sense.

On this construal of grammatical terms, we obtain three ***Cover-all Conceptions of Grammatical Terms***, the ***Cover-all D-Conception***, the ***Cover-all-σ-Conception***, and the ***Cover-all S-Conception***, for languages D, language systems σ, and idiolect systems S, respectively. In each case, a grammatical term of a grammar taken over from the presupposed theory of language retains its general interpretation, denoting the set of all relevant linguistic entities that belong to arbitrary languages D / language systems σ / idiolect systems S.

The most explicit example for such a conception appears to be Lehmann (*forthc.*), where categories are construed as 'classes of linguistic signs' and categories at a lower level of abstraction are taken to be related to corresponding categories at a higher level by a relation that is akin to hyponymy.

Cover-all Conceptions of Grammatical Terms should still be rejected, though, even if technically feasible. They create a problem that may be exemplified by the term 'article' construed as a one-place predicate. (The problem provides a reason for rejection that is still different from the reasons informally given in Haspelmath 2010a – see above, Section 4.6 – against a position that amounts to a Cover-all-D-Conception or Cover-all-σ-Conception in our sense.)

In language comparison, we wish to speak of properties that articles have in *some* but not *all* languages. If only the one-place term 'article' with its general interpretation were available for use in a grammar, we would have to introduce a separate concept of *belonging* for relating articles to languages or language varieties D / systems σ for entities D / idiolect systems S: 'X is an article & X belongs to D / belongs to σ / belongs to S'. (In fact, assuming that a theory of language is presupposed in an HPSG grammar, the Language-Feature Conception may be understood as an attempt to interpret 'belongs'.)

Suppose that we have found an interpretation for 'belongs', such as 'is a lexical word of'. There is still a major problem with the variable 'D' in any proper definition of 'X is an article' in its general sense: 'D' ought to be free in the definiens but would not be so in the definiendum, which is a serious definitional mistake. (Taking 'X belongs to D' as the antecedent in a *conditional* definition would not help.)

Therefore, D must be free in the definiendum, too, that is, we arrive at two-place relational definienda: 'X is an article *of* D / *of* σ / *of* S' – the term 'article' is *relativized* to languages / language systems / idiolect systems, it is two-place.

The example throws into doubt the very possibility in the present context of constants that are one-place predicate constants and are defined – but definability should not be excluded.

6.5 Fourth Solution: Relativizing Conceptions of Grammatical Terms

We now assume a theory of language that contains terms like 'article' and 'subject' in a relativized form, representing the major types of terms to be considered. (Closed grammatical terms other than constants could be allowed but will be disregarded.)

'article' relativized
The term 'article' – exemplifying grammatical terms of a first type – is understood as 'article of', denoting a two-place relation; the relata are 'words' on the one hand and are, on the other, either arbitrary languages D, or arbitrary language systems σ, or arbitrary idiolect systems S: the term is *relativized* to entities D, entities σ, or entities S.

Technically, there is a second way of achieving a relativization to arbitrary D / σ / S: a constant like 'article' is construed as a one-place functor, naming a function that assigns to any D / σ / S a certain set of 'words' of D / σ / S. The two solutions are, in a sense, equivalent. However, the predicate solution appears to be closer to the informal use of terms like 'article' in linguistics (use of 'is an article of' rather than 'article in') and will therefore be the only one to be pursued in the present essay.

Suppose that 'article of' is relativized to *idiolect systems S*. (The following discussion carries over directly to the case when terms are relativized not to idiolect systems S but to languages D or language systems σ.) The term may then be used, already in the theory of language, to form a logically complex term such as 'article of S', formally 'article(-, S)', where S is any idiolect system in any language. Technically, the term 'article of S' or 'article(-, S)' is an open predicate expression that denotes the set of linguistic entities – 'words' – which stand in the relation article-of to S (compare Carnap 1958: § 33d for the hyphen notation; expressions with hyphens are equivalent to lambda-expressions whose operand is a sentence formula). The set of 'words' denoted by 'article(-, S)' is a certain *grammatical category of* S. The term 'article of' is an example of a *grammatical constant* of the theory of language, and 'article of S' or 'article(-, S)', obtained from this constant by a purely logical step and containing a free variable, is an *open grammatical term* of the theory of language. If this theory is axiomatic, both the constant and the open term are axiomatic terms of the theory.

Now assume that the theory of language is presupposed in the grammar of a specific idiolect system, say, of an ***English*** idiolect system S_E. Both the grammatical constants and the open grammatical terms of the theory of language are then available in the grammar as ***grammatical constants*** and ***open grammatical terms of the grammar***. In addition, substituting the system name 'S_E' for the variable in an open grammatical term like 'article of S', we obtain a ***closed grammatical term*** of the grammar, such as 'article of S_E' or 'article(-, S_E)'; this is a ***complex name of a category of*** S_E, viz. of the set of all articles of S_E. If the grammar is an axiomatic theory, the closed term is an axiomatic term of the grammar; the constant and the open term are not, but are still available in the basic language of the grammar.

Next, consider a ***German*** system S_G and the closed grammatical term 'article(-, S_G)' of a grammar of S_G that presupposes the same theory of language. This term is a name of a category, too: of the set of all articles of $\mathbf{S}_G$. The two categories are, as a matter of fact, completely different. However, the grammatical constant 'article' ('article of') is the same in both names; since 'article' is a grammatical constant of the theory of language that is presupposed in both grammars, 'article' is used in each category name in exactly the same sense: the grammatical constant 'article' is ***idiolect-system overarching***, in contrast to the two closed grammatical terms formed from 'article' – the category names 'article of $\mathbf{S}_E$' and 'article of $\mathbf{S}_G$'. The grammatical constant 'article' of the theory of language and the open grammatical terms formed from it, such as 'article of S', can be used in comparative statements as well as in individual idiolect grammars.

'subject' relativized

Terms like 'subject' must be dealt with in a way different from 'article', but their treatment – exemplified by the construal of 'copular complement' in later Examples (Section 8.1, (11) and (12d)) – may still have the same generalizing effect.

Consider 'subject', exemplifying grammatical terms of a second type. This may be treated as the name of a function that takes arbitrary D / σ / S and assigns to each another function, 'the subject function in D / σ / S', or subj(D) / subj(σ) / subj(S). This is a function (possibly empty) that takes arbitrary sentences of D / σ / S (the sentences need not have a standard form) as arguments and assigns to each sentence a certain two-place relation (possibly empty) between constituents of the sentence (this relation is 'the subject/predicate relation' in the given sentence).

Technically, we obtain *open* functor expressions like 'subj(S)' and *open* predicate expressions like 'subj(S)(f, s, e)', where ⟨f, s, e⟩ is a sentence of S, including a syntactic structure s and lexical interpretation e; and we also obtain corresponding *closed* expressions by substituting constants for the variables, such as (on a Relativizing D-Conception): 'subj(English)', read as 'subject in English'; or such as 'subj(English) (*Smoking is dangerous*.)', read as 'subject in English of *Smoking is dangerous*.' and denoting the set {⟨*smoking*, *is*⟩}, a two-place relation, informally: the 'subject/predicate relation' in English of the sentence *Smoking is dangerous*. (The English sentence – a triple ⟨f, s, e⟩ – has been named orthographically.)

A treatment of this kind can be considered whenever we are confronted with grammatical terms that should be construed as function terms. In Lieb (2013), word-formation notions like 'compounding' are dealt with in this way. The approach applies throughout grammar, including phonology.

In summary, it is ***Relativizing Conceptions of Grammatical Terms*** that have been characterized here, using 'article' and 'subject' as sample terms and allowing for relativization to (i) languages and language varieties, (ii) language or variety systems, and (iii) idiolect systems. I will speak of ***the Relativizing D-Conception*** in case (i); ***the Relativizing σ-Conception*** in case (ii); and of ***the Relativizing***

S-Conception in case (iii). Each Conception is compatible with the use of 'article' in an informal statement such as (6).

A Relativizing Conception of Grammatical Terms might also work for model-theoretic, constraint-based grammars if the framework for them is modified so as to include a general theory of language to which grammars have access. I leave this for others to decide.

6.6 Overcoming the opposition of 'comparative concept' vs. 'descriptive category'

The Relativizing Conceptions throw new light on Haspelmath's opposition of 'comparative concepts' vs. 'descriptive categories' (Haspelmath 2010a; see above, Section 4.6). The following picture evolves from Section 6.5.

As argued in Section 4.6, 'comparative concepts' as characterized by Haspelmath are easily reconstructed as terms of a theory of language, more specifically, as grammatical constants (or other closed grammatical terms) of such a theory where they are conceived on a Relativizing D-Conception or σ-Conception, i.e., relativization to languages or to their systems should be assumed. Presupposing a theory of language of this kind, its constants just as its open grammatical terms may be used in comparative studies, which is implicitly Greenberg's approach in formulating his language universals (Greenberg 1963; compare Section 4, 'Logical Structure of Universals', esp. Section 4.2 'Universal implications', in Greenberg, Osgood & Jenkins 1963; similarly emphasized in Lehmann *forthc.*: Section 2.4). Actually, Haspelmath's own proposals for universals may be understood on a Greenbergian pattern, as in Haspelmath (2015: 294): "*Universal 1*: In all languages, plenimorphs are larger than minimorphs." Haspelmath is right in referring back to Greenberg and the Greenberg tradition (e.g., 2010a: 664), which never lost its importance in comparative linguistics. (A more advanced conception of universals was subsequently proposed, though, in Lieb 1975.)

Strictly speaking, it is not only a theory of language that may figure in this context but also an ***extension*** of such a theory, in the sense explained below (Section 11.4); in particular, the extension may be a ***typological theory***.

Grammatical terms such as 'article(-, D)' or 'article of D' (assuming a D-Conception is adopted, analogously for σ- or S-conceptions) are not constants but open predicate expressions of the theory of language that each denote a *category* of D, such as the set of all articles of D; similarly, for open functor expressions like 'subj(D)': this is a term denoting the subject function in D, which assigns to each sentence of D the set of subject/predicate pairs of the sentence. (In the Haspelmath / Newmeyer debate, terms like 'subject' appear to be treated on a par with terms like 'article', a dubious move in the light of Section 6.5, above.)

The open predicate or functor expressions may be used in making general statements on grammatical categories or functions, that is, in the formulation of language universals, and may be used in language comparison, too.

However, the same theory of language extended or presupposed in comparative studies may also be presupposed in the grammars of individual languages. The open grammatical terms of the theory of language or its extension may then be used in the grammars to obtain closed grammatical terms of the grammars by making use of the names of the languages, such as 'article(-, English)' or 'article of English' or 'subj(English)' (again presupposing a D-Conception). In a given grammar, the closed grammatical terms so obtained are complex names of individual grammatical categories, or individual functions, of the language that is an object of the grammar, such as the set of all articles of English or the subject function in English; that is, the terms are exactly Haspelmath's 'descriptive categories' understood as *terms*.

The possibility of relating 'descriptive categories' to 'comparative concepts' in this way is not recognized in Haspelmath (2010a), which is due to a lack of logical explicitness (Haspelmath is even less explicit than Greenberg, Osgood & Jenkins (1963: Section 4) were in their characterization of the concept of universal), and is also due to an obvious distrust of theories of language. As a consequence, comparative and typological work is opposed to descriptive work in a partly untenable way, even admitting "that the distinction between comparative concepts and descriptive categories helps language describers and typologists to benefit from each other" (Haspelmath 2016b: 299) – the distinction is drawn in a wrong way.

The discussion among typologists that originated from Haspelmath's paper ([Lingtyp] 2016a and 2016b, [Plank ed.] 2016), continued in Workshop 11 of the *2017 Congress of the Societas Linguistica Europaea*, shares with it the lack of formal explicitness and, with some exceptions, the same distrust of theories of language. Suppose that such theories had been considered. Only the Third Solution seems to be compatible then with what is tacitly presupposed or assumed by most discussants in their discussion; it is, however, the Fourth Solution that is to be preferred for solving the problems. In this respect, the discussion appears to be somewhat off the mark.

The Fourth Solution seems preferable also for interpreting glossing labels, along the lines of Lieb and Drude (2000), to which Volker Gast draws attention in [Lingtyp] (2016b). Following Haspelmath (2016b: 301), a member of the group to whom the Leipzig Glossing Rules are due, "Interlinear glosses are not abbreviations of deep analyses, but reading aids to the reader." Even so, the interpretation problem remains: glosses that cannot be interpreted as an abbreviation of some analysis, however superficial, are hardly useful even as reading aids.

It should be noted that both the Third and the Fourth Solutions can be generalized to 'linguistic complexes' of arbitrary size, be they language varieties, languages, or larger complexes (even non-linguistic complexes may be included): we simply allow the variables 'D' and 'σ' to range over arbitrary 'communication complexes' and their systems in the sense of Lieb (1968), (1993). This would allow us to give a more precise meaning to the term 'languoid' as discussed in Gil (2016) or as used in the Glottolog enterprise (Hammarström, Forkel, Haspelmath & Bank 2016), where the term is to apply to language families, languages, and dialects: languages and their varieties can be construed as communication complexes of a specific kind, and language families as sets of such complexes.

6.7 Conclusion

Apparently, only a Relativizing Conception of grammatical terms will allow us to properly relate theories of language in General Linguistics, comparative theories in Typology, and grammars in Descriptive Linguistics. More specifically, it is the ***Relativizing S-Conception of Grammatical Terms*** that should be chosen, to account for language variability right from the start. I will adopt this conception with a qualification: it will be used not only in idiolect grammars but also in grammars of languages and language varieties, understood as sets of idiolects. In a grammar, there will be quantification over the systems of some or all idiolects in the language or variety, using open grammatical terms of the presupposed theory of language, rather than formulation of closed grammatical terms – introduced derivatively – to name categories of the language or variety or of its system.

I am in a position now to characterize my framework for formal and semi-formal grammars, which is different from existing ones: I am going to propose for such grammars the format of axiomatic theories of a certain type. This conception allows us to achieve a better understanding of informal grammars, and to avoid the problems that have been discussed.

C. Grammars and theories of language: Motivating axiomatization

7. Advantages of an axiomatic grammar format

7.1 Introduction

We are considering grammars in the sense of grammars as texts. As a special case, we may have a grammar construed as an axiomatic theory. A conception of linguistic grammars as axiomatic theories is proposed, made explicit and evaluated in my own work mainly in two places, in Part G of Lieb (1983) and Chs 20 and

21 of Lieb (1993); these are further developments of the original conception in Lieb (1974), (1976) (which still contains a mistake in the underlying logic, pointed out in Falkenberg 1996). An axiomatic format is also advocated in Neef (2014) for grammars that are written in a realist framework. Lieb's conception of axiomatic grammars includes a conception of theories of language as axiomatic, too (informally assumed now for theories of language also in Lehmann *forthc.*: Section 2.2.1); for this reason, a theory of language may be treated as part of the language in which a grammar is written.

It may be suggested that constraint-based grammars can be conceived, or reconceived, as axiomatic theories. Constraints are, or can be formulated as, sentences of a formal language; but this is hardly sufficient for assigning an axiomatic status to a grammar. Again, I leave this question for others to decide.

My own conception of axiomatic grammars, to be partially outlined in Part D of the present essay, is a further development and application to linguistics of axiomatic theories of a conservative type, essentially characterized as early as in Carnap (1958: Section 42). Carnap provides good examples for axiomatic theories of this kind, drawn from a number of different fields (Carnap 1958: Part 2).

I begin by remarks on this standard conception of axiomatic theories, and then consider the potential role of axiomatic grammars, especially partial ones, for achieving a better understanding of informal grammars. The potential and the limits of axiomatization in grammar writing are thus evaluated before the proposals for axiomatic grammars are made in Part D.

7.2 Important features of axiomatic theories

The following features of axiomatic theories of the standard type are of special importance in the present context:

i. A systematic distinction is made between definitions, axioms, and theorems in the narrow sense (excluding axioms).
ii. Variables are distinguished from constants.
iii. Axioms and theorems are sentences, i.e. closed formulas (not containing free variables) that can be assigned a truth-value, in some sense; definitions may be construed as sentences, or else as open sentential formulas.
iv. A set of axiomatic constants, defined or undefined, can be identified.
v. In a non-logical theory, the axiomatic constants are non-logical, descriptive.
vi. The theory is formulated in a formal language that either is an interpreted language of logic, or else is based on such a language.
vii. In the theory's language, or axiomatic language, a basic part (the basic language) can be distinguished: the basic part does not contain expressions that contain axiomatic constants of the theory.

viii. In an empirical theory, there are axioms that may be called *application axioms*, or such axioms may be added; these are axioms that directly or indirectly relate certain axiomatic constants of the theory, 'theoretic terms', to other axiomatic constants of the theory that are 'observational terms', i.e., are fully interpreted to refer to entities considered to be directly accessible to experience.

7.3 Discussion and explanations

To this day, there are problems especially with Point (vi), the nature of the theory's language, and Point (viii), the relation to experience given an empirical theory.

A condition such as (vi) is essential. It allows us to construe grammars as theories in a strictly 'declarative' sense (above, Section 4.1) and renders inference and deduction possible in a grammar. This again allows us, among other things, to use general concepts of *explanation* in connection with grammars and theories of language – concepts developed in the philosophy of science – either employing them in a traditional standard form, as in Nolda (2018) in relation to word formation, or modifying them, in view of known problems, to arrive at concepts of explanation that apply in linguistics generally, as in Lieb (1978).

The availability of inference and deduction does not imply that we are dealing with 'proof-theoretic' grammars in a sense where such a grammar is a means for enumerating 'sentences' by means of inference or deduction. For an example of an informal proof in a hypothetical grammar of the type that will eventually be assumed, see Section 10.6, below.

One problem with (vi) in linguistics is choice of a formal language that is adequate for dealing with the semantics of natural languages. The problem is most serious on an approach by which the semantics of a natural language is to be specified *indirectly*: specified through translations into a language of logic that has a sufficiently strong model-theoretic semantics. The problem arises in a weaker form when the semantics is to be *described directly*, using the language in which the axiomatic theory is written.

In the case of an empirical theory, the language of the axiomatic theory may or may not have a model-theoretic semantics. It is rarely appreciated, though, that having such a semantics is by itself insufficient for relating the theory to reality; application axioms are still needed. An axiomatic theory that has application axioms may be called ***applied***; a theory that does not have them but can be expanded to include them will be called ***potentially applied*** (also compare Lieb 1983: Chapter 30; Point (viii) goes beyond Carnap 1958).

Suppose that we restrict ourselves to the non-semantic aspects of natural languages. Even then choice of a language in which a grammar is to be written may be problematic. However, judging by the discussion of 'description languages' for model-theoretic grammars, demands on grammar languages in the context of (vi) may be less severe if the semantics of natural languages is excluded: "Nothing greater than the power of context-free grammars seems to be needed for English" (Pullum 2013: 499, where it is also claimed that a corresponding description language can deal with "the description of non-context free string sets" by simply generalizing the class of models). Again, Clément et al. (2015: 95):

> it seems that linguistic constraints do not need extra logical power. The most expressive and concise logic that is known in this class [of logical languages that only define regular tree languages, H.L.] is Monadic Second-Order Logic (MSOL), but *various kinds of first-order or modal logics may suit very well the needs of linguistics*. [Emphasis: H.L.]

The formulation in (iii) of Section 7.2, 'can be assigned a truth-value, *in some sense*", points to a problem: just as concepts of truth cannot be naively applied in relation to model-theoretic grammars (see Richter 2007), caution is needed when they are applied in the context of axiomatic theories. This is due to interpretation problems raised by 'theoretic terms' (see (viii)). A radical solution to these problems is adopted in Lieb (1974); it consists in interpreting theoretic terms incompletely, fixing only the *type* of entity they refer to. A concept of truth may then be defined that applies to sentences containing theoretic terms but is relativized to axiomatic theories (details in Lieb 1974: Section 3.3). This is not the place for getting more deeply into one of the most debated topics in philosophy. For an informal overview of conceptions of 'the truth of linguistic propositions', see Bobrowski (2015: 183–200). – Despite these problems, axiomatic grammars are an ideal means for answering questions raised by informal ones.

7.4 Clarifying informal grammars by partial axiomatic reformulation

Very likely, Points (i) to (viii) listed above have never been satisfied by any grammar written for normal descriptive or educational purposes; such grammars are informal. This means that questions concerning the systematic order of the grammar as opposed to the order of presentation cannot as a rule be answered, questions such as:

(8) a. What are the basic assumptions made in the grammar, and what are simply their consequences?
b. Is a certain sentence of the grammar meant as a definition of a term or as a factual statement on the language described?
c. What is the level of abstraction assumed by the grammar?
d. How are we to understand the grammatical terms used in the grammar?

Answers to such questions are needed for a deeper understanding of an informal grammar – even admitting that a deeper understanding may not always be needed.

Suppose then that we reformulate relevant parts of an informal grammar as an axiomatic theory. This forces us to pay attention to Points (i) to (viii). Many of the systematic questions raised but not answered by the informal grammar can now be answered for the axiomatic theory. To the extent that the theory may count as a reformulation of the relevant parts of the grammar, the answers obtained for the axiomatic theory may count as answers to the questions raised by the informal grammar. Since the axiomatic reformulation satisfies (i) to (viii) in Section 7.2, we will be able to recognize, or partly recognize, in the informal grammar:

(9) a. The sentences that should be taken as definitions (including recursive definitions), or as formulating basic assumptions, or as formulating consequences of definitions and basic assumptions. (Points (i), (iii) and (vi))
b. The ontological status that may be assumed for the linguistic objects described by the grammar. (Points (ii), (iv), (v), and (vi))
c. The definitional status that may be assigned to the terms in the grammar (defined vs. undefined), their logical status (logical vs. non-logical, in particular, axiomatic), and their interpretation. (Points (i) to (viii))
d. The relationship of the grammar to potential data. (Point (viii))

It is this consequence that makes axiomatic grammars an ideal reference point for informal ones, as I will show now by continuing Example (6). (The analysis is formal only to the extent that this is required for the argumentation; details are preliminary.)

8. Grammatical statements: Informal and semi-formal

8.1 Example

Sentence (6) is chosen, once again, for exemplification:

(6) Unlike many other languages, English requires an article with singular count nouns as complements […].

The sentence will be considered in a reduced form, without the reference to other languages:

(10) English requires an article with singular count nouns as complements.

This is now interpreted as a claim made for all systems S of idiolects C that are elements of English, which is construed as a set of idiolects. As a first approximation, and adding just a few theoretic distinctions, (10) can be understood as follows (I read 'article with' in (10) as 'article as a part of', a debatable decision; complements will be restricted to 'copular complements': to the *non-subject complements of copular verb occurrences*):

(11) Consider any system S of any English idiolect C. Let the syntactic unit f together with its structure s and lexical meaning assignment e be a sentence of S. Let f_1 be a copular complement in S of some f_2 given f, s, and e. Suppose f_1 is an occurrence in f of a Singular form f_1' of some count noun W_1 of S. Then there is a part f_3 of f_1 that is an occurrence in f of a form f_3' of some article W_2 of S.

Somewhat more formally, and more clearly:

(12) For all C, S, f, s, e, f_1, and f_1', ***if***:
a. C ϵ English,
b. S is a system of C,
c. ⟨f, s, e⟩ is a sentence of S,
d. for some f_2, ⟨f_1, f_2⟩ ϵ cop-comp(S) (f, s, e),
e. f_1 is an occurrence in f of f_1',
f. for some W_1,
 i. W_1 is a count noun of S,
 ii. f_1' is a Singular form of W_1,
 then for some f_3:
g. f_3 is a part of f_1,
h. for some f_3' and $W_{2,}$
 i. f_3 is an occurrence in f of f_3',
 ii. W_2 is an article of S,
 iii. f_3' is a form of W_2.

In (f.ii), 'is a Singular form of' should be understood as: 'is categorized as Singular-form-of-S in', and a condition (f.iii) may have to be added such as: 'the meaning assigned by e to f_1 = the meaning of W_1' (f_1'may be a Singular form shared by the count noun W_1 with a non-count noun W_2, pointed out to me by Monica Budde, personal communication, which requires 'fixing the meaning'). The semantic requirement is implicit at best in statement (6) as formulated by Leech and Svartvik (1975: 207).

Statement (12) is easily reformulated as a sentence – a universal implication – of set theory as formulated in a first-order (or a higher-order) language of predicate logic, a sentence of which (12) may then be considered a reading. In an axiomatic grammar, this sentence would have the status of a theorem. For the sake of simplicity, I am going to stay with the semi-formal sentence (12).

8.2 Variables and constants

First, let us have a look at the ***variables*** in (12). These indicate the *kinds of abstract entities* assumed in formulating (12). Variables with the same letter parts are interpreted to cover entities of a single kind. The entities covered by a given variable are as follows:

(13) a. 'C': entities of the kind of elements of English (i.e., of the kind of English idiolects)
b. 'S': entities of the kind of systems of English idiolects
c. 'f'-variables: entities of the kind of: sentence forms; forms of lexical words; parts of sentence forms or word forms
d. 's': entities of the kind of structures of sentences
e. 'e': entities of the kind of lexical interpretations of sentences
f. 'W'-variables: entities of the kind of lexical words

The ***constants*** that are used in (12) can be classified as follows (constants lacking a corresponding term in the informal version (10) are in italics):

(14) a. ***Language name***
English

b. ***System-related linguistic terms***
system of
sentence of
cop-comp in
count noun of
Singular form of
article of

c. ***Non-linguistic terms***
∊ (element of)
occurrence in … of …
part of
form of

A number of important points appear from the reformulation of (10).

8.3 Evaluation

Variables do not occur in the informal version (10); they have to be newly introduced as in (13). This is typical of statements in informal grammars. For this reason, the ontological status of the linguistic objects referred to in informal grammars remains largely undetermined. If the position of Modified Realism is adopted for clarification, the various entities are abstract but are ultimately constructs based on objects and events in space and time; this suggests that for formulating a theory of language or a grammar, a language should be used that admits a number of specific domains of individuals, such as a many-sorted language of logic (cf. Section 3.1, above). Now what about the constants? Here, the situation is more complex.

'English', the name of a language, appears in both the informal statement (10) and its semi-formal version (12). As to the system-related linguistic terms in (14b), the two most general ones, 'system of' and 'sentence of', are without corresponding terms in the informal version (10). This is typical: we do not have to repeat in an informal grammar each time that we are dealing with systems, or sentences.

All non-linguistic terms in the semi-formal version, listed in (14c) and including 'form of', can be understood as logical ones. They are not represented by any terms in the informal version (10). This again is typical of statements made in informal grammars: logical constants that appear in a formal restatement are rarely represented directly by logical terms in the informal version.

Conversely, 'requires' in the informal sentence (10) is a term that may count as logical but is not represented by a logical constant in the semi-formal restatement (12). 'Requires' is rendered by the logical structure of sentence (12). This is a universal implication that may be reformulated in a usual way as follows: for all C, S, etc., it is *a necessary condition* for (a) to (f) that for some f_3, (g) and (h). Typically, a logical term contained in a sentence from an informal grammar may be represented in this way in a formal restatement: not by a term again, but by complex logical properties of the restatement.

Generally, a vast amount of information needed for understanding the informal sentence (10) is either presupposed by it or left implicit, as demonstrated by our reformulation. This is typical of the grammatical statements made in an informal grammar. At the same time, what is presupposed or left implicit will be made explicit in a reformulation that uses the means available in an axiomatic theory.

In summary, grammars formulated as axiomatic theories of a standard type provide an ideal reference point for informal grammars. The role of theories of language, used implicitly in an informal grammar, appears more clearly, too, in a correlated axiomatic theory. This is true especially of the nature of grammatical terms.

9. Grammatical terms taken over from a theory of language

9.1 A basic assumption

Following the discussion in Section 6, especially in 6.5, grammatical terms of a grammar that can be construed as constants or as open terms get their language-overarching properties by also being grammatical terms of a theory of language (which may well be partial), a theory that is (implicitly or explicitly) presupposed in the grammar. This is now stated explicitly as an assumption of my informal theory of grammars, added to the previous requirements (2) to (5) (Section 4.2, above):

(15) Typically, grammatical terms in a grammar that are, or can be construed as, constants or open terms are taken over into the grammar from a presupposed theory of language, where they occur as constants or open terms of the theory.

Closed grammatical terms that are not constants but contain the language name are language-specific. 'Typically' in (15) allows for grammatical *constants* of a grammar that are language specific, are not taken over from the presupposed theory of language but are newly introduced in the grammar by definitions that restrict them to the object language of the grammar.

Grammatical terms that are taken over from a theory of language may – but need not be – defined, that is, defined in the theory of language. Their *definition* in the theory of language must be carefully distinguished from their *use* in factual statements that are part of the grammar, an important point in the following exemplification.

9.2 Defining terms in a theory of language: Example

Consider the system-related linguistic terms in (14b). We assume a theory of language that is formulated as an axiomatic theory, contains these terms as axiomatic constants and is presupposed in the grammar.

Some of these constants may indeed be defined in the theory of language. In particular, the theory could contain a definition of 'article' saying that *an article is a lexical word with an empty lexical meaning whose forms may be used as the auxiliary parts of noun forms*. More precisely, the theory of language may contain the following definition for the term 'article' as a relational constant ('article of'):

(16) W is an ***article*** of S if and only if:
   a. S is an idiolect system,
   b. W is a lexical word of S,
   c. for some P, W = $\langle P, b^0 \rangle$,
   d. for every f: if f is a form of W, then for some f_1 and f',
      i. f_1 is a noun form of S,
      ii. f' is an occurrence of f in f_1,
      iii. $f' =$ the auxiliary part of f_1 in S.

b^0 in (16c) is 'the empty concept', defined in a presupposed (non-linguistic) theory of concepts and figuring as 'the empty lexical meaning' of W (the property of being a conception or perception whose content is empty, cf. Lieb 1983: 208).

The variables in definition (16) are interpreted in the theory of language, and are interpreted as indicated in (13), excepting ‘P’. ‘S’ stands for any entity of the kind of idiolect systems in any language; ‘W’ for any entity of the kind of lexical words of idiolect systems. Lexical words may be construed as *paradigm/concept pairs*, the *forms* of a word being the forms of its paradigm (there may be only one form), and the concept being the word’s *meaning*. ‘P’ stands for any entity of the type of word paradigms. ‘f’, ‘f_1’ and ‘f′’ each stand for any entity of the kind of forms of lexical words and parts of such forms.

The defining linguistic constants in (16) are ‘idiolect system’, ‘lexical word’, ‘noun form’, and ‘auxiliary part’. (I am assuming a theory of language that allows noun forms, not only verb forms, to have auxiliary parts. A move to an analogous position is appearing even in Generative Grammar, see Giusti 2015.) The four constants may indeed be available in a theory of language, where they are independent of any grammar. – Definition (16) is construed as an *open* sentence formula; therefore, no truth value is assigned to it.

Retaining the interpretation of the variables in (16), we may now transfer the definition of the grammatical constant ‘article’ from the theory of language into a grammar of English. Its status changes in the grammar, though: while still a grammatical term of the grammar, it is not an *axiomatic* constant of the grammar; the definition of ‘article’ appears in the *basic* language of the grammar (see (vi) in Section 7.2, above, for ‘basic language’, and the view of axiomatic grammars in Section 10, below). The definition may still be used in the grammar for deductions, and the constant employed in definitions, axioms, or theorems of the grammar.

9.3 Using terms in a grammar as defined in a theory of language: Example

Assume that we have a grammar of English, written as an axiomatic theory in which the definition of the constant ‘article’ in (16) is taken over from the presupposed theory of language. We may then have a theorem of the grammar that *identifies* the articles in English idiolect systems, a theorem that uses the term ‘article’ as defined in the theory of language but employs non-definitional criteria for the identification, criteria involving linguistic form (reference to linguistic form is considered essential to identifying morphological, lexical or syntactic categories or their members). The theorem might be as follows:

(17) For all C and S, if C $\in$ English and S is a system of C, then for all W, W is an article of S if and only if:
   a. W is a lexical word of S,
   b. for some f,
      i. f is a form of W,
      ii. f $\in$ {*the*, *a*, *sòme*, *àny*, *nò*}.

Informally: the articles in English idiolect systems are the lexical words that have a form that is either *the* or *a* or is *some*, *any*, or *no* with inherent secondary word stress (indicated by the accent sign).

This is an empirical claim, and sentence (17), like any axiom or theorem of an empirical theory, may be empirically false. The sentence is obviously false if the following is true of some or all English idiolect systems: there are no noun forms with auxiliary parts that are occurrences of *some*, *any*, or *no* with inherent secondary word stress. It may indeed be argued (in contrast to Lieb 2005: Section 4) that *some*, *any*, and *no* with inherent secondary word stress, if assumed at all, exist only as indefinite pronoun forms. In this case, there are no analytic noun forms whose auxiliary part is *sòme*, *àny*, or *nò*; instead, we have noun *groups* with modifying pronoun forms.

The important point is this: even if sentence (17) is false, this leaves definition (16) entirely unaffected.

In summary, sentence (17) is a theorem of the grammar, it is not a definition of the term 'article of' (quite apart from the fact that (17) is a sentence not – as (16) is – an open sentential formula); nor is (17) a conditional definition of the term 'article of' restricted to English. On the contrary, the term 'article of' is used in the theorem as defined in the theory of language.

9.4 The identification of categories

The term 'article' ('article of') is an axiomatic constant of the presupposed theory of language taken over into the grammar as a grammatical constant. As explained in Section 6.5, we also have the expression 'article of S' or 'article(-, S)' as an open predicate expression of the theory of language, available now in the grammar in the same way as the grammatical constant and denoting, for any S, the following set: $\{W \mid \langle W, S \rangle \in \text{article}\}$ – the set of all W such that W is in the relation article-of to S; article of S is a *category* of S. (If lexical words W are construed as paradigm/concept pairs $\langle P, b \rangle$, as suggested above, in Section 9.2, 'article(-, S)' is to be replaced by 'article(-, -, S)', and '$\{W \mid \langle W, S \rangle \in \text{article}\}$' by '$\{\langle P, b \rangle \mid \langle P, b, S \rangle \in \text{article}\}$'.) The category may be ***identified*** for any English idiolect system S as follows:

(17′) For all C and S, if C ϵ English and S is a system of C, then article of *S* = {W | W and S satisfy (17a) and (17b)}.

(17) and (17′) are obviously logically equivalent; hence, (17′) may be empirically false under the same conditions as (17). (Identification sentences like these two are of course not the only theorems of a grammar to make empirical claims on a category or its members; identification sentences are meant to identify, not to provide complete descriptions.)

Again, (17′) is not a definition, not even a conditional one, for 'article of S' understood as 'article(-, S)', restricting use of the term to English idiolect systems S. There is a simple formal reason for this: the requirements for definitions (see Suppes 1957: § 8.6, a classic formulation) are not met. Nor would *closed* grammatical terms like 'article(-, **S**)' or 'subj(**S**)', with a constant '**S**' denoting a specific idiolect system, qualify as definienda.

But suppose that we remove 'article of' in its general sense from the grammar, eliminating the interpretation of 'article of S' as 'article(-, S)', and go on to construe (17′) as a conditional definition of a function term 'article' that restricts its use to English idiolect systems. This gives rise to exactly the problems discussed in Section 4.6, above, in relation to Haspelmath's conception of 'comparative concepts' vs. 'descriptive categories', as pointed out again in Lieb (2017). (In the discussion following the presentation of Lieb (2017), Haspelmath, unperturbed, simply insisted on sentences like (17′) as definitions, apparently failing to understand the problem.)

It may still be proposed to call (17′) a 'definition' of the *category* article of S, for any English idiolect system S. However, 'definition' is then used in the sense of 'identification'. Speaking of the *definition of a term* and also of the *definition of a category* means that we use 'definition' in two different senses; confusing the two has caused, not only in linguistics, endless misunderstanding and fruitless discussion.

The identification of categories may be recursive, i.e. by means of axioms or theorems that differ from (17′) by having a form analogous to the form of recursive definitions. But again, recursive identification sentences for categories must not be confused with recursive definitions of terms (which may well occur in the underlying theory of language), by the same argument as before.

D. Grammars as axiomatic theories

10. Grammars G of D and σ (1): The format

10.1 The theory-of-language requirement

The following claim is justified by discussion in Parts B and C. A theory of language by which languages are independent of grammars must be available for use *in* grammars of any type if these are to satisfy two basic conditions:

i. Most grammatical terms occurring in a grammar can be understood as language overarching.
ii. A fundamental feature of informal grammars may be assumed for any grammar: making claims on languages that can be empirically false.

As argued before, neither theories of generative-enumerative grammars nor theories of model-theoretic, constraint-based grammars foresee theories of language – of languages independent of grammars – that may be used in grammars. And indeed, generative-enumerative grammars are flawed with respect to both (i), the condition of language-overarching grammatical terms, and (ii), the condition of making claims on languages that can be empirically false (depending on how such grammars are understood, they either do not contain statements, or do not contain statements on natural human languages). Model-theoretic, constraint-based grammars may in principle satisfy condition (ii). However, such grammars appear to be flawed by a lack of language-overarching terms; they do not satisfy condition (i). Instead of comparing languages independently of grammars, followers of either approach must be satisfied with comparing grammars, or at best, comparing languages as objects of grammars.

In summary, theories of language are needed. My own work during the past quarter of a century has concentrated on developing a theory of language that would be adequate for being presupposed in descriptive grammars of arbitrary languages and in language comparison (Lieb *ed*. 2017 may be compared now). Only recently have I turned, once again, to questions of grammatical description (in a part of Lieb 2013).

So far, 'presuppose' has been used informally. Different ways can be considered (below, Section 11) for integrating a theory of language into a grammar without the two getting confused; 'presupposition', in a technical sense, is one of them.

10.2 Presupposing a theory of language

Consider a grammar G that is a grammar of a set D of idiolects and a system σ for set D. G is to be a potentially applied axiomatic theory: an axiomatic theory that can be expanded by adding axioms that establish a relationship to 'observational' terms.

Let T be an axiomatic theory of language whose language is compatible with the language in which G is formulated. Grammar G itself may then be formulated so as to ***presuppose*** theory T, in roughly the following sense:

> Some or all valid non-logical sentences of the language of the theory of language, in particular, axioms, theorems, or definitions of the theory, are included among the valid non-logical sentences of the language of the grammar but not among the axioms, theorems, or definitions of the grammar. (Lieb 1983: 425)

(Definitions are assumed here to be closed sentence formulas, not open ones as in (16), above.) In a case of presupposition, the theory of language, T, or some part of T reappears in the language of grammar G. Such parts must still be kept separate from G as an axiomatic theory.

In particular, there are non-logical constants of the theory of language, possibly axiomatic, that are taken over into the language of grammar G. These constants, which include the grammatical constants of the theory of language, axiomatic in that theory, are *excluded* from the axiomatic constants of the grammar by the way the grammar is constructed.

For example, all system-related terms listed in (14b) – 'system of', 'count noun of', etc. – and the non-logical constants used in the definition of 'article' in (16) may well be axiomatic constants of the presupposed theory of language T. However, they are *excluded as axiomatic constants of grammar G* by being relegated to the grammar's *basic* language, which does not involve axiomatic constants of the grammar (see Section 7.2, (vii), above). This means that the basic language of the grammar is no longer purely a language of logic.

On our conception it is not only *grammars* that presuppose a theory of language; this is also true of a *theory of grammars* due to the fact that in a theory of grammars we must be able to refer to the objects of grammars; compare assumptions (2) to (5) as listed in Section 4.2, above, which belong to an informal theory of grammars but imply specific assumptions on idiolects, languages, language varieties and corresponding systems; these assumptions are taken over from a theory of language that is presupposed by the theory of grammars.

As a special case of presupposition, there is ***formulation in terms of***. Grammars may presuppose a theory of language by being formulated in terms of the theory, in a sense made precise in Lieb (1983: Section 28.3) and intuitively exemplified above, in Sections 9.3 and 9.4. Formulation-in-terms-of implies a Relativizing Conception of Grammatical Terms (Section 6.5, above).

The following discussion will be restricted to languages but applies to varieties just as well, with one qualification: a grammar of a variety of a language should, ideally, presuppose not only a theory of language but also a grammar of the language in question. The following terminology for languages may be transferred to varieties by substituting 'variety' for 'language'. – What, then, are the axioms and axiomatic constants of an axiomatic grammar as I conceive it?

10.3 The language name. Language-determination and language-identification sentences

There are at least two constants that must be assumed for a grammar G of a language D and a system σ: *the language name D**, a term denoting D (such as 'SBE' or 'Standard British English', treating this, for the sake of discussion, as a language rather than a variety), and *the language system name σ**, a term denoting the system σ for D. First, consider the language name D* of G.

This is an ***axiomatic primitive of G***: it is an axiomatic constant of G and is undefined in G. Moreover, D* may well be the only axiomatic primitive of G.

A complex expression of G that directly or indirectly involves the language name of G will be called ***language-name dependent in G***, otherwise, ***language-name free***. The distinction applies, in particular, to sentences of the grammar.

A grammar G of D and σ will contain ***language-determination sentences***, that is, valid non-logical sentences of the following form:

(18) $t_C \subseteq D^*$,

where t_C is a term (a constant or a complex term) that denotes a set of idiolects C, and D* is the language name. A language-determination sentence may be an axiom or a theorem of the grammar, depending on the way the grammar is set up; if a theorem, it cannot be obtained by adding the 'language-name axiom' (below, Section 10.5) to the grammar's basic language: determination sentences formulate what is *specific* to the language.

A grammar may but need not include a ***language-identification sentence***, a valid non-logical sentence that has the following form but must not be construed as a definition (see Section 10.5, below, for the reason):

(19) $D^* = t_C$.

Obviously, all language-identification sentences are equivalent to determination sentences but not conversely, and sentences of either type are language-name dependent.

A grammar G of D and σ will be called ***language complete*** if it includes a language-identification sentence. In linguistic practice, we will have to be satisfied with incomplete grammars in most cases, using determination sentences rather than identification sentences, but this is a point of methodology that leaves the conception of grammars unaffected.

10.4 The language system name. System-determination sentences

Consider a theory of grammars as assumed above, in (2) to (5) of Section 4.2. Given the way the objects of a grammar are characterized, the theory of grammars presupposes a theory of language that contains an axiom or theorem to the following effect:

(20) Any system σ ***for*** a set D of idiolects C is a set of component-specifying properties of idiolect systems S such that for any system ***of*** any idiolect in D, the system has each one of the properties.

(A more complex construct than a *set* of properties may eventually be needed.) Therefore, in any grammar of D and σ, the language system name σ* is a term that denotes a set of properties related to D in this way. It may well be a constant. Is σ* also an *axiomatic* constant of the grammar? Two cases may be distinguished.

Case 1. In contrast to the language name, the language system name may be ***defined*** in the language of the grammar; the definition then takes the place of an identification sentence. If defined, the system name is an ***axiomatic constant*** of the grammar if, and only if, the definition is language-name dependent. This need not be the case: the system σ may be quite abstract and as such may be a system not only for language D but for a much larger 'communication complex'. Indeed, it is not only a single system σ that should be assumed for a given language D, but it is an entire chain $\sigma_1, \ldots, \sigma_n$ of increasingly abstract systems that should be associated with a language (compare Lieb 1983: Section 1.6, and 1993: Section 4.4, for details). Assuming a chain is important for doing comparative and historical linguistics directly on languages, rather than comparing grammars for shared parts, to which projects such as the CoreGram project (Müller 2015) are reduced. If the definition of the language system name is language-name independent, it belongs to the basic language of the grammar and is not an *axiomatic* constant.

Case 2. Suppose that there is ***no definition*** of σ* in the grammar. In this case, the language system name is another ***primitive axiomatic constant*** of the grammar, occurring, in particular, in ***system-determination sentences*** of the grammar. These are analogous to the language-determination sentences but have the form '$t_S \subset \sigma^*$', where t_S is a term (primitive or complex) that denotes a set not of idiolects C, but of *properties of idiolect systems S*. Consider the following simple example for a system-determination sentence.

Let G be a grammar of Standard British English and σ a system for Standard British English. σ might contain the following property as an element:

(21) the property of being an S such that: For all W, W is an article of S if and only if:
  a. W is a lexical word of S,
  b. for some f,
    i. f is a form of W,
    ii. f ϵ {*the*, *a*, *sòme*, *àny*, *nò*}.

Grammar G would then contain a system-determination sentence of the following form:

(22) $\{\phi\} \subset \sigma^*$,

where ϕ is the property of being an idiolect system S as in (21). The property identifies a category of idiolect systems S, the set of articles of S (compare Sections 9.3 and 9.4, above). The determination sentence (22) partly determines a system for Standard British English.

Grammars are ***system complete*** if and only if the system name is defined, otherwise ***system incomplete***. Again, in practical work it is mostly system-incomplete grammars that we will be dealing with.

10.5 The axioms

Two axioms are needed in any grammar G of D and σ: ***the language-name axiom***, which uses the language name D* in stating that D is a language; and ***the language-system axiom***, which uses D* and the language-system name in stating that σ is a system for D. There is at least one additional axiom, a language-determination sentence or, if the grammar is language complete, a language-identification sentence. If the grammar is system incomplete, at least one system-determination sentence is added as a further axiom.

Due to the *language-name axiom*, anything that can be said of languages in general can also be derived in the grammar in relation to D, to the extent that the presupposed theory of language has been taken over into the grammar. Analogously, for the *language-system axiom*: due to this axiom, anything that holds of all language systems may be derived for σ, to the extent that the theory of language has been presupposed. Each one of the *additional axioms* is a claim on something that is 'specific to D'.

In a grammar that is ***language complete***, there is a *language-identification sentence*, an additional language axiom or a theorem identifying D on the basis of σ. The identification sentence may well satisfy the form requirements for a definition of the language name, e.g., could have the following form: ***D* = the greatest set D´ such that σ* is a system for D´.***

It would still be a fundamental mistake to construe the identification sentence as a definition, for the following reason. It must be possible for a language-identification sentence to be empirically false. However, *definition* sentences of a theory are either logically true or are neither true nor false, depending on one's theory of definition. Thus, the empirical nature of the grammar evaporates if the language-identification sentence is misconceived as a ('nominal') definition. At the same time, the possibility of a language-identification sentence in a grammar removes any reason for considering the language name as a vague term, best treated as extra-theoretic and therefore to be eliminated from a grammar. Indeed, it is a major flaw of formal grammars as currently assumed, even 'declarative' ones, that they do not allow for language-identification sentences, be it only for excluding language names from grammars.

