

Theorization and Representations in Linguistics

Edited by
Viviane Arigne and
Christiane Rocq-Migette

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Cambridge
Scholars
Publishing



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This book first published 2018

Cambridge Scholars Publishing

Lady Stephenson Library, Newcastle upon Tyne, NE6 2PA, UK

British Library Cataloguing in Publication Data

A catalogue record for this book is available from the British Library

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ISBN (10): 1-5275-0645-2

ISBN (13): 978-1-5275-0645-9

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ACKNOWLEDGEMENTS

Many colleagues have contributed in a variety of ways to bringing this publication about and we are grateful to each and every one of them. Our thanks are due to Nicolas Ballier, Philippe Bourdin, Evelio Cabrejo-Parra, Jean-François Chiantaretto, Pierre Cotte, Philippe De Brabanter, Ilse Depraetere, Jean-Pierre Desclés, Jean-Michel Fortis, Brendan Gillon, Geneviève Girard-Gillet, Jacqueline Guéron, Albert Hamm, James Hampton, Daniel Hirst, Jacques Jayez, Paul Larreya, Philip Miller, Sara Mondini, Eithne O'Neill, Jean Pamiès, Susan Pickford, Graham Ranger, Raphael Salkie, Michael Stubbs and Jennifer Vince. Our gratitude for their help and support also goes to the staff of the UFR LLSHS, notably, Rosa Abdesselam, Maud Loubet, Clara Picone, Houda Schiratti and Bérénice Waty, while the competence of our kind editorial assistant François-Xavier Mas was invaluable in bringing the book to completion. Lastly, we are indebted to the following members of the Pléiade research unit (EA 7338), Frédéric Alexandre, Maruszka Meinard and Françoise Palleau-Papin, as well as Anne-Valérie Dulac, Yann Fuchs, Mathilde Lévêque and Claire Parfait.

INTRODUCTION

VIVIANE ARIGNE
AND CHRISTIANE ROCQ-MIGETTE

The aim of this book is to question the representations related to theoretical frameworks in linguistics and at the same time reach a certain degree of insight into the way natural languages function. Access to theoretical representations can only be gained through an analysis of metalinguistic discourses, so that the volume can, in this respect, be seen as a sequel to our former publication *Metalinguistic Discourses* (2015). However, the contributions assembled here approach the topic of metalinguistic discourses from a new perspective. While still mainly addressing issues internal to linguistics, they also deal with the relationships between linguistics and other disciplines, studying how linguistics interfaces with related fields of study such as philosophy, logic, psychology and other cognitive sciences. The idea of this publication sprang from an international conference held on the subject of metalinguistic discourses in 2015 at Paris 13 University, in which interdisciplinary approaches were encouraged. While some of the chapters are authored by researchers who took part in the conference as speakers and then submitted a written version of their presentation for this publication, others are written by members of the scientific committee who volunteered to contribute their reflections on the conference themes.

The book is divided into two parts. The first part is entitled “Systems of Representation” and questions the very concept of metalanguage. The contributions gathered in the second part, “Linguistics and Cognition,” are more specifically dedicated to the relations between linguistics, language and cognition from different angles. Needless to say, considering topics such as representations and cognition, some themes will be seen to recur at various points in the volume independently of the part they belong to. One might cite, for example, the question of categorization or the interdisciplinary approach to linguistics.

In the first chapter, entitled “The Relations between Object-Language and Metalanguage in Formalised and Natural Languages,” Philippe de