The situation is somewhat different for the system name. In a grammar that is ***system complete***, there is indeed a *definition* of the system name. In this case, the empirical nature of statements on the language that directly or indirectly involve the system is guaranteed by the language-system axiom.

Let me emphasize an important feature of axiomatic grammars as I am conceiving them: the object ⟨D, σ⟩ of a grammar G is characterized directly by sentences of the grammar, not indirectly by imposing restrictions on the language of the grammar, restrictions, in particular, on the form or sequential order of expressions of the grammar. As long as we pay attention to the requirements imposed on axiomatic theories, the language and the form of a grammar can be freely chosen.

10.6 The theorems: Example

Given the axioms and the definitions of the grammar, we may derive theorems, as in the following example. Assume a grammar G such that:

a. G presupposes a theory of language that
   α. states that any language is a set of idiolects and
   β. includes (20) as an axiom or theorem on the systems for such sets.
b. The language-name axiom of G states that Standard British English is a language.
c. For a certain σ, the language-system axiom of G states that σ is a system for SBE.
d. G contains the determination sentence {ϕ} ⊂ σ as a theorem, where ϕ = property (21), a property of idiolect systems; implying: property (21) is an element of σ, the language system for Standard British English.

Given (a) to (d), we obtain the following theorem of grammar G (same as (17) in Section 9.3, except for involving *Standard British English* instead of English):

(23) For all C and S, if C ϵ Standard British English and S is a system of C, then for all W, W is an article of S if and only if:
  a. W is a lexical word of S,
  b. for some f,
    i. f is a form of W,
    ii. f ϵ {*the*, *a*, *sòme*, *àny*, *nò*}.

Proof. Assume the antecedent of (23). SBE is a language, by (b). Therefore, SBE is a set of idiolects, by (a.α). σ is a system for SBE, by (c). Property (21) is an element of σ, by (d). Therefore, S has property (21), by (a.β) and (20). Hence, the consequent of (23) holds. Q.E.D.

(23) is a theorem of G, which is a grammar, possibly language and system incomplete, of Standard British English and σ; the theorem demonstrates how the presupposed theory of language, the language-name axiom and the language-system axiom cooperate in the derivation of theorems of the grammar that make language-specific claims. Such theorems may or may not be true.

The language-specific objects in (23) are abstract. A relation of the grammar to concrete data can be established through *theory application*, and a relation to other theories, through *theory integration*.

11. Grammars G of D and σ (2): Application and integration

11.1 Application

A grammar G of a set D of idiolects and a system σ is interpreted to the extent that this is true of the language in which it is formulated, but this does not mean G is *applied*: explicitly related to potential data; adopting Modified Realism, the data are determined in agreement with the Intentionality Hypothesis (above, Sections 2.1 and 2.2). In Lieb (1983: Chapter 30) it is argued that the application problem can be solved, or solved in principle, by combining grammars of *sets* of idiolects with grammars of idiolects; in general outline, and glossing over the role of idiolect grammars, application may proceed as follows.

Consider the theory of language that is presupposed by a given grammar of D and σ. The presupposed theory of language is to have a part – as assumed in Lieb (1983) – that deals with ***normal utterances***. Such utterances are to relate speech objects or speech events V, persons (potential speakers) V_1, idiolect systems S, and sentences of S: 'V is a normal utterance in S by V_1 of …', where the dots are to be replaced by a name of an ***interpreted sentence*** of S (which need not be of a standard form; it may well be elliptic). Making use of the 'utterance-part' of the presupposed theory of language, a grammar G with the language name D* will allow us to derive, among others, theorems of the following form:

(24) For all C, S, V, and V_1, if C ϵ D* and S is a system of C and V is a normal utterance in S by V_1 of …, then .. VV_1 ..

Here, '.. VV_1 ..' stands for an open sentential formula whose only free variables are 'V' and 'V_1', i.e. stands for a formula that imposes a condition on the speech object or event V and the speaker V_1.

11.2 Example

The following example concerns the meaning, not the form of the sentence that will be chosen. Assuming Modified Realism, an example for the form would be largely analogous, due to the fact that the form is to be a construct ultimately based on objects or events in space and time.

Suppose that G is a ***potentially applied grammar*** of Standard British English (SBE) and a system σ. We may then have a ***theorem of G*** of the following form:

(25) For all C, S, V, and V_1, if C ∊ SBE and S is a system of C and V is a normal utterance in S by V_1 of *smoking is deadly* meaning 'smoking can kill', then V_1 wants some addressee of V to believe, because of V: (i) smoking can kill, and (ii) V_1 believes that smoking can kill.

'.. VV_1 ..' in (24) is represented in (25) by: 'V_1 wants some addressee of V to believe, because of V: (i) smoking can kill, and (ii) V_1 believes that smoking can kill.'

As grammar G is potentially applied, application axioms – any number, however large – may be added to G, creating an ***applied grammar G′***, i.e. a grammar that is an applied theory; different applied grammars may be created from G. There may be ***application axioms*** that have the following form:

(26) For some C ∊ SBE and some S, S is a system of C, and V* is a normal utterance in S by V_1* of ….

V* denotes a specific speech object or event, and V_1* denotes a specific person. The constants V* and V_1* are taken to be ***observation terms***. Since G′ is an applied theory of SBE and σ, (26) specifically allows for an ***application axiom*** of the following form:

(27) For some C ∊ SBE and some S, S is a system of C, and V* is a normal utterance in S by V_1* of *smoking is deadly* meaning 'smoking can kill'.

We now use (25) as a theorem of G, hence, of G′, in conjunction with the application axiom (27) of G′ to derive a ***theorem of G′*** that has the following form:

(28) V_1* wants some addressee of V* to believe, because of V*: (i) smoking can kill, and (ii) V_1* believes that smoking can kill.

11.3 Discussion

Theorem (28), a theorem of the applied grammar G′, no longer contains any axiomatic terms of G but only axiomatic terms of G′ that are observation terms relative to G′ ('want' etc. are not axiomatic constants of the grammar but are terms from the semantic part of the presupposed theory of language), and (28) is a valid non-logical sentence of the language of G′. The theorem is therefore an ***observation sentence with respect to G′***.

The ***truth*** of theorem (28) can be checked by various kinds of observational evidence. If true, the theorem specifies a ***positive datum*** for G′ and for G: a state of affairs that (i) is designated by an observation sentence with respect to G′; (ii) is connected – in a non-contingent way – with speaking, understanding speech, or judging speech from a communicative point of view; and (iii) is a fact. The positive datum confirms, to some extent, all valid non-logical sentences of the language of G′ that are used, directly or indirectly, in the theorem's derivation. If false, theorem

(28) specifies a ***negative datum*** for G′ and for G: a state of affairs as before, satisfying (i) and (ii), except for *not* being a fact. The negative datum disproves at least one of the valid non-logical sentences of the language of G′ that are used, directly or indirectly, in the theorem's derivation. If the theorem is false, one or more of the sentences used in its derivation must be removed from the valid sentences of the language of G′ to prevent the derivation of the theorem, or else it is decided that for some reason the state of affairs is to be disregarded as a datum. How to proceed is a question of methodology.

Data for grammars, even in the narrow sense of 'observational data', need not be restricted to states of affairs concerning individual speakers, speech objects, or speech events: corresponding *sets* may also be involved, as in corpus linguistics. Nor are data for grammars the only kind of data in linguistics.

11.4 Integration

The conception of grammars that has been outlined implies one specific solution – presupposition – to the general problem of interrelating different theories while keeping them distinct. This is the ***integration problem*** as raised by theories of language and grammars. The general problem is just as pertinent when arbitrary linguistic theories are concerned, or even linguistic and non-linguistic ones.

A number of different solutions to the integration problem in linguistics are outlined in Lieb (1983: Chapter 29), accounting for the problem's different aspects. The results are summarized as follows:

a. A **grammar** is completely formulated in terms of a theory of language that presupposes a theory of communication and may presuppose other non-linguistic theories, such as psychological and sociological ones (alternatively: a grammar is completely formulated in terms of a theory of linguistic communication that may presuppose non-linguistic theories).
b. Ideally, a **grammar of a variety** of a language is formulated, at least partly, in terms of a grammar of the language that is formulated in terms of the same theory of language (linguistic communication) as the grammar of the variety.
c. Ideally, embedded in a **grammar of an idiolect** there is a grammar of a language or variety formulated, at least partly, in terms of the same theory of language (linguistic communication) as the grammar of the idiolect.
d. If certain compatibility requirements are met, the conflation of two **grammars of the same kind** again is a grammar (if the two grammars are partial grammars of the same variety or language) or at least a theory (if the two grammars are grammars of different varieties or languages).

e. A **linguistic theory** and a **non-linguistic theory** may be related by presupposition in either direction, extension in either direction, or co-extension, as specified in (29.10). (Lieb 1983: 444; emphasis added, H.L.)

The following ***kinds of theory integration*** are assumed: complete formulation in terms of – presupposition – partial formulation in terms of – embedding – conflation – extension – co-extension; for explanations, see Lieb (1983: Chapter 29), (1993: Chapter 21). Formulation-in-terms-of is a restrictive subcase of presupposition; for complete or partial presupposition, see Lieb (1983: 425), quoted above in Section 10.2 (compare 'some or all' in the quote). Since idiolect grammars are not discussed in the present essay, I leave the notion of embedding unexplained. Conflation is closely related to extension and may not be independently needed. The notions of extension and co-extension also apply when only linguistic theories are involved. An *extension* of a theory is, roughly, another theory obtained by adding axiomatic constants, axioms and, possibly, definitions. Two theories are co-extended by a third if the third is an extension of either theory (for (29.10), referred to in (e), see below, Section 11.6).

Some improvements, outlined in Lieb (1983: Chapter 30), of the standard conception of axiomatic theories are required if linguistic theories, in particular, grammars, are to be construed as axiomatic theories to which (a) to (e) apply. Provided certain formal requirements are met, Points (a) to (e) may also apply to non-axiomatic theories.

11.5 Accounting for quantitative aspects

We may have to admit 'usage-based' *quantitative* properties of languages, varieties, or idiolects that should be accounted for in grammars; as stated by Müller (2016) for Lexical Functional Grammar (more generally, Müller 2017: Section 9):

> Many of the LFG systems combine linguistically motivated grammars with a statistical component. Such a component can help to find preferred readings of a sentence first, it can increase the efficiency of processing and make the complete processing robust. (Müller 2016: 214)

Given the conception of theory integration in (a) to (e), above, quantitative properties can be taken into account, using mathematics as required and linguistic corpora as a source of data. The proper relationship of quantitative vs. 'qualitative' aspects in grammars is, in my view, as follows.

Components of linguistic systems (of idiolect systems, on an Integrational conception) are formally *identified* without having explicit – as opposed to implicit – recourse to quantitative properties. Identification may but need not make use of prototypes and similarity relations. (For an emphasis on prototypes, see, most recently, Lehmann *forthc.*; for a formal account of the classic version of 'prototype theory', compare Lieb 1980.) Given the identification of, say, categories of the system, we specify in the grammar quantitative properties of what has been identified. Either-or conceptions of quantitative vs. qualitative do not seem adequate for grammars. In particular, neglecting the identification task in favour of quantitative aspects is problematic, as appears, for example, from an important new book by Blevins (2016), where (the mathematical) information theory is applied to word paradigms without first indicating how the paradigms are identified. Quantifying categories by reconstructing a category as, say, a set of pairs of a number and a linguistic expression (which might correspond to the result of introducing 'weighted constraints' into a constraint-based grammar, considered by Müller 2017: Section 9, in his discussion of Fluid Construction Grammar), would still not eliminate the identification requirement, unless categories are given up entirely in favour of graded distinctions.

Quantitative properties of languages, varieties, or idiolects, even if 'usage-based', should be distinguished from quantitative properties of items in linguistic corpora. Applying statistical methods to corpora is a major means of ultimately arriving at imitations of human language use and understanding by machines, possibly without much involvement of linguistic grammars. Still, combining the two approaches – grammar writing and applying statistical and other mathematical methods to corpora – may well be needed for optimal results in this area of language technology (as appears to be recognized by researchers actively involved in it; compare Ettinger 2017).

11.6 Integrating linguistic and non-linguistic theories: The interconnection problem

Linguistic theories must be integrated with linguistic ones, but also with non-linguistic theories:

> A linguistic and a non-linguistic theory may be related by one of the following forms of theory integration:
> a. The linguistic theory is an extension of the non-linguistic. (Example: theory of language learning and theory of learning.)
> b. The non-linguistic theory is an extension of the linguistic. (Example: theory of language learning and theory of language. Note that the theory of language learning, being an inter-theory, is both linguistic and non-linguistic.)
> c. The linguistic and the non-linguistic theories are co-extended by a third theory. (Example: theory of language and theory of learning.)
> d. The linguistic theory presupposes the non-linguistic. (Example: theory of language change and theory of physical time.)

e. The non-linguistic theory presupposes the linguistic. (Lieb 1983: 440–441, (29.10))

Consider (a) to (c). A theory of language learning is non-linguistic (has a non-linguistic basis) because it is an extension of a theory of learning, which is a non-linguistic, psychological theory. At the same time, a theory of language learning is linguistic (has a linguistic basis) because it is also an extension of a theory of language. A theory of language learning is an inter-theory, belonging to psycholinguistics, a shared branch of linguistics and psychology. At the same time, the theory of learning and the theory of language are co-extended by the theory of language learning. – The example for (d) should be obvious. (e) would be exemplified by a neurological theory of aphasia that *presupposes* a specific theory of language rather than *extending* it.

It is theories of language that are chosen as linguistic theories in the above examples, but grammars might have served as well.

Given a solution to the integration problem, we are in a position to also solve ***the problem of interconnections***: how to relate linguistics to non-linguistic disciplines in a non-reductionist way, by means of 'inter-disciplines'. The solution was outlined above, in Section 3.3.

11.7 Concluding remarks

Adopting the conceptions of theory application and integration, it is possible to describe linguistic objects from every point of view that is relevant in linguistics or in related fields, and describe them in a realist way. This is directly true of grammars conceived as formal or semi-formal axiomatic theories, but is indirectly true also of the informal grammars that prevail in practical grammar writing.

The essentials of theory application and integration in linguistics were published some thirty years ago, providing linguistics with an antidote to the poison of reductionism, all pervasive at the time and not yet eliminated from the body linguistic. I suggest we administer it.

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CHAPTER 6

Languages and other abstract structures

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My aim in this chapter is to extend the Realist account of the foundations of linguistics offered by Postal, Katz and others. I first argue against the idea that naive Platonism can capture the necessary requirements on what I call a 'mixed realist' view of linguistics, which takes aspects of Platonism, Nominalism and Mentalism into consideration. I then advocate three desiderata for an appropriate 'mixed realist' account of linguistic ontology and foundations, namely (1) linguistic creativity and infinity, (2) linguistics as a theory of types (and not tokens) and (3) independence but structural respect between language and the linguistic competence thereof. My own brand of mixed realism, what I call *ante rem* realism, is defended along the lines of an *ante rem* or non-eliminative structuralism, the likes of which has been offered for mathematics by Resnik (1997) and Shapiro (1997). In other words, grammars describe a mind-independent (but not necessarily unconnected) linguistic reality in terms of linguistic patterns or structures also known as natural languages. I further amend this picture to allow for the possibility of a naturalistic account of language acquisition and evolution by arguing against a particular view of the type-token distinction.

Keywords: foundations of linguistics, ontology, linguistic realism, Platonism, philosophy of mathematics

1. Introduction

The dominant picture of the foundations of linguistics and the ontological status of linguistic objects is provided by the conceptualism founded by the generative movement of Chomsky (1965). On this account, languages are mental states, or I-languages, of the individual language users. To 'cognize', or more controversially to *know*, a language is thus to be in a particular cognitive state of the language faculty. This is a physicalist view. All talk of the mind or mental states is just

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physical talk about the brain at a different level of description (see Chomsky 1986). Hence linguistics is really biolinguistics and is to be subsumed by neuroscience or biology itself.

In the wake of this picture of the foundations of linguistics, Linguistic Realism emerged. Drawing strength from the analogy with mathematics (specifically arithmetic and set theory) and issues within ontology that proved difficult for a physicalist account of the science (in its current form at least), Katz (1981), Katz and Postal (1991) and most recently Postal (2003) offer a radically different account of the objects of linguistic theory and the place of its science. It is my purpose among other things to show that this alternative holds genuine insights and approaches the field with bold honesty in interpreting linguistics as *it is* rather than as we *hope it to be*.

In this chapter, I will not mount a direct attack against Conceptualism. Rather I will take seriously the challenge presented by Platonism while attempting to develop a novel account which makes use of some of its core features. In many cases, I think the words of Katz and Postal lend themselves to my account more so than to naive Platonism (see Section 4).[1] I plan to depart from their picture to adopt a mathematical structuralist analog for linguistics itself. However, in so doing I will take a route which separates my view from Realism proper in linguistics to a rather more mixed account. Here, I take Realism to be the view that the subject matter of linguistic theory is a mind-independent realm of objects not purely characterisable in strictly physicalist terms.

Specifically, the strategy I plan to employ will be to identify three essential desiderata or properties of natural language for which any mixed realist theory of linguistic foundations and ontology ought to account. These properties stem from critiques of the biolinguistic or generative program offered by Realists such as Katz and Postal (and Nominalists such as Devitt). I will then show that one version of Platonism offers an approach to dealing with these desiderata at too large a cost. Finally, I shall suggest a non-eliminative structuralism for the foundations of linguistics as a competitor in accordance with a similar interpretation of mathematics (Shapiro 1997; Resnik 1997), thus maintaining an analogy with the formal sciences.

In the next section, I draw from the Platonistic (and Nominalistic) critiques of generative grammar in identifying the essential characteristics of a mixed realist theory of linguistic foundations. In Section 3, I hope to show that one variety

1. As pointed out to me by David Pitt, the Platonism of Katz and Postal is by no means the only game in town. Thus, in keeping with this volume, I reserve the term ‘Linguistic Realism’ for their account and Platonism for a more general ontological view found in metaphysics.

of Platonism cannot meet this challenge. In Section 4, I describe an alternative view which might fare better with these requirements and in addition offer a potentially more naturalistic account of the foundations of linguistic theory and its objects. This account offers a mathematical structuralist foundation for linguistic theory in which linguistics is a science of natural languages conceived as quasi-concrete structures (in terms of Parsons 1990). In order to tailor the mathematical structuralism of Shapiro (1997) and Resnik (1997) to linguistics, viewed here as a semi-empirical enterprise (or semi-formal, depending on your perspective), I argue against a particular view of the type-token distinction currently prevalent within the philosophy of language and linguistics. This is the primary task of Section 4.3.

2. Three desiderata for Mixed Realism

In this section, I shall outline three important properties of natural language that a Mixed Realist theory of linguistics should consider. Most of these properties are familiar from various critiques of the generative or biolinguistic program. The first argues for a central place for the concept of linguistic infinity, despite the fact that linguistic infinity is potentially irrelevant for linguistic creativity. The second concerns the so-called correct 'level of abstraction' for the objects of linguistic theory, namely sentences. Lastly, the final property deals with the relationship between a grammar as theory of linguistic structures and a theory of competence. While it denies their identification (in line with Platonism), it also argues for a particular account of their interaction (in line with Devitt 2006), namely that linguistic competence should respect aspects of the structure rules of the grammars and *vice versa*.

2.1 Linguistic creativity and infinity

2.1.1 *Creativity*

One of the most discussed properties of natural language is that of linguistic creativity (Drach 1981; Chomsky 1982; D'Agostino 1984; Pullum & Scholz 2010). Despite being assumed to be a cross-linguistic universal component of competence, the notion has not always been clearly described. Part of the problem is that the phenomenon of creativity has not always been separated from the concepts and terms used to model it, such as 'linguistic infinity', 'discrete infinity', 'generatively enumerable' etc.

Infinity issues have dominated the foundations of linguistics and often informed the rejection or acceptance of various frameworks (Langendoen &

Postal 1984; Katz 1996). For instance, as Searle (1972) notes "[w]ithin structuralist assumptions it is not easy to account for the fact that languages have an infinite number of sentences". For years, Chomskyans have placed the need for a computational system with recursive elements at the forefront of their linguistic project. Katz (1996) argued that due to the infinity of natural language, both Bloomfieldian Nominalism and Chomskyan Conceptualism fail as interpretations of linguistics because there are simply not enough concrete tokens to capture the generalizations of grammatical theories (essentially restaging the debate between nominalists and Platonists within the philosophy of mathematics). Langendoen and Postal (1984) produced a proof to the effect that the cardinality of natural language exceeds generative capacities, and thus standard accounts of competence, in being of the same magnitude as a proper class (see Langendoen (2018) for a refinement of this result).

However, it is not at all clear what linguistic creativity is or even if it requires linguistic infinity (and in fact the contrary has been convincingly argued by Pullum & Scholz 2010). I will not rehash this entire debate here, I will however try to make sense of the creativity claim and determine to what extent it goes hand-in-hand with the theoretical posit of infinity. My conclusion will be that infinity should in principle be accommodated within an account of the science of linguistics for reasons other than those usually offered for creativity, but only if one is a Realist.

What is linguistic creativity? A natural starting point to this discussion can be found in the comments of Chomsky who placed this property at the forefront of the discipline. For instance, consider Chomsky (1964) and (1966) respectively.

> The central fact to which any significant linguistic theory must address itself is this: a mature speaker can produce a new sentence of his language on the appropriate occasion, and other speakers can understand it immediately, though it is equally new to them. (Chomsky 1964: 50)

> The most striking aspect of linguistic competence is what we may call the 'creativity of language', that is, the speaker's ability to produce new sentences that are immediately understood by other speakers although they bear no physical resemblance to sentences that are 'familiar'. (Chomsky 1966: 74)[2]

There are a few things to notice about the above quotations. The first is that there is no mention of the concept of infinity in either. Given that the number of expressions which language users actually encounter can only be finite, the above

2. This criterion is not very strong. Simple cross-linguistic and even intralinguistic evidence can cast doubt on it.

characterizations potentially allow for an upper bound on the capacity to produce new sentences i.e. a limit to creativity. The second thing to note is the idea that creativity so conceived involves the cognitive ability to interpret novel expressions without prior analogy. Note the emphasis on 'new' or unfamiliar sentences here. Part of the reason behind this insistence is to block Hockett-like accounts involving creativity by analogy. Hockett (1968) attempts to cast doubt on the ubiquity of linguistic creativity by suggesting that corpus data indicates that most sentences encountered in daily life are merely variations of a more commonly used/ heard set of sentences (perhaps a precursor to contemporary Construction Grammar accounts?). Chomsky, however, is careful to distinguish the creative 'use' of language from the 'creative aspect of language' itself. The former may indeed be constrained by various limitations but the latter allows for much more freedom of expression, at least in principle (see Chomsky 1982 for discussion).[3] Nevertheless, freedom of expression still puts us quite significantly shy of infinity claims.

Consider a statement from Chomsky (1972) in which the concept of 'indefiniteness' of size surfaces.

> Having mastered a language, one is able to understand an indefinite number of expressions that are new to one's experience, that bear no simple physical resemblance and are in no simple way analogous to the expressions that constitute one's linguistic experience. (Chomsky 1972: 100)

Again in the above quotation, empiricist or analogy-based accounts of creativity are explicitly blocked but the idea of an indefinite number of expressions is also introduced, denying the possibility of a fixed upper bound on creativity. It is at this stage that one may be tempted to introduce infinity into the picture. However, we are still some distance from requiring linguistic infinity for the notion of creativity under discussion.

Consider the example, presented in Pullum and Scholz (2010), of a standard *haiku*. A haiku typically involves 3 lines with a maximum of 17 syllables (5 in the first and last lines and 7 in the second). The possibilities for haiku creation are clearly finite, yet seemingly 'indefinite' in the required sense (somewhere in the region of 10^{34} in Japanese). As Pullum and Scholz (2010: 127) note, "the set is large enough that the competitions for haiku composition could proceed continuously throughout the entire future history of the human race […] without a single repetition coming up accidentally". This is meant to be a case for the non-necessity of

3. It should be noted Chomsky has admitted over the years that linguistic creativity might be in some sense inexplicable or one of the 'hidden mysteries' of the science (Chomsky 2009).

infinity for creativity. We can see that if we relax the parameters on composition, the cardinality of the creative capacity increases dramatically, yet we are still well-within the bounds of the finite.

A similar sentiment on the separation between creativity and infinity is suggested in Evans (1981).

> It is unfortunate that Chomsky's writings have led people to equate the creativity of language use with the unboundedness natural languages display. Linguistic creativity is manifested in the capacity to understand new sentences, and the speaker of a finite language such as the one I have described can manifest it. (Evans 1981: 327)

Evans provides us with a simple language (with 20 axioms linked to a finite vocabulary and a composition axiom) which (similar to a haiku case) allows for a wide range of combinatorial expression (100 sentences) and a disposition towards the understanding of novel expressions. So far, we seem to have a few core components of an account of linguistic creativity, of which infinity is not one. These components include, genuine novelty in terms of non-analogy, indefiniteness in number and flexible composition.[4] It seems to me that all of these features can be comfortably accommodated by means of the principle of compositionality.

The principle of compositionality states that the semantic value of a complex expression is determined by the semantic value of its parts and their syntactic combination.[5] For one, compositionality is not usually thought of as a property of a given semantics or syntax. Following Montague, it is defined as a relationship between a syntax *X* and a semantics *Y*, often modelled as a homomorphism between generated algebras (see Janssen 1986 for details).[6] Clearly creativity is an important property of natural language and any theory of linguistics, realist or otherwise, should be able to account for it.

So at which point does infinity enter into the picture? The usual story is linked to recursion, iteration and infinite generation. However, I think this issue might go deeper than these specific mechanisms to the very idea of rule-following in linguistics and the philosophy of language.

4. Technically, 'indefiniteness' is not a property of Evans' example or the Haiku case.

5. This principle is also used in morphology.

6. The literature on compositionality is much too vast to go beyond an intuitive sketch here. Suffice to say that almost every aspect of its definition is up for grabs. See Shieber and Schabes (1991) for a promising account in terms of synchronous grammars.

2.1.2 *Rule-following and infinity*

In this section, I hope to show that realism places an added burden on linguistic theory in terms of infinity claims than do strictly physicalist frameworks. The idea that the theories of natural language are provided by rule-based grammar formalisms has held sway since the seminal *Syntactic Structures* (Chomsky 1957). Two related ideas informed both the inception of formal language theory and the centrality of syntax within the generative tradition in general. The first is that a language can be seen as a collection of sentences of finite length over a finite vocabulary and secondly that a grammar (viewed as a theory of language) generatively enumerates the sentences of that language. Chomsky (1959: 137) goes on to add "[s]ince any language *L* in which we are likely to be interested is an infinite set, we can investigate the structure of *L* only through the study of the finite devices (grammars) which are capable of enumerating its sentences". The rules or functions which we specify for a given language are informed by the specific constructions of the natural language under study.

Natural languages such as English allow for iterative constructions such as those involved in conjunction, subordinate clauses and adverbial modification. Consider the examples from Pullum and Scholz (2010: 114) below.

> It is evident that *I exist* is a declarative clause, and so is *I know that I exist*, and so is *I know that I know that I exist*; that *came in and went out* is a verb phrase coordination, and so is *came in, turned round, and went out*, and so is *came in, saw us, turned round, and went out*; that *very nice* is an adjective phrase, and so is *very very nice*, and so is *very very very nice*; and so on for many other examples and types of example.

The idea is that at no non-arbitrary point can we stop the chain of grammatical constructions; or rather that at no stage in the sentence production can we say 'this is no longer English'. Thus, natural language seems to be 'closed' under recursive rules such as the rules characterizing the constructions mentioned above. In this way, we are confronted, in the philosophy of language, with a parallel of the Sorites cases. Given the nature of certain vague predicates such as *bald* or *tall*, we cannot determine the point at which the predicate disapplies to an object (which can have effects on the validity of rules such as *modus ponens* or principles such as bivalence in certain systems used to model the phenomenon).[7] If indeed we are dealing with 'closure' principles as in first-order logic (FOL), then the generated set (or 'theory' in the logical sense) would be unproblematic and denumerably infinite. However, in the case of natural languages as they are used, things are generally not this

7. I thank Henk Zeevat for suggesting this possible connection to me.

precise. The recursive rules of formal languages do not perfectly capture the nature of natural language use. If they did, then there would be no difference between formal and natural languages, or between ideal competence and actual performance, but presumably there is such a difference. Natural languages as they are used are sloppy and imprecise, their rules are malleable and violable. More controversially put, there might indeed be a point at which a further iteration of *very* yields an ungrammatical sentence (to borrow a phrase from David Pitt, we might 'generate ourselves out of the language'). Nothing I am saying here depends on taking 'grammatical' to be a vague predicate (although I think 'acceptable' certainly is). The point is that recursion might indeed be a useful element of the grammars we use to model natural language constructions but it is not necessarily a feature of the languages themselves, *mutatis mutandis* for infinity. Of course, if we follow Postal, natural languages are sets, or sets of sentences, and indeed they are capable of such characterization.

An important element of the above characterization and connection with Sorites series is that of natural languages as concrete objects and linguistic rules as modelling something in the messy physical world. However, if we accept that linguistics is a formal science, concerned with abstract objects, similar to mathematics and mathematical logic, this limitation is lifted. On this account, the rules of our grammars specify (not model) the features of our natural languages, much like the syntactic rules of, say, propositional logic (PL) specify the *wff*'s it generates. If sentences are not constituents of mental states or concrete tokens, then we are free and indeed required to treat the rules of our grammars as determining the structures of our languages. Generativists themselves often make use of this formal analogy, for instance consider Pinker (1994: 86).

> By the same logic that shows that there are an infinite number of integers – if you ever think you have the largest integer, just add 1 to it and you will have another – there must be an infinite number of sentences.

As Pullum and Scholz (2010) correctly counter, the case for the discrete infinity of the natural numbers is established by the axioms of Peano arithmetic which include a successor function (and an induction axiom schema), and there is no analogy of this operation in the case of natural languages. But a Platonist (or Realist) could insist that there are other mathematical avenues available to arriving at the requisite cardinality (denumerable infinity or α_0). Perhaps one could avail oneself of the idea of weak limit cardinals which do not require anything like a successor function to arrive at denumerable infinity. Postal (2003) has a somewhat nuanced argument for the connection between natural numbers and natural languages. He argues, by *reductio*, that if one assumes an upper bound on an iterative series of sentences in English, then one can show that its logical implications (that the iterations stop at sentence m rather than $m+1$ or $m-1$) cannot be met. The above reasoning is meant to show that the posit of an upper-bound or fixed upper limit on the set of sentences is to be rejected (this is also compatible with my suggestion above that such constructions are vague not infinite).[8] Nevertheless, the realist has no principled reason for rejecting the idea of closure operations in natural language nor that of languages as sets or collections of expressions (as Chomskyans genuinely adherent to the concept of 'I-languages' are wont to do). The original 'vastness result' of Langendoen and Postal (1984) is testament to the limits of logico-linguistic reasoning. Returning to Katz,

> [G]rammars are theories of the structure of sentences, conceived of as abstract objects in the way that Platonists in the philosophy of mathematics conceive of numbers […] They are entities whose structure we discover by intuition and reason, not by perception and induction. (Katz 1984: 18)

On this view, natural languages themselves are systems of these sentences, and the rules of the grammars governing their interaction are proven in the same way as we would prove theorems in number theory, such as Fermat's last theorem.[9] Thus, linguistic infinity should be an element of any realist account of linguistic ontology and the foundations of the science, notwithstanding its relation or lack thereof to creativity. If recursion is an aspect of our best linguistic theory (grammar) then recursive structures are aspects of linguistic reality. If the set of sentences of a given natural language is closed under conjunction or other recursive operations, then much like the case for formal languages such as PL or FOL, NL is discretely (and trivially) infinite. In Section 3.1. I will discuss how a realist might escape a strict reading of this infinity requirement while maintaining the rule-following commitment.

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8. I am not sure that this argument necessarily entails infinity. Following Hockett (1968), consider the rules of any baseball game. It is easy to see that for any real game, the ultimate score could always have been higher or lower than it in fact was but this does not mean that the score of any baseball game is potentially infinite. I think the analogy here is not with the denumerable infinity of the natural numbers but rather with their 'countability' which can be finite in set theory (i.e. a finite subset of n). In addition, it assumes that no sequence of sentences has a maximal length.

9. For example, proving that $a^n + b^n = c^n$ is true for any positive integers where $n > 2$ might be a similar task to proving $a^n b^n c^n d^n$ where $n \geq 1$ is a string not accepted by a context-free grammar. But the former is certainly a different task from showing that Swiss-German is not such a language (see Shieber 1985 for details).

2.2 Of tokens and types

Another core component of the realist persuasion in linguistics is the emphasis on the correct 'level of abstraction' for the interpretation of its theories. Originally presented in Katz (1984), it has undergone some variation and revision in Katz (1996) and Postal (2003, 2009). Thus, there are a number of related strands to this line of reasoning and I hope to do them justice in this section.

The idea can be summarized as follows. The same species of problem that befell the nominalist or American structuralist project affects the biolinguistic or mentalistic one, namely they were pitched at an insufficient level of abstractness. Linguistic theory needs to posit grammars which can account for natural language properties at the right level of abstraction.

> Thus, with conceptualism [mentalism], as with nominalism, there is a possibility of conflict between a demand that grammars satisfy an extrinsic, ideologically inspired constraint and the traditional demand that grammars meet intrinsic constraints concerning the successful description and explanation of the grammatical structure. (Katz 1984: 195)

In order to correctly meet the 'intrinsic' constraints such as infinity, recursion and structural hierarchy, the psychological level is inadequately abstract on this view (another way to understand the quotation above is that grammars do not need to go beyond 'descriptive adequacy').[10] Therefore, we need to ascend to a higher level of abstraction to capture these linguistic properties. In the absence of a systematic correspondence between the formal structure and the physical system, an extreme interpretation of this problem could be expressed as a charge of a category mistake at the heart of the biolinguistic movement (or 'incoherence' in Postal (2009)). Thus, a physical system (a human brain) is not something capable of possessing properties such as infinity (or capable of description in terms of the set-theoretic merge operation). Mental states and physical tokens cannot be recursive or infinite, only sets and other mathematical objects are amenable to such description.

The problem does not disappear with the limitation of structure either. In the Minimalist Program (1995), Chomsky investigates the *minimal* structural requirements needed to explain the gulf between the child's initial state and the adult's later competence, as well as language evolution. This marks a departure from the putative complex linguistic architectures of the Extended Standard theory (*circa* 1970) and Government and Binding (Chomsky 1981) which posit various levels of representation and interfaces between these levels. Once again, the central

10. Katz and Postal (1991) argue that the psychological level is also inadequate for the task of interpreting the various necessities that are involved in linguistic theorising.

explananda of linguistics is to account for the perceived discrete infinity of linguistic expression and the hierarchical nature of syntactic organization. According to Minimalism, in order to explain these features, one needs to only posit a binary merge function which takes two syntactic objects and outputs one. Technically, there are two merge operations, *external merge* which takes two distinct objects as input and *internal merge* which allows embedding and thus allows for recursion. Furthermore, *internal merge* involves duplicating items within the operation. For instance, if we merge syntactic objects α and β to form the unordered set {α, β} and there is a γ such that γ is a member of α and we merge this object with{α, β}, we would have two copies of γ in the resulting structure (see Langendoen 2003 for more details). In this way, we are supposed to be able to account for all the usual movement operations with very minimal apparatus in the syntax (and various constraints on the operations).

Merge, however, is a set-theoretic operation. The universe of set theory (non-denumerably captured by the universe *V*) generally takes sets to be outside of space and time, finite or infinite, and abstract. Before continuing, it is important to clear up one potential confusion here. The objection is not supposed to be that mathematical models are being used to describe a physical system. This is a commonplace practice in science and does not presuppose that all mathematical modelling generates incoherent ontologies. The reason for the specific problem in the biolinguistics tradition can be couched in terms of the lack of a systematic correspondence between elements of the model and elements of the target system. Behme (2015) admits to the coherence of such accounts for a notion of mathematical modelling in physical systems. But she adds that such a story is not available for a Chomskyan concept of I-language since "there is currently no proposal providing a systematic correspondence between neurophysiological structures in the brain and the elements of the set-theoretic linguistic model" (Behme 2015: 33). What is more is that we have no idea how elements such as the set-theoretic operation of merge could correspond to neurophysical structures and furthermore it is argued that there in fact cannot be such a correspondence.

On the one hand, we want to explain discrete infinity, recursion and syntactic hierarchy through the all-encompassing set-theoretic operation of merge. On the other hand, we want to provide a naturalistic explanation of language in terms of the human brain and biology. Postal (2009) believes that these requirements pull in opposite directions and thus cannot be met in the same object simultaneously, namely an I-language. Thus, biolinguistics is stuck with an untoward or 'incoherent' ontology (at least at its current stage). Or as Postal (2003: 242) puts it "[t]he received view claims that an NL is something psychological/ biological […] a state of an organ […] And yet it has been unvaryingly claimed in the same tradition at issue that NL is somehow infinite. These two views are not consistent".

The move made by Platonists then is simply to raise the level of abstraction of sentences to that of sets and other abstract objects, thereby proffering a coherent ontology for the interpretation of linguistics. Returning to Katz,

> Sentences, on this view, are not taken to be located here or there in physical space like sound waves or deposits of ink, and they are not taken to occur either at one time or another or in one subjectivity or another in the manner of mental events and states. Rather, sentences are taken to be abstract and objective. (Katz 1984: 18)

Postal (2009) presents a similar argument to this effect. However, he follows Katz (1996) in making use of the type-token distinction. If linguistic theory or grammars were indeed about brain-states etc. as the biolinguist would have it, then the sentences of these theories would have to be at the level of tokens, not types (which are here conceived of as abstract objects). There are two issues with this position, he claims. For one thing, it seems out of touch with linguistic practice in which grammars usually deal with "island constraints, conditions on parasitic gaps, binding issues, negatively polarity etc." (Postal 2009: 107). Importantly, these accounts are rarely, if ever, informed by evidence from neuroscience or psychology (as one would expect if they were truly concerned with brainstates). Therefore, he concludes that these accounts are concerned with sentence types conceived abstractly.[11]

> Sentence tokens exist in time and space, have causes (e.g. vocal movements), can cause things (e.g. ear strain, etc.). Tokens have physical properties, are composed of ink on paper, sounds in the air […] Sentences have none of these properties. Where is the French sentence *Ça signifie quoi?* – is it in France, the French Consulate in New York, President Sarkozy's brain? When did it begin, when will it end? […] Such questions are nonsensical because they advance the false presupposition that sentences are physical objects. (Postal 2009: 107)

11. For Katz (1996) the abstractness concern in linguistics is a special case of the general problem of abstractness in the formal sciences. An account such as the strict finitism or 'inscriptionalist nominalism' characterized by the Hilbert program, for instance, failed as an appropriate interpretation of mathematics according to Katz. In order to capture the infinity of mathematics via the empiricist scruples of nominalism, only reconstructed language about the infinite is permitted, "mathematics is about mathematical expressions" (Katz 1996: 273). The objection is simply that to make sense of such talk, we need either expression types, which take us back to abstract objects, or expression tokens, which need to allow for unactualized *possibilia* which in turn are no less metaphysically suspect than abstract objects. Katz, however, neglected the vast literature on actualist reinterpretations of quantified modal logic, some varieties of which posit *contingently nonconcrete* objects in an attempt to avoid commitment to *possibilia*.

These considerations lead Platonists to conclude that linguistics is concerned with sentences on the level of abstract objects, in the sense of non-spatio-temporally extended entities. Truth in linguistic theory or in its grammars is then determined by correspondences between the sentences of the theory and these objects. Thus, there is some kernel of truth to the notion that linguistic grammars and the theories they inform do possess a formal and abstract level of description through the analysis of sentence types (or whichever basic unit with which one begins). Furthermore, a Realist account of linguistics should provide an appropriate interpretation of this aforementioned level of abstraction and linguistic practice as it is.

2.3 Mixed Realism and respect

The properties or desiderata of the previous sections emerged mostly from Platonist critiques of Mentalism. Thus, they pushed a specific agenda and ontological attitude. The next series of arguments stem from a very different ontological approach to linguistics, similar in its focus on *concreta* to mentalism but in line with Platonism in its rejection of representationalism or the idea that speakers of a language represent/ know/ cognize the grammar rules of their language. The chief proponent of what is called the 'linguistic conception' (as opposed to the 'psychological' of generative grammar) is Michael Devitt in his book *Ignorance of Language* (2006). Devitt claims to be a realist in some sense but not a Platonist. It might then be useful to consider his stance and its intersection with those of Katz and Postal. The goal of this section is to establish a *sui generis* position between Realism/ Platonism and Devitt's nominalism.

Given what I have said above, we might be tempted to consider Realism to be non-ontologically-committing (although this is not how it would be interpreted by Katz and Postal). One common claim between Linguistic realists and nominalists of Devitt's kind is that linguistics is *about* something outside of psychological reality. Theories of language, i.e. grammars, tend to describe this extra-mental reality and not the linguistic competence of speakers. In saying something more precise about what exactly this non-psychological reality is, these camps diverge. Platonists hold that it is an abstract extra-physical reality, while nominalists, such as Devitt, prefer a physicalist account. My own account will draw from aspects of both ontologies. Another way of putting this point is that both Platonists and Nominalists hold that language *qua* object of linguistic inquiry is not in the brain but where they say it is differs quite drastically from one view to the next.

However, the above characterization will lead to confusion. In the spirit of this chapter I will maintain the use of the term Mixed Realism to refer to the amalgamation of desiderata informed by Platonism and Nominalism.

So given the above characterization of Mixed Realism, unsurprisingly, part of the next desideratum of a mixed realist account of linguistic foundations will be the rejection of competencism or the view that linguistics concerns the psychological states of language users (what Devitt calls 'the psychological view'). Devitt (2008) describes his positon in the following way.

> [A]ccording to my 'linguistic conception' a grammar explains the nature of linguistic expressions. These expressions are concrete entities external to the mind, exemplified by the very words on this page. (Devitt 2008: 249)

I will follow Devitt one step further in adding another aspect to this desideratum and that is a notion of RESPECT between the posits of the grammars and the processing rules of competence.

> [A] theory of a competence must posit processing rules that respect the structure rules of the outputs. Similarly, a theory of the outputs must posit structure rules that are respected by the competence and its processing rules. (Devitt 2006: 23)

This is what Devitt calls the 'Respect Constraint'. This constraint is motivated by various examples, mostly designed to distinguish between mental competence in a particular act and the output of that competence. Although in the case of natural language these features are distinct, any of the former must respect the structures of the latter and *vice versa* according to Devitt. He goes on to describe his 'linguistic conception' or the view that "a grammar is about a non-psychological realm of linguistic expressions, physical entities forming symbolic or representational systems" (Devitt 2008: 203). Devitt claims that grammars of linguistics are true of linguistic reality (not to be confused with an abstract linguistic reality) and not human psychology. From this conception of grammars he defines his minimal position (M) below.

> A competence in a language, and the processing rules that govern its exercise, respect the structure rules of the language: the processing rules of language comprehension take sentences of the language as inputs; the processing rules of language production yield sentences of the language as outputs. (Devitt 2006: 57)

The onus is on the generativists or biolinguists to prove that we need more than this minimal posit, i.e. prove that representationalism is correct. This has been a notoriously difficult task, in most cases representationalism was merely assumed. In addition, early psycholinguistics was initially meant to determine the connection between the processing rules of performance and the grammar rules of competence. This was generally considered to be an unsuccessful venture (even by its own proponents at the time). Nevertheless, it is not my concern here to challenge Devitt's position from a conceptualist or mentalist perspective (see Collins 2007, 2008a, 2008b; Lawrence 2003; Rey 2006; Slezak 2007 for such arguments).

Unfortunately, there are a number of problems with this view, or Devitt's 'linguistic conception', in light of the other desiderata I consider and general issues about which a Mixed Realist might be concerned. For one thing, in this chapter (and Devitt's book) a lot is said about what linguistics is *not* about but so far we have not delved into the question of what linguistics *is* about and here lie the problems for the position I take in this chapter.

In the preface to *Ignorance of Language*, Devitt describes both his initial fascination with and initial resistance to linguistics. He states (of his thoughts during his graduate years) "Surely, I thought, the grammar is describing the syntactic properties of (idealized) linguistic expressions, certain sounds in the air, inscriptions on paper, and the like […] It rather looked to me as if linguists were conflating a theory of language with a theory of linguistic competence" (Devitt 2006: v). This thought is apparently the seed out of which the main ideas of the book grew. Now most Realists would agree on the last statement, in fact Katz (1981), (1984) and Postal (2003) stress the fallacy of conflating the knowledge of language and language itself allegedly present in generative linguistics. It is the first claim, that grammars are about 'sounds in the air' and 'inscriptions on paper', that seems to be at odds with Linguistic Realism (and Mixed Realism). Once again, we seem to be at the wrong level of abstractness. Concrete tokens are insufficiently abstract for the interpretation of most of what linguists do. We saw in the previous section that there is some kernel of truth to the type talk of Platonists and in so far as 'idealized token' means type, we are fine but I doubt that this is what Devitt has in mind.[12] To reiterate, grammars, on this view, describe structure rules which constitute representational systems outside of internal mental representational systems (but are respected by them). As we saw with the above characterization of (M), sentences are supposed to be inputs for processing and they are also outputs of processing, but what are sentences on this view? Are they physical tokens, 'inscriptions on paper' or 'sounds in the air', i.e. utterances? Surely not, since this would not be sufficient to interpret the theories of actual linguists as *per* Postal's objection in 2.2.

Another issue is brought out by Ludlow (2009: 394) when he claims that "while Devitt purports to be offering a proposal that is faithful to linguistic practice, the range of linguistic phenomena and explanation he surveys is limited". This limitation cannot, for instance, deal with postulates of covert material in syntax (which have no phonological expression), such as PRO (also see Collins 2007, 2008a) or traces and the like. If our structure rules concern physical tokens (sounds, writings etc.), then elements which do not overtly appear through these media pose a problem. Much of linguistic practice and methodology involves the use of assumed or

12. See quotation from Devitt (2008) above.

covert entities and items. Katz (1971) linked the Chomskyan revolution in linguistics to the Democritean revolution in early scientific thought in that it aimed to expose the underlying physical reality behind appearances.

Consider the PRO postulate in syntax. This element is a null noun phrase or a type of empty category which means it goes unpronounced phonologically. A common example is found in infinitival constructions in which PRO operates as a subject of infinitives, *Susan wanted John [PRO] to help her.* The behaviour of PRO is different from that of general anaphors, referring expressions and pronouns hence its need for its own category. PRO does not, however, figure in physical tokens as it is unpronounced.

On a related note, in Section 2.1.2, I argued that Mixed Realists have to take posits of the grammars (and their consequences) to be actual features of linguistic reality. Such posits include recursive structure rules and closure principles which seem to lead to infinity claims. Thus, either we need to be able to ascend beyond the level of physical tokens which fail to interpret such claims or provide a naturalistic account of infinity claims in linguistics (I attempt to do both below).

Lastly, despite the issues with the nominalism of this proposal, we will incorporate an element (or two related elements) from its core, namely that linguistics is the study of language not the study of linguistic competence (or knowledge) and that the study of language and the study of competence need to be connected by a RESPECT constraint (the latter is the specific contribution of Devitt's account).[13,14] My specific aim is to be able to tell a story about the production and comprehension of natural languages, even if linguistics itself is the independent study of language systems conceived of in a realist or nominalist manner. In this way, I hope to use insights from Platonism and nominalism to good effect. More on this task in the next section.

2.4 Taking stock

So far, I have been attempting to determine the key aspects of a Mixed Realist account of linguistics. I have argued that although potentially unrelated to

13. This moves in a direction that Realists of the Katz and Postal persuasion would deem unconvincing for a variety of reasons. One of these reasons is that linguistics need not concern itself with the 'knowledge of language' and only the language itself conceived of as an abstract object.

14. Seen in this light, Lewis (1975) can also be considered a Mixed Realist view. It posits that languages are abstract objects of a certain sort (functions from sounds to meanings) while holding that these *abstracta* model social patterns of linguistic behaviour in terms of truth and trust, i.e. a different take of the respect constraint.

creativity (which requires compositionality), linguistic infinity cannot be ignored by Realists. I affirmed the need to ascend beyond a level of physical tokens or mental ones in the interpretation of grammatical theory as argued by Katz (1996) and Postal (2009). And lastly, I accepted that linguistics is the study of a competence-independent linguistic reality but I restricted this claim by insisting (with Devitt 2006) that this reality be linked to linguistic competence *via* a structural respect constraint. The last aspect goes beyond Realism or Platonism. For clarity, I provide the list below as a guide for the ensuing discussion.

1. A mixed realist interpretation of linguistics ought to (a) account for creativity in terms of novelty, compositionality etc. and (b) account for the potential infinity (denumerable or otherwise) of natural language(s).
2. Linguistic theory is a theory of sentences at the level of types or more generally Mixed Realism needs to be pitched at the correct level of abstraction.
3. (a) Linguistics is the study of natural language, not the study of the knowledge of or competence in that language, and (b) grammatical structures (and rules) need to be respected by the structures of competence and *vice versa*.

What remains to be shown is that Platonism does not necessarily capture these three conditions on a mixed realist account of linguistic foundations.[15] This is the task of the rest of the chapter. Before I attempt to do this, however, I will briefly compare the view so far presented with a similar account in the literature, namely Hans Heinrich Lieb's 'Modified Realism'.

2.4.1 *Mixed Realism and Modified Realism*

At this stage of exposition, I think it expedient to compare and contrast my view with a similar account in the form of Lieb's 'Modified Realism' (Lieb 1992, Lieb 2018). Lieb offers an alternative account of linguistics to both the mainstream mentalist picture and the linguistic realism discussed in this volume. I will outline three aspects of his view and consider how they interact with the desiderata for a Mixed Realist account as I have described it above. I will ignore aspects of the view which do not directly concern my present purpose. The interested reader is directed toward Lieb's chapter in this volume for more details.

Lieb's account is said to be realist in that it considers the proper treatment of linguistics to require an 'abstract, extra-mental objects' interpretation of the linguistic structures of phonology and syntax, linguistic meanings, idiolects, languages conceived of as abstractions from idiolects and the respective systems

15. Again, Platonists are not aiming to do so. For instance, they would reject (3b) outright.

thereof. He identifies an ontological hierarchy in which lower levels are comprised of *concreta* and at higher levels *abstracta* generally pertain. Additionally, "[a] hierarchy of ontological levels is constructive in the following weak sense: entities of higher levels are ontological constructs based on lower-level entities" (Lieb 1992: 46).

Thus, he envisages a bottom-up Platonism similar to Bromberger (1989) in which linguistic types are archetypes which model tokens conceived of as (quasi) natural kinds. This process is based on the interrelationships between linguistic tokens (or 'projectible questions' that receive the same answers for all the tokens). However, Bromberger's account fails to capture certain properties of linguistic types not possessed by their tokens such as the number of centre-embeddings and the like (see Wetzel 2014). In terms of my dialectic, infinity judgements are left without explanation on Bromberger's account since properties such as infinity or infinite generation are generally not aspects of tokens or lower level ontological objects in Lieb's system. Lieb claims that:

> "This notion of 'abstract' as 'non-concrete' differs from a Katzian conception by being relativized to a hierarchy of ontological levels where the entities on a higher level are 'constructs' from entities of lower levels, in a purely ontological sense that does not imply anybody 'constructing' them in a literal sense.
>
> (Lieb 2018: 83)

The difference between the two views is that Bromberger's account involves a certain artefactual notion of construction or model in a bottom-up fashion as is common in scientific modelling.[16] Lieb, on the other hand, maintains that the objects at higher levels are not literally constructed from lower level entities but given by the ontological hierarchy posited in his 'Intentionality Hypothesis' (see below). In addition, he considers a purely bottom-up analysis to be too restrictive and thus allows for data to be derived from various sources (presumably including pure logic). This strategy may avoid the issue above but I think it might also run into some other problems as we will see in Section 4.3.[17]

One important commonality between Lieb's framework and the one outlined in this chapter is what he refers to as the 'Intentionality Hypothesis'. This claim requires that for anything to qualify as an 'object of linguistics' it has to be needed in some way "for describing the content of intentional mental states or events that

16. For an account of scientific modelling in linguistics, see Nefdt (2016a) and Nefdt (2016b).

17. Lieb conceives of linguistic grammars as texts or systems to which normal objects need to adhere, a special case of which are Carnapian axiomatic theories (see Tomalin 2006 for a discussion of Carnap's influence on the development of generative grammar).

are connected – in a non-contingent way – with (i) speaking, (ii) understanding speech, or (iii) judging speech from a communicative point of view" (Lieb 1992: 61–62). This is a means of fulfilling the respect constraint mentioned above. In addition, Lieb's view also pitches linguistic reality at the appropriate level of types, thus fulfilling the second desideratum as well.

Modified Realism is constituted by a specific construal of the respect constraint. It maintains that the *objects* of linguistics are extra-mental abstract entities but that the *data* (or source of data) of linguistics can be mental states. What is the relationship between these objects and the data? The answer is that mental states have *contents*. "Mental states and events have a content; the content is not considered an actual part of the states or events" (Lieb 2018: 83). These contents are abstract and extra-mental and thus appropriate objects of linguistic theory, the states of which they are not a proper part are spatiotemporal and thus serve only as data for such theories.[18]

The final aspect of Lieb's theory, relevant for this discussion, is a version of functionalism for speech acts with a proviso that such a functionalist account can be extended to language systems in general. In the following section, I aim to provide insights that dovetail with this latter possibility. Specifically, functionalism in the philosophy of mind acknowledges the definitive roles played by mental states in their characterization. Similarly, structuralism in mathematics identifies mathematical objects by the roles they play in larger mathematical structures. More details forthcoming in the following sections. Admittedly, my view does involve a more ontologically committing position in terms of the objects of actual structures.

18. Another mixed realist account which makes similar distinction is Stainton's 'Linguistic Pluralism' (see Stainton 2014). He distinguishes between difference senses of physical, mental and abstract. Physical$_1$ is related to the use of the word 'physical' in the hard sciences such as physics, i.e. *quantities*. Weeds, defined as unwanted plants, would not count as physical on this definition. On an extensional physical$_2$ definition, weeds show up since they have spatio-temporal and other physical properties. Mental$_1$ includes individual mental states such as pains and hallucinations etc., these too have physical$_2$ status ("[o]n a materialist position, mental states and events are spatiotemporal and concrete" (Lieb 2018: 85). Mental$_2$ involves a specialized notion of secondary qualities conditioned by the mental but not identifiable with mental items. Stainton uses the term 'mentally conditioned' to capture this variety of mind-dependence. Another way to go would be to separate the content of mental states from the mental states themselves (i.e. Lieb's route). Lastly, he contrasts abstract objects *qua* Platonic objects, with what he calls 'abstractish' objects, neither in the mind nor concrete particulars. Musical scores, models of cars and legislation form part of this latter category. The linguistic structures of the following sections might be amenable to such interpretation.

I think the best way to think of Lieb's framework is as a specific variety of Mixed Realism, of which another alternative will be provided in Section 4. The task of the next section, however, is to discuss the advantages a Mixed Realist perspective might have over its Platonist rival.

3. Against Platonism

In this section, I will be rather brief since my argument is straightforward. Simply put, Platonism is not the best way of capturing the three desiderata placed on linguistics above. I will start with an argument to the effect that the Platonism often associated with Linguistic Realism (but by no means necessarily for it) does not account for either the creativity or the kind of infinity I take to be associated with linguistics. Then I will argue on the basis of Benaceraff's famous dilemma for mathematical truth (1973) that the respect constraint cannot be met by Platonists in any plausible way and therefore as with mathematical Platonism a gulf is created between the truth of our linguistic theories and our knowledge of this truth (competence). Lastly, I will make a general claim (following Soames (1984)) that mathematics (as well as logic) and linguistics are conceptually distinct and if indeed linguistics is a formal science, it is a *sui generis* one.

Before we get to this task, however, let us review what the Platonist position is. Essentially, Platonism is an account of linguistic foundations which holds that linguistics is the study of abstract mind-independent objects. The Platonist takes all of the syntactic and semantic (and morphological, phonological and grapholinguistic, see Neef 2018) structure posited by grammars not merely as useful tools for describing mental states or physical tokens but as constituting an independently existing linguistic reality. A natural language, like a formal language, is an abstract object in the sense of being non-spatio-temporally extended and comprised of sets of sentences. On the view we have been considering (that of Katz and Postal), sentences are ontologically similar to numbers, sets and geometric figures.[19] Natural languages are simply systems of these sentences, describable by us through reason and intuition *a priori*.

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19. Postal (2003: 237) states that "an NL is a set-theoretic object, a collection, in fact, a bit more precisely, a collection of sets, where each set is a complex object composed of syntactic, semantic, and expression objects. The traditional term for these sets is 'sentence', so that it is appropriate to say that an NL is a collection of sentences".

3.1 The right kind of 'wrong view'

In this section, I aim to distinguish my view from what a naive Platonist conception of language might look like. An aspect often assumed of the Platonist position (dubbed the 'Wrong View' by Fodor (1981)) is that it contends that there is a static universe of natural languages (and the sentences of which they are comprised) atemporally existing independently of human beings and language users and somehow known to us *a priori*. However, this view is not necessarily a part of Linguistic Realism (see Neef 2018) and thus the following criticism is rather limited in scope and designed to show how a Linguistic Realism or Mixed Realist view can avoid certain philosophical pitfalls associated with Platonism *simpliciter*. Nevertheless, it might be useful to separate the present view from what linguists in the mentalist camp often consider to be the Platonist position (see Chomsky 1990 on P-languages). On this naive view, we discover languages, we do not create them. Much like numbers and sets exist independently of mathematicians who study them or the bean counters who use them, if there were no speakers or users of natural languages, there would still be natural languages and sentences.

Once this metaphysical point is appreciated, I think naive Platonism's incompatibility with the type of creativity discussed previously can be gleaned. In Section 2.1.1. we looked at creativity in language and its role in linguistic theory. I argued that it involved the use and appreciation of novel sentences (to the user), the manipulation of composition rules and the indefiniteness of the number of expressions for which it allowed. The problem is that according to one variety of Platonism (the one mentioned above), every sentence of every language exists in an atemporal sense prior to being used or thought of. This opens up a new question. Is the mere instantiation of existing objects through production or comprehension to be considered novelty? A child counting to a previously uncounted number might be performing an impressive feat but it would not be deemed 'creative' in the sense that the term is used in linguistics. Of course, there is certainly an argument to be made that the mastery of sentences as Platonic objects is 'creative' in the sense above much like a theorem of mathematics still possesses insight whether or not it illuminates an ever-existing mathematical object or property.

Nevertheless, naive Platonism (not identical to Linguistic Realism necessarily) bears the burden of defining creativity in the absence of some more ontic notion of creation. Certainly, this sort of Platonism can accommodate a notion of novelty-to-a-speaker similar to the new number-to-a-counter but the stronger notion (involving genuine creation) would be inaccessible on this ontological account. And if we are redefining creativity in light of this view, then we should at least admit that the subject has changed from the concept discussed by Chomsky, Evans and others.

Platonists might want to bite the bullet on this one. But I think that it is related to a different issue in terms of linguistic methodology as is evinced by the problem of infinity often thought to be a virtue of Platonism. Part of the motivation for Platonism was to better capture infinity claims and the 'vastness of natural language'. In fact, Katz (1996) argues that without Platonism, the vastness result of Langendoen and Postal (1984) (the Cantorian proof that the cardinality of natural language is the size of the continuum or a proper class) does not go through as an objection to generativism or competencism. It seems as though Platonism and infinity go hand-in-hand conceptually. So how then, can I claim that Platonism is at odds with linguistic infinity?

My contention is that the infinity with which Platonism provides us is the wrong kind of infinity for linguistics, which is better underpinned with a rather more constructivist approach to infinite expression in my view. Before I present my case, it is important to remember that we are in the interpretation game not the revision one. Part of the merit I attributed to Katz and Postal's tenacious defense of Realism was due to their bold honesty in the face of often ideological opposition. In this section, I argue that linguistic infinity is not to be understood *statically*, as *per* naive Platonism, but rather *dynamically*, as *per* constructivism (or even strict finitism).

A brief history of the foundations of mathematics might be in order here. Constructivism, or intuitionism, starts with the idea that mathematics is the product of human thought and therefore should be accessible to human mental capabilities. Iemhoff (2015) describes Brouwer's initial conception as follows.

> The truth of a mathematical statement can only be conceived via a mental construction that proves it to be true, and the communication between mathematicians only serves as a means to create the same mental process in different minds.

A famous example is the rejection of rule of double-negation in classical logic which states the following equivalence: $\neg\neg p \equiv p$. Intuitionistic logic rejects this rule since the proof of the negation of a negated sentence is not the same as a proof of the sentence, or as Heyting (1956: 17) put it: "a proof of the impossibility of the impossibility of a property is not in every case a proof of the property itself". One consequence of the above reasoning is the failure of the law of excluded middle in intuitionistic logic. The reasoning goes that since there are statements in mathematics (such as the Continuum hypothesis or the Riemann hypothesis) for which there is neither a positive proof nor a refutation (nor a clear path to either), and since having a refutation means being able to show the positive proof false, the principle cannot hold in every case (and at every time). The underlying intuitionistic move responsible for the various departures from classical logic mentioned above (and beyond) is the link between truth and knowability present in the framework.

The notion of proof and construction appear within this redefinition of mathematics through the relocation of the human mathematician to the subject role in the mathematical process. For example, in Hilbert (1899) the claim 'one can draw' in geometry is taken to be synonymous with 'there exists'. Here again we see why classical principles such as excluded middle fail. Existence claims in intuitionism are equivalent to the production of exemplars and there are certain claims (such as the Continuum hypothesis etc.) for which we cannot do so (nor produce refutations). This is in turn coupled with a mentalistic approach to construction. As Heyting notes,

> Isolating an object, focusing our attention on it, is a fundamental function of our mind. No thinking is possible without it. In isolating objects the mind is active. Our perception at a given moment is not given as a collection of entities; it is a whole in which we isolate entities by a more or less conscious mental act.
> (Heyting 1974: 80)

Naturally, much of the philosophical motivation behind constructivism and intuitionism centered around the concept of infinity. The idea of an infinite series incapable of comprehension in its entirety by a human mind was contrary to the core precepts of this position. For instance, instead of starting with the successor function and the axioms of Peano arithmetic, for the intuitionist the natural numbers start with the process of counting. According to Heyting, this is the mental process of isolating perceptions of entities and then creating more of these entities in one's mind (and in time, importantly). A fuller survey of intuitionism in mathematics is unfortunately outside the scope of the present work. I do, however, want to draw a comparison between this picture of mathematics and the initial idealizations of the nature of linguistics as a science. Shapiro offers us a helpful way of thinking about constructions.

> I propose that we think of the constructions as performed by an imaginary, idealized constructor, obtained in thought by extending the abilities of actual human constructors. Then we can sharpen dynamic language and the various 'construction problems' by articulating exactly what abilities are attributed to the ideal constructor. (Shapiro 1997: 184)

The idea is that we can interpret dynamic talk of 'constructing' mathematical objects or following mathematical rules in terms of these ideal mathematicians not limited in the same way as actual mathematicians are. Thus, certain moves might still not be permitted by intuitionists (such as inferring p from $\neg\neg p$) but we are also not stuck in the very literal readings of such talk (bound by actual performance). Compare this to the opening lines of Chomsky's *Aspects of a Theory of Syntax*.

> Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its (the speech community's) language perfectly and is unaffected by such grammatically irrelevant

> conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of this language in actual performance. (Chomsky 1965: 4)

Indeed, much of the talk surrounding the concept of generative grammars, recursively enumerable sets and discrete infinity is constructivist in linguistics. An ideal speaker is capable of expressing an infinite number of sentences of her language (has a generative grammar in her mind), but the infinity in question is a constructive not an actual one. It is the product of mental competence, it is a mental activity like counting is for intuitionists such as Brouwer and Heyting.[20] The ideal speaker is following a procedure set out by the rules of her grammar or the language, in that sense, provides instructions to performance systems (see Chomsky 2000). In addition, with this understanding of infinity, novelty can also be rescued. We, as human language users, genuinely create the structures of our languages as we produce and comprehend them. Thus, new sentences can be produced by following certain rules (the rules of the grammar of our language). In this way, linguistic infinity is understood as an infinite capacity to produce sentences of the language.

3.2 Benacerraf's dilemma and respect

The failure of the respect constraint, I argue, is due to another issue with Platonism in the philosophy of mathematics. The problem was famously identified by Benacerraf (1973) and has significantly altered the landscape in the foundations and philosophy of mathematics since. The dilemma posed by Benacerraf makes the claim that the quest for mathematical truth pulls in two opposing directions with relation to a uniform semantics and a (causal) epistemology. The argument takes the form of placing two demands on any theory of our knowledge of mathematics. Namely, that

> (1) the concern for having a homogeneous semantical theory in which semantics for the propositions of mathematics parallel the semantics for the rest of the language, and (2) the concern that the account of mathematical truth mesh with a reasonable epistemology. (Benacerraf 1973: 661)

Benacerraf held that all (or most) accounts of mathematical truth fail to find the appropriate balance between these two demands, in fact more than that, the

20. Pylyshyn (1973) makes similar comparisons between Chomsky and intuitionists like Heyting. Chomsky (1982: 16) himself states that "[o]ne could perhaps take the intuitionist view of mathematics as being not unlike the linguistic view of grammar".

demands seem inversely proportional in these accounts.[21] Consider Platonism, in providing a standard truth-conditional semantic account which dovetails with the semantics for the rest of language, Platonists make reference to abstract objects. In other words, the truth of mathematical statements about numbers, sets and the like is determined by their correspondence to abstract objects, non-spatio-temporally extended, in a similar way to how reference to physical objects is supposed to be fixed (in a Tarski-style semantics). However, in providing such a semantic account, we cannot begin to make sense of our causal contact with the former objects (by definition) and thus are left with no (causal) account of our mathematical knowledge the likes of which we have for ordinary physical objects. In the opposite direction, empiricist accounts of mathematical knowledge tend to root it in the familiar physical causal world (the Hilbert program or Devitt's analysis for linguistics) but fail to then specify how the necessary truth of these objects is obtained in a uniform semantics for ordinary discourse.

This is not the place to go into too many details about Benacerraf's dilemma, but suffice to say that by endorsing Platonism for linguistics, Katz and Postal essentially accept its lot.[22] Postal (2003: 251) admits that "[a] formal, abstract object-based view of linguistic ontology, of course, faces the classic epistemological problem often raised in connection with mathematics and logic of how knowledge of abstract objects can be obtained". He defers discussion, however, to Katz' *Realistic Rationalism* (1998). We will get to a discussion of some of these ideas below but for the sake of this dialectic I would like to recast Benacerraf's dilemma in terms of the 'respect constraint' discussed in the previous section.

In the previous section, in accordance with Devitt (2006), I advocated the need for a mixed realist condition on the relationship between the structure rules of grammars and the structures of linguistic competence (whatever these may be). This move was made in part to 'ground' realist accounts of linguistic theories (of the outputs of language comprehension and production) in the mental activities of language users and *vice versa*. In relation to this point, I further argued for an interpretation of all talk of infinity and generative grammars in terms of

21. Of course, many contemporary philosophers of mathematics are unconvinced by this dilemma on either the reference or epistemic side.

22. Katz's (1995) response to this dilemma utilises what I call an *argument from linguistics* (now prevalent in the philosophy of language, see Stanley and Szabo (2000) for one such case), to dismantle Benacerraf's case. He argues that surface form is not always a guide to deep structure (by means of the famous *eager to please* versus *easy to please* case) and that causal theories of knowledge are not the only game in town. His idea is that Platonism needs neither a uniform semantics for countenancing its objects nor a causal theory of knowledge. Unfortunately, in the absence of concrete proposals on either side, this position is hard to evaluate.

constructivist mathematics. In a sense, this condition was suggested (imposed) to prevent language use from getting away from linguistic theory.

The issue with Platonism in linguistics is that, much like the Benacerraf problems for Platonism in mathematics, its ontology pulls in an opposite direction *vis-á-vis* the respect constraint. More precisely, if the structure rules of the grammars designate objects in a Platonic realm, i.e. abstract objects without spatial or temporal dimensions, then how are we to account for their relationship with the physical competence of language users in their use or acquisition of such objects? In other words, how does the abstract ontology of linguistic Platonism account for our knowledge of language, i.e. our linguistic competence?[23] Furthermore, if we take linguistic constructivism seriously, there might indeed be mathematical structures which are incapable of being comprehended by a human mind but surely there are no such linguistic structures. We cannot impose the condition that competence respects the structures of linguistic reality if it is possible that this reality completely outstrips human comprehension. In the other direction, why would linguistic grammar rules or the structures they posit *qua* abstract objects need to correspond in any way to real world constraints any more than higher-order set-theoretic entities should respect our abilities to conceive of them? In this way, intuitionism in mathematics can be interpreted as the attempt to establish a RESPECT constraint on mathematical theory and the mental competence from which it is spawned. Nevertheless, whatever the status of Platonism is for mathematics, it could be seen as posing a particular problem for understanding or respecting the relationship between natural languages and the speakers (or knowers) of these languages. A mixed realist account might fare better in this way.

3.3 Conceptual distinctness

In the previous subsections, I aimed to show that Platonism cannot meet my first and last desiderata of a mixed realist theory of linguistic foundations. In this section, I will briefly concern myself with another corollary of the Platonist view of linguistic objects. This is the view that given Realism, linguistics itself must be a formal science on par with mathematics and logic. In order to show this reasoning to be fallacious, I will apply a similar (realist) strategy employed by Soames (1984) to the effect that linguistics is not cognitive psychology.

The strategy proceeds in the following way. In order to establish that two types of theories are *conceptually distinct*, one has "to show that they are concerned with

23. Again, Platonists would argue that they do not need to offer such an account.

different domains, make different claims, and are established by different means" (Soames 1984: 155).[24] Challenge accepted.

I think the first two requirements are relatively uncontroversial (although potentially question-begging against Platonists), namely that linguistics and mathematics are concerned with different domains and make different claims. Linguists are concerned with natural languages such as English, Swahili, and Tamil. They care about the structures of these languages, their cross-linguistic similarities and differences and how they change over time. When linguists write grammars for specific languages or attempt to capture certain formal properties of various constructions, they are constantly required to make sure that their grammars and properties correspond to actual languages spoken (or signed) in the world. This is accomplished sometimes by means of checking linguistic intuitions (their own and those of other native speakers) or corpus data. To put the point somewhat differently: the linguistics practiced on a planet of speakers cognitively and socially distinct from humans, might look very different from our own, or at least the grammars and constructions might (the linguist's job might still be the same though). Linguists might ask the same questions but the content of their answers would be different. On a standard Platonistic account of mathematical theory, this is not the case. Set theory on earth looks exactly the same as set theory on Pluto or Mars (even if they were populated with different sorts of creatures).[25] I think that this is generally the case because the two types of theories are 'established by different means'. Mathematicians consult their intuitions *a priori* while linguists are bound by certain contingent linguistic phenomena and behaviour (at least in part).

Linguists, like empirical scientists, might use mathematics (as in formal language theory and truth-conditional semantics) as tools or even essential tools but this is different from mathematics as method. Even in its strongest form, the disanalogy persists. Without sets, functions, morphisms etc. linguists might not be able to describe linguistic reality (or competence). But there is a difference between saying 'we can't describe-without-mathematics linguistic reality' and

24. Soames also uses the tool of what he calls *empirical divergence*, i.e. linguistic structures are unlikely to be isomorphic to psychological structures, which on the face of it seems to be in contrast to my RESPECT constraint. Empirical divergence, however, is a much stronger claim on the relationship between linguistic theory and the theory of competence, and respect certainly does not require anything as strong as a morphism or structure-mapping.

25. Of course, these creatures could have a different logic and this might affect the mathematical structures they discover or postulate. But certain structural relations seem to be ubiquitous. Consider group theory which deals with a basic notion of symmetries. By studying the symmetries of structures, we shed light on the nature of these structures themselves whatever they may be.

'we can't describe linguistic reality-without-mathematics'. This is the Berkeley fallacy, mentioned by Yablo (2013: 1016), that statements like 'we can't imagine a tree non-perceptually' do not entail statements of the form 'we can't imagine an unperceived tree'. I would opine that linguistic research constitutes, at most, the use of (perhaps essential) mathematical tools but not necessarily mathematical methodology.

Furthermore, mathematical methods are different from tools. The methods of mathematics involve things like postulation, induction, implicit definition, impredicative definition and construction.[26] Such methods are generally absent from linguistic theorizing and grammar construction. The linguist's job is not done after postulating a mathematical possibility; the possibility only becomes linguistic if it is instantiated by some real world language. For decades, research into finite-state grammars was abandoned due to Chomsky's claim to have shown that such formalisms did not concern natural language constructions in any significant way (see Pullum (2011) for the falsity of that claim). In specific cases, if a particular syntactic or phonological combinations are never realized in any natural language, they lose linguistic relevance. The task of a mathematician has no such empirical restriction.[27] In opposition to this, Postal (2003: 240) claims that there are natural languages for which no knowledge exists (or even could exist).[28] To be a natural language is just to obey certain constitutive laws and if we can specify an object that obeys these laws and is unlearnable, then there are unlearnable natural languages. This is an implicit definition and a corollary of the Vastness theorem (see Langendoen & Postal 1984). Still, it is not clear to me why learnability is not one of the constitutive laws of natural languages as formalisability might be for their formal counterparts. In addition, allowing for such unrestricted uses of implicit definition violates the *respect* constraint.

Linguistics certainly seems to use mathematical tools in identifying the properties of its objects (as do many sciences) but it does not seem to mathematically define the objects of its inquiry *a priori* or rather use mathematical methods. In Lewisian terms, linguistic and mathematical objects seem to be *orthogonal* to one another or as I have put it (following Soames), the fields are 'conceptually distinct'.

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26. See Chapter 5 of Shapiro (1997) for an overview of the place of these methods in the history of mathematics.

27. This point is debatable. Although much linguistic research focus has been spent of the discovery of universals (following Greenberg 1963), unrealized patterns could also shed light on realized structure.

28. This might be a point at which Postal's idiosyncrasy diverges from the strict Linguistic Realist position.

Of course, one could argue that not all formal sciences are alike and linguistics is unique (a similar line is taken in Katz (1981)). In the rest of the paper, I aim to lend some credence to this idea.

4. *Ante Rem* realism and the foundations of linguistics

So far I have argued that Platonism (and nominalism) do not aim to capture certain conditions or desiderata of a mixed realist interpretation of linguistic theory.[29] I proffered these desiderata in accordance with arguments presented for these very positions. What remains to be shown is that there is an alternative that can account for (1) linguistic creativity and infinity, (2) the appropriate level of abstraction present in current linguistic accounts or grammars and (3) both the separation of linguistic reality from competence and the mutual respect constraint between them.

In the following sections, I will describe a view of the foundations of linguistics in terms of a non-eliminative structuralism similar to that offered for mathematics by Shapiro (1997) and independently by Resnik (1997), I call this view *ante rem* realism. I hope to show that the ontology that this position brings with it is coherent in the spirit in which Platonism was offered but does not suffer from some of the problems as described in the previous section, such as Benacerraf worries. Furthermore, this account allows for a more naturalistic interpretation of linguistics as an empirical science with formal aspects by offering an alternative account of abstract objects.

4.1 Mathematical structuralism

The motivation behind mathematical structuralism can be traced back to Benacerraf and the dilemma he presented (see Section 3.2). The core idea of this foundational picture in mathematics is that mathematics is a theory of structures and systems of these structures. In this way there is a shift from the traditional (perhaps) Fregean concept that numbers, sets and other mathematical entities are abstract objects, unidentifiable in space and time. The important insight is that it is structures and not objects which are the vehicles of mathematical truth (and knowledge). This presents an entirely different conception of the nature of the enterprise as well as the concept of a mathematical object itself. Structuralism is a broad framework with historical antecedents ranging from the Bourbaki group

29. Stainton (2014) can also be thought of as aiming for a mixed realist account of linguistic theory in which physical, mental and abstract(ish) objects are countenanced.

and Dedekind to Hilbert and even Benacerraf himself. Thus, there are a number of varieties of the idea at work within the contemporary philosophy of mathematics. I will try to stay as broad as possible for the moment, although I do plan to endorse and develop a particular variety of what is referred to as *ante rem* or non-eliminative structuralism for linguistics in the next section.

In order to understand this view on the foundations of mathematics, we need to answer a few preliminary questions. Firstly, what are structures on this view? And how do they relate to traditional objects of mathematics? Secondly, whatever they are, how do we come to know about them? Then finally how does understanding mathematics as a theory of these structures get us out of Benacerraf-types worries? I hope to provide some potential answers to these questions in this section.

Shapiro starts his book with the slogan 'mathematics is the science of structure'. He continues by way of example,

> The subject matter of arithmetic is the *natural-number structure*, the pattern common to any system of objects that has a distinguished initial object and a successor relation that satisfies the induction principle. Roughly speaking, the essence of a natural number is the relations it has with other natural numbers.
>
> (Shapiro 1997: 5)

This holds true for groups, topoi, Euclidean spaces and whichever mathematical structure is studied by mathematicians. Let us focus on the natural-number structure for a moment and consider its objects. What is a number on this view? Essentially, it is nothing more than a place in a natural-number structure. The only way to talk about the number 2 or 5 or 4892001 is with relation to other places in that structure, i.e. 2 is the successor of the successor of 0 or the number 2 is the third place (if we start from 0 as Frege did) of a natural-number structure, it is in the second place of an even-number structure and the first place of a prime number structure and so on. The same holds for other mathematical objects, the idea being that these objects are only interpretable in accordance with some background theory. As Parsons (2004: 57) puts it, "the idea behind the structuralist view of mathematical objects is that such objects have no more of a 'nature' than is given by the basic relations of a structure to which they belong".

The concept of a group is often taken as a canonical example of a structure. A group *G* consists of a finite or infinite domain of objects and a two-place function called the group operation. This function satisfies four properties (or axioms). It is associative (associative property), there is some identity element (identity property), it is closed (closure property), and every element in the domain must have a reciprocal or inverse (inverse property). Now there are many different types of groups which mathematicians may wish to study. We could look at finite groups (groups with finite domains) or Abelian groups (groups whose elements are also commutative). The basic group structure is the same and the structure is given to us by the relations its objects have to one another (according to the four properties). The objects themselves are of no importance to us, they might as well be point-particles, Martians, jelly-beans or Rice Krispies, it doesn't matter.[30] What matters is the structural relations one object (whatever it is) has to another in the group, we only care about the structures. In fact, we can even talk about structures in isolation from any objects. Shapiro characterises his own position in the following way.

> The first [*ante rem* structuralism] takes structures, and their places, to exist independently of whether there are any systems of objects that exemplify them. The natural-number structure, the real-number structure, the set-theoretic hierarchy, and so forth, all exist whether or not there are systems of objects structured that way. (Shapiro 1997: 9)

The other versions of structuralism offer similar accounts. They differ, however, in important respects. For instance, the question of whether or not structures can themselves be considered mathematical objects. For set-theoretic structuralists, inspired by model theory, the answer is yes. Structures are set-theoretic entities themselves. For modal structuralists, structures are not objects of study. Hellman (1989) utilizes this framework to avoid reference to individual mathematical objects altogether (by replacing such talk with talk of *possible* mathematical objects or number-systems in his case); it is thoroughly eliminative. The point is that there is no one answer to the question of the nature of structures themselves, different structuralists will provide radically different accounts. Another question concerns the background logic, which varies from first-order with identity to second-order and modal logic given different accounts of structuralism.

We have looked at the question of what structures are and what traditional mathematical objects are within them, i.e. merely places-in-structures devoid of individual meaning or importance. The last question to confront in this section is how this framework aims to avoid Benacerraf's dilemma. Recall that Benacerraf's claim was that the more uniform the semantics, i.e. the more the objects of mathematics were treated on par with the objects of ordinary discourse in terms of reference, the further we get from a tractable epistemology. The semantic problem was that we were forced to the treatment of abstract objects as singular terms referring to non-spatio-temporal entities. This created an ontological gap untraversable

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30. Compare this to the desciption of a category in category theory. "A category is *anything* satisfying the axioms. The objects need not have 'elements', nor need the morphisms be 'functions' […] we do not really care what non-categorical properties the objects and morphisms of a given category may have" (Awodey 1996: 213).

by standard causal accounts of knowledge. But with structuralism, there is no such reference since there is an ontological difference between an object and a place in a structure. Neither numbers nor sets commit us to individual abstract objects (as with Platonism),[31] but merely to places-as-objects in natural-number structures or set-theoretic structures. The existence of these kinds of objects is provided by the axioms (as we saw with group theory) or relational properties of the structures. These axioms and structural relations, in turn, can be known by us in a presumably sounder epistemic manner.[32] I shall leave matters here for now and more details will follow when we consider a specific structuralist proposal for linguistics in the next section.

4.2 Linguistic structures

Previously, I described a general framework, neither obviously Platonist nor nominalist in nature, which confronted Benacerraf's dilemma by eliminating the need for reference to individual abstract objects. Importantly for our purposes, the *ante rem* structuralism of Shapiro and Resnik is a realist theory of the foundations of mathematics. As Shapiro (1997: 6) states, "as articulated here structuralism is a variety of realism". He distinguishes between two kinds of realism within a model-theoretic semantics (such as Tarski's). 'Realism in ontology' or the idea that singular terms in the language of mathematics denote mathematical objects which genuinely exist and 'realism in truth value' which states that grammatical sentences in mathematics have definite truth values (either true or false). He claims that his version of structuralism is realist in both senses.

In order to appreciate the realism of this proposal, one has to delve into the notion of an 'object' – as a position in a structure – which it incorporates. The claim is that natural language provides as with two uses of the concept. In the one more frequent case, we treat positions as offices or roles, which are multiply realizable in terms of entities. For instance, some uses of *President* or *rook* are examples of these cases. They do not denote individual objects as in *The President has the right*

31. Although they are referential in a manner consonant with ordinary discourse as I will show in the next section.

32. Of course, knowledge of axioms also results in further epistemological questions but of a much different order to knowledge of Platonic objects. For example, for Gödel, the truth of axioms of set theory "force themselves upon us" so much so that "despite their remoteness from sense experience, we do have something like a perception of the objects of set theory" (Boolos 2000: 266). Boolos attenuates this extreme claim somewhat to suggest that perhaps only certain axioms have the desired effect (e.g. extensionality and pairing). Parsons (1990) attempts to pick up on the 'perception' analogy for mathematical intuition and claims that there is indeed a phenomenon which answers to it.

to overrule the senate or *The rook can move three places*. Shapiro calls this 'places-as-offices'. There is another sense of the term in which we treat positions not as the offices or roles they occupy but as genuine singular terms denoting objects. Examples are sentences such as *The President had lunch with the Dalai Lama today* or *The rook ate the queen at d7*. This is the 'places-are-objects' perspective. *Ante rem* structuralism takes this latter concept as primary. Of course, as Shapiro (1997: 11) notes, "[w]hat is an office from one perspective is an object – and a potential officeholder – from another".

Now from the above, we can see how this form of structuralism is realist in ontology and realist in truth value (albeit in a different sense to the 'realism' of Linguistic Platonism). In arithmetic or number theory we take numbers to be objects, but in set theory they are offices. Consider the number 2, "[i]n one system, [finite von Neumann ordinals] {Ø{Ø}} occupies the 2 place, and in the other [Zermelo numerals] {{Ø}} occupies that place" (Shapiro 1997: 11). In either case, the numeral 2 is a name picking out an object *qua* position in a structure and statements involving the numeral are true or false but in neither case are we committed to an individually existing number in the Platonic sense. All we need is for the structure to exist (and there are various ways of ensuring this, see Chapter 3 of Shapiro (1997)). In fact, this example presents one of the advantages of this theory over Platonism. According to Platonists, numbers are individual mathematical objects and mathematical objects are sets. If this is the case, then there is a fact of the matter as to which sets constitute the natural numbers. But von Neumann ordinals and Zermelo numerals have different set-theoretic consequences for numbers, since on the former account '$2 \in 4$' is true while on the latter it is not. How do we decide which theory is correct? With structuralism we don't have to decide, since both theories are true in virtue of being concerned with the same natural-number structure, not the individual numbers and their correspondence to specific abstract entities or individual sets.

The account I offer essentially makes use of the same claims. If 'mathematics is the science of structures', then linguistics is the science of linguistic structures. In this sense, my view is a Linguistic Realistic one, since it admits for *abstracta*. *Ante rem* realism is the position on the ontology of language that states that linguistics is concerned with abstract patterns or structures and grammars are theories of those structures. My account does depart from that of Shapiro (and Resnik) in significant ways by specifying what kind of *abstracta* linguists are committed to. Consider the following remark made by Resnik concerning linguistics.

> Take the case of linguistics. Let us imagine that by using the abstractive process […] a grammarian arrives at a complex structure which he calls *English*. Now suppose that it later turns out that the English corpus fails in significant ways to instantiate this pattern, so that many of the claims which our linguist made

> concerning his structure will be falsified. Derisively, linguists rename the structure *Tenglish*. Nonetheless, much of our linguist's knowledge about *Tenglish qua* pattern stands; for he has managed to describe *some* pattern and to discuss some of its properties. Similarly, I claim that we know much about Euclidean space despite its failure to be instantiated physically. (Resnik 1982: 101)

In linguistics we seem to be concerned with a specific class of structures, those which are instantiated in the real world. These are the structures that are produced by human linguistic competence, i.e. the outputs of competence. In this way, I amend the structuralism of Shapiro to include what Parsons (1990) calls quasi-concrete objects. These objects or positions-in-structures, in my view, are comprised of a mixed ontology. Parsons offers the existence of such objects as an objection to structuralism but I see no serious reason for why this cannot be compatible with it for the case of linguistics (Shapiro himself takes this concept as a friendly amendment). Parsons (1990: 304) states that there are "certain abstract objects that I call quasi-concrete, because they are directly 'represented' or 'instantiated' in the concrete" and he includes as an example of such an object "symbols whose tokens are physical utterances or inscriptions". The idea is that there is an additional relation to the axioms of such structures that goes beyond pure structuralism, a 'representational' (or instantiation) relation. I think that this third kind of ontological category merely marks the boundary (which is vague) between the structures of pure mathematics and those of applied sciences in which I place linguistics. This marks a departure from the Realism of Katz and Postal but not from commonsense to a certain extent. Consider Boolos' comments below.

> Numbers do not twinkle. We do not engage in physical interactions with them, in which energy is transmitted, or whatever. But we twentieth-century city dwellers deal with abstract objects *all the time*. We note with horror our *bank balances*. We listen to *radio programs: All Things Considered* […] Some of us write *pieces of software* […] And we draw *triangles* in the sand or on the board. Moreover bank balances, reviews, palindromes, and triangles are 'given' to us 'in experience,' whatever it may mean to say that. (Boolos 2000: 265)[33]

What Boolos calls 'abstract objects', I call *quasi-concrete*. And 'what it means to say that' they are 'given to us in experience' is just to say they have either instantiation or representation relations in the concrete. The difference between Boolos' list and

33. Within the context of linguistics, Stainton seems to describe a similar class of objects. "There is another sense of abstract, however – namely, things that are not inside the mind yet are not concrete particulars either. They are neither fish nor fowl. Let me coin the term abstractish for these" (Stainton 2014: 6). Within this list he mentions objects very similar to those found in Boolos' catalogue above.

linguistic (and some mathematical) objects is that many of the abstract objects on his list are fully determined by the physical objects to which they relate whereas linguistic objects, as I conceive of them, have a generally structural nature in addition to concrete instantiation or representation.

In *Realistic Rationalism* (1998), Katz offers a similar account for what he calls 'composite objects'. Examples of objects like the equator or impure sets (which have physical objects as members) push him towards accepting a third metaphysical category of objects. These are not just objects with dualist parts or feet in both worlds but they stand in a 'creative' relationship with one another, i.e. their composition creates a new object distinct from either part.[34] For instance, the equator is neither a perfect circle nor a line that exactly bisects the circumference of the earth, since "[i]t didn't exist before the earth was formed and will cease to exist when the earth ceases to exist" (Kaufman 2002: 219). In terms of impure sets, in *Skeptical Linguistic Essays* (2003), Postal identifies classes of sentences, involving direct discourse, whose ancestral elements actually include physical objects.

> This entails that the sets that comprise NL sentences must be able to contain as members or submembers something that can instantiate the endlessly distinct physical properties involved in direct speech. The only way I see that this can be the case is if direct speech segments involve sets that contain the physical properties themselves and not, as in the case of more standard (regimented) linguistic elements, symbols that represent instructions (to a fixed physical apparatus) to produce physical things. (Postal 2003: 193)

My account in some ways corresponds to the position Katz and Postal suggest at times despite differing from the one they officially endorse. Furthermore, I think that this is a very intuitive picture of the science of linguistics. What after all is syntax, if not the study of the structural relationships between sentences and their subphrases? Of course, these structures should be additionally exemplified by real world languages but this is merely the addition of the respect constraint for which I argued earlier. The syntax of a particular language is an abstract object much like the *University of St Andrews*. Following Ryle, we cannot ask where the university is exactly since it is the organization of different ever-changing units, it is a quasi-concrete structure. The positions various buildings occupy could change, the chemistry building could house the biology faculty at some stage and thus change its assignment, some buildings can be removed and others erected. If the entire structure is destroyed, then it no longer exists *in toto*. But it existed once in

34. This creation relation vastly overgenerates and thus in the end fails to maintain the concept of a concrete object since concrete objects stand in indefinitely many relations to abstract objects. See Kaufman (2002) for details.

a temporal and partially physical sense. The syntax (and semantics) of a particular natural language is similarly abstract, it is the organization of linguistic units or sentences in terms of their structural relationships to one another. If the language dies, so do the systems (physical instantiations of structures) which governed it. Of course through records we could still study the language on a more abstract / formal level as with the University blueprints, we could even resurrect the language based on the structures as in the case of Hebrew. Hale (1987) assumes that natural languages, like mental states, have temporal parts notwithstanding their lack of physical dimensions.

We can now see that this account can meet all of the desiderata of a realist theory of linguistics. Linguistic creativity and infinity are easily represented as there are no size limits to the linguistic constructions we employ. In addition, we can avail ourselves of the *dynamic* discourse of constructivists, as the linguistic structures which we create as language users could be conceived of as direct products of our mental faculties, despite being amenable to study independent of those faculties. Much like the natural-number structure could have been created or constructed by initial counting procedures of human agents through abstraction (see Shapiro (1997) Chapter 4 for a suggestion and Resnik (1982) for a more speculative account), natural language patterns or structures could have been created by the dual need for thought and communication among human cognizers. The rules of either activity lead to a potential or constructive infinity.[35] In terms of the appropriate 'level of abstraction', we have an arguably more sound account than Platonism offered us. After all, *ante rem* structuralism drew inspiration from the classical position on universals and particulars (as Hellman calls structures on this view '*sui generis* universals'). Unlike the previous dualist picture, we have a potentially naturalistic picture available to us. Linguistic grammars are concerned with sentences as positions-in-linguistic-structures. Immediately, we do not run into Benacerraf-type worries about how we as physical beings use abstract objects like sentences if they are not extended in space-time. Sentences, like numbers, have purely relational and structural components, c-command, governance, scoping relations etc. But unlike numbers, I argue, sentences are part of quasi-concrete structures which include instantiation relations. In the same sense as the non-eliminative or *ante rem* structuralism discussed above, sentences on this account

35. The research of Simon Kirby is especially interesting with relation to this point. Kirby (1999) designed a series of experiments to computationally test the emergence of structure in a population over time with the result that "[t]he simulation results […] show that compositional, recursive language emerges in a population which initially has no language […] Purely through the process of being repeatedly mapped from an internal form as a grammar to an external form as utterances and back again, language evolves" (Kirby 1999: 14).

are *bona fide* objects (in the places-as-objects sense) and linguistic statements concerning them have definite truth-values. Thus, sentences are not to be taken as tokens or 'words on a page' and 'sounds in the air' or mental states for that matter but abstract objects conceived as places or positions in linguistic structures which are in turn represented or instantiated by those tokens.