Brabanter aims at clarifying the manner in which terms borrowed from logic can legitimately apply to natural languages. In the case of formalised languages, logicians assemble metalanguages to construct the former and describe their semantics. The metalanguage, which has its own lexicon, and the object-language therefore belong to two separate hierarchical levels. Things are far less obvious and the distinction blurred in the case of natural languages, as no clear distinction can be made between object-language and metalanguage, the object-language being the set of metalinguistic sentences of a natural language. As lexical items are apt to display various degrees of metalinguisticity, any neat hierarchy such as is found in logic is difficult to establish and this object-language can by no means be considered as a language in its own right. A better, more adequate theory of quotation seems to be a much more desirable objective than attempting to theorize about natural metalanguages. Nicolas Ballier agrees with Philip de Brabanter as regards the “impossibility of an abstract formal language (of the Fregean type).” In the second chapter, “What do Linguists Do when they Write Something: the Art of Stenography,” he draws our attention to the role of notation in linguistic theories which, although not a metalanguage proper, is a vital component of linguistic metadiscourse. He develops the concept of stenography, examining the origins of the word understood as short hand and its use in linguistics and semiotics. He then goes on to show that a system of metalinguistic representations is entrenched in its notational procedures. This point is exemplified by the work of eighteenth century orthoepists and their reconceptualization of syllables and Nicolas Ballier compares the stenography of syllable division over a century of notational systems with the explicit metadiscourse found in the preface and entries of orthoepists’ treaties. In his chapter “Metalinguistic Discourse in Chomskyan Theory and ‘Representation’,” Jean Pamiès also answers a need for clarification. After a definition of what is subsumed by metalinguistic discourse, he examines the protean use of the term ‘representation’ in Chomsky’s work through the successive states of the theory. The word ‘representation’ is thus understood as mental representation, cerebral representation, theoretical representation, symbolic representation, semantic representation or intentional representation. For Jean Pamiès, these different uses of the term are a thread enabling one to follow the evolution of Chomsky’s thought. Using and analysing quotations from the latter’s work, he shows that, on the one hand, the interface between the different levels of representation cannot be scientifically defined and established and, on the other hand, that the link between mind and brain, absent in the theory, cannot be accounted for by the well-known metaphor of software and

hardware in a computer. In conclusion, it is impossible for Chomskyan theory to qualify as a science on a par with physics: in fact, the theory has become so cut off from observation and empirical data that it has drifted into pre-scientific speculation.

Although the issue of cognition lies at the core of the analysis of representations as seen through linguists' metalinguistic discourses, it is more specifically addressed in the second part of the book. With "Cognitive Linguistics as One of the Cognitive Sciences: a Question of Terminology," Katarzyna Kwapisz-Osadnik presents a review of fundamental notions which are embedded in the history of cognitive linguistics and to be found in a variety of approaches and methodologies. Cognition not being the sole property of linguistics, she also studies the use of these notions in other disciplines, namely philosophy, psychology, mathematics and ethnology. The comparison reveals that identical terms in the lexicon may relate to different concepts, a state of affairs which is quite understandably conducive to a measure of confusion and misinterpretation. Focusing on Anglo-Saxon, French and Polish points of views, she insists on the fundamental holistic nature of the study of linguistic phenomena and the interdisciplinary nature of cognitive linguistics, a stand-point which may lead us to reassess the epistemological status of the various fields of research at work in the study of language. A specific example of a term being used in different scientific domains is that of 'prototype.' Jean-Michel Fortis, in his chapter entitled "Prototype Theory: the Origins of a Theoretical Patchwork," gives a detailed historical account of the origins and development of prototype theory, from its initial elaboration by Rosch in the 1970s to its transfer from psychology to linguistics by semanticists. The early days of the concept were contemporaneous with the categorization of colours, from Lenneberg's to Berlin and Kay's research. It was then that Rosch elaborated her own theory of categorization, extending its scope from colours and spatial shapes to 'semantic categories,' which she analysed in terms of discrete features. The transfer to linguistics occurred in the context of generative semantics, when Lakoff and Ross were working on their project of 'fuzzy grammar.' The version of the prototype theory implemented in this new lexical semantics was, however, drastically reduced to a distinction between basic meanings and, in the case of polysemous words, different and distinct meanings linked by family resemblance. To conclude, Jean-Michel Fortis notes that the concept of prototype has found other applications, in phonology and functional linguistics, as well as in attempts to isolate schematic units abstracted from usage patterns. Semantic categorization is also at the heart of "That is Conjecture'. On English Assertive Shell-Nouns." Carla