Once again, the emerging picture seems rather intuitive in light of actual linguistic practice. Consider a determiner phrase (DP). On most syntactic accounts, it is a structurally designated linguistic item in a hierarchical structure or tree, and any word or object (sometimes nothing, as in the case of null determiners) can satisfy the position. And whatever is in that position is a DP. The postulation of covert material is usually supported by structural reasoning in linguistics, i.e. something must be there since this structure requires it or it stands in a structural relation to something else. The UG hypothesis itself can be considered structuralist in that it aims to discover the underlying structures of the human faculty of language, the particular items or objects of various languages are rendered inconsequential (this is often a criticism of the claim). Furthermore, consider Jackendoff's Parallel Architecture, a highly modularized account of the language faculty which consists of various individual generative systems with interface principles or relations between them. On this view, the syntax is not the only generative system (as it is with traditional generative accounts) but semantics and phonology are systems (or "a collection of objects with certain relations" (Shapiro 1997: 73)) in their own right. The interfaces are concerned with the structures, i.e. the systems at a higher level of abstraction, where non-relational elements are ignored.

With relation to realism, one significant advantage of this foundational framework is that it can provide an answer to Quine's (1972) famous challenge to Chomsky concerning equivalent grammar formalisms. Quine's challenge was initially posed to a conceptualist framework, i.e. if two grammar formalisms are weakly equivalent (generate the same set of sentences) then how can we divine which one is cognitively realized in the human mind/brain? Similarly for the Platonist, if sentences are sets and two weakly equivalent grammar formalisms pick out the same sets of sentences (sets of sets), how can we tell which sets constitute the language in question? This is essentially a parallel of the arithmetic case involving the finite von Neumann ordinals and the Zermelo numerals (and also Benacerraf's (1965) objection to Quine's version of Platonism). The answer for the *ante rem* realist is analogous, they both pick out the same natural language structure and thus we have no reason to decide between them.

Another related aspect in favour of this view over its Platonist alternative might be the level at which languages themselves are pitched. As previously mentioned, sentences are abstract objects for Platonists. But so too are languages as they are defined as ‘systems of sentences’. As Carr (1990: 123) put it, “while it is perfectly reasonable to assume that sentences are linguistic objects and thus susceptible to such Platonic interpretation, it is rather novel to argue that particular languages […] should be taken to be objects of linguistic theory”. The worry here is that even if we consider the notion of ‘sentence’ to be a theoretical one, considering an entire language as a theoretical concept seems singular.[36] Generally, the boundaries between external languages like Dutch, English and German are not sharply defined. Likewise, the Platonist claim is that there is a fact of the matter as to which distinct abstract objects (or sets) Serbian and Croatian correspond to respectively. However, languages in this sense are often politically defined and classified (hence Chomsky’s initial reservations about E-languages). In general, these types of languages are within the realm of sociolinguistics and not objects of grammatical theory. On the *ante rem* realist account, Serbian and Croatian, Urdu and Hindi and other such cases have structural overlap. The systems of sentences to which our grammars of these languages correspond are the same or similar natural language structures identified by syntactic theory; they need not be identical to achieve this end nor need there be a fact of the matter as to which structures they correspond to exactly.

For the last desideratum, I propose we treat the instantiation relation of our quasi-concrete linguistic structures as the RESPECT constraint itself. Thus, the way in which our linguistic structures or patterns are instantiated in the physical world is by respecting the rules of our competence and by those same rules respecting the rules of the structures in turn. This could be achieved by persisting with the idea that the quasi-concrete linguistic structures are comprised of sentences which are the output of our linguistic competence but distinct from that competence, like the waggle dances of Devitt’s bees. I think that on this view we have even more options than these available to us for capturing the interdependence of structure and mind while maintaining their distinct natures. Furthermore, if linguistic structures are the outputs of competence and competence is within the evolutionary order of things in the physical world, then given the RESPECT constraint, our linguistic structures are also related to a naturalistic story of language evolution. However, much more needs to be said about this matter before it could be considered an advantage over rival views.

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36. Of course, Platonism about language is not a theoretical point about how we should treat natural languages but an ontological point about what they in fact are. If I am correct in my interpretation of Carr’s point, then his line might be somewhat misguided.

4.3 Natural types

In Section 2.4., I promised that I would show that Platonism incorporated a type-token distinction that might question the correct level of abstraction of linguistic theory. In the previous section, I suggested a different (Aristotelian) notion of this distinction in terms of quasi-concrete structures in which "the relation of linguistic types to their tokens (and in general of quasi-concrete objects to their concrete 'representations') is not an external relation" (Parsons 1990: 337). Admittedly, this commits us to *abstracta* of a certain sort. I further claimed that this account of the requisite abstraction level was more in line with the *ante rem* realism I proposed for the foundations of linguistics as well as some comments and accounts suggested by Katz and Postal themselves. Despite the fact that a mixed ontological attitude towards abstraction is well-supported in the literature (Hale 1989, Parsons 1990, Stainton 2014 etc.), a Platonist could insist that there is no independent justification for jettisoning the clearer traditional account of types as abstract objects and tokens as their physical instantiations. The claim that quasi-concrete structures seem to 'go better' with the ontology I propose is not independent reason for accepting these structures nor sufficient justification for my earlier claim that Platonism fails to do abstraction justice. In this final section, I will make the case for questioning the traditional view of types as non-spatio-temporal abstract objects outside of the causal order. This will require a detour into the philosophy of science. First, however, consider these passages cited in both Katz (1996) and Postal (2009).

> There will ordinarily be about twenty *thes* on a page, and of course they count as twenty words. In another sense of the word *word*, however, there is but one *the* in the English language; […] it is impossible that this word should lie visibly on a page or be heard in any voice. (Peirce 1958: 423)

> ES IST DER GEIST DER SICH DEN KÖRPER BAUT: [S]uch is the nine word inscription on a Harvard museum. The count is nine because we count *der* both times; we are counting concrete physical objects, nine in a row. When on the other hand statistics are compiled regarding students' vocabularies, a firm line is drawn at repetitions; no cheating. Such are two contrasting senses in which we use the word *word*. A word in the second sense is not a physical object, not a dribble of ink or an incision in granite, but an abstract object. In the second sense of the word *word* it is not two words *der* that turn up in the inscription, but one word *der* that gets inscribed twice. Words in the first sense have come to be called tokens; words in the second sense are called types. (Quine 1987: 216–217)

Characterizations of objects such as those presented in the quotations above aim to establish a distinction between abstract and ordinary objects (e.g. tables, chairs, Chomskies etc.). Once this distinction is in place, there are two options for describing the relationship between these respective types of objects. We could go the traditional Platonist route of removing abstract objects from the causal order by conceiving of them without physical and temporal parts. This might be less desirable for the reasons we saw in Section 3.2. Another option is adopting a position called 'Naturalized Platonism' (Linsky & Zalta (1995)). This position makes the empiricist claim that properties and sets and other *abstracta* are well-within the causal order and knowable *a posteriori*. In some ways, Quine falls within this camp by constraining abstract objects through the same principles (such as Ockam's razor) that constrain other theoretical entities. Still we are left in some confusion as to how we come to know these entities in the first place.

In order to offer a genuinely naturalized account of Platonistic underpinnings and abstract objects, Linsky and Zalta (1995) propose what they call 'Platonized Naturalism' (not to be confused with Naturalized Platonism above). The details of this proposal are tangential to my purpose here.[37] However, the aspect of the project that does have significance for the current discussion is their particular identification of the genesis of the issues with the Platonistic positions mentioned in the previous paragraph.

> We believe that there are two mistakes in that conception: (i) the model of abstract objects as physical objects, and (ii) the piecemeal approach to theorizing about abstract objects. (Linsky & Zalta 1995: 9)

The first prong of this analysis is particularly important here and I think the main issue with the some accounts of the type/ token distinction as presented by Quine and repeated by Katz (1996) and Postal (2009) above. Some philosophers have taken abstract objects to be analogous to physical objects (Armstrong, Maddy) within the causal order. "Most Platonists conceive of abstract objects on the model of physical objects. That is, they understand the objectivity and mind-independence of abstract objects by analogy with the following three features" (Linsky & Zalta 1995: 9), namely: if physical objects are 'sparse', then so are the abstract objects to which they correspond; if physical objects are 'complete' as in have more properties than we know and are entirely determinate, then abstract objects are knowable in their entirety and determinate in detail (either true or false for all properties); and lastly if physical objects have 'backsides' or underlying hidden structures, then abstract objects are similarly complex. In some sense,

37. Although a careful reader might note the similar motivations behind this view and Mixed Realism itself. However, within *ante rem* realism, structures can be defined independently of theoretical or naturalistic concerns. For instance, we could be interested the amalgams of unrealized linguistics patterns for the purpose of shedding light on realized ones. These structures can be described outside of the respect constraint.

this picture is natural since abstract objects are often determined by 'abstracting' from physical objects. But this dichotomy brings with it some problems. Linsky and Zalta go as far as to assert that it is the root of Platonism's conflict with naturalism. I take no stance on those issues here. I do, however, believe that the analogy with physical objects is responsible for some of the properties of type/token distinction presented by some Platonists (such as Bromberger (1989)), specifically by forcing a singular denoting term reading of abstract objects analogous to that of physical objects.

If we persist in modelling the type/ token distinction with this definition of abstract objects as abstract physical objects, we might be stuck with an irreconcilable ontology and an epistemological problem as to how we can know the latter in the first place. *Ante rem* realism aims to avoid this particular issue, among other things. For instance, if types are on the level of offices (in the sense discussed above) the analogy with physical objects is dropped, since these offices are not complete, do not have hidden natures and are certainly not sparse (offices can be created *ad infinitum* independently of entities to fill those positions). For instance, for Millikan (2005) two semantic tokens are of the same type only if they are copied from the same pool of linguistic patterns or 'reproducing conventions' within a given community. Once we relax the strict type-token distinction of Quine and Peirce (above), many of the alternatives can be favourably illuminated.

5. Conclusion

Since the late 1950's linguists have discussed linguistic structures, their implementation in grammar formalisms and their interrelations. Very little has been said specifically about what these structures are and how they relate to other non-linguistic structures. I have attempted to give the beginnings of an account here. Much work still needs to be done. Nevertheless, *ante rem* realism provides not only an account of the foundations of linguistics and its subject matter but also aims to demystify the concept of structure used throughout the discipline as an abstract pattern produced by competence but distinct from it in ontology.

The question remains, what kind of science is linguistics? Is it a formal science in terms of mathematics or an empirical science like psychology? On the view I have been pushing, the answer is that it is a little bit of both. One could either take it to be an empirical science with formal aspects or a formal science with empirical aspects (depending on your funding grant). Linguistics lies in the same disciplinary lacuna that most applied sciences do.

In this paper, I have attempted to extend the contributions of Katz and Postal in the foundations of linguistics by considering a mixed ontology and a mathematical structuralist interpretation thereof. In many ways, my project can be seen as a natural progression of the ideas presented in Katz (1981) and defended in Postal (2003). I argued for three conditions or desiderata on a mixed realist account of linguistic ontology in light of critiques found in the Platonist and Nominalist literature, namely creativity and infinity, the correct level of scientific abstraction and RESPECT between the distinct structures of the mind and linguistic world respectively.

I then drew from the philosophy of mathematics to suggest a novel account of the nature of the linguistic enterprise and the natural languages it studies, in terms of an *ante rem* or non-eliminative structuralism with the inclusion of quasi-concrete structures, which I called *ante rem* realism. This account aimed to meet all of the aforementioned desiderata of a mixed realist linguistic account in a way more amenable to naturalism.

However, there are still many questions to answer. Some of these pertaining to *ante rem* structuralism and whether or not it is in fact an advancement on Platonism or simply 'Platonism in disguise' (Hellman 1989)? Other questions pertain to the exact relationship between *abstract* structures and concrete realizations, i.e. between linguistic types and their tokens. In light of these challenges, the present work serves to continue a conversation started by Katz and Postal many years ago and to show that this conversation still has many avenues and insights to offer contemporary theoretical linguistics and its philosophy.

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CHAPTER 7

Autonomous Declarative Phonology

A realist approach to the phonology of German

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In the paradigm of Linguistic Realism, phonology deals with abstract objects. Consequently, phonological units cannot be derived from phonetics as the material side of speech production and reception. I suggest a theoretical approach that conceives of phonology as autonomous from phonetics; hence Autonomous Declarative Phonology. The central questions of this kind of phonology are: What are the phonological elements of a specific language system (in particular: German)? How can these phonological elements be combined in the language under analysis? I give a definition of what phonology is and show how individual phonological units can be motivated. In order to give a model-theoretic reconstruction of phonological sequences, I make reference to the concept of syllable, employing and re-interpreting ideas from generative phonology and other approaches such as hierarchical syllable structure, CV-phonology, and sonority.

Keywords: declarative phonology, minimal pairs, syllable, phonological word, sonority

1. Fields of linguistics

Linguistics is the scientific study of language. Linguists do not agree upon the question what kind of object language is. Saussure (1916) distinguishes the three distinct concepts *langue*, *langage* and *parole*. These French expressions can all be translated into English as *language*. Katz (1981) identifies three different kinds of linguistic approaches with distinct assumptions concerning the epistemological status of language. Based on these differentiations, I regard the following as the central objects of linguistics (Table 1):

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Table 1. Three conceptions of 'language'

language use	use that individuals make of specific languages	empirical object
knowledge of language	knowledge that individuals have of specific languages	mental object
language system	specific language as a system	abstract object

Language in the sense of 'language use' or *parole* is an object that can be observed empirically using scientific methods that are customary in the social sciences. Language in the sense of 'knowledge of language' (a sense that is loosely related to Saussure's term *langage*) is a mental object and as such is studied with methods that are prevalent in psychology. If language can be used, its users have to have some knowledge of it. If, furthermore, people have knowledge of language, language has to have an existence independent of this knowledge. In other words, language is a concept fundamentally different from knowledge of language; knowledge of something always has a different epistemological quality than the object known. These considerations give rise to the third conception of language which is, as stated, 'language as such' (or *langue*). Like the objects of study in mathematics or logic, it is an object of abstract quality. Abstract objects cannot be observed directly; rather, they can only be reconstructed as systems in specific theoretical frameworks. This is the task of theoretical linguistics, while other branches of linguistics deal with knowledge of language and use of language, respectively.

Needless to say, a number of linguists disagree about this conceptualization. Many generative linguists seem to claim that the object of study of theoretical linguistics is a mental object, i.e. knowledge of language (e.g. Chomsky 1986), while others like Harris (1951) seem to follow the idea that language use, hence an empirical object, is the only valid object of study of theoretical linguistics. Katz (1981) denounced such opinions as category errors in linguistics. In the following, I will characterize what phonology is from the viewpoint of theoretical linguistics dealing with the abstract object language. In this sense, the following considerations pertain to the paradigm of Linguistic Realism.

2. A theory for languages as abstract objects

To be more explicit, I regard individual languages as the primary objects of study in theoretical linguistics. Consequently, a specific language is the starting point for linguistic research. The object of the following study is the German language. There are many different kinds (usually termed 'varieties') of what can be called German. Some varieties may be regarded as systems in their own right. This is how

I conceive of dialects on the one hand and older stages of a language on the other. Other varieties may be seen as specific ways of using the standard system. This may hold for registers like youth language or technical language. Without further justification, I take Modern Standard High German (in short: German) to be the object of the present study.

In order to study German as an abstract object (i.e. to reconstruct it as a system), one needs to have a theory of it. Within the paradigm of Linguistic Realism, there may be different competing theories.[1] Theories can be distinguished by the basic assumptions they make beyond the general assumption that the language system is an abstract object, which is the definitional criterion for the paradigm of Linguistic Realism. Such basic assumptions are axiomatic in that they cannot be deduced from within the theory itself; rather, they have to be stipulated. These assumptions may or may not be convincing. What eventually makes a basic assumption convincing is the extent to which it allows for developing a sound theory. A sound theory is one that conforms to the common scientific standards of explicitness, consistency, conciseness, and completeness. Moreover, a theory in Linguistic Realism has to be faithful to the object it intends to model since this object, i.e. a specific language, exists prior to its description. Given that there is no external evidence available to evaluate a theory of an abstract object, it is ultimately the reception of the theory in the linguistic discourse that determines its success.

According to the above considerations, any individual language may be studied in detail without making reference to data from other languages. In this sense, analyses of specific languages are self-contained. In a next step, applying the same theory to different languages may allow for a comparison of these languages. In this way, it may turn out that there are properties that are shared by many language systems or, potentially, by all. Such properties would then be universals. Whether or not universals exist is a result of extensive cross-linguistic research and not something that is given a priori by the framework of the theory.

The specific theoretical framework that I assume is what I call 'Declarative Grammar' (cf. Neef 1996 et passim). The defining properties of this theoretical approach within Linguistic Realism will become clearer as I present an analysis of the phonology of German. In doing so, I provide a model of the assumed structure

1. In the field of phonology, such competing theories exist, though it is somewhat difficult to clearly relate specific phonological theories to the paradigm of Linguistic Realism. This is because few researchers explicitly follow the distinction of general linguistic paradigms sketched in Table 1. Besides Lieb (1998, 2008), I would integrate the approach by Brandão de Carvalho (2002) into this linguistic paradigm as well as attempts following the glossematic tradition founded by Hjelmslev (1961).

of language systems in general. This model follows standard assumptions, particularly those outlined in Bloomfield (1933), even though the relevant terms are defined slightly differently.

In order to reconstruct language as a system, two areas have to be distinguished: one area comprising the regular aspects of the system and the other capturing the irregular ones. These areas are termed 'grammar' and 'lexicon', respectively. The distinction between regular and irregular linguistic expressions is a main feature of linguistic theories and distinguishes linguistics from other sciences like, e.g., physics. Simple signs in the Saussurean sense are irregular as there is no regular relation between the signifier and the signified. A number of other irregularities exist as well. Of course, it is not the linguistic expressions themselves that show whether they are regular or irregular; rather, it is the theoretical analysis that decides upon this classification.

In the regular area of the language system, i.e. in grammar, I distinguish – as is widespread – the four modules phonology, morphology, syntax, and semantics. This gives the following diagram of a language system (Figure 1):

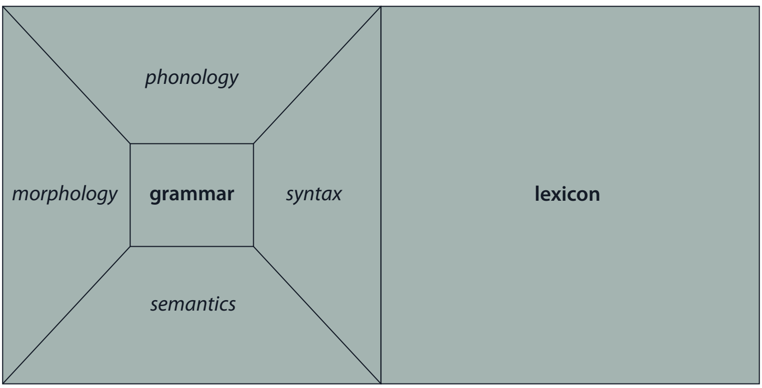

Figure 1. General model of a language system

The central property of language systems is to establish relations between forms and meanings. Among the elements of a language system, there are those that bear meaning and those that merely have the property to distinguish between meanings. Meaning-bearing elements basically come in two kinds: words and phrases. The formal properties of words are subject of morphology while syntax deals with different kinds of phrases, including the sentence as a particular phrase type having a finite verb as the element that determines its nature. Semantics deals with regular aspects of meaning of both words and phrases. Meaning-distinguishing elements, finally, are the subject of phonology, a module of grammar that is treated in detail in the next section.

3. Autonomous Declarative Phonology

3.1 Defining phonology

In phonological theory, there are basically two competing approaches to defining the basic units of phonology: One tradition takes phonetics to be basic and claims that phonological units can be gained by abstraction from phonetic properties (e.g. Chomsky & Halle 1968, Browman & Goldstein 1992, Port & Leary 2005, Becker 2012). Usually, it is the articulatory properties of sounds that are taken to be relevant for phonology, although there is no independent argument why the analysis should be restricted to this subarea of phonetics and not extended to auditory and acoustic phonetics. A comprehensive approach of a phonology grounded in phonetics is hard to imagine, however, because from phonetics alone it is not even clear which phonetic elements should count as pertaining to language (What about groaning, laughing, coughing, clearing one's throat?). Moreover, some phonetic features that are usually used to define specific phonological elements upon closer inspection turn out not to be truly necessary. For example, 'round' vowels do not differ in rounding from unrounded vowels in ventriloquists and 'voiced' sounds are realized unvoiced while whispering. Moreover, when singing, shouting, or speaking with a full mouth, sounds differ phonetically to a large extent from normal articulation, yet their phonology does not change.

From the viewpoint of Linguistic Realism, it should be evident that phonetics cannot be regarded the base of phonology, as phonology belongs to the abstract language system. In contrast, phonetics is a field from the realm of language use. Thus, for principled reasons, phonological units cannot be derived from phonetic units but have to have an independent motivation. In this sense, phonology is regarded as autonomous from phonetics[2] in my approach, an assumption that in part motivates the name of the theory, Autonomous Declarative Phonology. The term declarative refers to the mode of explanation used in this theory, i.e. the use of constraints instead of rules. In this respect, the theory is similar to some approaches to phonology within the generative paradigm developed in the 1990s like Declarative Phonology (cf. Scobbie, Bird & Coleman 1996) and Optimality Theory (cf. Prince & Smolensky 1993), but also, e.g., in work in the framework of Integrational Linguistics (cf. Lieb 1998, 2008).

The other traditional conception of phonology is to regard it as the component of grammar that is concerned with meaning distinction. Obviously, this is an approach suitable for Linguistic Realism. A prominent view of this kind is given

2. Autonomous approaches to phonology also exist in generative linguistics; e.g. Samuels (2009).

in Bloomfield (1933), but the general idea behind it is much older. In Neef (2005), I added the proviso 'have the potential to' to the definition of phonology of the Bloomfield-type, yielding the following formulation:

(1) *Definition of the term 'phonology'*
Phonology is the study of units that have the potential to distinguish meaning in a specific language system. Both paradigmatic and syntagmatic properties of these units have to be investigated.

According to this definition, a unit need not distinguish meaning in all contexts in order to qualify as a phonological unit, as long as it does so in some contexts. Of course, if a putative unit distinguishes meaning in only a few examples, it may be regarded as irregular rather than as a phonological unit proper. However, as there is generally no clear-cut border line between units and exceptions on the factual level, the relevant decision depends – as always – on the detailed theoretical modeling.

In the phonology of German, schwa is the element that shows this ambivalent character most clearly. A pair of words like *Lehre* [ˈle.ʀə][3] 'apprenticeship' vs. *Lehrer* [ˈle.ʀɐ] 'teacher' indicates that the elements schwa and vocalic r [ɐ] serve to distinguish meaning and therefore constitute different phonological elements. Here, phonological representations are given in square brackets, a terminological convention suggested in Vennemann and Jacobs (1982: 36). The representations given count as theory-driven hypotheses; they are not located on a level of empirical fact. For the current purposes, an orthographic representation would not suffice to make clear the crucial differences between the words under consideration.

Schwa not only contrasts with the reduced vowel vocalic r but also with full vowels like in *Motte* [ˈmɔ̣tə] 'moth' vs. *Motto* [ˈmɔ̣to] 'motto' as well as with null like in *Schale* [ˈʃa.lə] 'bowl' vs. *Schal* [ʃal] 'shawl'. On the other hand, it does not make a difference in meaning whether or not schwa is present in a word like *Hafen* 'harbour'; both [ˈha.fən] and [ˈha.fn] represent the same word. The case that the sequence schwa plus sonorant alternates with a syllabic sonorant (without an attendant change in meaning) is known by the term 'alternation condition' in German phonology (cf. e.g. Neef & Neugebauer 2002). A way to analyze this alternation is to attribute the two variants of the word to different stylistic levels. The variant with schwa would then be assigned to the level of 'explicit articulation' while the form without schwa represents the level of 'standard articulation' (cf. e.g. Duden 2016: 51–57). Such levels are subject to different phonological constraints

3. The notation given in square brackets needs justification. The remainder of this text serves this task.

(which is to be further specified in a detailed analysis). In the absence of the words 'have the potential to' in the definition in (1), it would be unclear whether or not schwa in German is a phonological unit.

3.2 Determining the set of phonological units

If phonology is defined as in (1) above, the first task of a phonological analysis of a language system is to determine the set of phonological elements. A heuristic method to identify phonological units is to use minimal pairs, as indicated by the above given pairs. This method played an important role in structuralism but has a much longer history (cf. Coleman 1998: 54). In a realist approach, minimal pairs do not count as proof of the existence of phonological units because it may turn out that a putative unit may better be analyzed as a sequence of two units, or the other way round. Still, they are a good and probably the best heuristic method. Table 2 gives minimal pairs for a sample of five putative phonological units of German (provisionally labeled 'sonorant consonants'). In order to be able to refer to these phonological units more easily, I label and number them consecutively from 41 to 45. These labels are given in square brackets in order to indicate their status as phonological segments.

In this approach, minimal pairs consist of two words (in the sense of grammatical words and not in that of lexemes) with a different meaning and with a phonological representation that is hypothesized to differ in only one segment. Since the method aims at identifying phonological units, morphological features like word class or inflectional category are irrelevant. In Table 2, examples are given in standard orthography as a convenient way (but not a sufficient one) to refer to words. The orthographic letter(s) that correspond to the respective phonological unit are marked by underlining. The first word in the cells containing minimal pairs indicates the phonological unit in the column and the third word indicates the unit in the row. English glosses are given in lines two and four. In Table 2, minimal pairs are given for all phonological units of the sample. Of course, it is not necessary to give such a complete set of minimal pairs in order to motivate the status of a unit as phonologically relevant, but such an exhaustive list may be more convincing than giving one minimal pair only for each unit under consideration.

Along these lines, a hypothesis about the set of phonological units of German can be formulated. The validity of the hypothesis depends in parts on answering the question which words belong to the vocabulary of German and which ones do not, which is in itself a notoriously difficult question that does not have a definitive answer. In order to give a presentation as convincing as possible, I left out foreign words as well as names, interjections, and abbreviations.

Table 2. Minimal pairs for sonorant consonants in German

[41]				
mein ‘my’ nein ‘no’	[42]			
Lamm ‘lamb’ lang ‘long’	sinnen ‘to reflect’ singen ‘to sing’	[43]		
mein ‘my’ rein ‘pure’	Naht ‘fissure’ Rad ‘wheel’	bangen ‘to tremble’ Barren ‘bar’	[44]	
Maus ‘mouse’ Laus ‘louse’	Not ‘hardship’ Lot ‘plumbline’	bangen ‘to tremble’ Ballen ‘bale’	Brei ‘pulp’ Blei ‘lead’	[45]

Clearly, the minimal pair method is not conclusive as to the exact number of phonological units, since relevant data may have been overlooked when assembling the set of minimal pairs.

While a notation of phonological units using numbers is fully sufficient, it may not be most appropriate for scholars because of the complete arbitrariness of the designations. Moreover, such a notation makes it difficult to compare the phonological systems of different languages. For these reasons, using an established notational system is advisable. One such system is the International Phonetic Alphabet (IPA). This script supplies a large set of symbols as written names for phonological units. It consists of symbols in relation to the prototypical articulation of a phonological unit in a specific language. Given that element [41] is prototypically articulated as a bilabial nasal in German, the IPA-symbol [m] is an adequate notation of this unit. Based on such an evaluation of prototypical articulation, the phonological units from Table 2 can be assigned the following IPA-symbols:

(2) *IPA-symbols for sonorant consonants in German*
[41] → [m]
[42] → [n]
[43] → [ŋ]
[44] → [l]
[45] → [r]

The symbol chosen for element [45] is disputed in German linguistics (as it is in phonetics in general; cf. Hall 1997: 105). Wiese (1996: 8) characterizes the prototypical articulation of this phonological element as a uvular approximant and criticizes the IPA because it does not have a symbol for this kind of articulation. Others see the uvular trill as the prototypical articulation of this element (e.g. Vennemann 1982: 275), a category that also does not exist in the current IPA chart. There is even more variation in the articulation of this consonant. Therefore, it is preferable to address it with a term not rooted in articulation. A reasonable term may be 'rhotic' (cf. e.g. Hall 1997: 104). I give it a symbol that resembles the other symbols for sonorant consonants, namely the small letter [r]. More precisely, this is the symbol for the consonantal rhotic.

In a similar way as in Table 2, minimal pairs for other phonological units can be given, resulting in a set of about 40 phonological units in German (cf. Table 3 below, derived from Neef 2005). In accordance with the phonemic principle of IPA (1999: 159), I give each phonological unit with a distinct symbol without diacritics. This is particularly important for tense vowels (often called long vowels, which is rather misleading) that are usually given by a colon added to the simple symbol. Interestingly, this widespread custom already violates the IPA-principles (cf. IPA 1999: 30). In the case of diphthongs, however, IPA does not supply adequate (simple) symbols for these elements if they are regarded as units. The reason for regarding diphthongs as units is that the model works better in this way, something which would have to be made explicit in a more detailed elaboration. In order to make the phonological notation of diphthongs comprehensible, I refrain from creating new simple symbols for these units and render them in the following way:

(3) *Phonological notation of diphthongs as phonological units*
a. [a͡ʊ] Haus 'house'
b. [a͡ɪ] heiß 'hot'
c. [ɔ͡ɪ] Häuser 'houses'

The phonological units discussed so far may be called phonemes (if one focuses on their capability of distinguishing meaning) or phones (given they are elements of a phonological surface representation, traditionally often called '(broad) phonetic transcription'). In a more neutral way, I refer to them as 'phonological segments'. Besides these segments, there are two other kinds of phonological units at the word level, both relating to the level of syllable. The first kind of phonological unit of this type is word stress, indicated in notation by a raised vertical stroke before the first phonological segment of the stressed syllable (cf. (4a)). German has only a small number of minimal pairs that vary by the phonological quality of stress. The other kind of unit is the number of syllables of a word, which is indicated indirectly by dots as signs for syllable boundaries inside of words (cf. (4b)). In Neef (2005: 235), I give six minimal pairs for this kind of unit, following Vennemann (1982: 265) as one of the few linguists to acknowledge the phonological status of this type.

(4) *Other types of phonological units in German*

a.	*stress*	perfekt	'ideal'	[pɛɐ.ˈfɛkt]	vs.	Perfekt	'perfect tense'	[ˈpɛɐ.fɛkt]
b.	*syllable number*	sehr	'very'	[ˈzeɐ]	vs.	Seher	'seer'	[ˈze.ɐ]

To repeat, a phonological representation (or notation) is not a linguistic fact to be derived in some way from a phonetic reality but a theory-driven hypothesis. Such representations have to be consistent with regard to the specific theory chosen as their basis. A phonological representation comprises all and only the relevant phonological units, with written symbols conventionally related to these units. Beyond that, phonological notations of individual words can only be more or less convincing in the eyes of the reader.

3.3 Paradigmatic properties of phonological units

In my attempt to identify the phonological units of German, I have occasionally made use of terms like 'vowel' and 'sonorant' that relate to classes of units. Such phonological classes have to be assumed if they facilitate phonological explanation. What essentially needs to be explained by a phonological theory is how phonological units can be combined in a specific language system. These are syntagmatic properties of phonological units. Whether a specific phonological class is relevant therefore depends on the role it plays in modeling syntagmatic well-formedness. Thus, phonological classes are theory-dependent; they demonstrate their meaningfulness only in view of the complete model of the phonological component of a language.

The names of phonological classes are arbitrary. Such class names may derive from phonetic features that are prototypically connected with their members. A good class name is one that is telling and therefore easy to memorize, while a bad name is one that is misleading. Table 3 lists phonological classes that may be relevant for modeling the word phonology of German. Whether these are actually the relevant classes of phonological units is what needs to be shown. Phonetic similarity, in any case, does not count as a decisive criterion, as in the current approach phonological classes do not derive from phonetics. What counts in the first place is similar behavior of the members of a class with respect to their distribution. Class names can also be regarded as features of phonological segments.

Table 3. Names of assumed classes of phonological units of German and symbols of class members (based on Neef 2005)

<table>
<tr><th colspan="5">Names of phonological (sub-) classes</th><th>Symbols of class members</th></tr>
<tr><td rowspan="11">phonological segment</td><td rowspan="4">vowel</td><td rowspan="2">full vowel</td><td colspan="2">tense</td><td>[ɑ] [e] [i] [o] [u] [æ] [ø] [y]</td></tr>
<tr><td colspan="2">lax</td><td>[a] [ɛ] [ɪ] [ɔ] [ʊ] [œ] [ʏ]</td></tr>
<tr><td colspan="3">reduced vowel</td><td>[ə] [ɐ]</td></tr>
<tr><td colspan="3">diphthong</td><td>[a͡ʊ] [a͡ɪ] [ɔ͡ɪ]</td></tr>
<tr><td rowspan="7">consonant</td><td rowspan="4">obstruent</td><td rowspan="2">stop</td><td>voiced</td><td>[b] [d] [g]</td></tr>
<tr><td>voiceless</td><td>[p] [t] [k]</td></tr>
<tr><td rowspan="2">fricative</td><td>voiced</td><td>[v] [z] [j]</td></tr>
<tr><td>voiceless</td><td>[f] [s] [ʃ] [ç] [x] [h]</td></tr>
<tr><td rowspan="3">sonorant</td><td colspan="2">nasal</td><td>[m] [n] [ŋ]</td></tr>
<tr><td colspan="2">lateral</td><td>[l]</td></tr>
<tr><td colspan="2">rhotic</td><td>[r]</td></tr>
<tr><td colspan="5">stress</td><td>[ˈ]</td></tr>
<tr><td colspan="5">syllable break</td><td>[.]</td></tr>
</table>

In phonology, classes can be defined extensionally due to the fixed and small number of members. For example, voiced stops form a subgroup of obstruent consonants in the set of phonological segments and comprise the elements [b], [d], and [g]. A relation to phonetics is given by the further characterization of the individual phonological segments. For instance, the segment symbolized by [b] is a phonological segment in the first place and it is furthermore prototypically (but not necessarily) articulated as a bilabial voiced plosive. It is important to recall that articulation features do not count as definitional criteria for phonological segments. Prototypical articulation properties may not be necessary ones, as e.g. [b] can also be articulated voiceless when whispering. Actually, the unit symbolized by [b] is one of the units that have the potential to distinguish meaning in German.

Among the phonological units that may be regarded as missing in Table 3 is, on the one hand, the voiced fricative [ʒ] that shows up only in (assimilated) foreign words like *Garage* 'garage' and *Dschungel* 'jungle' and, on the other hand, the diphthong [ɛɪ] that can be assumed in interjections like *hey* 'hey'. Another such phonological segment that is included in many studies of German is the glottal stop [ʔ]. Each case would need accurate argumentation to make comprehensible the decisions that underlie Table 3 (cf. Neef 2005 for some arguments in this direction).

3.4 Syntagmatic properties of phonological units

The main task of phonology is to give a model of well-formed sequences of phonological units of specific languages. If an explicit model is given stating which sequences of phonological units are possible and which are not, the syntagmatic part of phonology is explained. For this purpose, domains are required in which conditions of well-formedness apply. In the pertinent literature on prosodic phonology, a number of different domains have been suggested, usually conceived as a hierarchy (e.g. Nespor & Vogel 2007). In such structural, non-phonetic approaches,[4] the syllable is typically regarded as the smallest constituent of the prosodic hierarchy. By and large, I follow this tradition.

Among the phonological units given in Table 3, there are two types that go beyond the domain of the syllable, namely stress and syllable number. Hence, the syntagmatic explanation of both these types needs a domain larger than the syllable. This is plainly evident for the latter type, but it also holds for stress, as this unit captures a relation of at least two syllables. Therefore, I regard the syllable as the domain to model the well-formedness of sequences of phonological segments, and I assume another domain above the syllable, to be called 'phonological word', to determine the distribution of both stress and syllable number. This leads to the definitions in (5) and (6).

(5) *Definition of the term 'syllable'*
A syllable is a domain to capture the distribution of phonological segments.

(6) *Definition of the term 'phonological word'*
A phonological word is a domain to capture the distribution of both stress and syllable number.

4. This is how Nespor and Vogel (2007: 61) characterize their own approach.

Obviously, these definitions are language-specific in the first place, as particularly stress and syllable number may not be universal phonological units. However, the definition of syllable may be appropriate for phonological systems in general. What possibly needs to be added to the definition of phonological word is the relation of this domain to morphological concepts.[5]

The task of phonological theory, then, is to formulate the well-formedness conditions for syllables and phonological words, respectively. To illustrate how this task can be achieved, I give a sketch of some concepts that seem promising for arriving at a formal model. In particular, I adopt concepts from older approaches to phonology and adjust them to the current needs. In a declarative approach like the one assumed here, there is no need for a structure-building component as in generative phonology. The phonological module merely captures whether or not a given sequence of phonological units is well-formed.

An approach to modeling the question how many elements can be placed in a syllable is CV-phonology (Clements & Keyser 1983). In this approach, the relation between a phonological segment and a position on the CV-tier is usually one to one. Deviations occur in two directions: I stipulate that tense vowels[6] as well as diphthongs are phonological segments that each relate to two positions on the CV-tier, while affricates though consisting of two phonological segments relate to only one such position. Based on these determinations, a syllable in German has at most five positions on the CV tier. These positions form the 'core syllable'. In addition to the core syllable, one more position at the beginning and two at the end of the syllable have to be assumed. These X-positions, which are often called 'extrasyllabic' (e.g. Wiese 1996: 47) but which I take to be positions inside the syllable, are dedicated for specific phonological segments, namely [s], [ʃ], and [t]. The diagram in (7) – presented as a table as an alternative to the common tree diagram – gives the syllable structure of a monosyllabic word in German where all possible positions on the CV-tier are occupied:

(7) *Syllable structure of the word <u>streichst</u> 'to paint, 2.Ps.Sg.Pres.'*

<table>
<tr><th colspan="8">σ</th></tr>
<tr><th colspan="3">onset</th><th colspan="5">rhyme</th></tr>
<tr><td>X</td><td>C</td><td>C</td><td>V</td><td>C</td><td>C</td><td>X</td><td>X</td></tr>
<tr><td>[ʃ</td><td>t</td><td>r</td><td colspan="2">aɪ</td><td>ç</td><td>s</td><td>t]</td></tr>
</table>

5. It also needs to be considered whether other domains are necessary for phonological explanations.

6. Clements and Keyser (1983: 12) make this assumption for long vowels.

I adopt the view that a syllable is divided into the two sub-domains of onset and rhyme. A syllable is only well-formed if the two sub-domains are well-formed. While the onset may be empty, the rhyme needs two CV-positions to be filled in the case of a full syllable (i.e. a syllable that contains either a full vowel or a diphthong) and one position filled in the case of a reduced syllable (i.e. a syllable that does not contain a full vowel or a diphthong), respectively. In the rhyme, the first element on the CV-tier is a designated element, marked by 'V'. This is the most sonorous element of the syllable, the syllable peak. In an approach autonomous from phonetics, sonority serves the task of constraining the possible order of segments in a syllable. Basically, I assume the following sonority hierarchy (cf. Neef 2004: 266):

Table 4. Sonority hierarchy of German

degree of sonority	✓	✓	✓				
		✓					
			✓	✓			
					✓		
						✓	
							✓
	lax vowel	tense vowel	diph- thong	reduced vowel	[r] [l]	nasal	obstruent, affricate

I assume that sonority applies in the core syllable only. I subscribe to the following constraints for the sonority contour from Selkirk (1984: 116):

(8) "*Constraints for the sonority contour of syllables*
  a. In the onset of a core syllable, sonority has to rise strictly monotonically.
  b. In the rhyme of a core syllable, sonority has to fall strictly monotonically.
  c. The first element of the rhyme has to be of higher sonority than the final element of the onset of the same syllable."

Relating sonority to the concept of core syllable suggests that it is not the level of phonological segments but the CV-positions to which sonority values are connected. What follows from this assumption is that phonological segments that occupy two positions on the CV-tier have to have an inherently falling sonority contour in order to meet the constraints for the sonority contour of syllables. This idea, first formulated in Neef (2004), is implemented in Table 4.

With these assumptions, a number of syntagmatic properties of German syllables can be modeled and thereby explained. Clearly, this sketch of the model is not fully comprehensive and needs extending and refining. What I hope to have exemplified, though, is how a phonology that is autonomous from phonetics can work. Such an autonomy is required in Linguistic Realism.

4. Conclusion

In the present approach, well-formedness of phonological domains like the syllable is modeled by constraints that are in principle language-specific, though it may turn out that some constraints can be transferred to the modeling of other phonological systems as well. Evidently, a contrastive approach to phonological systems requires the application of the same theoretical framework. In the present framework, the constraints are principally unviolable; violation of a constraint leads to ungrammaticality (or exceptionalism). Constraints may be specific to certain levels of the vocabulary as well as to different stylistic levels like explicit or standard articulation. What needs to be added to the model is the relation to morphological concepts like the root, given that it is word phonology that I try to capture with the model outlined. It is in the relation of lexematic representations of roots to phonological representations of words where the traditional concept of phonological rules like final devoicing is kept (cf. Neef 2005: 214).

The present approach to phonology in terms of Autonomous Declarative Phonology addresses aspects of the language system conceived of as an abstract object. Questions relating to knowledge of phonology or use of such knowledge in speaking and hearing require fundamentally different approaches.[7]

7. I thank Christina Behme, Tracy Alan Hall, Holger Hopp, and Hans Heinrich Lieb for helpful comments.

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CHAPTER 8

Explaining linguistic facts in a realist theory of word formation

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The present paper examines foundational issues of a realist word-formation theory. A realist linguistic theory, as it is understood here, takes linguistic units and the linguistic systems that determine them to be abstract entities. With respect to such a word-formation theory, the following two questions are discussed:

1. What are the word-formation facts to be described and explained or predicted?
2. What linguistic objects are those word-formation facts about?

Presupposing the axiomatically formalized Pattern-and-Restriction Theory (PR), it is proposed that the word-formation facts to be described and explained or predicted are true statements of word-formation relations in the linguistic system under consideration, and that those facts are about abstract lexical units in the sense of the realist framework of Integrational Linguistics (IL). On the example of a word-formation pattern in some spoken Modern German system it is shown how deductive-nomological (DN) explanations or predictions of word-formation facts can be logically derived from theorems of the PR theory and theorems of a grammar and a dictionary of the linguistic system.

Keywords: axiomatic theory, word formation, conversion, deductive-nomological explanation

1. Introduction

Katz and Postal (1991) distinguish three views of natural language: the *nominalist* view of language as concrete physical phenomena (linguistic performance in Chomskyan terms), the *conceptual* view of language as psychological phenomena (linguistic competence), and the *realist* view of language, which sees it as an abstract entity, underlying both linguistic competence and performance. In the

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latter view, "linguistics is an autonomous formal science with its own goals and domain of facts" (Katz & Postal 1991: 515). Taking this general view for granted, I shall examine in this paper what such a position means for a realist word-formation theory.

In particular, I shall discuss the two questions stated below:

1. What are the word-formation facts to be described and explained or predicted?
2. What linguistic objects are those word-formation facts about?

The following answers will be proposed here:

1. The word-formation facts to be described and explained or predicted are true statements of *word-formation relations* in the linguistic system under consideration.
2. Statements of word-formation relations in a linguistic system are about abstract *lexical units* in the system.

By 'statement', I understand an abstract declarative sentence in some natural or formal language. Statements in this sense can be true or false of the objects they are about, given that they contain no explicit or implicit free variables. A true statement is a fact. As an example, consider the following, pretheoretic statement of a word-formation relation in Modern German systems:

(1) The noun *Fall* meaning 'falling event' is formed from the verb *fallen* meaning 'to fall' in Modern German.

This statement is about two lexical units in Modern German systems:

– a noun with the citation form *Fall* and a lexical meaning paraphrased as 'falling event';
– a verb with the citation form *fallen* and the lexical meaning 'to fall'.

Implicitly, it will be argued, such a statement also involves a *word-formation process* – here: conversion – and an appropriate *word-formation pattern* by means of which formal, categorial, and semantic properties of the lexical units can be related. Insofar as the statement in (1) is true, it is a fact – a word-formation fact, to be exact.

The discussion of these questions and answers will be couched in terms of the *Pattern-and-Restriction Theory* (*PR*), which was developed and axiomatically formalized in Nolda (2012), a study on conversion in Modern German. PR aims at describing and explaining or predicting word-formation relations in a linguistic system, which underlie lexical motivation relations. In particular, the theory allows for explaining word-formation relations between conventionalized, 'existing' lexical units as well as for predicting word-formation relations also involving non-conventionalized, but still 'possible' lexical units. Lexical units are understood in the sense of the realist framework of *Integrational Linguistics* (*IL*) as abstract pairings of a paradigm and a lexical meaning (Lieb 1983, 1985, 2005). The finite set of conventionalized lexical units in a linguistic system – those which are 'known' by speakers of the corresponding idiolect, variety, or language – constitute the *vocabulary* of the system. As a rule, the vocabulary of a system is a proper subset of its *lexicon*, which also contains non-conventionalized lexical units and is potentially infinite, provided that word formation is recursive in the system.