Vergaro studies the metarepresentation of utterance acts, as named by illocutionary nouns in their function for shell-nouns. Basing her research on theoretical insights from cognitive linguistics and an empirical-conceptual approach to verbal communication, she contends that conceptualization can be studied through language, albeit in an indirect way only. In this respect, illocutionary shell-nouns are metalinguistic devices which are particularly appropriate for this kind of study. Nouns that name assertive speech acts are thus selected to conduct a corpus-based study and are then categorized on the basis of their complementation patterns and the potential collocations with determiners. This categorization is supported by descriptive and exploratory statistics. The study shows that both complementation and complement selection are linked to the semantics of the noun, exhibiting a correlation between semantic similarity and distributional similarity. In the final chapter, "Inference Processes Expressed by Languages: Deduction of a Probable Consequent vs. Abduction," Jean-Pierre Desclés and Zatkla Guentchéva also deal with categorization, which, in this instance, concerns mental operations, namely the different types of inference. Logical inference through deduction should not be confused with inference by abduction, the latter leading to the formulation of a more or less plausible hypothesis. As far as deduction is concerned, they underline the necessity to distinguish between a simple type of deduction and deduction that results in a probable consequent, contexts and inference processes being different in either case. The modal notion of uncertainty is shared by both abduction and deduction with a probable consequent, leading to the potential use of modalities such as possibility, probability, improbability, or to the explicit statement that what is expressed is a plausible hypothesis or a probable consequent. Inference is a cognitive process used in scientific reasoning as well as in the everyday use of language. Whereas scientific reasoning tries to make inferential processes explicit, they often remain implicit in the everyday use of language. As a matter of fact, the different types of inference may or may not be grammaticalised, depending on the language used. English lacks this possibility but Albanian, Bulgarian, Pomo or North-Amazonian languages, to name but a few, resort to specific formal devices to express abductive inference based on clues.

This book is an attempt to clarify some of the complex issues raised by theorization in linguistics. Metalinguistic discourses are intimately related to, and dependent on, theoretical frameworks and constructs, and one must also consider that the theory itself may also be seen as influenced by them. Metalinguistic discourses being held in natural languages, their lexicons

exhibit considerable instability, due to the symbolic and dynamic nature of language as well as to the theoretical framework. Taking into account other disciplines in an interdisciplinary perspective may lead to even more instability of the shared lexicon, as has been seen in the cases where cognition and cognitive sciences other than linguistics were concerned. Taking cognition into explicit consideration in the analysis of (meta)language reminds us that language is a symbolic system of representation processed by the human, cognitive, faculty of language. This is why the theme of representation is a fundamental concept in both the analysis of metalanguage and that of ordinary language, and in a number of cases closely associated to the issue of linguistic categorization. Language can probably be said to be a cognitive activity *par excellence* so that, as Katarzyna Kwapisz-Osadnik points out, “language [...] proves to be the most direct and natural source of knowledge about human beings in all their intellectual, psycho-affective and behavioral complexity,” and linguistics should be given pride of place among all the cognitive sciences.

PART I

SYSTEMS OF REPRESENTATION

CHAPTER ONE

THE RELATIONS BETWEEN OBJECT- LANGUAGE AND METALANGUAGE IN FORMALISED AND NATURAL LANGUAGES¹

PHILIPPE DE BRABANTER

Introduction

The terms ‘object-language’ and ‘metalanguage’ form an inseparable pair,² so much so that there is no point in talking of one if there is no other. They were introduced into scientific parlance by logicians, in the early 1930s. Alfred Tarski is usually credited with coining, in 1931 or 1933, the term *metajęzyk*, later to be translated into English as *metalanguage* (cf. Rey-Debove 1978, 7; Jakobson 1981, 25; Blackburn 1994, 239). However, in Schilpp (1963, 54), Carnap explains that he used the term *Metasprache* as early as 1931, in unpublished lectures on metalogic given in the Vienna Circle.

Be that as it may, it is clear that logicians of the early 1930s had a genuine need for terms like *metalanguage* and *object-language*. Just as they were striving to set up formalised languages that could do duty as ‘languages of science’, in the place of what they saw as flawed everyday languages, there had been a realisation that any description or definition of the semantics of formalised languages above a basic level of complexity had to be provided in a different language, on pain of generating semantic paradoxes such as the ‘Liar paradox.’ This different language had to be of a higher-order than the one whose semantic properties were being described or defined. For instance, for truth—a, perhaps *the*, central property in formal semantics—Tarski devised partial definitions which took the form of bi-conditional statements. These statements were couched not in the formalised language the truth of whose formulas was at stake, but in a higher-order metalanguage. (An illustration is provided in Section 2.1.)