According to the PR conception, word-formation relations between lexical units explicitly or implicitly involve word-formation processes and multidimensional word-formation patterns which each combine four *formation means*:

1. a *formal means*, which determines form-related properties,
2. a *paradigmatic means*, which determines paradigmatic categorial properties,
3. a *lexical means*, which determines lexical categorial properties, and
4. a *semantic means*, which determines semantic properties.

For every word-formation pattern, there is an associated *formation restriction* which accounts for distributional properties of the pattern.

Statements of word-formation relations in a linguistic system can be logically derived in PR from lawlike sentences – theorems of the word-formation theory – and particular statements concerning system-specific properties of the involved lexical units, processes, patterns, and restrictions. As I shall demonstrate in this paper, such a logical derivation represents a *deductive-nomological* (*DN*) *explanation* or *prediction* in the sense of Hempel (1965).[1] Roughly speaking, a word-formation relation exists between a lexical product and one or more lexical bases in the system if, and only if, product properties are obtained through the application of a word-formation pattern in the system to base properties in the associated formation restriction.

As a word-formation theory in the Item-and-Process tradition, PR is related to, and has been influenced by, theories such as Aronoff's (1976) theory of

1. Hempel's explication of the notion of (non-inductive, non-statistical) scientific explanation in terms of deductive-nomological explanation was much discussed in the subsequent literature, and pragmatic alternatives were proposed (for a thorough overview cf. Stegmüller 1983). For the sake of logical and expository simplicity, I shall stick to Hempel's classic conception here. This should not be mistaken as implying that PR is incompatible with alternative notions of scientific explanation.

word-formation rules and morphological restrictions, Beard's (1995) Lexeme-Morpheme Base Morphology, and in particular Lieb's (2013) Process Model of Word Formation, another IL-based word-formation theory. Albeit being Item-and-Process with respect to word formation proper, understood as a component in the morphosyntactic part of linguistic systems, PR is Word-and-Paradigm as far as the lexicon is concerned, for which IL's Word-and-Paradigm notion of lexical units is presupposed. IL's axiomatically constructed theory of language has also influenced PR's axiomatic formalization.[2]

The present paper is organized as follows. Section 2 outlines how lexical units are understood here, *i.e.* the objects statements of word-formation relations in PR are about. Word-formation relations and the involved word-formation processes and patterns are introduced in Section 3 on the example of conversion by means of a selected word-formation pattern in some spoken Modern German system. In Section 4 it is shown how statements of word-formation relations can be explained or predicted in PR. Section 5 concludes the discussion with a summary of the realist approach to word formation advocated here. An appendix lists the symbols used below as well as the presupposed axioms and definitions of PR's theoretical core. Despite the fact that all of the examples in this paper are taken from German, it should be clear that the argument made here is not restricted to systems of that language.

2. Lexical units

As proposed in the introduction, abstract lexical units in a linguistic system are the objects statements of word-formation facts in the system are about. Lexical objects come in several kinds. *Lexical words* are provided by the *word lexicon* of the system. The word lexicon contains the *word vocabulary* of the conventionalized, 'existing' lexical words in the system as a proper subset. In addition, the word lexicon contains non-conventionalized, but still 'possible' lexical words. In a linguistic system with a non-empty morphological component, there is also a *stem lexicon*, providing *lexical stems*. The conventionalized lexical stems in the system are members of its *stem vocabulary*. Besides word and stem vocabularies,

2. As a companion for word-formation description in PR, there is a computer program called 'PPR' ('System for Processing Formation Patterns and Restrictions', available at http://andreas.nolda.org/software.html#ppr), which can be used for testing the soundness of theoretical and empirical hypotheses. Currently, it provides a small set of lexical entries and selected word-formation patterns for spoken and written Modern German systems, including the word-formation pattern used as an example here.

linguistic systems typically have a *collocation vocabulary* containing conventionalized idiomatic or non-idiomatic *lexical word groups*. Besides conventionalized word groups, phrasal word formation (*i.e.* word formation based on groups) may in addition involve non-conventionalized word or stem groups as bases.

According to the IL theory of language, a lexical word is a pairing of a word paradigm and a lexical meaning. For lexical words, the following notation will be used in this paper:[3]

/ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to fall’}}$: pairing of a word paradigm with the citation form /ˈfal[ə]n/ and the lexical meaning ‘to fall’.

/ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to decrease’}}$: pairing of a word paradigm with the citation form /ˈfal[ə]n/ and the lexical meaning ‘to decrease’.

The monosemous lexical words /ˈfal[ə]n/n/$^{\mathrm{W}}_{\text{‘to fall’}}$ and /ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to decrease’}}$ correspond to the first two of nine major readings the *Wörterbuch der deutschen Gegenwartssprache* (Klappenbach & Steinitz 1980: vol. 2, 1204–1207) distinguishes in the lexical entry for this citation form. The lexical entry as a whole describes a polysemous *lexicological word* in the sense of Nolda (2012: Section 4.3, 2016): a set of lexical words of the same part of speech with identical or overlapping paradigms and related lexical meanings.[4] For heuristic reasons, PR subscribes to the view that the objects of word-formation theory and word-formation description are monosemous lexical units, not potentially polysemous lexicological units, thereby reducing descriptive complexity. In the PR sense, then, word formation may be understood as the formation of lexical units.

A *word paradigm* – *i.e.* the paradigm of a lexical word – specifies the (spoken or written) *forms* of the word as well as the *paradigmatic categorizations* they realize. IL models paradigms as set-theoretic relations between forms and categorizations (Lieb 1983: Chapters 6 and 11; Lieb 2005). In the paradigm of /ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to fall’}}$, the word form /ˈfal[ə]n/, for example, realizes paradigmatic categorizations such as:[5]

3. ‘/ˈfal[ə]n/’ is an informal phonological notation for the form *fallen* in spoken Modern German. (For details, see below.)

4. The term ‘lexicological word’ is due to François Filandre. Similar distinctions are made by Cruse (1986: Chapter 3) in terms of ‘lexical units’ and ‘lexemes’ and by Mel’čuk (1995: 206–207, 250) in terms of ‘lexical units’ and ‘vocables’. The range of semantic relations which can hold between members of the same lexicological word are discussed by Blank (1997, 2003).

5. Traditional tenses like ‘present tense’ or ‘present perfect’ are decomposed here into a proper tense category (present tense) and an anteriority category (non-perfect or perfect) (Nolda 2012: Section 4.2.2, appendix A; cf. also Teuber 2005; Thieroff & Vogel 2012). Non-clausal

(2) a. non-perfect, active, non-clausal infinitival verb form
b. first person, plural, indicative, present tense, non-perfect, active verb form

From a set-theoretic point of view, a paradigmatic categorization is a set of *paradigmatic categories*:[6]

(3) a. {NonPerf-Vf, Act-Vf, NonClausInf-Vf}
b. {1Pers-Vf, Plur-Vf, Ind-Vf, Pres-Vf, NonPerf-Vf, Act-Vf}

Paradigmatic categories, in turn, are sets of forms. Categories of word forms are obtained on the basis of classification systems on the set of word forms in the linguistic system, and likewise for categories of stem forms (for the presupposed notion of classification system cf. Lieb 1993: Chapter 9; a proposal for such classification systems in Modern German can be found in Nolda 2012: appendix A).[7]

Lexical stems (in particular, stems of lexical words) are taken to be pairings of a stem paradigm and a lexical meaning in IL. Informally, lexical stems will be notated in the following way:

/ˈfal/$^{\mathrm{St}}_{\text{‘to fall’}}$: pairing of a stem paradigm with the citation form /ˈfal/ and the lexical meaning ‘to fall’.
/ˈfal/$^{\mathrm{St}}_{\text{‘to decrease’}}$: pairing of a stem paradigm with the citation form /ˈfal/ and the lexical meaning ‘to decrease’.

As a rule, the lexical meaning of a lexical word is identical to the lexical meaning of its stem (if any).[8]

In analogy to lexicological words, Nolda (2012, 2016) also assumes potentially polysemous *lexicological stems*. Like lexicological words, they are not considered to be objects of word-formation theory or word-formation description.

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infinitival verb forms are infinitives without *zu* (‘nicht satzwertige’, ‘reine Infinitive’ in German grammatical tradition).

6. The symbolic categories used in this paper and their intended readings are listed in the appendix.

7. Paradigmatic categories are the paradigm-related subset of what is called *unit categories* or *type 1 categories* in IL.

8. Examples of lexical words without corresponding stems are ‘nominalized adjectives’ in Modern German, such as the noun /ˈklain[ə]r/$^{\mathrm{W}}_{\text{‘small person’}}$, which is directly formed from the adjective /ˈklain/$^{\mathrm{W}}_{\text{‘small’}}$. The word paradigm of the former does not result from morphological inflection of a noun stem; rather, it is inherited from (a subset of) the word paradigm of the latter (for discussion cf. Nolda 2012: Section 3.2.2 and 8.2).

A *stem paradigm* – the paradigm of a lexical stem – specifies the forms of the stem as well as the paradigmatic categorizations they realize. The categorizations the stem form /ˈfal/ realizes in the paradigm of /ˈfal/$^{\mathrm{St}}_{\text{‘to fall’}}$ include:[9]

(4) a. basic verb-stem form
 b. infinitival verb-stem from
 c. non-second-or-third-person-singular, indicative, present tense verb-stem form

In set-theoretic notation:

(5) a. {Basic-VStf}
 b. {Inf-VStf}
 c. {Non2/3PersSing-VStf, Ind-VStf, Pres-VStf}

In addition to basic and inflectional categorizations, the paradigm of /ˈfal/$^{\mathrm{St}}_{\text{‘to fall’}}$ also assigns word-formation related categorizations to /ˈfal/ such as:

(6) a. compounding verb-stem form
 b. conversion verb-stem form

(7) a. {Comp-VStf}
 b. {Conv-VStf}

As a compounding stem form, /ˈfal/ can be used in the formation of compounding products like /ˈfal/ /ˌʃirm/$^{\mathrm{St}}_{\text{‘parachute’}}$; as a conversion stem form, it is used in the formation of conversion products like /ˈfal/$^{\mathrm{St}}_{\text{‘falling event’}}$. Note that word-formation stem forms need not coincide with inflectional ones, as in the case of the conversion stem forms /ˈfluːG/ and /ˈap/ /ˌfluːG/ of the verb stems /ˈfliːG/$^{\mathrm{St}}_{\text{‘to fly’}}$ and /ˈap/ /ˌfliːG/$^{\mathrm{St}}_{\text{‘to take off’}}$, respectively, which are used in the formation of the noun stems /ˌfluːG/$^{\mathrm{St}}_{\text{‘flying event’}}$ and /ˈap/ /ˌfluːG/$^{\mathrm{St}}_{\text{‘take-off event’}}$.[10] This example also shows that word-formation stem forms may be inherited by derivative lexical stems in the same way as inflection stems forms are. (For a discussion of compounding and derivation stem forms from a broader IL perspective cf. Fuhrhop 1998; Eisenberg

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9. For the concept of *basic stem form* (or *Grundstammform*) cf. Fuhrhop (1998: 27, passim). In (4c), there are no anteriority or voice categories involved: those categories are not marked morphologically in Modern German, but syntactically (*i.e.* by means of auxiliaries).

10. According to the analysis proposed in Nolda (2012: Section 5.1.2), the lexical stem /ˈap/ /ˌfliːG/$^{\mathrm{St}}_{\text{‘to take off’}}$ has word-formation stem forms only. The word forms of the corresponding lexical word /ˈap/ /ˌfliːG[ə]n/$^{\mathrm{W}}_{\text{‘to take off’}}$ are not formed from /ˈap/ /ˌfliːG/$^{\mathrm{St}}_{\text{‘to take off’}}$ through morphological inflection, but from /ˈfliːG[ə]n/$^{\mathrm{W}}_{\text{‘to fly’}}$ through syntactic derivation by means of deaccentuation and prefixation of the particle form /ˈap/.

2013: Chapters 6 and 7; conversion stem forms are introduced in Nolda 2012: Chapter 3, 4, and 7.)

Word and stem forms are regarded in IL as sequences of *morphosyntactic atoms*: word forms are sequences of one or more *syntactic atoms* (*syntactic base forms* in IL terms) and stem forms are sequences of one or more *morphological atoms* (*morphological base forms*). In spoken linguistic systems, morphosyntactic atoms are phonological units.

PR as a theory of word formation (and, more recently, inflection) is, in principle, neutral with respect to questions of phonological representation. In this paper, I shall make the minimal assumption that phonological representations specify not only segmental phonological properties but also suprasegmental ones, in particular lexical accents of syllables. *Primary lexical accent* is understood in IL as the potential of a syllable for bearing a non-contrastive syntactic accent (Lieb 1999a, 1999b). In the informal phonological notation used here,[11] such accents are indicated by the usual IPA symbol 'ˈ'. The IPA symbol 'ˌ' is used for *deaccented lexical accents* ('secondary lexical accents') in word forms like /ˈgastproˌfesor/ ('visiting professor') and /ˈgastprofeˌsoːr[ə]n/ ('visiting professors'), where they result from deaccentuation of the primary lexical accents in /proˈfesor/ and /profeˈsoːr[ə]n/, respectively.[12] Syllables with primary lexical accent will be called 'accented syllables' for short. In order to keep the informal phonological notation as simple as possible, syllable borders are left unspecified, while atom borders are delimited by '/ /'. In syllables with primary or deaccented lexical accents, the IPA symbol 'ː' is used to mark vowels which are phonetically realized as long tense ones, while unmarked vowels are phonetically realized as short lax ones. Phonologically the former may be analyzed in Modern German as being 'smoothly cut' or 'in loose contact', and the latter as being 'abruptly cut' or 'in close contact', as proposed by Becker (1998), Restle (2003), and others. For phonetic realizations of vowels in syllables without primary or deaccented lexical accent cf. Becker (1998: 82–99). '[ə]' represents epenthetic schwa (for discussion cf. Wiese 2000: 106–114, 242–248). Capital letters like 'G' stand for archiphonemic consonants which, in Modern German, are unspecified for voice (or tenseness) and are realized as voiced (or, for that matter, lax) consonants unless they undergo final devoicing (tensing) or spirantization (cf. Lieb 1999a: 374–375).

11. This is also the phonological notation used in the user interface of PPR (cf. Note 2).

12. As convincingly argued by Becker (1998: 82–84) for German, 'smoothly cut', phonetically long tense vowels in syllables without primary lexical accent require a deaccented lexical accent ('morphologischer Nebenakzent').

Lexical meanings like 'to fall' are understood in IL as *(potential) concepts* – certain perceptual or conceptual properties involving attributes of concrete or abstract entities (Lieb 1983: Chapter 13; Lieb 1985). For the sake of the argument, let us assume that the attribute TO-FALL is a three-place intensional relation whose name is defined in (8):

(8) *Definition (tentative)*
TO-FALL = the intensional relation between x_1, x_2, and x_3 such that x_1 is a movement process of x_2 and x_3 is a directional property such that 'x_2 moves downwards towards x_3 in x_1 through gravitational force'.

Then the name of the concept 'to fall' can be defined as follows:

(9) *Definition*
'to fall' = the property of being a perception or conception whose content contains the attribute TO-FALL.

Thus, 'to fall' is a concept denoting certain kinds of movement processes.

Lexical units are elements of *lexical categories* such as part-of-speech categories or lexical gender. The lexical word /ˈfal/$^{W}_{\text{'falling event'}}$, for instance, realizes a *lexical categorization* with the following lexical categories:[13]

(10) noun, masculine nominal word

Again, lexical categorizations are modelled as sets of lexical categories:

(11) {Noun, Masc-N}

The corresponding lexical categorization realized by the lexical stem /ˈfal/$^{St}_{\text{'falling event'}}$ is:

(12) noun stem, stem of a masculine nominal word

Or, in set-theoretic notation:

(13) {NounSt, Masc-NSt}

Lexical categories of lexical words are obtained on the basis of a classification system on the word lexicon in the linguistic system, while lexical categories of lexical stems are based on a classification system on the stem lexicon.

13. In Modern German, the set of masculine nominal words is not limited to nouns, but does also include masculine pronouns.

3. Word-formation relations, processes, and patterns

In the PR view, the word-formation facts to be described and explained or predicted are statements of *word-formation relations* between lexical units in a linguistic system, underlying *lexical motivation relations*. Lexical motivation relations in a linguistic system relate a lexical unit to one or more lexical units it is 'dependent' upon in formal, categorial, and/or semantic respects (for an overview of lexical motivation cf. Marzo 2015). Let '**S**' be an ambiguous constant for some specific spoken Modern German system. Then there are motivation relations in **S** such as:

(14) /ˈfal[ə]n/$^{\text{W}}_{\text{'to decrease'}}$ is motivated by /ˈfal[ə]n/$^{\text{W}}_{\text{'to fall'}}$ in **S**.

(15) /ˈfal/$^{\text{W}}_{\text{'falling event'}}$ is motivated by /ˈfal[ə]n/$^{\text{W}}_{\text{'to fall'}}$ in **S**.

Implicitly, such motivation relations involve a motivating process:[14]

(16) /ˈfal[ə]n/$^{\text{W}}_{\text{'to decrease'}}$ is motivated by /ˈfal[ə]n/$^{\text{W}}_{\text{'to fall'}}$ through metaphor in **S**.

(17) /ˈfal/$^{\text{W}}_{\text{'falling event'}}$ is motivated by /ˈfal[ə]n/$^{\text{W}}_{\text{'to fall'}}$ through conversion in **S**.

Word-formation relations in the PR sense correspond to lexical motivation relations which are established through word-formation processes. They come in two kinds: *direct word-formation relations* and *indirect word-formation relations*. Basically, a direct word-formation relation involves lexical units which are directly related through the word-formation process by means of an appropriate word-formation pattern; an indirect word-formation relation, on the contrary, presupposes a direct word-formation relation between *lexically equivalent* units (*e.g.* the stems of the relata). As an example, consider the two statements of word-formation relations below, corresponding to the lexical motivation relation (17):[15]

(18) a. /ˈfal/$^{\text{W}}_{\text{'falling event'}}$ is indirectly formed from /ˈfal[ə]n/$^{\text{W}}_{\text{'to fall'}}$ through conversion in **S**.

b. /ˈfal/$^{\text{St}}_{\text{'falling event'}}$ is directly formed from /ˈfal/$^{\text{St}}_{\text{'to fall'}}$ through conversion in **S**.

As a shortcut, the following notation will be used for stating indirect and direct word-formation relations through conversion in **S**:

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14. In the case of (16), /ˈfal[ə]n/$^{\text{W}}_{\text{'to decrease'}}$ may be motivated by /ˈfal[ə]n/$^{\text{W}}_{\text{'to fall'}}$ through metaphorical comparison of, say, the decrease of temperature with the downward movement of liquid in a liquid-in-glass thermometer.

15. Statements of word-formation relations of these kinds are inspired from the 'word-formation statements' in the Process Model of Word Formation (Lieb 2013).

(19) a. /ˈfal/$^{\mathrm{W}}_{\text{‘falling event’}}$ $\lessdot^{\mathrm{S}}_{\mathrm{conv}}$ /ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to fall’}}$

b. /ˈfal/$^{\mathrm{St}}_{\text{‘falling event’}}$ $\leqslant^{\mathrm{S}}_{\mathrm{conv}}$ /ˈfal/$^{\mathrm{St}}_{\text{‘to fall’}}$

In such a notation, the lexical unit denoted by the term left of ‘$\lessdot$’ is an *indirect (lexical) product* and the lexical units denoted by the term or terms right of ‘$\lessdot$’ are *indirect (lexical) bases*. Similarly, the lexical unit denoted by the term left of “$\leqslant$’ is a *direct (lexical) product* and the lexical units denoted by the term(s) right of ‘$\leqslant$’ are *direct (lexical) bases*. In contexts where the distinction between direct and indirect word-formation relations is irrelevant, the familiar ‘<’ is used for notating word-formation relations between products and bases:

(20) a. /ˈfal/$^{\mathrm{W}}_{\text{‘falling event’}}$ $<^{\mathrm{S}}_{\mathrm{conv}}$ /ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to fall’}}$

b. /ˈfal/$^{\mathrm{St}}_{\text{‘falling event’}}$ $<^{\mathrm{S}}_{\mathrm{conv}}$ /ˈfal/$^{\mathrm{St}}_{\text{‘to fall’}}$

Explicit word-formation relations, with explicit reference to a word-formation process, imply *implicit* word-formation relations without such a reference:

(21) a. /ˈfal/$^{\mathrm{W}}_{\text{‘falling event’}}$ is indirectly formed from /ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to fall’}}$ in **S**.

b. /ˈfal/$^{\mathrm{St}}_{\text{‘falling event’}}$ is directly formed from /ˈfal/$^{\mathrm{St}}_{\text{‘to fall’}}$ in **S**.

(22) a. /ˈfal/$^{\mathrm{W}}_{\text{‘falling event’}}$ $\lessdot^{\mathrm{S}}$ /ˈfal[ə]n/$^{\mathrm{W}}_{\text{‘to fall’}}$

b. /ˈfal/$^{\mathrm{St}}_{\text{‘falling event’}}$ $\leqslant^{\mathrm{S}}$ /ˈfal/$^{\mathrm{St}}_{\text{‘to fall’}}$

Although word-formation relations in this sense are *directional*, *i.e.* non-symmetric, relations, PR does not exclude cases where word-formation relations involve the same lexical units in different order. This may be considered, for example, for the following pairs of lexical units:[16]

(23) a. /ˈhaːg[ə]l/$^{\mathrm{W}}_{\text{‘hailing event’}}$ $\lessdot^{\mathrm{S}}$ /ˈhaːg[ə]ln/$^{\mathrm{W}}_{\text{‘to hail’}}$

b. /ˈhaːg[ə]l/$^{\mathrm{St}}_{\text{‘hailing event’}}$ $\leqslant^{\mathrm{S}}$ /ˈhaːg[ə]l/$^{\mathrm{St}}_{\text{‘to hail’}}$

(24) a. /ˈhaːg[ə]ln/$^{\mathrm{W}}_{\text{‘to hail’}}$ $\lessdot^{\mathrm{S}}$ /ˈhaːg[ə]l/$^{\mathrm{W}}_{\text{‘hailing event’}}$

b. /ˈhaːg[ə]l/$^{\mathrm{St}}_{\text{‘to hail’}}$ $\leqslant^{\mathrm{S}}$ /ˈhaːg[ə]l/$^{\mathrm{St}}_{\text{‘hailing event’}}$

Put differently, word-formation relations need not be asymmetric (cf. Eschenlohr 1999: Section 3.1.2 and Nolda 2012: Section 5.1.3 for discussion).

Both implicit and explicit word-formation relations are based on *fully explicit* word-formation relations, not only specifying a word-formation process, but also an appropriate *word-formation pattern*, such as the one informally given in (27):

16. This presupposes that for both directions, there are appropriate word-formation processes and word-formation patterns, left implicit in (23) and (24).

(25) a. $/ˈ\text{fal}/^{\text{W}}_{\text{‘falling event’}}$ is indirectly formed from $/ˈ\text{fal[ə]n}/^{\text{W}}_{\text{‘to fall’}}$ through conversion by means of (27) in **S**.

b. $/ˈ\text{fal}/^{\text{St}}_{\text{‘falling event’}}$ is directly formed from $/ˈ\text{fal}/^{\text{St}}_{\text{‘to fall’}}$ through conversion by means of (27) in **S**.

(26) a. $/ˈ\text{fal}/^{\text{W}}_{\text{‘falling event’}} <^{\text{S}}_{\text{conv((27))}} /ˈ\text{fal[ə]n}/^{\text{W}}_{\text{‘to fall’}}$

b. $/ˈ\text{fal}/^{\text{St}}_{\text{‘falling event’}} <^{\text{S}}_{\text{conv((27))}} /ˈ\text{fal}/^{\text{St}}_{\text{‘to fall’}}$

(27) Formal means: initial accentuation.
Paradigmatic means: formation of a basic noun-stem form.
Lexical means: formation of a noun stem in the masculine.
Semantic means: formation of a concept according to the scheme ‘event consisting in a non-state denoted by the base meaning’.

(For the empirical motivation of (27) cf. Nolda 2012: Section 7.2.) From a mathematical point of view, a *one-place* pattern like (27) with *one-place formation means* is a quadruple of simple or composed one-place set-theoretic operations:

(28) ⟨ini-acc,
basic-nstf,
masc-nst ∘ nounst,
event⟩

(‘∘’ denotes function composition.) Since Nolda (2012), I have generalized the theoretical core of PR in such a way as not only to account for word formation, but also for inflection. In order to do so, both word-formation patterns and inflection patterns are introduced as special cases of *formation patterns* which are quadruples of formation means of the kind illustrated above.[17]

Besides one-place patterns for *one-place* word-formation processes like conversion, derivation, shortening, or reduplication, PR also assumes patterns with at least two places for word-formation processes like compounding or blending. Generally speaking, *n*-place word-formation patterns with *n*-place formation

17. Inflection patterns are distinguished from word-formation patterns with respect to their function. An inflection pattern is used to determine forms and their categorizations in the paradigm of a lexical unit l on the basis of forms and categorizations in the paradigms of one or more lexical units $l_1,\ldots, l_n$ such that l is lexically equivalent to at least one l_i ($1 \leq i \leq n$; $n > 1$ is needed for analytic inflection); l_i and l are lexically equivalent if they are identical or if one of them is a *morphological correspondence* of the other (*e.g.* its stem). As a consequence, lexical and semantic means of inflection patterns are the identity operation or other trivial operations. A word-formation pattern, on the contrary, is used to relate a lexical product to lexical bases which are not lexically equivalent to it. (Cf. Definition Schema 5, Definition 9, Definition 10, and Definition Schema 11 in the appendix.) This captures the traditional idea that word formation, but not inflection, ‘creates new words’.

means are used for describing or explaining statements of *n*-place word-formation relations between *n* lexical bases and one lexical product, involving *n*-place word-formation processes.

I shall now briefly discuss formal, paradigmatic, lexical, and semantic formation means on the example of pattern (28). *Formal means* operate on forms. The formal means of (28) accents the first stressable syllable in its arguments (basically, the first non-schwa syllable) and deaccents any other accented syllables (cf. Nolda 2012: Section 3.1.3 and 7.2.1):[18]

(29) ini-acc (/ˌyːb[ə]r/ /ˈfal/) = /ˈyːb[ə]r/ /ˌfal/

In cases where the first stressable syllable is already accented, this operation amounts to the identity operation:

(30) a. ini-acc (/ˈfal/) = /ˈfal/
b. ini-acc (/ˈap/ /ˌfluːG/) = /ˈap/ /ˌfluːG/
c. ini-acc (/f[ə]r/ /ˈzuːx/) = /f[ə]r/ /ˈzuːx/

Syllables like /f[ə]r/ are unstressable in spoken Modern German. As can be seen from these examples, formal formation means are not limited in PR to *arrangement operations* like concatenation or affixation, but may also specify phonological properties by means of *phonological operations*, insofar as the latter are consequences of word formation.[19] Such a strategy is already considered by Anderson (1992: 224):

> With a shift in conception to a morphology based on Word Formation Rules, however, the motivation for such a class of mixed rules [*i.e.* morphologically conditioned phonological rules; A.N.] becomes much less secure. This is because it is generally possible to incorporate all of the phonological 'side effects' of a given category into the Word Formation Rule that creates the category in the first place.

18. Since formation means are set-theoretic, extensional operations, there are many equivalent ways of how to specify them. 'ini-acc', for instance, is defined in Nolda (2012: Section 7.2.1) as acc∘deacc, *i.e.* as a composed operation deaccenting all accented syllables in its arguments and then accenting their first stressable syllable. Such an operation is identical to an operation accenting the first stressable syllable and then deaccenting any other accented syllables. What matters in PR is which arguments and values are related by the means, not the way this is achieved. Thus, PR clearly is a declarative theory of word formation and inflection, and not a derivational or transformational one.

19. This view is also taken by the Process Model of Word Formation (Lieb 2013), where 'construction modes' – the counterparts of PR's word-formation patterns (cf. Note 21 below) – include not only 'arrangement functions' and 'shortening functions', but also phonological 'form-change functions'.

Paradigmatic and *lexical means* operate on paradigmatic and lexical categorizations, respectively. In (28), the paradigmatic and lexical means are constant functions, assigning the same value to any argument:

(31) basic-nstf ({Conv-VStf}) = {Basic-NStf}

(32) masc-nst ∘ nounst ({VSt}) = masc-nst (nounst ({VSt}))
= masc-nst ({NounSt})
= {NounSt, Masc-NSt}

There are also paradigmatic and lexical inheritance functions, copying (one of) their argument(s) to the value. In Modern German systems, this is the case in the formation of 'right-headed' compounds.

Semantic means operate on concepts. The values of the semantic means of (28) are concepts denoting events (in the narrow sense of 'event'), derived from concepts denoting arbitrary non-states:[20]

(33) event('to fall') = 'falling event'

Those values are determined by means of a suboperation on the corresponding attributes, called 'EVENT' in (34):

(34) EVENT (TO-FALL) = EVENT-OF-FALLING

(For definition and empirical discussion cf. Nolda 2012: Section 7.2.2.)

Formation instances combine the arguments and values of the formal, paradigmatic, lexical, and semantic formation means of a formation pattern. (35) and (36) are examples for such quadruples:

(35) ⟨/ˈfal/,
{Conv-VStf},
{VSt},
'to fall'⟩

(36) ⟨/ˈfal/,
{Basic-NStf},
{NounSt, Masc-NSt},
'falling event'⟩

(35) and (36) *instantiate* the lexical units /ˈfal/$^{\text{St}}_{\text{'to fall'}}$ and /ˈfal/$^{\text{St}}_{\text{'falling event'}}$ in **S**, respectively. While the third and fourth components of a formation instance directly represent categorial and semantic properties of the instantiated lexical units, the first and second components represent formal and categorial properties of one of their

20. With Mourelatos (1978) I distinguish between *states* and *non-states*, the latter being classified into *processes* and *events*. Typically, events are discrete and telic, while processes may be homogeneous and atelic.

forms. Note that the components in a formation instance may be underspecified with respect to the actual properties they represent. This holds in particular for the semantic component, which needs not be identical to the actual lexical meaning as long as it is implied by the latter (cf. Definition 1 in the appendix).

Word-formation processes in a linguistic system *S* like *conversion in S* (conv_S) are functions from patterns to *formation operations* on formation instances in *S*. In (37), for example, conv_S assigns a formation operation to the word-formation pattern (28) by means of which the *product instance* (36) can be formed from the *base instance* (35):

(37)

$$\text{conv}_s\left(\begin{array}{l}\langle\text{ini-acc,}\\ \text{basic-nstf,}\\ \text{masc-nst} \circ \text{nounst,}\\ \text{event}\rangle\end{array}\right)\left(\begin{array}{l}\langle\text{/ˈfal/,}\\ \text{\{Conv-VStf\},}\\ \text{\{VSt\},}\\ \text{‘to fall’}\rangle\end{array}\right) = \begin{array}{l}\langle\text{ini-acc(/ˈfal/),}\\ \text{basic-nstf(\{Conv-VStf\}),}\\ \text{masc-nst} \circ \text{nounst(\{VSt\}),}\\ \text{event(‘to fall’)}\rangle\end{array}$$

$$= \begin{array}{l}\langle\text{/ˈfal/,}\\ \text{\{Basic-NStf\},}\\ \text{\{NounSt, Masc-NSt\},}\end{array}$$

The same holds, in principle, for inflection processes and inflection patterns. As a rule, an *n*-place *formation process* assigns an *n-place* formation operation to an *n*-place formation pattern.[21]

For every formation pattern in a linguistic system, there is an associated *formation restriction* which accounts for distributional properties of the pattern by specifying logical or system-specific restrictions on the formation instances to which the corresponding formation operation may be applied.[22] PR models formation restrictions as set-theoretic entities. The formation restriction of the one-place pattern (28), for instance, is a set with the following elements:

21. Word-formation processes and formation operations in PR jointly correspond to the word-formation processes in the Process Model of Word Formation (Lieb 2013), from which they are inspired. There, a word-formation process in a linguistic system is conceived of as a 13-place function from a ‘basic triple’, an ‘added triple’, and a ‘construction mode’ to a ‘result triple’. The triples – so-called ‘fully specified forms’ – correspond to PR’s base instances, minus the lexical component. The ‘construction mode’, being a 7-tuple of five formal, one paradigmatic, and one semantic means, roughly corresponds to PR’s word-formation patterns, without the lexical means. Note that all word-formation processes in the Process Model are two-place in PR’s sense: in the case of derivation by means of affixation, the ‘added triple’ consists of an affix form, a more or less trivial paradigmatic categorization, and the empty concept; in the case of conversion, its first and second components are further reduced to the empty set. For an in-depth comparison of PR with the Process Model cf. Lieb (2013: Section 8).

22. Formation restrictions of formation patterns in PR correspond to what Aronoff (1976: 36) calls “morphological restrictions on the sorts of words one may use as the base of certain WFRs [word-formation rules; A.N.]”. Besides morphological conditions proper, formation restrictions in PR may specify any logical or system-specific condition relevant for the application of the pattern. (For the relation between formation restrictions and productivity cf. Note 26 in Section 4.)

(38) {⟨/ˈfal/,
{Conv-VStf},
{VSt},
'to fall'⟩,
⟨/ˌyːb[ə]r/ /ˈfal/,
{Conv-VStf},
{VSt},
'to raid'⟩,
⟨/ˈfluːG/,
{Conv-VStf},
{VSt},
'to fly'⟩,
⟨/ˈap/ /ˌfluːG/,
{Conv-VStf},
{VSt},
'to take off'⟩,
⟨/f[ə]r/ /ˈzuːx/,
{Conv-VStf},
{VSt},
'to try'⟩,
...}

Formation restrictions of two-place patterns are two-place extensional relations, and so on.

In a grammar of **S**, the formation restriction (38) can partially or totally be identified by a hypothesis of the following kind:[23]

(39) *Hypothesis (tentative)*
The formation restriction of pattern (28) in **S** is the set of all ⟨*f*, *P*, *L*, *c*⟩ in **S** for which the following conditions hold:
1. *P* contains Conv-VStf,
2. *L* contains VSt, and
3. *c* denotes a non-state.

Formation patterns and their associated formation restrictions are provided for by the *formation component* in the morphosyntactic part of the linguistic system

23. Being set-theoretic, extensional entities, formation restrictions can, in principal, be identified by various intensional criteria. That is, one and the same formation restriction may be identified by different logically or materially equivalent hypotheses. Thus, formation restrictions are independent from the form of their description. What is more, 'output-related' hypotheses on product instances in the range of a formation operation can be accounted for by 'input-related' restrictions on corresponding base instances in the domain of the formation operation.

(cf. Axiom 1, Definition 2, Definition Schema 3, Definition 4, and Definition Schema 5 in the appendix). Word-formation processes and word-formation relations, however, are not given by the system; rather, they are ultimately determined by the word-formation patterns in the system and their formation restrictions (cf. in particular Definition Schemes 13, 15, and 18; definitions of names of individual word-formation processes such as conversion, derivation, or compounding can be found in Nolda 2012: Section 6.3, appendix B).[24]

4. Explanation and prediction of statements of word-formation relations

Statements of word-formation relations between lexical units, explicitly or implicitly involving word-formation processes and word-formation patterns, can be explained or predicted in PR in terms of *deductive-nomological* (DN) explanations or predictions. According to the classic definition in Hempel (1965), a *DN explanation* is an argument with the following logical structure:

(40) $$\frac{\begin{array}{c} C_1 \wedge C_2 \wedge \ldots \wedge C_k \\ L_1 \wedge L_2 \wedge \ldots \wedge L_r \end{array}}{E}$$

The variables used in (40) are interpreted in Hempel (1965: 336) as follows:

> Here, $C_1, C_2, \ldots, C_k$ are sentences describing the particular facts invoked; $L_1, L_2, \ldots, L_r$ are the general laws on which the explanation rests. Jointly these sentences will be said to form the *explanans S* […]. The conclusion *E* of the argument is a sentence describing the explanandum-phenomenon.

Thus, read in bottom-up direction, an argument of the form (40) represents a DN explanation of the *explanandum E* by an *explanans* consisting of *lawlike sentences* $L_1, L_2, \ldots, L_r$ ('general laws') and *particular statements* $C_1, C_2, \ldots, C_k$ ('sentences describing particular facts'; Hempel 1965: 336–337). In opposite direction, such an argument represents a *DN prediction* of *E* from $C_1, C_2, \ldots, C_k$ and $L_1, L_2, \ldots, L_r$ (Hempel 1965: 365–366). *True* explanations or predictions are logically valid

24. 'Conversion in *S*' ('conv_S'), for example, is defined in Nolda (2012: Section 6.3.3, appendix B) as a one-place word-formation process – *i.e.* a function from one-place word-formation patterns to one-place formation operations – whose arguments have a formal formation means that does not change segmental properties of its arguments. A somewhat different possibility would be to define it negatively as a one-place word-formation process whose arguments are not arguments of other one-place word-formation processes like derivation, shortening, or reduplication.

arguments and have a true explanans and a true explanandum (Hempel 1965: 338). True explananda are true statements of the phenomenon to be explained, *i.e.* facts.

As examples for explananda in PR, consider the statements of word-formation relations in (41), already mentioned in Section 3 above and repeated here for convenience:

(41) a. $/\text{ˈfal}/^{\mathrm{W}}_{\text{‘falling event’}} \lessdot^{\mathrm{S}} /\text{ˈfal[ə]n}/^{\mathrm{W}}_{\text{‘to fall’}}$

b. $/\text{ˈfal}/^{\mathrm{St}}_{\text{‘falling event’}} \lessdot^{\mathrm{S}} /\text{ˈfal}/^{\mathrm{St}}_{\text{‘to fall’}}$

(41a) states an indirect word-formation relation between the lexical words $/\text{ˈfal}/^{\mathrm{W}}_{\text{‘falling event’}}$ and $/\text{ˈfal[ə]n}/^{\mathrm{W}}_{\text{‘to fall’}}$ in some specific spoken Modern German system **S**, and (41b) states a direct word-formation relation between the corresponding lexical stems $/\text{ˈfal}/^{\mathrm{St}}_{\text{‘falling event’}}$ and $/\text{ˈfal}/^{\mathrm{St}}_{\text{‘to fall’}}$ in **S**. I shall show now how to construct DN explanations of these explananda in PR, starting with (41b) and then turning to (41a).

A lawlike sentence that can be used in the explanans of an explanation of (41b) is the following one:

(42) *Theorem*

For every l, l_1, and S, if:

1. l and l_1 are lexical units in S and
2. there is an $\langle f, P, L, c\rangle$, $\langle f_1, P_1, L_1, c_1\rangle$, $\langle \varphi^1, \pi^1, \lambda^1, \sigma^1\rangle$, and ρ^1 such that:
   a. $\langle f, P, L, c\rangle$ and $\langle f_1, P_1, L_1, c_1\rangle$ instantiate l and l_1 in S, respectively,
   b. $\langle f_1, P_1, L_1, c_1\rangle$ is in the formation restriction of $\langle \varphi^1, \pi^1, \lambda^1, \sigma^1\rangle$ in S,
   c. ρ^1 is a one-place word-formation process in S,
   d. $\langle \varphi^1, \pi^1, \lambda^1, \sigma^1\rangle$ is in the domain of ρ^1, and
   e. $\rho^1\,(\langle \varphi^1, \pi^1, \lambda^1, \sigma^1\rangle)(\langle f_1, P_1, L_1, c_1\rangle) = \langle f, P, L, c\rangle$,

then $l \lessdot^{S} l_1$.

This is a valid theorem of PR which follows from axioms and definitions of PR's theoretical core, listed in the appendix (cf. in particular Axiom Schema 3 and Definition Schemes 15, 16, and 17 as well as the axioms and definitions presupposed there; for the intended variable interpretations cf. the appendix, too). Put in a nutshell, the theorem stated in (42) amounts to the following: a lexical unit l is directly formed from a lexical unit l_1 in a linguistic system S if a formation instance $\langle f, P, L, c\rangle$ instantiating l can be formed in S through a one-place word-formation process ρ^1 by means of an appropriate one-place word-formation pattern $\langle \varphi^1, \pi^1, \lambda^1, \sigma^1\rangle$ from a formation instance $\langle f_1, P_1, L_1, c_1\rangle$ instantiating l_1 in the formation restriction of the pattern.

In (37) in Section 3 it was illustrated how a formation instance instantiating $/\text{ˈfal}/^{\mathrm{St}}_{\text{‘falling event’}}$ is formed from a formation instance instantiating $/\text{ˈfal}/^{\mathrm{St}}_{\text{‘to fall’}}$ in **S**. There, the following entities were involved:

1. a one-place word-formation process in **S**, conversion in **S** ($conv_S$);
2. a word-formation pattern in **S**, repeated here as (43);
3. a formation instance instantiating $/\text{ˈfal}/^{St}_{\text{‘to fall’}}$ in **S** in the formation restriction of pattern (43), repeated here as (44);
4. a formation instance instantiating $/\text{ˈfal}/^{St}_{\text{‘falling event’}}$ in **S**, repeated here as (45).

(43) ⟨ini-acc,
basic-nstf,
masc-nst ∘ nounst,
event⟩

(44) ⟨/ˈfal/,
{Conv-VStf},
{VSt},
‘to fall’⟩

(45) ⟨/ˈfal/,
{Basic-NStf},
{NounSt, Masc-NSt},
‘falling event’⟩

With reference to these entities specific to the linguistic system **S**, the following states of affairs can be stated as potential theorems following from identificational sentences in a grammar and a dictionary of **S**:[25]

(46) a. *Theorem*
$/\text{ˈfal}/^{St}_{\text{‘falling event’}}$ and $/\text{ˈfal}/^{St}_{\text{‘to fall’}}$ are lexical units in **S**.
b. *Theorem*
(44) and (45) instantiate $/\text{ˈfal}/^{St}_{\text{‘to fall’}}$ and $/\text{ˈfal}/^{St}_{\text{‘falling event’}}$ in **S**, respectively.
c. *Theorem*
(44) is in the formation restriction of (43) in **S**.
d. *Theorem*
$conv_S$ is a one-place word-formation process.
e. *Theorem*
(43) is in the domain of $conv_S$.
f. *Theorem*
$conv_S$ ((43))((44)) = (45).

25. As argued for in Budde (2000) from an IL perspective, it is the task of linguistic theory to provide definitions for general linguistic terms, whose extensions are identified by system-specific theories, such as a grammar or a dictionary.

These theorems, corresponding to the clauses in the antecedent in (42), will function as particular statements in the explanans of our explanation of (41b).

We can now logically derive the explanandum (41b) from the explanans (46) and (42) as follows:

(47) particular statements (46)
lawlike sentence (42)
—————————————
statement (41b)

Basically, this argument takes the form of *modus ponens*: (42) is a universal implication whose antecedent and consequent parts are instantiated by (46) and (41b), respectively, where variables are replaced by system-specific constants. Being a logically valid argument, (47) represents a DN explanation (or, for that matter, as a DN prediction) of the explanandum (41b). Provided that (41b) is a true statement – a fact – and that the explanans is true, too, (47) is a true explanation (or a true prediction) of a word-formation fact: the fact that there is a direct word-formation relation between the lexical stems /ˈfal/$^{\text{St}}_{\text{‘falling event’}}$ and /ˈfal/$^{\text{St}}_{\text{‘to fall’}}$ in **S**.

In a similar way, the statement in (41a) of an indirect word-formation relation between the corresponding lexical words /ˈfal/$^{\text{W}}_{\text{‘falling event’}}$ and /ˈfal[ə]n/$^{\text{W}}_{\text{‘to fall’}}$ can be explained. For this explanandum, the following PR theorem can be used as a lawlike sentence in the explanans:

(48) *Theorem*
For every l, l_1, and S, if there is an l' and l'_1 such that:
1. l' and l'_1 are morphological correspondences of l and l_1 in S, respectively, and
2. $l' \mathrel{<\!\!\!-}^{S} l'_1$,

then $l \lessdot^{S} l_1$.

This theorem follows again from PR's theoretical core, listed in the appendix (cf. Definition Schemes 18, 19, and 20 as well as the definitions presupposed there). It states, roughly, that a lexical unit l is indirectly formed from a lexical unit l_1 in a linguistic system S if there is a direct word-formation relation between lexical units l' and l'_1 in S such that l' is a morphological correspondence of l in S and l'_1 is a morphological correspondence of l_1 in S. A lexical stem l' is a morphological correspondence of a lexical word l in a system S if l' is the stem of l in S (cf. Definition 9 in the appendix).

This lawlike sentence is again a universal implication. Instantiations of its antecedent, with variables replaced by system-specific constants, will function as particular statements in the explanans of our explanation of the explanandum (41a). These should be theorems of a grammar and a dictionary of the specific linguistic system **S**:

(49) a. /ˈfal/$^{\text{St}}_{\text{‘falling event’}}$ and /ˈfal/$^{\text{St}}_{\text{‘to fall’}}$ are morphological correspondences of /ˈfal/$^{\text{W}}_{\text{‘falling event’}}$ and /ˈfal[ə]n/$^{\text{W}}_{\text{‘to fall’}}$ in **S**, respectively.

b. /ˈfal/$^{\text{St}}_{\text{‘falling event’}}$ $<^{\text{S}}$ /ˈfal/$^{\text{St}}_{\text{‘to fall’}}$

Note that (49b) is identical to (41b), the statement of a direct word-formation relation between /ˈfal/$^{\text{St}}_{\text{‘falling event’}}$ and /ˈfal/$^{\text{St}}_{\text{‘to fall’}}$ which was explained above.

As before, the explanandum (41a) can be logically derived from the explanans (49) and (48) in an argument in basically *modus ponens* form:

(50) particular statements (49)
lawlike sentence (48)
―――――――――――――
statement (41a)

This argument represents again a DN explanation (or a DN prediction) of the explanandum. Insofar as the explanandum and the explanans are true, we have arrived at a true explanation (or a true prediction) of the word-formation fact that there is an indirect word-formation relation between the lexical words /ˈfal/$^{\text{W}}_{\text{‘falling event’}}$ and /ˈfal[ə]n/$^{\text{W}}_{\text{‘to fall’}}$ in **S**, which is based on the word-formation fact that there is a direct word-formation relation between the lexical stems /ˈfal/$^{\text{St}}_{\text{‘falling event’}}$ and /ˈfal/$^{\text{St}}_{\text{‘to fall’}}$ in **S**.

Explanations of statements of word-formation relations between lexical units $l, l_1,\ldots, l_n$ in a linguistic system *S* answer questions of the sort: Why is *l* formed from $l_1,\ldots, l_n$ in *S*? Typically, such questions pertain to conventionalized, ‘existing’ lexical units in the vocabulary of *S*. Predictions of such statements, however, answer questions like: Which *l* may be formed from $l_1,\ldots, l_n$ in *S*? Here, also non-conventionalized, but still ‘possible’ lexical units in the lexicon of *S* are relevant, provided that the word-formation pattern is not totally unproductive.[26] This even holds for the pattern in (43), which is occasionally used in Modern German to form non-conventionalized nouns like /ˈtsupf/$^{\text{W}}_{\text{‘plucking event’}}$ from verbs like /ˈtsupf[ə]n/$^{\text{W}}_{\text{‘to pluck’}}$ (cf. Rapp 2006: 425 for more examples):

(51) *dem allerletzten Zupf an der Saite*
the very.last plucking.event at the string
(St. Galler Tagblatt 75, 2008: 45; cited from the German Reference Corpus)

26. In the PR view, productivity is not a question of the word-formation patterns themselves but rather of their *actuation* in the sense of Koefoed & Marle (2000). In particular, the productivity of a certain word-formation pattern may be independent of its formation restriction (the *morphological domain* in the sense of Koefoed & Marle 2000; for discussion cf. Becker 1990: Section 3.7).

As mentioned in the introduction, 'statement' is understood here as an abstract declarative sentence. Abstract sentences are, by definition, non-observable. What is more, the entities statements of word-formation relations explicitly or implicitly involve are abstract, too: lexical units, linguistic systems, word-formation processes, and word-formation patterns. Sentences like these, however, may figure in sentences about spatiotemporal, observable entities, for instance: speaker *s* of an idiolect determined by system *S* behaves at place *p* and time *t* in a way by which *s* expresses that *s* believes that *l* is formed from $l_1,\ldots, l_n$ in *S*. A sentence of this form might describe, for example, some observed behaviour of a subject in a psycholinguistic experiment which examines word-formation relations in the system of a certain language or variety to which the idiolect spoken by the subject belongs. Sentences of this kind can, of course, themselves be explananda of DN explanations or predictions. An appropriate lawlike sentence for the explanans could be a sentence stating that such a state of affairs is to be expected if *l* is formed from $l_1,\ldots, l_n$ in *S* and if certain further (psychological or methodological) conditions are fulfilled. One of the particular statements of the explanans would then be a statement of the word-formation relation in question – in a similar way as statement (41b) of a direct word-formation relation (itself explained in (47)) functions as a particular statement in the explanans in (50), the explanation provided above for statement (41a) of an indirect word-formation relation.

5. Summary and conclusions

The present paper discussed foundational issues of a realist word-formation theory, which takes linguistic units and the linguistic systems that determine them to be abstract entities, underlying both linguistic competence and performance. The discussion was couched in terms of the axiomatically formalized Pattern-and-Restriction Theory (PR) on the example of a selected word-formation pattern in some spoken Modern German system. It was proposed that the word-formation facts to be described and explained or predicted are true statements of word-formation relations between abstract lexical units in the linguistic system under consideration. Word-formation relations, underlying lexical motivation relations, involve one lexical product and one or more lexical bases as well as – explicitly or implicitly – a word-formation process and a word-formation pattern. It was shown how statements of word-formation relations can be explained or predicted in terms of deductive-nomological (DN) explanations or predictions by logically deriving them from general theorems of the word-formation theory and system-specific theorems of a grammar and a dictionary of the linguistic system. Those theorems concern properties of lexical units, word-formation processes, word-formation patterns, and their associated formation restrictions. Lexical units are understood in the sense of the realist framework of Integrational Linguistics (IL) as abstract pairings of a paradigm and a lexical meaning. Conventionalized, 'existing' lexical units in a linguistic system, 'known' by speakers of the corresponding idiolect, variety, or language, as well as non-conventionalized, but still 'possible' lexical units are given by the lexicon of the system. Word-formation patterns as well as inflection patterns and the associated formation restrictions are provided for by the formation component in the morphosyntactic part of the system, while word-formation processes and word-formation relations are ultimately determined by the word-formation patterns in the system and their formation restrictions.

Since lexical units are given independently from word formation, word-formation relations between them can be identified in word-formation description by only partially specifying formal, categorial, and semantic properties of lexical bases and products in terms of word-formation patterns and formation restrictions. Thus, word-formation description in PR, such as in the word-formation part of a system-specific grammar, basically amounts to stating word-formation patterns and hypotheses about their associated formation restrictions (hence the theory's name). In addition, this realist word-formation theory does not restrict the objects word-formation facts are about to conventionalized lexical units, nor does it exclude word-formation relations through processes by means of more or less unproductive patterns from consideration. Thereby word-formation theory and word-formation description can account both for the formation of 'existing' lexical units and for the potential formation of 'new' lexical units. Both accounts are, according to Aronoff (1976: 1), tasks of (word-formation) morphology:[27]

> It [morphology; A.N.] is […] concerned with words which are not simple signs, but which are made up of more elementary ones. This concern encompasses two distinct but related matters: first, the analysis of existing composite words, and second, the formation of new composite words. A unified theory of morphology should be capable of dealing with both of these areas in a unified and coherent manner […].

PR, I hope to have shown, is such a theory.

27. As a matter of fact, PR is a theory of word formation (and inflection) in morphology *and* syntax, situating the formation component of a linguistic system in its morphosyntactic part. In this view, which is also shared by the Process Model of Word Formation (Lieb 2013), morphological word formation is the direct formation of lexical stems, while syntactic word formation is the direct formation of lexical words, such as the formation of 'nominalized adjectives' in Modern German like /ˈklain[ə]r/$^{W}_{\text{'small person'}}$ (cf. Note 8 in Section 2 above).

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Appendix

List of symbols

Notational conventions

‘St’:	lexical stem.
‘W’:	lexical word.

Symbols for categories

‘1Pers-Vf’:	first person verb form.
‘Act-Vf’:	active verb form.
‘Basic-NStf’:	basic noun-stem form.
‘Basic-VStf’:	basic verb-stem form.
‘Comp-VStf’:	compounding verb-stem form.
‘Conv-VStf’:	conversion verb-stem form.
‘Ind-Vf’:	indicative verb form.
‘Ind-VStf’:	indicative verb-stem form.
‘Inf-VStf’:	infinitival verb-stem form.
‘Masc-N’:	masculine nominal word.
‘Masc-NSt’:	stem of a masculine nominal word.
‘Non2/3PersSing-VStf’:	non-second-or-third-person-singular verb-stem form.
‘NonClausInf-Vf’:	non-clausal infinitival verb form.
‘NonPerf-Vf’:	non-perfect verb form.
‘Noun’:	noun.
‘NounSt’:	noun stem.
‘Plur-Vf’:	plural verb form.
‘Pres-Vf’:	present tense verb form.
‘Pres-VStf’:	present tense verb-stem form.
‘VSt’:	verb stem.

Symbols for relations, functions, and operations

‘$<$’:	word-formation relation.
‘⋲’:	direct word-formation relation.
‘$\lessdot$’:	indirect word-formation relation.
‘$\circ$’:	function composition.
‘basic-nstf’:	formation of a basic noun-stem form.
‘conv’:	conversion.
‘event’:	formation of a concept according to the scheme ‘event consisting in a non-state denoted by the base meaning’.
‘ini-acc’:	initial accentuation.
‘masc-nst’:	formation of a stem of a masculine nominal word.
‘nounst’:	formation of a noun stem.

Variables

‘B^n’: non-empty n-place relations between $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$ (in particular, n-place formation restrictions).
‘c’: (potential) concepts.
‘f’: sequences of morphological or syntactic atoms.
‘F^n’: non-empty one-place functions from entities $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ to relations B^n (in particular, functions from formation patterns to formation restrictions).
‘l’: pairings of a paradigm and a concept.
‘L’: sets of non-empty sets of entities l (in particular, paradigmatic categorizations of entities l).
‘n’: natural numbers ≥ 1.
‘P’: sets of non-empty sets of entities f (in particular, paradigmatic categorizations of entities f).
‘R^n’: n-place relations between entities l (in particular, n-place word-formation relations).
‘S’: linguistic systems.
‘K’: sets of functions F^n (with arbitrary n).
‘β^n’: n-place operations on entities $\langle f, P, L, c \rangle$ (in particular, n-place formation operations).
‘λ^n’: n-place operations on entities L (in particular, n-place lexical formation means).
‘π^n’: n-place operations on entities P (in particular, n-place paradigmatic formation means).
‘ρ^n’: one-place functions from entities $\langle f, P, L, c \rangle$ to entities β^n (in particular, n-place word-formation processes).
‘σ^n’: n-place operations on entities c (in particular, n-place semantic formation means).
‘φ^n’: n-place operations on entities f (in particular, n-place formal formation means).

Ambiguous constant

‘$\mathbf{S}$’: some specific spoken Modern German system.

Axiomatic formalization

Below are listed the axioms and definitions from the theoretical core of PR on the basis of which the theorems used for explaining statements of word-formation relations in Section 4 are derived. This is part of a revised and generalized version of the formalization of PR in Nolda (2012: appendix A).[28]

Definition 1

$\langle f, P, L, c \rangle$ *instantiates* l in S if, and only if:

1. f is a form of l in S or a citation variant of such a form,
2. P is a categorization of f in the paradigm of l in S or a subset of such a categorization,
3. L is a categorization of l in S or a subset of such a categorization, and
4. c is the meaning of l or a concept implied by that meaning.

Axiom 1

For every S, the morphosyntactic part of S contains exactly one component K such that, for every n, there is at most one F^n in K.

28. This concerns in particular its generalization for inflection (cf. Note 17 above).

Definition 2

The *formation component* of S = the component in the morphosyntactic part of S according to Axiom 1.

Definition Schema 3

F^n is the *n-place formation foundation* in S if, and only if, F^n is in the formation component of S.

Definition 4

B^n is the *formation restriction* of $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in S if, and only if, $\langle\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle, B^n \rangle$ is in the n-place formation foundation in S.

Axiom Schema 2

For every B^n, $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$, $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$, and S, if

1. B^n is the formation restriction of $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in S and
2. $\langle\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$ is in B^n,

then there is an $l_1, \ldots, ln$ such that:

3. $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$ instantiate $l_1, \ldots, l_n$ in S, respectively.

Definition Schema 5

$\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ is an *n-place formation pattern* in S if, and only if, there is a B^n such that B^n is the formation restriction of $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in S.

Definition Schema 6

$\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ is *applicable* to $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$ if, and only if:

1. $\langle f_1, \ldots, f_n \rangle$ is in the domain of φ^n,
2. $\langle P_1, \ldots, P_n \rangle$ is in the domain of π^n,
3. $\langle L_1, \ldots, L_n \rangle$ is in the domain of λ^n, and
4. $\langle c_1, \ldots, c_n \rangle$ is in the domain of σ^n.

Definition 7

The *operation specified by* $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ = that β^n for which the following holds:

1. β^n is an n-place operation on entities $\langle f, P, L, c \rangle$ and
2. for every $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$:
   a. $\langle\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle\rangle$ is in the domain of β^n if, and only if, $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ is applicable to $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$ and
   b. if $\langle\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle\rangle$ is in the domain of β^n, then the following holds:
   $$\beta^n (\langle\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle\rangle) = \langle \varphi^n (f_1, \ldots, f_n), \pi^n (P_1, \ldots, P_n), \lambda^n (L_1, \ldots, L_n), \sigma^n (c_1, \ldots, c_n) \rangle.$$

Definition Schema 8

β^n is an *n-place formation operation* in S if, and only if, there is an n-place formation pattern $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in S such that β^n is the operation specified by $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$.

Definition 9

l' is a *morphological correspondence* of l in S if, and only if, l' is the stem or another morphological variant of l in S.

Definition 10

l is *lexically equivalent* to l' in S if, and only if:

1. $l = l'$,
2. l is a morphological correspondence of l' in S, or
3. l' is a morphological correspondence of l in S.

Definition Schema 11

$\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ is an *n-place word-formation pattern* in S if, and only if:

1. $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ is an n-place formation pattern in S, and
2. for every $\langle \langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle \rangle$ in the domain of the operation β^n specified by $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$, there is an $l, l_1, \ldots, l_n$ such that:
   a. $\beta^n (\langle \langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle \rangle), \langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$ instantiate $l, l_1, \ldots, l_n$ in S, respectively, and
   b. l is not lexically equivalent to l_1 or … or l_n in S.

Definition Schema 12

ρ^n is an *n-place formation process* in S if, and only if:

1. ρ^n is a non-empty one-place function from n-place formation patterns in S to n-place formation operations in S and
2. for every $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in the domain of ρ^n, $\rho^n (\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle)$ is the operation specified by $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$.

Definition Schema 13

ρ^n is an *n-place word-formation process* in S if, and only if:

1. ρ^n is an n-place formation process in S and
2. the domain of ρ^n is a set of n-place word-formation patterns in S.

Axiom Schema 3

For every $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$, ρ^n, $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$, and S, if:

1. ρ^n is an n-place formation process in S,
2. $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ is in the domain of ρ^n, and
3. $\langle \langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle \rangle$ is in the formation restriction of $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in S,

then there is an $\langle f, P, L, c \rangle$ and l such that:

4. $\langle f, P, L, c \rangle$ instantiates l in S and
5. $\rho^n (\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle)(\langle \langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle \rangle) = \langle f, P, L, c \rangle$.

Definition Schema 14

$\langle f, P, L, c \rangle$ *can be formed* from $\langle f_1, P_1, L_1, c_1 \rangle, \ldots, \langle f_n, P_n, L_n, c_n \rangle$ through ρ^n by means of $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in S if, and only if:

1. ρ^n is an n-place formation process in S,
2. $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ is in the domain of ρ^n,

3. $\langle\langle f_1, P_1, L_1, c_1\rangle,\ldots,\langle f_n, P_n, L_n, c_n\rangle\rangle$ is in the formation restriction of $\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle$ in S, and
4. $\rho^n(\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle)(\langle f_1, P_1, L_1, c_1\rangle,\ldots,\langle f_n, P_n, L_n, c_n\rangle) = \langle f, P, L, c\rangle$.

Definition Schema 15

l is *directly formed* from $l_1,\cdots, ln$ through ρ^n by means of $\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle$ in S ($l \lessdot^S_{\rho^n(\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle)} l_1 + \cdots + l_n$) if, and only if:

1. $l, l_1,\ldots, l_n$ are lexical units in S,
2. ρ^n is an n-place word-formation process in S, and
3. there is an $\langle f, P, L, c\rangle, \langle f_1, P_1, L_1, c_1\rangle,\ldots,\langle f_n, P_n, L_n, c_n\rangle$ such that:
   a. $\langle f, P, L, c\rangle, \langle f_1, P_1, L_1, c_1\rangle,\cdots,\langle f_n, P_n, L_n, c_n\rangle$ instantiate $l, l_1,\cdots, ln$ in S, respectively, and
   b. $\langle f, P, L, c\rangle$ can be formed from $\langle f_1, P_1, L_1, c_1\rangle,\ldots,\langle f_n, P_n, L_n, c_n\rangle$ through ρ^n by means of $\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle$ in S.

Definition Schema 16

l is *directly formed* from $l_1,\ldots, l_n$ through ρ^n in S ($l \lessdot^S_{\rho^n} l_1 + \cdots + l_n$) if, and only if, there is a $\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle$ such that $l \lessdot^S_{\rho^n(\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle)} l_1 + \cdots + l_n$.

Definition Schema 17

l is *directly formed* from $l_1,\ldots, l_n$ in S ($l \lessdot^S l_1 + \ldots + l_n$) if, and only if, there is a ρ^n such that $l \lessdot^S_{\rho^n} l_1 + \cdots + l_n$.

Axiom Schema 4

For every $l, l_1,\ldots, l_n$, and S, if $l \lessdot^S l_1 + \ldots + l_n$, then:

1. l is a lexical stem or word in S and
2. $l_1,\ldots, l_n$ are lexical stems, stem groups, words, or word groups in S.

Axiom Schema 5

For every $l, l_1,\ldots, l_n$, and S, if:

1. $l \lessdot^S l_1 + \ldots + l_n$ and
2. l is a lexical word in S,

then $l_1,\ldots, l_n$ are lexical words or word groups in S.

Definition Schema 18

l is *indirectly formed* from $l_1,\ldots, l_n$ through ρ^n by means of $\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle$ in S ($l <^S_{\rho^n(\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle)} l_1 + \cdots + l_n$) if, and only if, there is an $l', l'_1, \ldots, l'_n$ such that:

1. $l, l_1,\ldots, l_n$ are lexically equivalent to $l', l'_1, \ldots, l'_n$ in S, respectively,
2. $l \neq l'$ or $l_1 \neq l'_1$ or … or $l_n \neq l'_n$, and
3. $l' \lessdot^S_{\rho^n(\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle)} l'_1 + \cdots + l'_n$.

Definition Schema 19

l is *indirectly formed* from $l_1,\ldots, l_n$ through ρ^n in S ($l <^S_{\rho^n} l_1 + \cdots + l_n$) if, and only if, there is a $\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle$ such that $l <^S_{\rho^n(\langle\varphi^n, \pi^n, \lambda^n, \sigma^n\rangle)} l_1 + \cdots + l_n$.

Definition Schema 20

l is *indirectly formed* from $l_1,\ldots, l_n$ in S ($l <^S l_1 + \ldots + l_n$) if, and only if, there is a ρ^n such that $l <^S_{\rho^n} l_1 + \cdots + l_n$.

Definition Schema 21

l is *formed* from $l_1, \ldots, l_n$ through ρ^n by means of $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ in S ($l <^S_{\rho^n(\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle)} l_1 + \cdots + l_n$) if, and only if:

1. $l \mathrel{<\!\!\!-}^S_{\rho^n(\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle)} l_1 + \cdots + l_n$ or
2. $l \lessdot^S_{\rho^n(\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle)} l_1 + \cdots + l_n$.

Definition Schema 22

l is *formed* from $l_1, \ldots, l_n$ through ρ^n in S ($l <^S_{\rho^n} l_1 + \cdots + l_n$) if, and only if, there is a $\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle$ such that $l <^S_{\rho^n(\langle \varphi^n, \pi^n, \lambda^n, \sigma^n \rangle)} l_1 + \cdots + l_n$..

Definition Schema 23

l is *formed* from $l_1, \ldots, l_n$ in S ($l <^S l_1 + \ldots + l_n$) if, and only if, there is a ρ^n such that $l <^S_{\rho^n} l_1 + \cdots + l_n$.

Definition 24

R^n is a *direct word-formation relation* in S if, and only if, there is a ρ^n such that $R^n = \{\langle l, l_1, \ldots, l_n \rangle \mid l \mathrel{<\!\!\!-}^S_{\rho^n} l_1 + \cdots + l_n\}$.

Definition 25

R^n is an *indirect word-formation relation* in S if, and only if, there is a ρ^n such that $R^n = \{\langle l, l_1, \ldots, l_n \rangle \mid l \lessdot^S_{\rho^n} l_1 + \cdots + l_n\}$.

Definition 26

R^n is a *word-formation relation* in S if, and only if, there is a ρ^n such that $R^n = \{\langle l, l_1, \ldots, l_n \rangle \mid l <^S_{\rho^n} l_1 + \cdots + l_n\}$.

CHAPTER 9

Cognitive propositions in realist linguistics

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The paper presents a cognitive conception of propositions as semantic contents of (some) declarative sentences. The conception expands solution spaces for previously intractable empirical problems in natural-language semantics and pragmatics, while also explaining how an agent who is unable to cognize propositions can know or believe them, and how sophisticated agents acquire the concept and believe things about them by monitoring their own cognitions. Finally, an account is given of what it is for a sentence to mean that p in a language that doesn't require having thoughts about p or L. Nevertheless, semantics isn't psychology; agents with different psychologies can speak semantically identical languages, while those with the same purely internal states (embedded in similar immediate environments) can speak different languages. Cognitive semantics can be realist and naturalistic without being a branch of psychology.

Keywords: semantics, hyperintensional, representational content, cognitive content, recognition of recurrence

1. Introduction

To be a realist about linguistic theories is to take them to be about real linguistic entities – the words, phrases, sentences, structures, and their properties that together make up languages. These, not fine-grained realizations of languages in speakers or populations are the subject matter of linguistics. Whenever it is possible for agents with different psychologies to speak the same language, individuated by the semantic, syntactic, and phonological properties of its expressions, the psychological respects in which agents differ don't individuate their languages. Linguistic realism contends that language individuation by linguistic properties is to a considerable extent extra-psychological. It is *not* part of linguistic realism,

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as I understand it, that broadly psychological facts have no role to play. I will illustrate with semantics.

Semantic realism requires meanings and semantic contents that standard versions of intensional semantics don't provide. At best those theories offer empirically inadequate models. To replace them with semantically real things, we must first recognize the artificiality of what we have been given. A semantic theory of L interprets its well-formed expressions, including sentences, explaining how interpretations of some relate to those of others. By the interpretation of an expression, I mean its semantic content, which is different from *what one who speaks the language understands when one understands it*. I will return to this distinction after saying more about *propositions*.

2. Propositions

2.1 Propositions in intensional semantics

Propositions are objects of attitudes, primary bearers of truth conditions, contents of some cognitive and perceptual states, meanings of some sentences, and semantic contents, at contexts, of others. They are not sets of truth-supporting circumstances. Nor, of course, are sentence meanings functions from contexts to such sets. Elsewhere I have argued that the coarse-grainedness problem for propositions as sets of truth-supporting circumstances can't be solved by substituting epistemologically possible states, logically possible states, or situations for metaphysically possible world-states (cf. Soames 1987, 2008b). Nor can it be solved by invoking so-called diagonal propositions, either pragmatically *a la* Stalnaker (1978) or semantically *a la* Chalmers (1996) (cf. Soames 2005, 2006). This is one reason why the truth-theoretic entities we have been given can't play the roles that have typically been assigned to them.

But it is only one reason. Another is that meanings, i.e. interpretations, don't, on pain of regress, require *further* interpretation. *But without interpretation by us, sets of truth-supporting circumstances don't represent anything as being any way*, and so don't have truth conditions (cf. Soames 2010a). Is the set containing just world-states 1, 2, 3 true or false? Since it doesn't represent anything as being this way or that, it can't be either. We could, if we wished, *interpret* it as representing *the actual world-state as being in the set*, and so as being true iff no state *outside* the set were instantiated. But we could equally well interpret it as representing the actual world-state as *not being in the set*, and so as being true iff no state *inside* it was instantiated. Without interpretation by us, the set doesn't represent anything, or have truth conditions.

The *function* assigning *truth* and *falsity* to world-states is no better. Why not?

(i) Truth is the property a proposition p has when the world is as p represents it. It is a property which, when predicated of p, gives us a claim we are warranted in accepting, believing, or doubting iff we are warranted in taking that attitude to p. This is what truth is. Since *propositions are conceptually prior to truth, truth can't be one of the things from which propositions are constructed*.

(ii) The illusion that a function assigning world-states 1–3 truth and others falsity represents something as being some way comes from presupposing a conceptually prior notion of propositions in which *world-states are properties predicated of the world* and each assignment of truth value to a world-state w is correlated with *the proposition that predicates w of the world*. Since that proposition is true iff the world is in state w, functions from world-states to truth values can be associated with disjunctions of such propositions. But this doesn't justify taking those functions to be propositions because it presupposes a prior notion of propositions on which they are *not* functions.

(iii) Taking propositions to be functions from world-states to truth values goes with taking properties to be functions from world-states to extensions. This conflicts with taking world-states to be properties, for surely a world-state *isn't* a function from world-states to anything. But if properties aren't such functions then, propositions aren't either.

(iv) World-states are properties of making complete world-stories, the constituents of which are propositions, true. *Since both truth and world-states* are conceptually downstream from propositions, they aren't building blocks from which propositions are constructed.[1]

In short, propositions aren't what intensional semanticists have said they are. Nor is the two-place predicate *is true at w* the undefined technical primitive it has often been said to be. If it were, then *nothing* more about the meaning of S would follow from the theorem *For all world-states w,* S *is true at w iff at w, the earth moves* than follows from the pseudo-theorem *For all world-states w, S is T at w iff at w, the earth moves*.[2] Suppose instead we analyzed *S is true at w* as saying that *if w were actual (instantiated), then S would be true*. Although this is a step in the right direction, it is not quite right, because S might fail to exist at some world-states at which the earth moves, or S might exist, but *not* mean (at some earth-moving states), what it actually means. Fortunately, this problem is easily fixed. To say that *S is*

1. See Chapter 5, Soames (2010b).

2. 'S' is here a metalinguistic variable ranging over sentences.

true at w is to say that *S expresses a proposition that would be true if w were actual (instantiated)*. To understand *is true at* in this way is to presuppose antecedent notions of *the proposition S expresses* and *the monadic notion of truth* applying to it. Taking them at face value, we invoke a pretheoretic triviality connecting meaning and truth – *if S means, or expresses, the proposition that the earth moves, then necessarily the proposition expressed by S is true iff the earth moves*. This triviality plus the theorem *S is true at w iff at w, the earth moves* entail that S means something *necessarily equivalent to the proposition that the earth moves*. Even this doesn't *fully* specify S's meaning, but it does give us information about S's meaning. Without this appeal to prior notions of truth and propositions, intensional truth theories *don't provide any information whatsoever about meaning*. To transform them into genuine *semantic* theories, we must map sentences to real propositions, the truth conditions of which are derived from their representational properties. We don't need real, representational propositions to *complete* intensional semantics. We need them to have any semantics at all.

2.2 Propositions as cognitive act types

This isn't an argument for traditional Fregean or Russellian propositions. Although their individuation conditions are better suited to accommodating the attitudes, they are still too coarse grained. Worse, the n-tuples of objects, properties, or senses they provide are merely models. Since n-tuples, or other formal structures, don't, without interpretation by us, represent anything as being any way, they aren't meanings or primary truth bearers.[3]

This, I believe, was the inchoate insight behind Donald Davidson's most telling objection to structured propositions as sentence meanings. Commenting on them in Davidson (1967), he remarked,

> Paradoxically, the one thing meanings do not seem to do is oil the wheels of a theory of meaning […] My objection to meanings in the theory of meaning is not that they are abstract or that their identity conditions are obscure, but that they have no demonstrated use. (Davidson 2001: 21–22)[4]

His point was correct; taking structured entities to be meanings (or *semantic contents*) of sentences doesn't help us give a theory of meaning, *unless one can read off which things a sentence represents to be which ways from the structured entity it*

3. Soames (2010a)

4. See the discussion on in Chapter 4 of Soames (2010a).

expresses. Since this information *can't* be read off traditional structured propositions, we need a new conception.

The needed conception inverts the traditional Frege/Russell idea that the intentionality of propositions is explanatorily *prior* to that of agents. On that idea, agents who entertain propositions cognitively represent things as bearing certain properties *because* the propositions entertained do. But that is mystery mongering. It is mysterious what such primitively representational entities are, it is mysterious what cognizing them amounts to, and it is mysterious how and why *our* cognizing them results in *our* representing things as bearing properties. The way to reduce the mystery is to start with the obvious fact that *agents* represent things as being various ways when they think of them as being those ways. Next we solve for two unknowns. What kind of entity P and what relation R can together play the roles of *propositions* and *entertaining* in our theories by guaranteeing that agents who bear R to something of kind P thereby represent things as being some way? If we find such P and R, we can *explain* the intentionality of things of kind P by *deriving* it from the intentionality of agents who bear R to them. If for A to bear R to p* *just is* for A to represent o as *being hot*, then p* may be deemed true iff o is as it is represented to be – hot.

Seen in this way, the answer to the question 'What are propositions and what is it to entertain one?' is obvious. *Propositions are repeatable, purely representational, cognitive act types or operations; to entertain one is not to cognize it but to perform it*. When I perceive or think of o as red, I *predicate* the property *being red* of o, which is to *represent o as red*. This act-type represents o as red in a sense similar to the derivative senses in which acts can be *insulting* or *irresponsible*. Roughly put, an act is insulting when for one to perform it is for one to insult someone; it is irresponsible when to perform it is to neglect one's responsibilities. The same sort of derivative sense of *representing* allows us to assess the accuracy of an agent's sayings or cognitions. When to perceive or think of o as P is to represent o as it really is, we identify an entity, a particular act-type of perceiving or thinking, plus a property it has when the cognition is accurate. The entity is a proposition, which is the cognitive act of representing o as P. The property is truth, which the act-type has iff to perform it is to represent o as o really is.

2.3 Predication

Although to entertain the proposition that *o is red* is to predicate redness of o, and so to represent o as red, it is not to commit oneself to o's being red. We often predicate a property of something without committing ourselves to its having the property, as when we *imagine* o to be red, or visualize it as red, or merely overhear it being described as red. Hence, predication isn't inherently committing.

Nevertheless, some instances of it, e.g. those involved in judging or believing, are either themselves committing, or essential to acts that are. In this, the act-type *predicating redness of o* is like the (determinable) act-type *traveling to work*, which, though not inherently effortful, has (determinate) instances, like biking to work, that are. Thus, to *judge* that o is red, is to predicate redness of o in a committal manner, which involves forming, or activating already formed, dispositions to act, cognitively and behaviorally, toward o in specific ways. To *believe* o to be red is (roughly) to be disposed to judge it to be. The story is similar for attitudes like doubting that *don't* aim at truth. The things doubted may be true or false, just as the things believed may be. Since what is believed by x may be doubted by y, truth- and non-truth-normed attitudes have the same propositional objects. Since propositions are act-types, and since for any act-type A, A is identical with the act-type *performing A*, entertaining a proposition is *the act-type* – because it is the proposition – in terms of which other attitudes with the same object are defined.

This is the basis of a naturalistic epistemology of propositions. Since believing p doesn't require cognizing p, any creature that can perceive or think of p's subject matter can believe p, whether or not it can predicate properties of propositions. Knowing things *about* propositions requires the further ability to distinguish one's cognitive acts from one another. One who can do this can ascribe attitudes to oneself and others, and predicate properties of propositions. Focusing on their own cognitions, self-conscious agents identify distinct propositions as distinct representational states or operations, and come to conceive of truth as a form of accuracy.

2.4 Complex propositions

So far I have spoken of simple propositions, which predicate properties of objects. Complex propositions involve additional operations. But the idea is always the same. How a proposition represents things is read off the act-type with which it is identified, from which we derive its truth conditions. In the simple case, the proposition *that o is red* predicates redness of o and so *represents o as being red*, which is what any conceivable agent who entertains it represents. Note, the way a proposition represents things to be is, by definition, the way any conceivable agent at any conceivable world-state represents things to be by entertaining (i.e. performing) it. Since what a proposition represents doesn't change from world-state to world-state, its truth conditions don't either. Thus, the proposition *that o is red* is true at any world-state w iff were w actual, things, in this case o, would be as the proposition represents them, in this case red. Since o may be red at w whether or not the proposition exists or is entertained at w, the proposition doesn't have to exist or be entertained in order to be true.[5]

3. Foundational and empirical advantages of cognitive propositions

This conception explains both how an organism without the ability to cognize a proposition can know or believe one and how sophisticated agents acquire the concept, and come to know things about propositions by monitoring their own cognitions. We also get a plausible story about what it is for a proposition to be *the meaning (semantic content) of* a sentence. *For S to mean p in L is (to a first approximation) for speakers of L to use S to perform p*. One who understands the sentence 'The earth is round' uses the name to pick out the planet and the predicate to ascribe *being round* to it. To do this is *to perform* the act-type that is the proposition (semantically) expressed by the sentence in a special way. It follows that one's use of the sentence *is* one's entertaining the proposition it expresses. Since no other cognition is needed, understanding what S means in L (in the sense of knowing its semantic content) doesn't require having any thoughts about p or L, let alone *knowing that* S stands in some relation R to p and L.

These are *foundational* advantages of cognitive propositions. Their *empirical* advantages for theories of language and information are equally important. Unlike arithmetic, the theorems of which didn't depend on the attempted logicist reductions of Frege and Russell, current empirical theories involving propositions yield different results when combined with different conceptions of propositions. As I argue in Soames (2015), many familiar, and seemingly recalcitrant, problems posed by hyperintensional constructions have arisen from conceptions of propositions that don't individuate them correctly. This problem is directly addressed by taking propositions to be a species of purely representational cognitive act-types of operations.[6] Consider the generic act-type of traveling to work and the more specific act-type of driving to work that relates an agent to the same start and end

5. Being repeatable act-types or operations that are capable of being performed with or without the use of language, propositions are neither sentences nor spatio-temporally locatable events in which agents perform them. In this sense they are abstract, rather than concrete, objects. Although I do not take it for granted that *all* abstract objects exist necessarily, i.e. at every world-state, the issue of whether or not propositions do is irrelevant to their utility for us. Whether or not propositions must exist at a world-state is irrelevant to whether or not they are true at that state.

6. The distinction between purely representational act-types that are propositions and those that are not is made in Chapter 2 of Soames (2015).

points, even though the latter constrains how the traveling occurs in a way that the former does not. It is the same with the generic act-type of predicating *being so-and-so* of an object o and the more specific act-type of doing so by identifying o as predication target in a certain way. Both represent the same thing as bearing the same property, even though the latter constrains how the object is cognized in a manner that the former doesn't. In this way, the cognitive conception of propositions provides individuation conditions that result in *cognitively distinct* but *representationally identical* propositions. These propositions represent the same things as being the same ways, and so impose identical truth conditions on the world, while imposing different conditions on minds that entertain them. This opens up new explanatory opportunities.[7]

3.1 Cognitively distinct but representationally identical propositions

Consider (1) and (2).

(1) a. Russell tried to prove (the proposition) that arithmetic is reducible to logic.
  b. Russell tried to prove logicism.

(2) a. Mary believes that Russell tried to prove that arithmetic is reducible to logic.
  b. Mary believes that Russell tried to prove logicism.

Let 'logicism' be a Millian name for the proposition L *that arithmetic is reducible to logic*, designated by the directly referential *that*-clause. Although L is what the two terms contribute to the representational contents of (1) and (2), (1a) and (1b) express different propositions, and (2a) and (2b) can differ in truth value. If Mary picked up the name 'logicism' by hearing it used to designate some thesis in the philosophy of mathematics that Russell tried to prove, (2b) may be true, even if she has no clue what he thought about arithmetic, in which case (2a) is false. How can this be? Although propositions (1a) and (1b) each require one who entertains it to predicate *trying to prove* of Russell and L, (1a) also requires one to identify L by entertaining it. Thus *to perform, i.e. to entertain (accept or believe), proposition (1a) is to perform, i.e. entertain (accept or believe), (1b), but not conversely* (just as to perform the act of driving to work is to perform the act of traveling to work, but not conversely).[8] From this, the different truth conditions of (2a) and (2b) follow.

7. See Chapters 2–8 of Soames (2015).

8. This observation holds necessarily when applied to the propositions that are the different semantic contents of (1a) and (1b). See Soames (2015: 39–43). In addition, sentence (1b) can

Because propositions are cognitive acts, they can place different constraints on how an agent cognizes an item, even when they predicate the same property of the same things.

Next consider (3) and (4).

(3) a. I am in danger. *Said by SS*
b. SS is in danger.

(4) a. I believe that I am in danger. *Said by SS*
b. SS believes that SS is in danger.

Since (3a) and (3b) express representationally identical but cognitively distinct propositions, (4a) can be false even if (4b) is true. This happens when I see SS in a mirror and believe him to be in danger, without believing I am in danger. Here, we distinguish predicating property P of an agent A cognized *in the 1st-person way* from predicating P of A however cognized. To do the first is to do the second, but not conversely, so the acts are different. Since the same property is predicated of the same agent, they are cognitively distinct but representationally identical propositions. In this way, we capture the fact that my epiphany – *I am the one in danger* – involves believing a truth I hadn't previously believed, even if my believing it is just my coming to believe, in a new way, something already believed.

If one wonders how I can report the 1st-person beliefs of others without being able to entertain the propositions I report them as believing, one should keep familiar examples like (5) in mind.

(5) (Every x: Fx) x believes that x is G

We can think of an utterance of (5) as asserting that the propositional function expressed by the matrix clause *is true of every x who is F*. Thus (5) is true iff each such x believes the singular proposition that predicates *being G* of x. *If we add that the believer identifies the predication target of G in the 1st-person way, we ascribe de se attitudes; if we don't, we ascribe de re attitudes*. This strategy generalizes to reports of attitudes born to all the other propositions of limited accessibility I will be talking about, including those in (6).

be used to assert a pragmatically enriched proposition p that requires the second argument of the *prove* relation to be cognized via the name 'logicism'. This proposition p is representationally identical to the semantic content of (1b), but neither belief in the semantic content of (1a) nor belief in the semantic content of (1b) guarantees belief in p. Believing p does necessitate belief in the semantic content of (1b), but not in the semantic content of (1a). See Soames (2015: 80–81).

(6) a. The meeting starts now! *Said at t*
b. I only just realized that the meeting starts now! *Said at t*

Just as for each person p there is a 1st-person way of cognizing p no one else can use to cognize p, so, for each time t there is a 'present-tense' way of cognizing t *at t* that can't be used at other times to cognize t. Suppose I plan to attend a meeting that will start at t – noon on July 1st. Not wanting to be late, I remind myself of this that morning. Nevertheless, as the morning wears on, I lose track of time. So, when I hear the clock strike noon, I utter (6a), and change my behavior. Coming to believe of t *in the present-tense way* that the meeting starts then motivates me to hurry off. Had I not believed this, I wouldn't have done so, even though I would have continued to believe, of t, that the meeting starts then. As before, I believe something new by coming to believe something old in a new way. What makes (6b) true is that the proposition to which I have only just come to bear the *realizing* relation requires *cognizing t in the present-tense way*.

3.2 Linguistic cognition

Linguistic cognition is another source of representational identity without cognitive identity. One who understands the sentence 'Plato was human' uses the name to pick out the man, the noun to pick out humanity, and the phrase 'was human' to predicate the property of the man – thereby *performing* the proposition p the sentence expresses. Since *using the sentence to predicate humanity of Plato* is itself a purely representational cognitive act, it too counts as a proposition p*. Since to entertain p* is to entertain p, but not conversely, they are cognitively distinct but representationally identical.

Next, consider the names, 'Hesperus' and 'Phosphorus', the representational content of which is their referent. These names are special in that *understanding* them requires having some standard information. Those well enough informed to use them are expected to know that those who do use them typically presuppose that 'Hesperus' stands for something visible in the evening while 'Phosphorus' stands for something visible in the morning. One who mixes this up misunderstands the names. With this in mind consider (7).

(7) a. Hesperus is a planet.
b. Phosphorus is a planet.
c. x is a planet (with Venus as value of 'x')

Let p be expressed by (7c). P_H is a proposition representationally identical to p that requires one to cognitively identify the predication target, Venus, of *being a planet* via the name 'Hesperus'. P_P requires cognition via the name 'Phosphorus'. Utterances of (7a) assert both P_H and p; utterances of (7b) assert P_P and p. With this, we reconcile a pair of hard-to-combine insights: one who accepts (7a) may, as Frege noted, believe something different from what one believes in accepting (7b) – thereby explaining the potentially different truth conditions of utterances of (8a) and (8b) – even though the propositions believed are representationally identical, as intimated by Kripke.[9]

(8) a. Mary believes that Hesperus is a planet.
b. Mary believes that Phosphorus is a planet.

Now consider A's use of (9) in addressing B, each presupposing that both *understand* the names.

(9) Hesperus is Phosphorus.

A asserts not only the bare singular proposition that predicates identity of Venus and Venus, but also the corresponding proposition entertainable only by identifying Venus via the two names. Although this proposition merely represents Venus as being Venus, B extracts more information from A's assertion. Presupposing that A *understands* the names, B reasons that A knows he will be taken to be committed to the claim that *the unique object that is both Hesperus and visible in the evening is the unique object that is both Phosphorus and visible in the morning*. Knowing that A expects him to so reason, B correctly concludes that A *asserted* the descriptively enriched proposition.

The extra representational content carried by A's remark arises from the linguistically enhanced proposition asserted, the presupposition that A and B understand the names, and the information that comes with this understanding. The conversation then continues as in (10).

(10) a. If Hesperus's orbit had been different it wouldn't have appeared in the evening. *Said by A*
b. In that case would Hesperus still have been Phosphorus? *Asked by B*
c. Of course. Hesperus would have been Phosphorus no matter what. *A again*

A's final utterance commits A to its being *necessary* that Hesperus is Phosphorus, but not to the absurdity that *no matter what, the unique thing that was both Hesperus and visible in the evening would have been the unique thing that was both Phosphorus and visible in the morning*. The difference between the enrichment of A's use of (9) and the lack of such enrichment of A's use of (10c) hinges on what

9. Chapter 4 of Soames (2015) discusses attitude ascriptions like (8), including those in which the agent of the reported attitudes uses words not used by the reporter because different languages are involved.

understanding the names requires. It requires knowing that most agents who use them take, and expect others to take, 'Hesperus' to stand for something seen in the evening and 'Phosphorus' to stand for something seen in the morning. Presupposing that both understand the names in this sense, A and B add descriptive content to A's utterance of (9). Since taking the names to refer to things *actually* seen at certain times tells one *nothing* about when they are seen at *possible* world-states, A and B don't descriptively enrich the occurrences of the names under the modal operator when evaluating assertive utterances of (10c).

This explanation depends on three points: (i) to cognize o via a name n does *not* involve *predicating being named n of o* (any more than cognizing oneself in the 1st-person way involves predicating that one is so-cognized); (ii) the linguistically enhanced propositions asserted by utterances of sentences containing names are representationally identical to, but cognitively distinct from, the bare semantic contents of the sentences uttered;[10] (iii) to *understand* an expression requires not only the ability to use it with its semantic content, but also the knowledge and recognitional ability needed to use it to communicate with others in ways widely presupposed in the linguistic community. This dynamic extends to natural kind terms, where it provides solutions to many instances of Frege's puzzle involving them.[11]

3.3 The Millian modes of perceiving and recognizing

So far I have mentioned four propositional sub acts that are different ways of identifying predication targets – identifying a propositional constituent of a complex proposition by *entertaining* it, identifying oneself by *cognizing oneself in the 1st-person way*, identifying a time by *cognizing it in the present-tense way*, and identifying something by *cognizing it linguistically*. Adding these constraints on *how* a predication target is identified to a more abstract propositional act-type that merely specifies *what the predication target is* doesn't change representational content. For this reason, I call these sub acts *Millian modes of presentation*.[12]

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10. See Chapter 4 of Soames (2015).

11. The key idea is essentially an updated and generalized version of the neglected suggestion in Putnam (1970, 1975b) that natural kind terms are associated with stereotypes that are regarded by speakers as crucial to understanding them, even though the stereotypes don't contribute to semantic contents. See Chapter 4 of Soames (2015) for this plus a solution to Kripke's puzzle about belief presented in Kripke (1979).

12. The first of the Millian modes mentioned here *is* included in the semantic content of, e.g., (1a). The 1st-person and present-tense Millian modes are *not* included in the semantic contents of sentences containing the 1st-person singular pronoun and the temporal 'now'. As

Perception is another such mode, or rather a vast family of modes. Agent A watches bird B, predicating *being red* of B *cognized visually*. Since A's perceptual predication is a sub case of the general act predicating *being red* of B, the two are distinct but representationally identical propositions. So are *predicating being Tom's pet* of B and *doing so cognitively identifying the predicate target B visually*. Even if A already knows the former – namely, that B is Tom's pet – from Tom's previous testimony, A may faultlessly respond to an utterance of (11a) by uttering (11b):

(11) a. That is Tom's pet. *Said to A demonstrating B*
b. I didn't realize it was Tom's pet. *Said by A looking at B*

A's assertion is *true*, because the proposition A claims not to have known is one the entertainment of which requires B to be visually identified.[13]

My final Millian mode is *recognizing* something previously cognized.[14] When one has predicated *being F* of x before and one now *recognizes x recurring* as predication target of *being G*, one doesn't need further premises to predicate *being both F and G* of x. To recognize recurrence is immediately and noninferentially to connect the information in one cognition with information in others. Ubiquitous in cognition, *recognition of recurrence* connects elements both *within* individual propositions and *across* multiple propositions we entertain.[15] Incorporating it within propositions generates trios of cognitively distinct but representationally identical propositions of the sort indicated by P1-P3.

P1. The act of predicating R of a pair of arguments, o and o, *recognizing o's recurrence*.
P2. The act of predicating R of the pair of arguments, *without recognizing o's recurrence*.
P3. The act of predicating R of the pair of arguments *whether or not one recognizes o's recurrence*.

with *linguistic* Millian modes, encoding the modes associated with these indexicals in the semantic contents of sentences containing them would misrepresent many attitude ascriptions in which they occur embedded under attitude verbs. See Chapters 2, 4 and 5 of Soames (2015) for discussion.

13. Chapter 5 of Soames (2015) extends this discussion and uses it to address puzzles in the philosophy of mind. As before, these perceptual Millian modes are generally not included in the semantic contents of the sentences that are used to assert propositions containing them.

14. The seminal discussions of *recognition of recurrence* are Fine (2007) and Salmon (2012).

15. Recognition of recurrence is discussed in Chapters 6–8 of Soames (2015).

Since I can fail to believe P1 while believing P2 and P3, I can use (12a) to say something true without saying anything false, even if (12b) is false and a = b.

(12) a. I don't believe that a R a.
b. I don't believe that a R b.