Another motivation for developing hierarchies of languages is remedial in intent. The philosopher-logician Quine noted that logicians and mathematicians routinely failed to observe the distinction between producing sentences of the object-language and making claims about that language (Quine 1940, Chapter 4). This can lead to confusions where absolute clarity is required. These errors, however, can be corrected through strict adherence to the use/mention distinction. The mention of a term or formula is achieved by using a quotation (Quine 1940, 23). Thus, in

- (1) ‘**Boston**’ is disyllabic

the subject ‘*Boston*’ is a quotation that names not the city, but the name of the city. Quotation is an unambiguous sign that the sentence that contains it is of a higher order than the term or formula mentioned. As we shall see, quotation is also of central importance in any discussion of the relations between object-language and metalanguage.

1. The relation between object-language (L_o) and metalanguage (L_m) in the theory of formal languages

Logicians *construct* object-languages and metalanguages. In so doing, they stipulate the features that these languages should display. Object-languages are formalised languages. Metalanguages are assembled in order to construct these formalised languages and/or describe their semantics. More often than not, at least in its initial stages, a metalanguage is informal, usually consisting of a (portion of a) natural language, suitably enriched with additional elements (Carnap 1947, 4; Church 1956, 47).³

1.1. The components of L_m

The views expressed in the literature as regards the building blocks of L_m vary slightly, but there is some consensus on the following elements. First, L_m must contain names of, and variables for, the symbols and the well-formed formulas of the object-language (Tarski 1983, 172–173;⁴ Carnap 1947, 4; Church 1956, 60). Those quotations, conceived as names, are typically formed by enclosing the L_o symbols or formulas in quotation marks. Second, L_m must include semantic predicates, such as *true (sentence)*, *satisfy*, *denote*, *designate*, etc. (Tarski 1983 passim; 1944, 345; Carnap 1947, 4; Gupta 1998, 266). Third, it must contain either all the L_o symbols and formulas, or translations of these. Some logicians mention

just one of these alternatives. Thus, Reichenbach (1947, 10–11) favours a conception on which Lm also contains words from Lo. Others, like Tarski (1944, 350) or Prior (1967, 230), do not seem to regard the alternatives as significantly different.⁵ A third group favour the use of translations as a “precaution against equivocation” (Church 1956, 63; cf. also Carnap 1947, 4).

The various components of Lm can be illustrated by an instance of Tarski’s famous bi-conditional statements:

- (2) ‘**Snow is white**’ is a true sentence (in English) if and only if snow is white.

The subject is a quotation-name. *True sentence (in English)* is a semantic predicate. The clause that occurs after *if and only if* is either a sentence of Lo, or an extensionally equivalent translation of it. Whatever other words there are (*is, if and only if*) are also either Lo expressions or translations of these.

1.2. Sentences and levels of language

For many logicians, $Lm \supset Lo$ (e.g. Tarski 1944, 350). Strictly speaking, the validity of this claim depends on whether one assumes that Lm includes translations of the symbols and formulas of Lo, or these symbols and formulas themselves. At any rate, logicians agree that Lm must be expressively richer than its Lo: it is made impossible to name an Lo element within Lo itself. Any sentence that contains a quotation-name is unambiguously a sentence of Lm. Likewise with sentences including a semantic predicate. What of those that include neither, e.g. *Snow is white*? If one assumes that Lm includes translations of Lo symbols and formulas, then there is no ambiguity as to the level of language: the form of the sentence will always reveal if it belongs to Lo or to Lm. If, on the other hand, one assumes that $Lm \supset Lo$ in the strict sense, then there are formulas that could be regarded as being either ‘object-level’ or ‘meta-level.’ Those strings are “ambiguous as to level of language,” as Reichenbach (1947, 10) puts it. This has two consequences. Thinking in sentential terms, a string like *Snow is white* can be both a sentence of Lo or of Lm. The lexical-level consequence is that one type of ‘building block’ of Lm sentences, such as *snow* or *is*, is also potentially an Lo element.

Does this ambiguity as to level of language pose problems for the hierarchy of languages? It does not, because there is never any point for the logician to produce an object-level sentence and intend it to be

understood as a sentence of Lm. Recall that Lm is set up to construct or describe Lo, so that any (relevant) sentence of Lm will contain a quotation-name and/or a semantic (or syntactic)⁶ predicate.