4. Cognitive propositions in a realist conception of linguistics

In all cases from (1) to (12), taking propositions to be purely representational cognitive acts allows us to derive correct but otherwise elusive results about what is believed, asserted, etc. Having indicated why linguistic theories should embrace cognitive propositions, I will close by sketching how they fit into a realist conception of linguistics. I begin by asserting two general claims illustrated by some of my examples. *First, many assertive utterances assert multiple cognitively distinct but representationally identical propositions that may be reported by non-equivalent attitude reports. Second, Millian modes of presentation in propositions asserted or communicated by utterances are often not parts of the semantic contents of the sentences uttered.*[16] Because these modes of presentation are antecedently occurring features of our cognition, they are routinely available to speaker-hearers, who add them when doing so results in illocutionary contents that make maximal sense of linguistic performances.

4.1 Semantic contents excluding Millian modes

Sometimes it is crucial that such modes *not* be included in semantic contents. For example, a speaker uttering one of the sentences of (13) will rely on *hearers* to recognize the recurrence of John, even though recognition of the recurrence *isn't* part of the semantic content of the sentence, since, if it were, the thought attributed to Mary would be transparently absurd.[17]

(13) a. *John* fooled Mary into thinking that *he* wasn't John.
b. *John* fooled Mary into thinking that *he*, John, wasn't John.
c. John fooled Mary into thinking that John wasn't John.

Although there are exceptions – including sentences (1a) and (2a) – the semantic contents assigned to sentences by a correct semantic theory generally won't include

16. Chapters 2, 4, 5, 6, and 7 of Soames (2015).

17. The italicized occurrences in (13a) and (13b) are anaphoric; cf. Soames (2012) and Chapter 6 of Soames (2015).

Millian modes of presentation used by speakers to extract information from utterances. This pervasive fact increases the already significant distance between semantic and assertive, or other illocutionary, content. Competent speakers can usually identify what is asserted by an utterance, but they aren't, and don't need to be, good at identifying the semantic content of the sentence uttered.[18] The correct theory of semantic content is the one which, when combined with independent pragmatic principles, does the best job of predicting assertive and other illocutionary content (cf. Soames 2008a). Such a theory isn't tested by *semantic intuitions*. Speakers do have a pretty good idea of what *they* would mean by a use of a sentence S in this or that context, and what information they would glean from others' use of it. But they don't have reliable opinions about what the linguistic meaning of S contributes to what S is used to assert or convey by competent speakers across all contexts, or about how much of what is communicated in particular cases is due to linguistic meaning.[19]

4.2 Semantic content vs. individual psychologies

The ability to use language requires that one's identification of the illocutionary contents of utterances match, to a sufficient degree, those of other speakers. How that match arises from the individual psychologies of speakers doesn't matter for determining whether they speak a common language. Even if, as I suspect, the required illocutionary match is compatible with individual differences in the information that is directly encoded psychologically vs. the information added inferentially, this needn't show that the *semantic contents* of sentences used by encoders are different from the *semantic contents* of those used by the inferers. Semantic contents can't be extracted from individual psychologies.[20]

With this in mind, suppose some speaker directly psychologically encodes the bare semantic content of the 'Hesperus'-'Phosphorus' sentence (9) and works out the assertive contents of utterances of it in the manner suggested earlier. The fact that this *can be done* shows that the asserted content *isn't* a second meaning. We

18. An independent argument for this is given in Soames (2009a).

19. See Chapter 3 of Soames (2002). To put the point most simply, the assertive content of a use of S on a given occasion is roughly what the speaker means by S on that occasion; the semantic content is the abstract, least common denominator associated with S across contexts. It must be mastered independently by the language user in order to recognize correct assertive contents across contexts. See Soames (2002, 2008a, 2009a) for details.

20. Semantic contents are always abstract in the sense of ft. 5. Semantic contents determined by users of the language are always part of the individuating conditions for the language, despite not being extractable from the psychology of an individual language user.

learned from Grice (1967) not to posit gratuitous new semantic contents to capture implicatures that can be explained by independently needed pragmatic principles governing linguistic exchanges. *The lesson is similar when it is assertive or other illocutionary contents that need to be captured*. Roughly, and with some qualifications, the semantic content of a sentence S is the minimal antecedently encoded information from which a well-informed, optimal reasoner *could* use rational pragmatic strategies to generate the illocutionary contents of uses of S across contexts.[21] It is not required that speakers *do,* in fact, systematically derive these illocutionary contents from the minimal semantic contents needed for the job.

For example, some speakers *might* directly encode not only the bare singular proposition that predicates Venus of Venus, which is the genuine semantic content of (9), but also the assertive content carried by utterances of (9) in contexts in which speaker-hearers *understand* it (in the sense discussed earlier). These speakers might access the two encodings in different linguistic environments, making *ad hoc* adjustments when needed. With enough fiddling, the illocutionary contents of their utterances might match those of other speakers, allowing them to communicate with all and sundry, despite the fact that they treat the unambiguous sentence (9) as if it were ambiguous. If semantic contents were extractable from individual psychologies, these speakers might count as speaking dialects in which (9) was ambiguous. But they don't; semantic contents aren't extractable from individual psychologies. There is no such thing as *semantic,* as opposed to *communicative,* competence.

The degree of illocutionary match required for communicative competence is less than one might think. Some overlap in representational content is certainly required. But, as illustrated by Example (9), much illocutionary content is due to what one's community presupposes communicatively competent agents will know. Since such presuppositions depend on widely-shared interests and beliefs, big differences in illocutionary contents are compatible with identical semantic contents of sentences uttered. Take the term 'water', the semantic content of which is the kind H_2O. Because the stuff is ubiquitous, as well as necessary and useful to us, the widely presupposed information associated with the term is pretty rich. An agent A lacking this information will miss much of the illocutionary content of linguistic performances involving the word, even if A uses it to stand for the kind, H_2O.

21. In certain cases, like 'I'm ready' and 'I'm finished', in which grammatically complete sentences are semantically incomplete, and so don't express propositions, their semantic contents may be understood as recipes for representational cognitive acts to be performed on contextually given constituents. See Bach (1994) and Soames, Chapter 7 of (2010b).

Despite the problems A would have communicating with us, his words wouldn't thereby differ from ours in semantic content.

Examples of this sort illustrate a general point. Although semantic content plays a role in individuating languages spoken by populations, it doesn't come close to determining the illocutionary contents of utterances. Differences in perceptual modes of presentation extend the point still further. The blind and color-blind can use color words with the same semantic contents we do, despite their inability to extract information in normally expected ways from utterances like, 'My car is the red one parked across the street'. Merely possible agents who perceive the same colors we do using an entirely different sense, with different observation conditions, might speak a language semantically identical to ours, despite finding it very difficult to communicate with us.

Defects in the ability to recognize *recurrence* have even greater effects on communication without affecting semantic content. Imagine an agent who suffered from a generalized version of the malady exemplified by the character Peter in Kripke (1979) who suspects different uses the name 'Paderewski' designate different men – one a statesman and one a musician. Since the men are identical, the name is semantically unambiguous. Unfortunately, Peter can't reliably detect recurrences of the same name as presenting recurrences of the same content. If his problem were extended to all names, natural kind terms, and other directly referential expressions, his ability to reason and communicate would essentially collapse, with no effect on the semantic contents of his words.

One key reason semantic contents are not extractable from individual psychologies is that participation in a social practice of the right sort allows agents to speak a language that is partially individuated by the semantic contents of its expressions. The social practice allows individuals access to contents of names, natural kind terms, and other expressions that would otherwise be inaccessible to them. This pattern of communal use, not any aggregate of independent individual cognitions, determines semantic contents of the expressions in the language spoken by members of a community. It also plays a large role in determining the representational contents of predications individuals perform by linguistic means. Because of this, the representational contents of many propositions these individuals use language to entertain, assert, or believe are not extractable from their individual psychologies. Although cognitive propositions are psychological in the broad sense of being objects of attitudes to which the bearer makes his or her own cognitive contribution, the representational contents of those attitudes are often determined in part by contributions made by other, sometimes distant, language users. There is only one aspiring science that studies this, and it isn't psychology.

5. Conclusion

I will close with a word about predication, which is ubiquitous in semantic contents of sentences. Like traveling from A to B – which is an abstract act-type that is performed by performing a more specific act-type in the same family (driving, bicycling, jogging, or walking, from A to B) – predicating the property *being red*, say, of an object o is an abstract act-type that is performed by seeing o as red, visualizing it as red, remembering it as red, imagining it as red, or by *any other possible way of perceiving or cognizing it as red*. Since there is no end to these more specific modes of representational perception and cognition, the primitive notion of predication employed in linguistic semantics is not reducible to concepts in any more specialized science.

Even if *predication by humans* proved to be reducible, as it conceivably might, this wouldn't touch the linguistic description of English. When I consider the possibility of alien beings believing or asserting certain propositions that are expressible in English, I *am* ascribing certain predications to them, but I am *not* ascribing the fine-grained neural realizations of those predications characteristic of normal human beings. The purely representational cognitive acts that are semantic contents of some English sentences are no more limited to human agents than they are to any other possible agents. Though English is an abstract object brought into being by humans, it is available to all comers. The primary object of the study of realist semantics is the language, not its causal origin or its realization in particular populations of speakers.

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CHAPTER 10

Languages as complete and distinct systems of reference

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Section 1 is an extended commentary on Edward Sapir's formulation nearly a century ago of what he considered the two most fundamental properties of human language, first that each one is a formally complete system of reference to experience and second that each one is formally distinct from every other. Section 2 considers some aspects of the development of these formulations, noting that they have been considered separately and not integrated as fully fleshed out systems of reference, as Sapir envisioned. Section 3 examines more closely what such an integration looks like in a case involving simple arithmetic. Section 4 begins with a brief review of the accomplishments of Greco-Roman logic and more recent developments in the theory of logic, leading to a consideration of what may be needed to fulfill Sapir's program. Section 5 summarizes some of my own recent research on extending first-order logic by replacing the unordered set of individuals with a specific ordering of a set of sets of individuals that is isomorphic to an ordering of sets of sets of numbers that contain no pairs of divisible numbers, which was investigated by Richard Dedekind shortly before the turn of the twentieth century.

Keywords: semantics, logic, form, system

1. Sapir (1924)

> Chaque langue forme un systeme où tout se tient. Saussure (1916)

Sapir (1924) appeared in the second issue of the first volume of a magazine edited by H. L. Mencken devoted to discussion of the contemporary American intellectual scene. In it he lamented "the very pallid state of linguistics in America", arising "because there is not and cannot be in this country that daily concern with foreign modes of expression that is so natural on the continent of Europe", and because

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“[t]here is something about language itself, or rather about linguistic differences, that offends the American spirit”. (Sapir 1924: 149) Nevertheless, he hoped to find a way “to give linguistics its requisite dignity as a science” by discussing “the psychological problem which most interests the linguist”, that of determining “the inner structure of language, in terms of unconscious psychic processes”. (Sapir 1924: 150) He recognized that this will be a hard sell because “linguistic studies demand at one and the same time an intense appreciation of a given form of expression and a readiness to accept a great variety of possible forms”. (Sapir 1924: 151)

In the paper Sapir developed two fundamental principles about language.

1. Each is a formally complete system of reference to experience (formal completeness).
2. Each is formally distinct from every other, from which it follows that reference to experience is relative to the forms of the language one uses (formal distinctness or, as he called it, relativity of the form of thought).

Concerning these, he wrote:

> The outstanding *fact* [emphasis mine] about any language is its formal completeness. […] The world of linguistic forms, held within the framework of a given language, is a complete system of reference, very much as a number system is a complete system of quantitative reference or as a set of geometrical axes of coordinates is a complete system of reference to all points of a given space. The mathematical analogy is by no means as fanciful as it appears to be. To pass from one language to another is psychologically parallel to passing from one geometric system of reference to another. The environing world which is referred to is the same for either language; the world of points is the same in either frame of reference. But the formal method of approach to the expressed item of experience, as to the given point of space, is so different that the resulting feeling of orientation can be the same neither in the two languages nor in the two frames of reference. Entirely distinct, or at least measurably distinct, formal adjustments have to be made and these differences have their psychological correlates. (Sapir 1924: 151)

By formal completeness, Sapir meant that every language has the capacity to satisfy every communicative need of its speakers.

> To put the matter of the formal completeness of speech in somewhat different words, […] a language is so constructed that no matter what any speaker of it may desire to communicate, no matter how original or bizarre his idea or fancy, the language is prepared to do his work. (Sapir 1924: 151)

Further, formal completeness also means that any language can expand its vocabulary to deal with new experiences and concepts without affecting its form.

> Formal completeness has nothing to do with the richness or the poverty of the vocabulary. It is sometimes convenient or, for practical reasons, necessary for the speakers of a language to borrow words from foreign sources as the range of their experience widens. They may extend the meanings of words which they already possess, create new words out of native resources on the analogy of existing terms, or take over from another people terms to apply to the new conceptions which they are introducing. None of these processes affects the form of the language, any more than the enriching of a certain portion of space by the introduction of new objects affects the geometrical form of that region as defined by an accepted mode of reference. (Sapir 1924: 151)

Consequently, all languages have the means to express any concept, even if their speakers are unable to express certain ones, for example some of those in Immanuel Kant's *Critique of Pure Reason*.

> If […] languages have not the requisite Kantian vocabulary, it is not the languages that are to be blamed but [the speakers] themselves. The languages as such are quite hospitable to the addition of a philosophic load to their lexical stock-in-trade. (Sapir 1924: 152)

Whereas the principle of formal completeness may easily be grasped by those without linguistic training, the principle of the relativity of the form of thought (i.e., formal distinctness) emerges only as a result of comparative study of the world's languages.

> The upshot of [such study] would be to make very real to us a kind of relativity that is generally hidden from us by our naïve acceptance of fixed habits of speech as guides to an objective understanding of the nature of experience. This is the relativity of concepts or, as it might be called, the relativity of the form of thought. […] For its understanding the comparative data of linguistics are a *sine qua non*. (Sapir 1924: 155)

To convey the idea of what he understood by formal distinctness, Sapir invited his readers to consider a situation in which speakers of a variety of languages witness the same event, such as a stone falling to earth, and how they might go about the task of describing what they have seen. He began by sketching what 'our naïve acceptance of fixed habits of speech' would lead us to believe.

> The natural or, at any rate, the naïve thing is to assume that when we wish to communicate a certain idea or impression, we make something like a rough and rapid inventory of the objective elements and relations involved in it, that such an inventory or analysis is quite inevitable, and that our linguistic task consists merely of the finding of the particular words and groupings of words that correspond to the terms of the objective analysis. (Sapir 1924: 154)

To convince us that the form of one's language dictates what types of elements and relations are used to express what happened, Sapir compared what an English speaker might say on this occasion with what a German, French, Russian, Chippewa, Kwakiutl, and Chinese speaker might say, and noted some of the formal differences among these expressions having to do with gender, definiteness, animacy, number, point of view, and tense. However, he recognized that an even more distinctive example may be needed to jar his readers from their preconceptions.

> These differences of analysis, one may object, […] do not invalidate the necessity of the fundamental concrete analysis of the situation into 'stone' and what the stone does, which […] is 'fall'. But this necessity, which we feel so strongly, is an illusion. (Sapir 1924: 155)

He found such an example in what a Nootka speaker would be likely to produce, a verb form that combines an element that refers to the position or movement of a stone-like object and another to downward direction. This is something that English speakers can get a hint of the feeling for by assuming the existence of a verb *to stone* that refers to the position or movement of a stone-like object occurring in a sentence like *It stones down*.

> In this type of expression the thing-quality of the stone is implied in the generalized verbal element 'to stone', while the specific kind of motion which is given us in experience when a stone falls is conceived as separable into a generalized notion of the movement of a class of objects and a more specific one of direction. (Sapir 1924: 155)

Moreover, the distinctive relationship between forms and their referents in each language is felt by its speakers, as underlying their ability to communicate with it.

> These forms establish a definite relational feeling or attitude towards all possible contents of expression and, through them, towards all possible contents of experience, in so far, of course, as experience is capable of expression in linguistic terms. (Sapir 1924: 151)

Sapir referred to this feeling or attitude as 'form-feeling' and used the causative relation as an example to point out the difference between knowing what is meant by causation, and being able to feel and express the causative relation.

> Now this ability to feel and express the causative relation is by no manner of means dependent on the ability to conceive of causality as such. The latter ability is conscious and intellectual in character; it is laborious, like most conscious processes, and it is late in developing. The former ability is unconscious and

> nonintellectual in character; exercises itself with great rapidity and with the utmost ease, and develops early in the life of the race and of the individual. (Sapir 1924: 152–153)

However, the ability to express such consciously acquired concepts such as causality is also possible in any language.

> If the particular language under consideration cannot readily adapt itself to this type of expression [i.e. of the idea of a certain type of action conceived of as a thing], what it can do is to resolve all contexts in which such forms are used in other languages into other [of its own] formal patterns that eventually do the same work. Hence, 'laughter is pleasurable', 'it is pleasant to laugh', 'one laughs with pleasure', and so on *ad infinitum*, are functionally equivalent expressions, but they canalize into entirely distinct form-feelings. (Sapir 1924: 152)

Both formal completeness and distinctness are required to ensure this outcome, as Sapir observed in several passages in the paper.

> [A speaker] will never need to create new forms or to force upon his language a new formal orientation – unless […] he is haunted by the form-feeling of another language and is subtly driven to the unconscious distortion of the one speech-system on the analogy of the other. (Sapir 1924: 151)

> All languages are set to do all the symbolic and expressive work that language is good for, either actually or potentially. The formal technique of this work is the secret of each language. (Sapir 1924: 152)

> Every language […] possesses a complete and psychologically satisfying formal orientation, but this orientation is only felt in the unconscious of its speakers – is not actually, that is, consciously, known by them. (Sapir 1924: 153)

Sapir described the study of form-feeling as both very important and very difficult, requiring the collaboration of experts from several different fields of study.

> It is very important to get some notion of the nature of this form-feeling, which is implicit in all language, however bewilderingly at variance its actual manifestations may be in different types of speech. There are many knotty problems here – and curiously elusive ones – that it will require the combined resources of the linguist, the logician, the psychologist, and the critical philosopher to clear up for us. (Sapir 1924: 152)

To embark on such an interdisciplinary effort, of course, the parties must be willing and able to work together, and Sapir was particularly concerned about whether suitable psychologists could be found.

> Our current psychology does not seem altogether adequate to explain the formation and transmission of such submerged formal systems as are disclosed to us in the languages of the world. (Sapir 1924: 153)

After summarizing the version of stimulus-response theory then coming into vogue in psychology, he pointed out its failings, and why it is not suitable for the work that would be required.

> I can only believe that explanations of this type are seriously incomplete and that they fail to do justice to a certain innate striving for formal elaboration and expression and to an unconscious patterning of sets of related elements of experience.
>
> The kind of mental processes that I am now referring to are, of course, of that compelling and little understood sort for which the name 'intuition' has been suggested. […] It is precisely because psychologists have not greatly ventured into these difficult reaches that they have so little of interest to offer in explanation of all those types of mental activity which lead to the problem of form, such as language, music, and mathematics. We have every reason to surmise that languages are the cultural deposits, as it were, of a vast and self-completing network of psychic processes which still remain to be clearly defined for us. (Sapir 1924: 153)

On the other hand, Sapir believed that linguists were ready to collaborate, apparently not yet aware that the behaviorist wave then sweeping over psychology would also engulf the field of linguistics and stifle such research for the next thirty-five years.

> Probably most linguists are convinced that the language-learning process, particularly the acquisition of a feeling for the formal set of the language, is very largely unconscious and involves mechanisms that are quite distinct in character from either sensation or reflection. (Sapir 1924: 153)

2. Some subsequent developments

Of the two principles that Sapir formulated, only one was further developed and debated, that of formal distinctness, better known as linguistic relativity because of Sapir's own wording of it, and as the Sapir-Whorf hypothesis, coined by Harry Hoijer (1954), who linked Sapir's name with that of his student Benjamin Lee Whorf, who promoted a version of it (Carroll 1956). The primary focus of the debate over these developments was the question of linguistic determinism, whether one's language determines and possibly also limits the form of one's thought processes. The first (weak) version is a paraphrase of Sapir's principle of formal distinctness, and the second (strong) version contradicts his principle of formal completeness. Linguists generally accepted that principle without further argument, but the furtherance of Sapir's overall program was postponed for a generation, when something like the kind of collaborations that he had hoped for among linguists, logicians, psychologists and philosophers began taking place. Logicians and philosophers, several decades before the publication of Sapir's paper, had made significant breakthroughs in the study of formal logic, whose consequences for semantic analysis were only appreciated when philosophy and to some extent logic itself made a 'linguistic turn' (Rorty 1967), and the question of the mapping between linguistic (grammatical) forms and logical (semantic) forms began to be seriously considered, for example by Donald Davidson (1965) and Richard Montague (1970), and adopted by linguists as the principle of semantic compositionality. The principle of formal completeness itself was reinterpreted by Jerrold Katz (1972) within his own framework for linguistic semantics as that of effability. Finally, psychologists began undertaking systematic study of the nature of language acquisition and of linguistic fluency as strictly behaviorist investigations gave way to studies of cognition and intuition. However, Sapir's unified conception of languages as formally complete but distinct systems of reference for experience was not picked up on.

3. Simple formally complete and distinct arithmetic systems of reference

The systems of reference that Sapir mentioned as arithmetical analogues to linguistic ones provide a method for identifying and relating all the members of sets numbers ordered by magnitude in one or more dimensions. The sets themselves vary in size and composition, depending on what work we want them to do. For example, if all we require is the ability to count to some maximal number *m*, then a one-dimensional set N_m of integers containing *1* and whose other members are obtained by application of the successor function $s(n) = n+1$ until *m* is reached is sufficient. A system of reference R_m for N_m is complete if and only if for every $n \in N_m$ there is an $r \in R_m$ such that *r* refers to *n*. For example, R_{999} = *{one,two,… nine hundred ninety-nine}* of English counting words and phrases is a complete system of reference for N_{999}. On the other hand, if we require the ability to identify the entire denumerably infinite set *N*, so that addition maps $N{\times}N$ onto *N* (i.e. is a total function), then no set of counting words or phrases in any language provides a complete system of reference (Greenberg 1978). Place systems consisting of a set D_b of non-empty strings of digits drawn from a set *B* = *{'0','1',…,'b-1'}*, in which the base '*b*' refers to an integer greater than *1* and whose first digit is not '*0*', can be mapped one-to-one onto *N*, and so provide complete systems of reference for *N*, as follows. Order the members of D_b by length, and for all members of the same length order them by the referent of the digit in the first distinct position (place) from the left. For example, let *r* be the string '$d_m \ldots d_1$'. Then *r* refers to the integer $(d_m{\times}b^{m-1})+\cdots+d_1$. Furthermore, each choice of the referent of '*b*' gives rise to a distinct system of reference, completing Sapir's analogy. For example, the digit strings that refer to the same integer as the English word *fourteen* for eight different place systems are shown in Table 1, in which values of *b* are referred to by decimal (base 10) strings and string delimiters are omitted for convenience from all the entries.

Table 1. Digit strings referring to the integer that English ***fourteen*** refers to in eight different place systems

b	*r*
2	1110
3	112
5	24
7	20
10	14
14	10
16	E
20	E

The lack of an unbounded system of counting words and phrases could be considered a counterexample to Sapir's claim of formal completeness for language since the task would seem to be within scope, and having to borrow a place system from arithmetic in order to accomplish it would seem to violate his claim that the existing formal apparatus for each language is sufficient for insuring its completeness. However the simple concatenation of expressions referring to the integers in some small base system (including for 0, which may have to be borrowed as a new lexical item, and for the remaining base integers if the language has no counting expressions at all) is presumably an existing formal resource in every language, and has in fact already been widely adopted, especially in text where it has largely replaced the use of native language counting words and phrases.

4. Some desiderata for formally complete and distinct system of reference for experience

The mapping between the forms of a given language L_i and the set E of possible experiences is not as straightforward as that between D_b and N described above, first because it is bound to be many-to-many rather than one-to-one, and second because the orderings of the relevant domains have yet to be fully worked out. To serve as an accurate system of reference for experience, the ordering of the forms in L_i, different as they are from language to language, must nevertheless track that of the members of E, keeping in mind that one's ability to follow the intended track may pose a considerable challenge in particular cases. For each L_i, we may suppose that the ordering of its forms is based on containment (constituency), with repetitions indexed, as in six_1 *hundred* six_2ty six_3, which yields a not surprisingly straightforward mapping to E, assuming that $N \subset E$, as follows.[1] The first occurrence of *six* (which maps to *6* in N) forms a constituent with *hundred* (which maps to 10^2 in N with the constituent mapping to $6 \times 10^2 = 600$), the second with *-ty* (which maps to 10^1 in N with the constituent mapping to $6 \times 10^1 = 60$), and the third with six_2ty (with the constituent mapping to $60+6 = 66$). The entire phrase then maps to *666* as desired. In practice, as in this example, the ordering of expressions based on containment has been primarily studied only up to the level of individual, isolated sentences, but that is not sufficient to account for how people use language to perform the tasks they rely on it for.[2]

The ordering of E or, more accurately, a tiny part of it, has been studied since antiquity as providing a means for insuring that a false conclusion cannot be drawn from true premises. Initially these logical investigations involved the relation between E and particular languages, notably classical Greek and Latin, but as the field progressed, it became more and more focused on E because of the spread of the belief among logicians that languages are inadequate vehicles for expressing logical relations. More recently, the pendulum has swung back, as it has been convincingly argued that languages not only are adequate in that regard, but also have certain properties that provide challenges for the design of logical systems. The core of logical investigations of language has been the study of vocabulary that refer to certain relations among concepts, such as conjunction, disjunction, negation, conditionality, necessity, possibility, identity, predication, instantiation, and quantity. In addition, the study of the ordering of the concepts and of their linguistic expressions, known as ontology, has also been going on since antiquity, and the success of Sapir's enterprise depends on bringing all these strands together and considering them in relation to all of the world's languages.

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1. A necessary and sufficient condition for L_i to be infinite (assuming that it has a finite vocabulary) on Sapir's account is that its network of its forms is arranged so that there is no upper bound on the number of repetitions of the members of its vocabulary, but of course speakers may have no feeling for whether or not there is, unless the bound is quite small.

2. Neither of the two attempts I have made to go beyond that level has been particularly satisfactory; in Langendoen (1982) I proposed a simple method for parsing stretches of text as a rooted tree, and in Langendoen (2002) a considerably more elaborate one for ordering sets of expressions that share vocabulary in directed acyclic graphs, using subsequence, rather than substring, as the ordering.

To illustrate the core of the Greco-Roman system of propositional logic, let $S \subset E$ be a set of propositions such that for all $p_1,\ldots,p_n,q \in S$, if $p_1,\ldots,p_n$ are true then so is q. S then is ordered by a truth-preserving entailment relation. A special case is of particular interest. Let $A \subseteq S$ be a set of atomic propositions to which truth values ('true' or 'false') are assigned independently of each other, and let S be closed under the operations of conjunction, disjunction and negation, defined as functions over truth values as follows. The conjunction of two or more propositions is true if and only if all of them are true. The disjunction of two or more propositions is false if and only if all of them are false. The negation of a proposition is false if and only if that proposition is true. If A_n has n members, then S_n has 2^{2n} members, in which conjunction and disjunction are functions of $S \times S$ onto S and negation is a function of S onto S. The ordering of S can be described in a lattice in which the conjunction of two propositions is their greatest lowest bound and their disjunction is their least upper bound, and the negation of a proposition is its reflection using the two axes of symmetry in the lattice. Since classical Greek and Latin, and (if Sapir's claim of formal completeness is correct) all other languages, have forms that can be mapped to any such S with an isomorphic ordering, people can use any language to evaluate the validity of arguments of the sort accounted for by propositional logic.[3]

Classical syllogistic logic was developed to account for truth-preserving entailments among propositions with a particular form, consisting of a subject and predicate as subforms, with the subject consisting either of a form referring to a single individual or to a quantified predicate, the quantifier being either universal or existential. The development of formal logic in the latter part of the nineteenth century extended the scope of reasoning with quantifiers beyond what syllogistic logic permitted and integrated that system with propositional logic into a system known as predicate logic. That system's most basic form is first-order logic, which extends predicate logic to include two sets in addition to S, a set I of individuals (entities) and a set P of predicates that map finite sequences I^* of members of I to members of S. For a given $p \in P$, the length of the sequence is the number of its arguments. A further refinement of the system allows for a classification of the arguments as to their semantic role (agent,

3. If the mapping of the referents to A in L_i is one-to-one, then the overall mapping to P will be many-to-one because of the logical equivalences obtained by distributing conjunction, disjunction and negation over each other; if not it will be many-to-many. The formal language mapping that is used in textbooks to teach propositional logic is many-to-many, since it does not care about the references of the atomic propositions, as long as they are logically independent.

patient, theme, location, etc.), which I do not consider here.[4] This penetration of logical analysis into the internal structure of propositions has not only allowed for a more adequate account of the mapping from L_i to S than is possible using the classical systems, but also stimulated research on how to improve it further, such as fixing the orderings of I and P such that the ordering of S is a consequence of the mapping. In the next section, I propose an ordering of I that accords with that of the linguistic forms, in languages like English at least, that refer to the members of I, which turns out to be isomorphic to an ordering of sets of positive integers discovered by Richard Dedekind (1897[1931]) based on their factorization into primes.

5. Extending first-order logic with a particular ordering for individuals

Let J be a set of discrete (i.e. non-overlapping) atomic (i.e. without proper subparts) individuals. Then form the set K consisting of the sums of the members of J using the sum operator $\oplus$ of the calculus of individuals of Leonard and Goodman (1940) and Goodman (1952). For example, if $J_2 = \{m,n\}$, then $K_2 = \{m,n,m\oplus n\}$. Now suppose that *Molly* names m and *Nelly* n. Then *Molly and Nelly* names $m\oplus n$, indicating that *and* refers to $\oplus$, which in turn suggests that $\oplus$ represents conjunction in K_2. If so then the ordering of K_2 is represented by the lattice in Figure 1. Now, let us ask, who if anyone does *Molly or Nelly* name? Surely not the calculus of individuals product $\otimes$ of *Molly* and *Nelly*, because that is either the empty individual ε or is undefined. Rather it is the union of the singleton individuals $\{m\}$ and $\{n\}$, i.e. the set $\{m,n\}$ whose members may be considered choices, or possibilities. Suppose then that $I_2 = K_2 \cup \{m,n\}$, resulting in the ordering shown in Figure 2, indicating that *or* refers to set union, and suggesting that set union represents disjunction in I_2.[5]

4. This exposition summarizes, with some simplification, the one given in Koslow (1992: 181–182), which includes the refinement omitted here, but without discussing how it supports (pun intended) the classification of arguments into roles.

5. According to Jennings (1994: 227–228), understanding the disjunction of nominals as the set of the referents of its disjuncts was first suggested by Geach (1962). Jennings also pointed out a resemblance of the semantics of disjunctive nominals to that of quantified ones, which I have followed up on elsewhere (Langendoen in press).

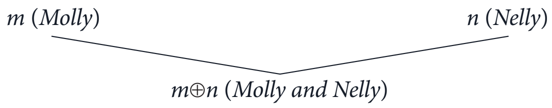

Figure 1. Ordering of K_2

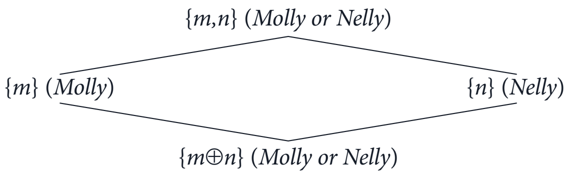

Figure 2. Ordering of I_2

Dedekind's problem can now be described as follows. Given a set P_n of n singletons consisting of prime numbers as atoms, and of their $2^n - 1$ multiples M_n, find the largest set of sets $F_n \subseteq \wp(M_n)$ that contain no divisible pairs.[6] Then P_n corresponds to J_n, M_n to K_n, and F_n to I_n. In particular, I_2 is isomorphic to the ordering of the set of sets of integers $F_2 = \{\{2\},\{3\},\{6\},\{2,3\}\}$ generated by conjunction and disjunction over the set $P_2 = \{\{2\},\{3\}\}$, where conjunction first yields $M_2 = \{\{2\},\{3\},\{6\}\}$, and then disjunction F_2.[7]

Now let $J_3 = \{m,n,o\}$ and suppose that O*llie* refers to o. Then I_3 has the 18 members listed in Table 2 with their disambiguated English names, and the ordering shown in Figure 3.[8]

Table 2. Members of I_3 and their English names

Member	Name
$\{m,n,o\}$	Molly, Nelly or Ollie
$\{m,n\}$	Molly or Nelly
$\{m,o\}$	Molly or Ollie
$\{n,o\}$	Nelly or Ollie
$\{m,n\oplus o\}$	Molly, or Nelly and Ollie

6. $\wp(M_n)$ is the power set of M_n.

7. Conjunction and disjunction of non-singletons are defined similarly, by taking cross-products.

8. Similarly, for $P_3 = \{2,3,5\}$, $F_3 = \{2,3,5,6,10,15,30,\{2,3\},\{2,5\},\{3,5\},\{2,15\},\{3,10\},\{5,6\},\{6,10\},$ $\{6,15\}, \{10,15\},\{2,3,5\},\{6,10,15\}\}$.

Table 2. (*Continued*)

Member	Name
$\{n,m\oplus o\}$	Nelly, or Molly and Ollie
$\{o,m\oplus n\}$	Ollie, or Molly and Nelly
m	Molly
n	Nelly
o	Ollie
$\{m\oplus n,m\oplus o,n\oplus o\}$	Molly and Nelly, Molly and Ollie, or Nelly and Ollie
$\{m\oplus n,m\oplus o\}$	Molly, and Nelly or Ollie
$\{m\oplus n,n\oplus o\}$	Nelly, and Molly or Ollie
$\{m\oplus o,n\oplus o\}$	Ollie, and Molly or Nelly
$m\oplus n$	Molly and Nelly
$m\oplus o$	Molly and Ollie
$n\oplus o$	Nelly and Ollie
$m\oplus n\oplus o$	Molly, Nelly and Ollie

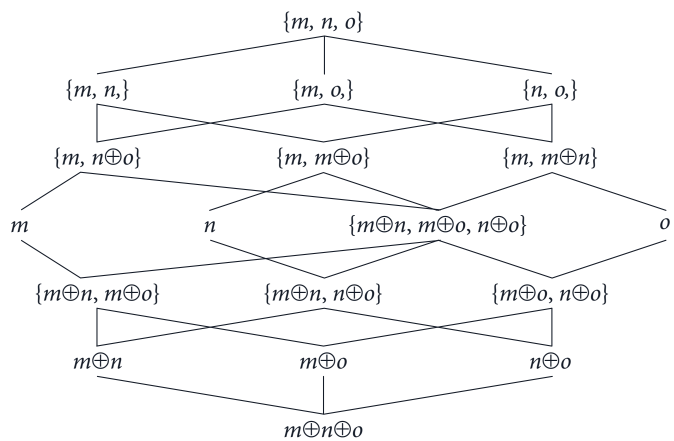

Figure 3. Ordering of I_3

I_n and F_n are free distributive lattices with n generators, their atoms (Davey & Priestley 2002). The size of such lattices is given by the Dedekind numbers, of which only the eight shown in Table 3 have been calculated so far.

The ordering proposed here for sets of sets of individuals in extended first-order logic may be appropriate (possibly with modifications) for other of its components, such as predicates and propositions. For example, let $H = \neg\neg I$ be

Table 3. The first eight Dedekind numbers and their sources

n	$\#n$	Source
1	1	Dedekind (1897)
2	4	“
3	18	“
4	166	“
5	7,579	Church (1940)
6	7,828,352	Ward (1946)
7	2,414,682,040,996	Church (1965)
8	56,130,437,228,687,557,907,786	Wiedermann (1991)

the image of double negation applied to the members of I, and let $\top$ represent the top (supremum) of the ordering of I. Then $H = K\cup\top$ is a Boolean structure with 2^n members. For example, $H_2 = K_2\cup\{m,n\} = I_2$, with 4 members, and $H_3 = K_3\cup\{m,n,o\}$ with 8 members. Now consider I_4 whose 166 members are generated by conjunction and disjunction over the atoms $J_4 = \{m,n,o,p\}$, where *Polly* names p. Then $H_4 = K_3\cup\{m,n,o,p\}$ is a Boolean structure with 16 members, whose ordering is given in Figure 4. H_4 in turn is isomorphic to the truth-preserving ordering S_2 of the classical propositional logic, in which the counterparts to $m\oplus n$ and $m\oplus o$ are the generators. What this exemplifies is the fact that for every S_n there is an equivalent H_{2^n} that makes no reference to truth values and in which mereological sum is conjunction, set union is disjunction (which resolves to mereological product except where the result is $\top$), and negation (defined in terms of the law of contradiction) are total functions. The generators moreover are atoms only for $H_{2^1} = H_2$; for all other values of n, they are sums of n atoms, which confirms the intuition that the larger the domain of experience, the more we need to specify in order to distinguish among its members.

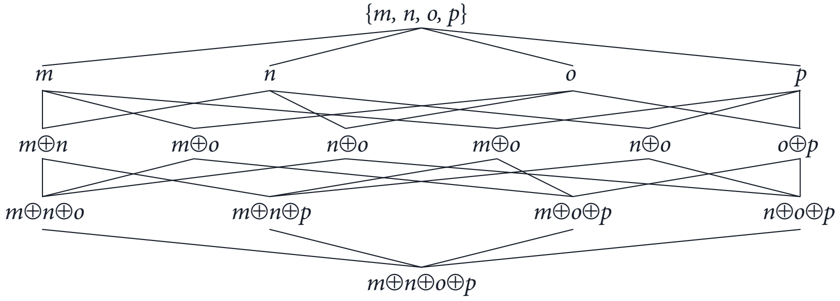

Figure 4. Ordering of H_4

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CHAPTER 11

The so-called arbitrariness of linguistic signs and Saussure's 'realism'

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Undoubtedly, F. de Saussure is among the founders of modern linguistics and 'semiology'/semiotics – though, strangely enough, his success mainly resulted from a book he did not write in the strict sense. Since the publication of the *Cours*, edited by his disciples in 1916, his notion of 'arbitrariness' has become one of the main canonical catchwords of linguistics that are hardly ever called into question. After a reconstruction of the Saussurean concept of the linguistic sign and its arbitrariness the attempt is made to work out its shortcomings and partly correct them. It is argued that instead of arbitrariness motivation must be taken as the central semiotic feature of linguistic signs which, after their formation, may be subject to a continuous historical process of increasing arbitrarization, possibly leading to complete arbitrariness in the end. In the final section the question of whether and to what extent the Geneva linguist may be considered a realist is tackled.

Keywords: arbitrarization, motivation, symbol, onomatopeia, proper names

Every word rings
With what conditioned it: its origins (Goethe, Faust II, 7094f.)

1. Introductory remarks

De Saussure's *Cours de linguistique générale* was one of the most influential linguistic books of the 20th century (cf. Fehr 1997: 19ff.), maybe even the most important one, and was considered as the 'Bible' of European Structuralism (cf. Leinfellner-Rupertsberger 1989: 271) or the 'Magna Carta of twentieth-century linguistics' (Harris 2001: 18). Saussure himself was conferred the inofficial title of 'father of

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modern linguistics' (Culler 1986: 104), and for almost one century the main ideas of this work and the structuralist way of thinking put forward by it[1] have been taught in basic courses in linguistics and explained in introductory books all over the world. In the meantime, however, it is almost commonplace and proven by philological research that Saussure may not be considered the author of his most famous work in the strict sense (cf. Jäger 2010: 13), which was compiled only posthumously on the basis of lecture notes by some of his disciples. Moreover, the Geneva series of lectures itself was merely an introduction to general linguistics for his students and not designed to present in every detail the authentic thinking of their master. Therefore, the Saussure of the *Cours* is nothing but a 'posthumous creature' (Jäger 2010: 25) without genuine life and biography,[2] and it may not only be said that we do not know the authentic Saussure (in the metonymic sense) but it is even the case that we do know for sure that Saussure himself actually did not write the world-famous book attributed to him (cf. e.g. Heringer 2013: 10).[3] Particularly the studies of Fehr (1997) and Jäger (2003) have shown that at least two Saussures must be distinguished: (i) the Saussure of his unpublished writings who insisted on the dynamic character of signs and the intrinsic diversity and instability of any language with regard to its historical and geographic appearance, and (ii) the 'structuralist' Saussure of the *Cours* of his disciples who seemingly advocated the strongly systemic character of 'langue' and insisted on the arbitrariness and almost static bilateralness of its signs and – to use Jäger's expression (2010: 15) – may be taken as the 'avatar' of Saussure's own Saussure.[4] However, as my purpose is neither philological nor biographical but semiotic it is still the well-known avatar I will mostly deal with here.

As a consequence of the reception of the *Cours*, 'arbitrariness' is generally considered as the decisive feature of those signs which are usually called 'symbols' in the Peircean tradition. Together with the doctrine of the bilateralness of the linguistic sign it is taken to be one of the basic insights of structuralism (cf. Jäger

1. However, the term 'structuralism' was never used by Saussure himself.

2. For a detailed description of the real Saussure's life and academic career see e.g. Jäger (2010: 25–75).

3. In addition, "it may be considered as one of the ironic punch-lines of the *Cours*' history of aftermath that exactly those theoretical ideas and conceptual dichotomies which have had determining influence on the paradigm of structuralism would have provoked Saussure's fundamental criticism." (Jäger 2010: 172)

4. Jäger (2010: 20) adds a 'comparatistic' Saussure to the former two. Harris (2001: 3) distinguishes between the putative author of the *Cours*, the actual scholarly person who gave the lecture, and the "putative theorist 'behind' the Geneva lectures".

2010: 20). In linguistics and semiotics, or as Saussureans would call it: semiology, it has become one of the most canonical catchwords and is hardly ever called into question. In what follows, I will (Chapter 2.) try to reconstruct the Saussurean concept of the linguistic sign and its arbitrariness and then (Chapter 3.) show its shortcomings and partly correct them.

European Structuralism, which followed Saussure (the avatar) and was inspired by his views, may not be considered a uniform paradigm. Some of its exponents should rather be classified as nominalists, others as conceptualists. It may be doubted that there were also realists among them. Now, which is the position of Sassure himself? Though his remarks concerning the ontological status of language were not at all clear and unequivocal, an investigation of his conception of *langue* and his concept of the sign and its arbitrariness may shed some light on this question. Finally (4.), therefore, I will briefly address the question of whether the Geneva linguist may be considered a realist or not, and if so, what kind of realism may be attributed to him or, more precisely, to the avatar posthumously constructed by his disciples.

2. Saussure's concept of the sign and its arbitrariness

Saussure's (the avatar's) *Cours* starts with an overview over the history and the aims of linguistics, his famous definition of language and the distinction between *langue*, *langage,* and *parole* (inspired by the useful conceptual trias offered by his romanic mother tongue), the description of the 'circuit de la parole' and interesting remarks on scripture and phonetics. Though undoubtedly equally important, the concepts and ideas outlined in the corresponding chapters cannot be dealt with here. I would just like to emphasize that, in his lectures, Saussure defines language (*langage*) as a 'social fact' from the very beginning (Saussure 1959: 6) and that he stresses the methodological priority of *langue* as the object of linguistic investigation[5] as well as the conventional character of any language as a sign system (like the game of chess) which makes it independent from the individual speaker and the objects it refers to at the same time: "a language is a convention, and the nature of the sign that is agreed upon does not matter" (Saussure 1959: 10). This quote shows that the conventionality of any *langue* and the arbitrary character of the signs it contains are presupposed from the beginning. The roots of these ideas may even be traced back to some of his earlier writings (cf. Fehr 1997: 73, 145ff.).

5. This important point was already made by Giovanni Nencioni in 1946 (see Berardi 1989: 45ff.).

Saussure's own concept of sign is outlined in detail in the chapter on 'General Principles'. Here the linguistic sign is defined as essentially bilateral: "The linguistic sign unites, not a thing and a name, but a concept and a sound-image." (Saussure 1959: 66) This sound-image is not the physical sound but its 'psychological imprint' (*empreinte psychique*). Let us look for a moment at what Saussure's avatar, created by his disciples Bally and Sechehaye, says about the concept of sign in his famous lectures:

> I call the combination of a concept and a sound-image a sign, but in current usage the term generally designates only a sound-image, a word, for example (*arbor*, etc.). One tends to forget that *arbor* is called a sign only because it carries the concept 'tree', with the result that the idea of the sensory part implies the idea of the whole. (Saussure 1959: 67)

This is an explicit refutation of the monolateral conception of the sign according to which language is but a 'nomenclature',[6] "a list of words, each corresponding to the thing that it names" (Saussure 1959: 65). The quoted passage further implies that the old so-called 'realist' conception of semantics (cf. e.g. Meggle 2010: 1–9) according to which meanings are the objects the signs correspond to is being rejected.[7] Upon careful reading it becomes clear that Saussure here stresses the idea that the sign is a combination of (mental) concept and sound-image or rather a unification or synthesis of the two (cf. also Saussure 1997: 357). On this view a sign unit consists of both sides; the 'sensory part' is significant just because it is associated with the concept it forms a whole with (though it does not 'carry' or 'contain' it in the strict sense). Consequently, Saussure makes his famous proposal

> to retain the word *sign* [*signe*] to designate the whole and to replace *concept* and *sound-image* respectively by *signified* [*signifié*] and *signifier* [*signifiant*]; the last two terms have the advantage of indicating the opposition that separates them from each other and from the whole of which they are parts. (Saussure 1959: 67)

In my opinion, this idea that a sign may only be a sign by associatively combining a signifier with a signified and thereby forming a meaningful linguistic unit which is bilateral by definition is the deepest semiotic insight of the Ferdinand de Saussure of the *Cours*. It means that meaning is part of the two-sided relation of mutual solidarity and, therefore, an indispensable feature of the sign. There is no sign without this correspondence (whatever substance may

6. This term is not quite properly translated by 'naming-process' in the English edition.

7. Alternatively, to be more precise, the existence of a factual correspondence-relation between signifier and object, which is a characteristic of proper names, is purely coincidential (cf. Raggiunti 1990: 178; 1982, 152; Jäger 2010: 40).

be attributed to the signified ontologically).[8] Saussure's insight becomes even more valuable and relevant to contemporary debates if one considers that (elsewhere) he explicitly expressed that beside the signifier there are no concepts or ideas or, as he puts it, "nothing distinct before the signifier" (Saussure 1968: 252, 1824–1825). This idea of the semiotic nature of (the bulk of) human thought is weakened or even obscured, however, by his choice of the term 'concept' which, to make matters worse, was translated as 'Vorstellung', which means a cognitive entity, into German.

Even if one does not go so far as to conceive of the sign as literally *consisting* of signifier and signified or as *carrying* or *containing* meaning in the literal sense, the conception of the non-natural sign as in itself bilateral remains, but it still looks quite static and inflexible. In his unpublished writings, however, Saussure had already added to the conception of the bilateral sign another important point, namely the insight in the central role of the process of 'circulation' or 'transmission' for its constitution or, as Fehr puts it:

> Signs cannot simply be considered as 'combinations' of 'articulations' and 'thoughts', but they must be conceived of as 'combinations' which can only exist in a process of transmission to the changing dynamics of which they are inevitably exposed. (Fehr 1997: 135)

Signs, therefore, exist as such by being 'thrown into circulation', i.e. by being "immersed into the social mass which newly establishes its value in every moment" (Saussure 1997: 419), and have their existence only there.

The next step taken by Saussure in his *Cours* is not as clear as it seems and deserves some detailed discussion. It is well known that, after establishing its bilateral character, he defines the linguistic sign as arbitrary, or to repeat it in his (or his disciples') words:

> The bond [in the French original: *le lien unissant*, i.e. 'the unifying tie'; A.B.] between the signifier and the signified is arbitrary. Since I mean by sign the whole that results from the associating of the signifier with the signified, I can simply say: *the linguistic sign is arbitrary*. (Saussure 1959: 67)

The concept of arbitrariness is prominent among the linguistic commonplaces that are taught in introductory linguistics courses and that are usually attributed to Ferdinand de Saussure (to the avatar, to be more precise). Of course, the real

8. It is interesting to note that Saussure (the avatar), in order to give a visible appearance to the invisible and to avoid the discussion about the ontological status of the signified, tends to use Latin (*arbor*, *equos*) as a metalanguage in his semantic descriptions of the signified, hereby following the nineteenth-century tradition.

Saussure, i.e. (in Harris' words) 'the actual scholarly person', would have known that the idea of the arbitrariness of most linguistic signs was at least already present in 18th century theory of language (cf. e.g. Coseriu 1968; Fehr 1997: 145). It was no less a thinker than John Locke who, in his *Essay Concerning Human Understanding* (1690), explicitly advocated the view that words came into use as signs of human ideas "not by any natural connection that there is between particular articulate sounds and certain *ideas*, for then there would be but one language amongst all men; but by a voluntary imposition whereby such a word is made arbitrarily the mark of such an *idea*." (Locke 1972: 12) Or, in other words, "the signification of sounds is not natural, but only imposed and arbitrary" (Locke 1972: 29).[9] And in Germany, almost one century later Johann Werner Meiner writes:

> Language, taken in its most common sense, is a picture of everything going on in our soul. Signs that are chosen arbitrarily and, as it were, agreed upon create this picture. I purposely say: by *arbitrary signs*, as there are also *natural signs* by which one can express one's desires, needs and inner sensations. […] In order to perfectly understand such a language consisting of sounds and letters, *two components* are necessary: I) a connection between the *concepts to be thought* and *the audible sounds* that is produced by intentional practice and has become so close that they call forth one another very easily and very quickly so that, when perceiving these or those sounds, the corresponding concepts or the corresponding sounds are called forth in us immediately. (Meiner 1781: 1, 4–5)

Writing that "all parts of speech […] signify concepts the content of which is defined by the establishing force of custom", Johann Severin Vater (1801: 149) also explicitly refers to the conventionality and arbitrariness of linguistic signs. Vater's contemporary August Ferdinand Bernhardi advocated a similar view in *Anfangsgründe der Sprachwissenschaft* (Bernhardi 1805). Vater and Bernhardi, however, base their idea of the arbitrariness of signs on a monolateral conception that identifies the sign with the signifier. Another half-century later, Locke's term and view were also adopted by William D. Whitney who defined language as "a system of arbitrary signs for thought" (1867: 410)[10] and was explicitly mentioned by Saussure in the context of his discussion of arbitrariness (cf. Saussure 1959: 76). In summary, the idea of the arbitrariness of linguistics signs, in the sense of their

9. For Locke, however, not only the connection between sign and the idea expressed by it is arbitrary, but in his view more complex ideas may also be arbitrarily composed of simple ones (cf. Locke 1972: 40–41).

10. Moreover, it must be addded that the arbitrary character of linguistic signs was not only expressed in passing by Whitney but emphasized repeatedly (cf. Koerner 1973: 84).

non-naturalness and conventionality, was already commonplace in Saussure's lifetime and the Geneva linguist must have been completely aware of this.[11]

Returning to Saussure's point of view, it is worth noticing, firstly, that not the sign itself but the 'bond' between its two components or aspects is called arbitrary[12] and, secondly, that it is not self-evident what exactly the notion of arbitrariness is intended to mean in the above quoted passage.

According to dictionaries, the German equivalent of *arbitrary* is *willkürlich*, which means something like 'just by chance' or 'by authoritative decision'. This term had already been used e.g. by Meiner (1781) more than a century before Saussure. However, the German translator Lommel (cf. Saussure 1967a: 79ff.) preferred the adjective *beliebig*, which means 'at will' or 'in accord with no reason or principle at all'. The English translation simply echoes the French term by using the adjective *arbitrary*. Does the French *arbitraire* mean 'just by chance' or 'in accord with no principle at all'? If we consider the Latin origin of the word which is derived from the verb *arbitrare* 'to observe, mean, believe, take for or regard as', we at least get a presentiment of the French connotation of an interior cognitive relation within the bilateral sign: speakers/listeners have learnt to take a word in a certain sense or react to it in a certain way and believe that the relation between signifier and signified subsists.[13] Considering Saussure's above-mentioned idea of the signs merely existing in 'circulation', one could even add that the sign-constitutive relation consists in the sign's social communicative repetition (and tradition). Saussure (the avatar) himself, however, explains *arbitrary* by *unmotivated*, "i.e. arbitrary in that it actually has no natural connection with the signified" (Saussure 1959: 69). He adds that the term does "not imply that the choice of the signifier is left entirely to the speaker (we shall see below that the individual does not have the power to

11. The idea of an alleged naturalness of signs and its refutation by the idea of arbitrariness, in the sense of there being no necessary connection between signifier and signified, may be traced back to Plato's dialogue *Cratylus* (see Reeve 1998). However, Plato's well-known *physei* – *thesei* 'by nature vs. by definition' distinction is not a genuine dichotomy, as signs and their meanings may well be introduced by human composition and definition and then develop or change by linguistic processes, which may be considered as perfectly natural nevertheless.

12. Some structuralist scholars, such as de Mauro and Raggiunti (see Berardi 1989: 172ff., 262ff.), have even extended Saussure's concept of arbitrariness to the view that the two sides of the sign, i.e. signifier and signified, each are arbitrary with regard to their internal – phonological or semantic – structure as well.

13. It is interesting to note that *arbiträr* in German and *arbitrary* in English, as loanwords, are arbitrary themselves whereas the French *arbitraire* possibly is not and the Latin *arbitrarium* surely was not.

change a sign in any way once it has become established in the linguistic community)" (Saussure 1959: 69).[14]

Saussure tries to strengthen his point by showing the variety of denominations for the same concept in different languages: "The idea of 'sister'", he writes, "is not linked by any inner relationship to the succession of sounds s-ö-r which serves as its signifier in French; that it could be represented equally by just any other sequence is proved by differences among languages and by the very existence of different languages" (Saussure 1959: 67–68). Different languages have very different words for female descendants of the same mother (*soror, sister, Schwester, zuster, sjestra, sœur, hermana, sorella, kardeş, imōto, mèimei* etc., just to mention a few). Does this mean that these linguistic signs are arbitrary in the sense of 'just by chance' or 'in accord with no reason or principle at all'? Saussure himself says that "every means of expression used in society is based, in principle, on collective behavior or – what amounts to the same thing – on convention" (Saussure 1959: 68). However, do such conventions arise at random? Or does this only mean that in any synchrony the relation between signifier and signified must be taken as it is, i.e. as it has been accepted by the speech community in the course of language history? Must such a view imply that there has never been any internal link between the two sides of the sign at all?

It is noteworthy that Saussure, in his chapter on the arbitrariness of signs, briefly discusses the notion of 'symbol' in the narrow sense, i.e. in the sense of a visual iconic sign with abstract ideological meaning (cf. Burkhardt 1996). Here he explicitly refutes the Peircean extension of the term's use to arbitrary signifiers such as linguistic signs (see also Heringer 2013: 61) and then adds:

> One characteristic of the *symbol* is that it is never wholly arbitrary; it is not empty, for there is the rudiment of a natural bond between the signifier and the signified. The symbol of justice, a pair of scales, could not be replaced by just any other symbol, such as a chariot. (Saussure 1959: 68)

Symbols in the narrow sense, therefore, are not completely arbitrary. Moreover, given that they always imply the existence of the 'rudiment of a natural bond', they cannot be arbitrary at all by definition. This seems to be the very reason why Saussure explicitly refutes the application of the term *symbol* to words (Saussure 1959: 68). As a consequence of this insight, we have to acknowledge that besides arbitrary signs there are others which are conventional and non-natural but not strictly arbitrary. However, these, too, must be distinguished from signs that were formed based on motivation:

14. In this sense, Heringer (2013: 7) rightly formulates that "Arbitrary does not mean at will. Neither speaker nor the language community are able to do as they want."

> There simply are not only signs which are perfectly arbitrary and signs which are perfectly motivated, but there is also a transitional sphere in which the linguistic units, to a certain degree, participate in both characteristics. (Wunderli 1981: 41)

3. A non-Saussurean approach to arbitrariness and motivation

3.1 A plea for motivation

In *Linguistik nach Saussure* ['Linguistics after Saussure'], Heringer emphasizes that in Saussure's lecture "After the basic assumption of bilateralness soon the most important principle follows: the linguistic sign is arbitrary." (Heringer 2013: 46) He stresses the importance of the principle of arbitrariness, but then says about the competing concept of motivation:

> It is surprising that in this context […] arbitrariness and syntagmatic motivation seem to be mingled. While e.g. *nine* and *ten* are arbitrary and unmotivated, *nineteen* is presented as relatively motivated and thus not completely arbitrary. Motivation and arbitrariness have little to do with one another. For *nineteen* is certainly arbitrary as well. If compounds were not arbitrary, one could forget about the whole principle of arbitrariness completely.
> (Heringer 2013: 46; cf. Saussure 1959: 131)

Heringer seems to suggest that *nineteen* is arbitrary because it is composed of morphemes, which are arbitrary themselves.[15] This is trivially true, but such a view does not refute Saussure's insight that compounds only have a reduced kind of arbitrariness: *nineteen* is not arbitrary exactly because it is the composition of two signs the respective signifiers and signifieds of which are arbitrarily combined (cf. Raggiunti 1990: 184; 1982: 154).

In the second part of the *Cours* ('Synchronic linguistics'), Saussure discusses motivation under the header 'Absolute and Relative Arbitrariness' (*L'arbitraire absolu et l'arbitraire relatif*). This implies that there are two subtypes of arbitrariness (motivation being one of them), and Saussure even speaks of 'degrees of abitrariness' (Saussure 1959: 131). Derivations and compounds are 'relatively motivated'.

Motivation, which, in any language, is predominant in derivations and compounds, for Saussure, is a way of limiting the scope of arbitrariness (which implies that the corresponding sign products are unmotivated by definition), by transmitting information about the signified by means of the morphological composition

15. A similar view was held by Giulio C. Lepschy (1962; see Berardi 1989: 118–119).

of the sign, i.e. by the interaction of 'associative'[16] and syntagmatic relations. Arbitrariness, Saussure explains, is the 'irrational' but prevailing semiotic principle in languages that, therefore, are 'chaotic' by nature. Motivation, on the other side, is the principle by which the human mind brings order into language (cf. Saussure 1959: 133), but for Saussure it is merely a secondary system:

> There is no language in which nothing is motivated, and our definition makes it impossible to conceive of a language in which everything is motivated. Between the two extremes – a minimum of organization and a minimum of arbitrariness – we find all possible varieties. (Saussure 1959: 133)

The Geneva linguist then shows that different languages, though to different extents, contain both: absolutely arbitrary and relatively motivated signs. In German, which is more 'grammatical', motivation plays a larger role than in English or Chinese, which appear to be more 'lexicological' (Saussure 1959: 133–134) and, therefore, more arbitrary, at least on the word level.

Does motivation, as relative arbitrariness, just 'bring order' into the 'chaotic' arbitrary world of linguistic signs or may it be considered as more fundamental? Recall that for Saussure (the avatar) arbitrariness is twofold: absolute and relative. In addition, relative arbitrariness may have different degrees. Symbols in the narrow (non-Peircean) sense, therefore, must be considered as relatively arbitrary. It is not quite clear whether the same applies for onomatopoeic expressions such as *glug-glug*, *tick-tock* and *bow-wow* and to interjections which are "closely related to onomatopeia" (Saussure 1959: 69), as they can be regarded as a "mixture of iconic and arbitrary" (Heringer 2013: 62). However, Saussure shows that even these are arbitrary 'to a certain extent' (*en quelque mesure*; Saussure 1967b: 102),[17] as they reflect the original noises, based on the respective linguistic phoneme system, only approximately and thus are 'demi conventionelle[s]'. Another reason is that "they are to a certain extent subjected to the same evolution – phonetic, morphological, etc. – that other words undergo" (Saussure 1959: 69). All these considerations support the idea that there are degrees of arbitrariness and show that symbols (in the narrow sense) and onomatopoetic expressions are partially arbitrary (or partially motivated, as you like) and that absolute arbitrariness and transparent motivation are but the extreme ends of a continuum.

I propose that there are basically two ways of creating a sign (or of its emergence in communication): [1] assigning to a signified a signifier which has no

16. 'Associative' [*associatif*] is Saussure's term for what was later called 'paradigmatic' (cf. Saussure 1959: 125–127).

17. The English translation "chosen somewhat arbitrarily" (Saussure 1959: 69) here is somewhat imprecise.

perceivable connection with it at all or [2] assigning to a signified a signifier which does have such a connection and may therefore be called 'motivated'. Motivation is a semiotic way of giving hints regarding the meaning or signified of a sign. From the current – synchronic – perspective, one can also put it this way: most (simple) linguistic signs, i.e. lexemes and grammemes, are arbitrary in the sense that there is nothing in their phonemic structure as a 'sound-image' that indicates or gives hints as to their meaning. This view may be verified by the factual phonemic differences of the equivalent signifiers in different languages (recall the example of *sister* and countless others). Therefore, it may appear as if arbitrariness were the true ideal of the conventional linguistic sign. However, this holds only in contrast to the 'natural' onomatopoeic sign (which is still not considered as purely iconic by Saussure, as shown above). But motivated signs, in the sense of words which are composed of arbitrary components (like compounds and derivations), are much more frequent in any sophisticated language, and as most of them are morphologically transparent they may not be considered to be arbitrary in the absolute sense. Despite this, their 'limiting of arbitrariness' (*la limitation de l'arbitraire*) is a viewpoint "which has scarcely received the attention of linguists" (Saussure 1959: 133). Moreover, the subsequent passage, in my opinion, has not found the appropiate attention either:

> In fact, the whole system of language is based on the irrational principle of the arbitrariness of the sign, which would lead to the worst sort of complication if applied without restriction. But the mind contrives to introduce a principle of order and regularity into certain parts of the mass of signs, and this is the role of relative motivation. (Saussure 1959: 133)

On this view, motivation is a "partial correction of a system that is by nature chaotic" (Saussure 1959: 133). On the other hand, put into contemporary terms: motivation is a welcome linguistic means of reducing complexity. For Saussure himself, arbitrariness is primary and more fundamental for any language as a sign system. However, if one does not merely consider the synchronic but also the diachronic perspective, just the opposite is the case: motivation, i.e. relative arbitrariness, is primary and the basic process while arbitrariness must be considered as secondary, namely as a result of processes of language change. Therefore, arbitrariness is mainly a matter of synchrony and of *langue*. That is, whoever wants to denominate something tries to find or form a sign, the signifier of which is constructed in a way that it gives hints or suggestions as to its meaning, or rather to the prominent features of the signified.[18] In addition, there is no reason to assume that this

18. There are very few exceptions to this rule that, as intentional violations of the general rule, strongly support its validity.

attitude should ever have been different in the history of humankind. The so-constructed motivated sign is then subject to phonemic and semantic change which may lead to its becoming arbitrary in the end:

> Within a given language, all evolutionary movement may be characterized by continuous passage from motivation to arbitrariness and from arbitrariness to motivation; this see-saw motion often results in a perceptible change in the proportion of the two signs. Thus with respect to Latin French is characterized, among other things, by a huge increase in arbitrariness. Latin *inimīcus* recalls *in-* and *amicus* and is motivated by them; against this, *ennemi* 'enemy' is motivated by nothing – it has reverted to absolute arbitrariness, which is really the prime characteristic of the linguistic sign. (Saussure 1959: 134)[19]

The German philosopher Eike von Savigny, a specialist in analytic philosophy of language, briefly discussed the example of the German word *Mehl* ('flour') when trying to refute the idea that words denominate objects. He referred to well-known paragraphs of Wittgenstein's *Philosophical Investigations*:

> We cannot talk about flour by using *Mehl* because *Mehl* is the denomination of flour, but *Mehl* is the denomination of flour as we talk about flour by means of *Mehl*. Why do we not talk about flour by *Meh*? The answer, *because 'Meh' is not the denomination of flour,* is empty. *Meh* is not the denomination of flour as we just talk about flour by *Mehl* and not by *Meh*. Who tries to explain why we can talk about flour by *Mehl* by stating that *'Mehl' simply is the denomination of flour*, when asked 'And how can *Mehl* be the denomination of flour?' will certainly not be happy to answer: 'As we talk about flour by *Mehl*'. He must find a completely new answer, an analysis of the naming relation which is independent from other uses of the word. (Savigny 1974: 26)

In addition, von Savigny refers to Wittgenstein's (1986: § 38) famous dictum "philosophical problems arise when language goes on holiday." However, to argue that *Mehl* is the denomination of flour in German because we use the word *Mehl* for talking about flour is simply begging the question. Though it is true that *Mehl* is used to refer to or speak about flour nowadays and that there is a conventional correlation between the signifier *Mehl* and the signified flour in present-day German synchrony, this does not explain at all why such a correlation came about. In fact, the German *Mehl* can be traced back to Germanic **melwa-* 'finely ground grain' which, according to the present state of etymological reconstruction, was derived from an Indoeuropean root *mel- 'to grind' and thus probably was originally motivated.

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19. The development of the Chinese scripture – from Bronze Age pictograms to modern hànzì – is a good example for continuous arbitrarization, i.e. the reduction of former motivation, as well.

The arbitrariness-conception of linguistic signs presupposes that it is constitutive for such a sign that there be no intrinsic relation between signifier and signified. However, this does not even hold for most of the words contained in today's lexicon which are motivated. If words had always been radically arbitrary, this would mean that the question 'why' is forever banned from reasoning in the field of linguistic semiology. However, every language teacher knows that questions like 'Why does the word X have the meaning Y?' or 'Why is Y called X in a certain language L?' are frequently asked in language teaching. There is no reason to exclude them, though it may often be quite difficult or even impossible to answer them.

3.2 Discussion of some examples

In this section, I will discuss three kinds of linguistic signs, which differ in their semiotic constitution: onomatopeia, proper names, and generic names. Additionally, I will examine their degree of arbitrariness.

The fact that there is a basic human desire for motivation is best illustrated with the example of onomatopoeic expressions. Onomatopoeic signs generally may be defined as imitations of natural sounds by linguistic means. Hence, they belong to the iconic subtype of signs.

Some language evolution researchers speculated whether the human faculty of language arose from the imitation of natural sounds, among them the bodily noises intuitively produced by early hominides themselves. The best known account is by Herder. In his famous *Treatise on the Origin of Language* (1772), he tried to show that linguistic signs originate from the direct imitation of natural sounds. Human language, on this view, results from a double metonymy, namely from associating imitations of sounds of nature (which are at first metonymically connected with their sources themselves) with the typical features of the respective objects: Man creates the language instrument by transforming the living creatures' sounds and expressions of emotion perceived by his ears into linguistic interjections as 'characteristic signs' or, to use Herder's original term, 'Merkzeichen'. From these, the corresponding verbs emerge, which, in Herder's view, are onomatopoeic at first (cf. Burkhardt 1998: 489).

Thus, the 'resounding' of the sheep's bleating first becomes a kind of name for the bleating activity and then for the wool-producing animal itself. "The first *characteristic mark* that I grasp is a *characteristic word* for me and a *communication word* for others!" (Herder 2002: 97; 1891: 17, 733). To give some genuine examples: the interjections *phew* and *yuk,* (*pfui* in German and *fe, fi, fu, tfu*, etc. in other languages; cf. Wierzbicka 1991: 302ff.)[20] at first were mere imitations of

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20. Deviating from etymological dictionaries, Wierzbicka (1991: 313) says that "The English *yuk* can be perceived as an imitation of the sound of retching. The English *phew*, the German the sound of spitting from distaste which may then have been used intentionally to express one's disgust to others. In the development process of a system of distinct sounds, they were transformed into chains of linguistic phonemes with a language-specific sound structure that still resembles the noise of the original spitting. Sound-imitating interjections like *bang, splash* or *whizz* are linguistic, i.e. phonemic imitations of typical non-linguistic sounds. A more complex example is the emotional German interjection *ach* (*ah, ahi, och,* etc. in other languages; cf. Wierzbicka 1991: 323ff.). The almost universal distribution of quite similar forms with very similar meanings supports the view that originally it was merely a repetition of the noise of groaning by exhaling loudly. Like the noise of bleating which, according to Herder's *Treatise,* first became the 'inner characteristic word' for the sheep, the noise of exhaling, which originally had only been the effect of groaning, in a second step, must have turned into the "characteristic word" for the pain and discontent which is the cause of groaning by a kind of metonymy. The imitation of the noise of exhaling then might have been used to express pain and discontent intentionally. Thereby, this sound became an iconic sign. But only after having developed a phonemic structure, e.g. /ax/, i.e. by fitting the principle of 'double articulation' (Martinet 1964) which is characteristic for human language, the sign became onomatopoeic in the literal sense. Other assumed related meanings may have been added later. Specimens of onomatopoeia, therefore, are neither completely motivated nor absolutely arbitrary. And Saussure was completely justified in calling them arbitrary 'to a certain extent'. Some of these signs, like the German *ach* and *pfui*, are arbitrarized to such an extent that today their onomatopoeic origin is hardly recognizeable.[21]

Proper names, too, are a good example of what I mean by arbitrarization. Usually, they are the result of an intentional act of denominating, a kind of verbal christening. The general idea underlying their choice – even if there will certainly be exceptions – is to find a sign form, i.e. a signifier, which particularly fits the referent by indicating his most important features and thereby conserving them in the collective memory. Proper names are used to refer to their respective referents, though it must be added that they also may have a lexical meaning that can be taken as their signified in the Saussurian sense.[22] Over time, their original

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pfui and the Scandinavian *fy* can be thought to imitate an attempt to breathe out of one's nose a repulsive smell". In any case, they are explained as sound-imitations and hence must be taken as motivated.

21. With regard to the development of iconographic scriptures, one could argue accordingly.

22. Proper names have a two- or three-sided kind of arbitrariness. Firstly, they arbitrarily, i.e. by convention, refer to a certain bearer as their corresponding referent. Secondly, the signifier motivation often becomes obscured by phonetic change and/or the change of their referents. Place-names like *Sevenoaks* still remind of a settlement that was characterized by the seven oak-trees that provided shade in summer. The name *Grantabricc* 'bridge on the (river) Granta' was derived from a Celtic river name meaning 'marshy river'. After the Norman Conquest, the settlement became until the late 14th century known as Cantebrigie. *Granta-* became *Canta-* and then *Cam-* by processes of phonetic assimilation.[23] The other numerous 'Cambridges', spread over the English speaking world, just echo the name of the famous British university town and thus are motivated by it.

By-names are also a good example for this process leading from motivation to arbitrariness. They result from intentional acts of naming and still show the original motivation. In the history of names, they often became family names later on: *Brown* refers back to a person with brown hair or skin, the *Taylors* or the *Millers* must have had a tailor or a miller for an ancestor, *Oxford* reminds of an immigrant from Oxford and *Sunday* originally was a metonymic name for a person born on a Sunday. Even modern web nicknames do not come about entirely 'by chance' but generally result from an intentional and significant choice which often even is designed to give hints as to the identity or properties of their bearers. At least in the situation of namegiving itself, proper names are hardly ever characterized by absolute arbitrariness.[24] Arbitrarization of family names in particular is the process of demotivation with regard to progeny as the non-original referents (though the examples show that as lexical meaning the essential criterion of the original naming may still be visible hundreds of years later). The well-known linguistic

arbitrarily activates the users' knowledge about the referent named by it. The arbitrariness of proper names therefore subsists in the relation between the name signifier and the bearer of the name in the first place (primary meaning, i.e. reference) (see also Giorgo Derossi according to Berardi 1989: 145–146) and the knowledge associated with it (secondary meaning) (cf. Burkhardt 2012). However, if the name's signifier corresponds to an appellative word or is motivated by its morphological components, it may also have a lexical meaning. If such name signifiers refer to properties of their referents by means of their lexical meanings, as in the case of most nicknames, they must be regarded as motivated. If not, they are arbitrary on a tertiary level as well.

23. One of the reasons may be that the Normans had difficulty pronouncing the Old English names. However, this is only speculation.

24. Even present-day choice of given names is far from being completely arbitrary. This can be seen in the behavior of parents. By choosing a certain name for their children, they want to show their adherence to a certain ideology/ religion or their sense of belonging to a certain culture or community. In other cases, they may have the intention to remind of historical or legendary persons (as e.g. in the case of *Armin*) or are at least influenced by euphonic preferences.

hero of a children's novel nicely illustrates that proper names have meanings that result from the original act of naming:

> 'My *name* is Alice, but –'
>
> 'It's a stupid name enough!' Humpty Dumpty interrupted impatiently. 'What does it mean?'
>
> '*Must* a name mean something?' Alice asked doubtfully.
>
> 'Of course it must,' Humpty Dumpty said with a short laugh: 'my name means the shape I am – and a good handsome shape it is, too. With a name like yours, you might be any shape, almost.' (Carroll 1975: 168)

Turning to generic names, it must again be admitted that Saussure (the avatar) is correct in saying that many words in contemporary languages are arbitrary. However, if they had always been arbitrary from their first use on, the question 'why do we call this an X (*Mehl*, for instance)?' would have been nonsensical from the beginning. Compounds are always motivated (but not completely determined) by their components (at least in a historical perspective): A nutshell is called a *nutshell* because it is the shell of a nut (though the word can also be used metaphorically) and a railway is a way made of rails (though nowadays the word may also be used metonymically for the whole means of transport). There are, however, many compounds that lost their original meaning over time and some reflection or even research may be necessary to uncover their original motivation: Today, a blackboard is no longer expected to be black and a landlord no longer needs to be a nobleman. We no longer associate *breakfast* with the old Christian custom of fasting between the supper meal of one day and receiving the Holy Communion the following morning. Nevertheless, we can still search for the original meaning that was not arbitrary in the narrow sense and describe the semantic and phonetic/ phonological changes which lead to the arbitrariness we observe now. The same applies to countless simple words that are arbitrary (and simple) today but were not arbitrary (and simple) when they were first used. A bicycle is called *bicycle* because it has two wheels. Metaphors are called *metaphors* because Aristotle wanted to express (in Ancient Greek) that words could be carried over into another semantic sphere. Paper – as a result of several processes of phonetic/ phonological change on its way from Latin via French into present-day English – is called *paper* because the original material was produced on the basis of the papyrus plant. One of the etymological explanations of the origin of the word *book* is that it was first used for writing boards made of beech-wood. It was then used to refer to several such boards sewn together and later on to denominate bundles of printed pages. Today, it can also be used metonymically to refer to the content of a book or to a chapter in it (or the electronic representation of its text). The relation between these meanings is not arbitrary at all. Even though one must admit that the original act of naming and the original motivation of the Indoeuropean **b^h aga* for 'beech-tree' cannot be retraced, I do not see any fundamental arbitrariness here, but only a process – to use Saussure's words – from relative to absolute arbitrariness. Saussure does not deny such changes when he discusses the examples of Latin *necare* 'kill' which became *noyer* 'drown' in French or of the Old German *dritteil* which became *Drittel* in Modern German. He calls this phenomenon "a shift in the relation between the signified and the signifier" (Saussure 1959: 75), but he does not clearly identify them as processes of increasing arbitrariness.

Not even seemingly artificial words like *gas, quark* or *spam* were introduced simply 'at random'. The word *gas*, introduced by the Belgian chemist J.B. van Helmont in the 17th century, was derived from the Greek *chaos*. *Quark* as a name for fundamental elementary particles was chosen in 1964 by the American physicist M. Gell-Mann to remind us of some shadowy creatures in Joyce's novel *Finnegan's Wake*. *Spam* was originally a portmanteau shorthand for *Shoulder Pork and hAM* or *SPiced hAM* and used as a brand name for canned precooked meat. This original SPAM played an important role in supplying the allied troops during World War II and was still widely distributed in post-war Britain. Therefore, the word SPAM could be used in a 1970 sketch in Monty Python's Flying Circus to make fun of a menu card in which only dishes containing spam were listed. Alluding to the well-known sketch and highlighting the features of omnipresence and unwelcomeness, it could be transferred metaphorically to junk mails swamping the internet later (cf. Kluge 2011: 861). It could be added that abbreviations generally tend to be arbitrarized (e.g. *radar* for *radio detection and ranging)* and that loanwords from English (or other languages), even compounds, are often used or adopted because they are taken to be 'more' arbitrary than equilvalents in the receiving language (e.g. *Bulldozer* vs. *Planierraupe* or *Highlight* vs. *Höhepunkt* in German). Moreover, if words are judged beautiful, such as *sonorous* or *ethereal*,[25] this is because people seem to have the impression that these signifiers fit their signified particularly well.

3.3 Arbitrariness vs. motivation: Conclusions

All the examples discussed above show that there is a fluid correlation between arbitrariness on the one hand (bringing, in Saussure's words, 'irrational' complication) and motivation creating order and regularity on the other. It results from the

25. See <https://de.pinterest.com/pin/350647520963878512/> (last access 3 September 2016).

fact that language history as the history of words must be understood as a process of increasing arbitrarization.[26] "Therefore, the concept of absolute arbitrariness is of a completely theoretical nature. Actually, the signifier of a language is always relatively arbitrary, i.e. historically motivated." (Raggiunti 1990: 187; 1982: 157)[27]

If we consider the question of arbitrariness in a more analytical way, we will recognize that Saussure perfectly grasped the arbitrary character of the sign but did not push its investigation far enough (cf. Raggiunti 1990: 191; 1982: 161), for he was not able to completely clarify the concept in question. Arbitrariness is not only a mere relation between signifier and signified but also refers to the interior structure of the signifier and the signified themselves, which must be considered as intrinsically arbitrary, or motivated, too. While the bond that ties the signifier to the signified, according to Raggiunti, is absolutely arbitrary, "the components of the relation, signifier and signified, on separate examination, can be relatively arbitrary or relatively motivated" (Raggiunti 1990: 193; 1982: 162). The relatively arbitrary character of the signifier is easily recognizable because it can be quite freely constructed within the limits of the phonemic rules of the respective system. And if one considers its distinctiveness with regard to all other signs within the system, it also becomes clear that the irreplaceability of the signified is one of the reasons for its arbitrariness. Moreover, signifieds are at least partly determined by extra-linguistic experience. Therefore, they must be determined as relatively arbitrary (cf. Raggiunti 1990: 188–189; 1982: 158–159). But, with Raggiunti, we also come to the conclusion that "for any sign whatever of any language whatever, it is [absolutely] arbitrary that exactly this particular group of phonemes is combined with that particular concept and this particular concept with that particular group of phonemes." (Raggiunti 1990: 195–196; 1982: 164) Arbitrariness, therefore, is but a reduction of the transparency of a word's motivation to zero. In the sense that a signifier of a signified could be replaced by any other combination of phonemes or graphemes whatsoever, it exists only theoretically.

Taken together, these considerations lead to the assumption that motivation and not arbitrariness is the real moving force of sign choice and sign formation. Linguistic signs, in most of their respects, are (or at least have been) only relatively arbitrary. Motivation stands at the beginning and arbitrariness at the end of

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26. At first glance, folk etymology could be referred to as a counterexample. However, as an etymologically false re-motivation of a word that has become arbitrary in the course of time (e.g. *sand-blind* from OE *sam-blind* 'half-blind') it is part of the eternal play of transition between motivation and arbitrarization, too.

27. Even the examples like *gas, quark*, and *spam*, discussed above, must be taken as cases of indirect motivation.

a word's historical career. In this light, onomatopoeia, which is in principle iconic, may just be taken as a basal special case of motivation, i.e. relative arbitrariness, which rarely occurs. Therefore, I propose to follow Ullmann (1957: 83–92) who counts onomatopeia as a subtype of motivation (beside the phonetic, morphological, and semantic).[28]

At least in the act of naming people, but not only there, human beings want to indicate an analogy between signifier and signified in order that the respective form refers or hints to the respective meaning.[29] With Pagliaro one may say that the original determination of signs is not to discern the signifiers from the signifieds, but "to imprint its meaning into the sign, i.e. to coin it" (Pagliaro 1957: 92). Over time, the original motivation may be reduced to arbitrariness by the mechanisms of phonetic/ phonological, morphological, and semantic change[30] step by step. From a certain point of the development on, it may no longer have any impact on the use or understanding of the sign. The resulting arbitrary sign, in turn, can now be used to motivate new signs by derivation or composition.

Absolute arbitrariness, as Raggiunti (1990: 187; 1982: 157) points out, is just a theoretical category, for it is true that theoretically every signifier could be replaced by any other linguistic form if a new convention was introduced. However, some signifiers will be considered as more suitable to denominate a certain signified than others because a semantic link between form and content may be seen. Therefore, signifiers, for the most part, are relatively arbitrary as they are historically motivated and cannot be changed easily within the communication system.[31] Ignoring for a moment the question of the origin of language, which some theorists, like Herder, base on onomatopeia, one may say the following: From a synchronic point of view, the simple signs of every language are mostly arbitrary, though to a different extent in different languages. Onomatopoeic signs continue to exist but are an exception to the rule, even with reduced iconicity. Instead, languages mainly consist of signs with reduced arbitrariness that are motivated in

28. For Ullmann metaphors and metonymies are semantically motivated (cf. 1957: 87–89). Ullmann also prefers the weeker term 'conventionality' to the Saussurean 'arbitrariness' (Ullmann 1957: 84–86), though the former is implied by the latter.

29. This seems to be one of the main reasons why Humpty Dumpty, in spite of the prevailing principle of arbitrariness, is wrong in advocating the view that words just mean what he chooses them to mean (cf. Carroll 1975: 173).

30. These mechanisms are described in a Saussurean context, e.g., by Heringer (2013: 80ff.).

31. By far the most linguistic signs are assigned to the objects and states of affairs they refer to "*by virtue of* a convention", though the "historical preamble of this assignment is unknown to the speaker of today" (Bühler 2011: 36).

a way that their linguistic form is composed of simple signs which, over time, have become arbitrary, as combinations of signifier and signified. From a diachronic perspective it appears that the arbitrary signs for the most part must have arisen from originally motivated – sometimes even onomatopoeic – signs. The original motivation of those signs then was gradually reduced by phonetic, morphological, and semantic change so that it is now difficult to perceive it in synchrony. The main attempts to reconstruct earlier motivation from later arbitrariness are undertaken by etymology and onomastics. Arbitrariness is the result of a historical process, namely that of arbitrarization which, therefore, is a main feature of the fundamentally historical character of language(s) (cf. Raggiunti 1990: 175; 1982: 149). Grammaticalization, which has been a very recent topic of scientific research and discourse, is but a special case of this more general process.

Meaning is constituted by establishing a relation between a signifier and a signified that is accepted by a speech community. This relation is designed to be transparent at first but may lose its transparency over time in the process of arbitrarization. This also means that arbitrariness is not merely a relation but the temporary end of a diachronic development. Unfortunately, by unduly focussing on arbitrariness instead of insisting on the priority of motivation, Saussure (the avatar) has turned their relation upside down (cf. Saussure 1959: 76).

Man is not only a technological animal but also the great name-giver who assigns names to objects and states of affairs and uses words to create and define abstract concepts. However, it seems hard to imagine that words in particular or signs in general have been arbitrary from the very beginning. What we perceive as arbitrary signs are but the results of the continuous historical process of arbitrarization. Moreover, arbitrariness, once it has developed, makes the linguistic signs and the *langue* to which they belong, in a certain sense, "independent of a pre-existing[32] world" (Heringer 2013: 67). Thanks to their doubly articulated structure, arbitrary signs undoubtedly have great advantages over such analogous sign systems as bee dancing or simple natural cries or shouts. Still, language is, mainly, the realm of – morphological and semantic – motivation.

4. Saussure – A realist?

Do Saussure's conception of language (*langue*), his binary conception of the sign and his insistence on the priority of arbitrariness lead him to a realist, to a nominalist, or to a conceptionalist view? I would like to conclude with some remarks on this question.

32. Or 'pre-semiological' (Jäger 2010: 143).

Saussure, the avatar, basically advocates the following:

1. Language (*langage*) exists in the form of languages (*langues*) which are 'social facts'.
2. These are in the heads of their speakers and do not exist as material objects outside the human brain.
3. A language (*langue*) is a system of non-natural, i.e. man-made artificial signs and rules for their combination which exists in relative independence from its aims and causes and is the true object of linguistics.
4. Linguistic signs are bilateral, i.e. every sign is a combination of expression and meaning, signifier and signified.
5. They are characterized by the relation of arbitrariness between signifier and signified.
6. Their meanings are abstract values, defined by their opposition in relation to one another within the system.
7. The language system does not exist in diachrony, but only in synchrony.
8. Language use (*parole*) is application or realization of the language system (*langue*).
9. The structure of the language system at least consists of the two relations syntagma (segmentation) and paradigm (classification).
10. Language (*langage*) is the interplay of 'langue' and 'parole'.[33]
11. Actual speech (*parole*) belongs to 'external linguistics'.
12. The speaking subject is not the object of linguistics.
13. As a science of signs, linguistics is part of semiology which itself is part of social psychology.
14. Linguistics is an analytical, empirical, and descriptive science and makes use of corresponding methods.
15. Its essential task is the analysis and description of languages (*langues*).[34]

May a scholar (or a scholarly avatar) who holds such views be rightly called a realist? Alternatively, must s/he be considered a nominalist or a conceptionalist? The answer will depend on the conception of realism one has in mind.

There is no reason to doubt that Saussure accepted the existence of a mind-independent world. In this respect, he would have to be called an 'ontological' and 'epistemic realist'. Would he also have believed in the material existence of

33. "Language is speech less speaking" (Saussure 1959: 77). The French original is terminologically clearer: „La langue est […] le langage moins la parole" (Saussure 1967b: 112).

34. This list mostly corresponds to Burkhardt (1997: 212).

universals? His conception of the meaning of a sign being its value in relation to others within the sign system, which includes that they mutually delimit one another, in connection with the opinion that no signified is delimited by nature or the objects referred to would at least count against such a view. Moreover, "Saussure does his best to demonstrate that ideas cannot exist without the language to identify them, however arbitrary that language may be" (O'Reilly 2012). If he did not emphasize the social character of *langue* and if his famous leaf metaphor did not exclude the separate existence of ideas ('concepts') at all, in a way that "one could neither divide sound from thought nor thought from sound" (Saussure 1959: 113), one might even be inclined to call him a conceptualist. Was he a 'scientific realist', then, postulating that ultimately scientific research will lead to a complete knowledge about the external world? That is also doubtful with regard to language (*langue*), as he never gets tired of emphasizing that it cannot be explored or described completely. May Saussure be considered a 'semantic realist' like, e.g. the Wittgenstein of the *Tractatus* who conceived meaning in terms of reference? His bilateral conception of the linguistic sign as composed of 'concept' and 'sound image' eliminates this possibility. Moreover, the question of truth and falsity is for him post-semiological, as it also is, for example, for Eco (cf. 1972: 73). With regard to his heuristic conception of a *langue* which does not really exist in the world but only as a necessary linguistic hypothesis, he is, at least in this particular point, very close to 'critical realism' and its view that no immediate but at most an approximative access to the external world is possible by sensual data.

Neef, in a recent article (2014), has defended a Katzian view according to which languages and the linguistic signs and grammatical rules as their components must be considered as abstract objects. This is a view Katz has defined as 'Platonic realism', distinguishing it from conceptualism and nominalism:

> Platonic realism holds that universals are real but distinct from physical or mental objects (i.e., non-spatial, non-temporal, and independent of minds). Conceptualism holds that universals are mental, with its particular forms arising from different specifications of the sense of 'mental'. Nominalism holds that only the sensible signs of language are real; the alleged use of them to name universals is nothing more than reference to space-time particulars with signs that apply generally on the basis of resemblance. (Katz 1981: 22)

In a follow up article, this view is further elaborated:

> In the early eighties, conceptualism was challenged by a new view of NLs [= natural languages]. This Platonist, or, as we say, realist, view takes NLs to be abstract objects, rather than concrete psychological or acoustic ones […]. This view is the linguistic analog of logical and mathematical realism, which takes propositions

> and numbers to be abstract objects […]. On a realist view, linguistics, like logic and mathematics, has no psychological goals, depends on no psychological data, and has no psychological status. (Katz & Postal 1991: 515)

At first glance, the expression 'abstract object' may sound a bit odd because of its seemingly oxymoronic construction. However, if 'object' refers to a mere topic for thinking and research without any material existence, a language (*langue*), as a matter of fact, may well be called an object which is *abstract* as opposed to *concrete* (which would only apply for *parole*). Language, as *langue*, is a *fait social*, 'something collective' (Heringer 2013: 26–27, 32–33) and, therefore, does not exist independently of the speakers. Nevertheless, its ontological character does neither include that universals are mental nor that only its sensible appearance is real. As language (*langue*) has no material but only a psychic collective existence which for speakers and linguists is the necessary fiction they need as "a common basis for mutual understanding" (Heringer 2013: 39) and which, therefore, is an object of 'social psychology', it may also be called an abstract object in the conceptualist sense. Oddly enough, Saussure himself (the avatar) explicitly determines it as "concrete, no less so than speaking" and, refuting the idea of linguistic signs being abstractions, he says that they are "realities that have their seat in the brain" and thereby attributes a psychic (though collective) 'reality' to them (Saussure 1959: 15). Now, what can a psychic collective reality be other than abstract, at least in a broader sense? I think the difference between Saussure's definition of language as a 'concrete object' and Katz's conception of language being an 'abstract object' is that the former insists on a psychic form of existence of language (particularly of the 'concepts' associated with the 'sound images') which may be called a conceptionalist position while the latter attributes a separate ideational existence to it which is realist by definition. But by calling language a 'concrete object', the Geneva linguist's, as it were, light version of conceptualism comes quite close to a realist position.

And if it is true that the 'other' or 'genuine' Saussure sees the existence of a language in its 'circulation' among the speakers (see Saussure 1997: 211, 225, 243), i.e. in the permanent re-creation and reproduction of signs and rules in the *circuit of parole*, then *langue* also cannot exist as a material object. Again, Saussure comes relatively close to the Plato/ Katzian position.

However, though it seems to be common to both versions of his thinking that Saussures' major concern was to avoid "hypostatizing a language behind speaking" (Krämer 2001: 11), it is also true that the 'other' (or genuine?) Saussure of the *Notes* (and other writings which remained unpublished during his lifetime) in many respects held views that were quite different from those of his avatar. With regard to the notion of arbitrariness, one may follow Jäger's interpretation:

> As […] there are no sources of possible meaning or a transcendent reason for a conceptual order beyond semiosis, the latter itself, as the space of performative discursiveness, becomes the place in which the constitution of meaning begins its infinite game. This is the punch-line of Saussure's conception of *arbitrariness*. (Jäger 2010: 162)

This would mean that arbitrariness, like *langue* itself, is something which exists only temporarily, as a volatile connection which may change in the course of parole-communication and therefore is valid only for a certain period of time, that is, in a synchrony. Moreover, arbitrarization in this light must be characterized as a never-ending story.[35] However, these views do not answer the question of what was the relation between signifier and signified in the naming or coining situation and how its arbitrariness first came about. In view of the permanent 'whirl of signs' [*Wirbel der Zeichen*; Saussure 1997: 355], as Saussure puts it (not his avatar this time), the question of how language can be an object and whether, as such, it is *abstract* or *concrete* may be answered by semiological reflections only with even more difficulty.

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35. "If an object, wherever it may be, could be the final point a sign is fixed on linguistics would immediately cease to be what it is" (Saussure 1997: 140).

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Index

This book contains new articles by leading philosophers and linguists discussing a promising philosophical framework distinct from currently dominant ones: Linguistic Realism. As opposed to Nominalism and Chomskyian Conceptualism, this approach distinguishes between use of language, knowledge of language, and language as such. The latter is conceived as part of the realm of abstract objects. The authors show how adopting Linguistic Realism overcomes entrenched problems with other frameworks and suggest that Linguistic Realism will best serve those interested in formal linguistics, the cognitive dimension of natural language, and linguistic philosophy. The essays offer different perspectives on Linguistic Realism, either supporting this paradigm or taking it as a starting point for developing modified conceptions of linguistics and for further tying linguistics to the kind of formal theories of sensory cognition that were pioneered in visual perception by David Marr – whose work is predicated on exactly the object/knowledge distinction made by Linguistic Realists.

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