1.3. Summary of the main properties of the Lo-Lm relation in the theory of formal languages

- (i) Both Lm and Lo are languages in their own right;
- (ii) Lm is set up to describe/construct *one* Lo;
- (iii) Lm has its own 'lexicon.' The members of this set are listed as 'components' in Section 2.1.
- (iv) For practical purposes, it is always possible to determine if a component of a sentence belongs to Lo or Lm;
- (v) Lo is contained within its own Lm. (However, on a strict interpretation, the relation does not obtain if Lm is conceived of as containing translations of all the expressions of Lo.);
- (vi) (In spite of (v)) Lm and Lo occur at distinct levels in a hierarchy of languages;
- (vii) For practical purposes, it is always possible to determine if a sentence belongs to Lo or Lm.⁷

The reason why the boundaries and relations are so definite is that formalised languages and their metalanguages are designed in such a way as to display the desirable features just recapitulated.

2. In the theory of natural languages

Logicians, among them those who elaborated the relations between formalised object-languages and their metalanguages, are very much aware that these relations cannot be replicated exactly when it comes to natural languages. To begin with, they point out the flaws of natural languages, such as vagueness, ambiguity, irregularities and their unsuitability as languages for scientific discourse (Carnap 1937, 2; Tarski 1944, 347; Reichenbach 1947, 6; Church 1956, 47). They also question the existence of a clear boundary between object-language and metalanguage (Reichenbach 1947, 16), readily assuming that the inclusion relation is inverted, with the natural object-language containing its metalanguage (Grelling 1936, 486). Relatedly, they also emphasise what Tarski (1983, 164) called the 'universality' of natural languages, i.e. their ability to talk about anything, which prevents them from being organised along a hierarchy of languages and, accordingly, lays them open to the semantic

paradoxes (Tarski 1983, 164). Tarski consequently questions the very possibility of constructing an exact definition of truth for natural languages. The only remedy would consist in imposing on everyday languages a division into a sequence of object languages and metalanguages. Such a ‘rationalization,’ however, would be pointless because it would likely deprive a natural language of the features that make it a natural language. (Tarski, 1983, 267, as discussed by Simmons 2009, 559).

In the rest of the quite substantial Section 2, I look into the applicability of ‘talk of metalanguage and object-language’ in the context of natural languages.

2.1. Can the relations be explained using two terms?

Let us start with the extreme view that the natural Lo is the same language as its Lm. Several writers seem to hold that view. Thus, Carnap writes that “[o]bject language and metalanguage may also be identical, e.g. when we are speaking in English about English grammar, literature, etc.” (1948, 4; also Gupta in Honderich 1995, 555). Initially, I will interpret this view literally, as a genuine identity statement, and examine its consequences.

Like Carnap, let us choose English as an example. The notion that needs to be captured here is ‘the metalanguage that uses English to describe the natural language English.’ The phrase is cumbersome, and will therefore be shortened to ‘the English metalanguage.’ Most authors conceive of the English metalanguage not as a language proper, but as a special use or function of English (e.g. Jakobson 1985, 117; Rey-Debove 1978, 7, 9; Gamut 1991, 27), or a particular discourse in English (Rey-Debove 1978, 42). In this context, *language* is used loosely. However, the English metalanguage can also be understood more strictly as a language, provided a set of lexical items is identified that, combined with a finite number of ‘formation’ rules, can be used to generate the set of all the sentences of the English metalanguage. (Section 2.2.3. is devoted to the ‘metalinguistic lexicon,’ or *metalexicon*, for short). For the moment, I will be content with pointing out that, based on the current state of our knowledge of English, no one can state a system of rules that, given the English lexicon, would generate all and only the sentences of the English metalanguage. Nor, as we will see in Section 2.4., is there any hope of ever achieving this goal. As a result, no substantial difference remains between the stricter and the looser interpretations, i.e. between ‘metalanguage as lexicon + formation rules,’ and ‘metalanguage as

particular use.’ For the sake of simplicity, in what follows, I will continue to talk of the English metalanguage as a language, i.e. as a set of sentences, all the time assuming that whatever point is made in terms of the set of sentences can also be made in ‘use’ terms.

Now, if English and its metalanguage are identical, then every English sentence must be about English. In other words, English is the only topic that can be talked about in English. This is an absurd consequence that flies in the face of the oft-recognised universality of natural languages, i.e. their ability to talk about ‘anything’ (e.g. Tarski 1983, 164; Hjelmlev 1966, 175; Droste 1989b, 931).

The view just discussed is unlikely to be seriously entertained by any scholar. Most probably it results from a loose use of *be* in the relevant passages. More promising as a characterisation of the relation between *Lo* and *Lm* in the theory of natural languages is a view according to which *Lo* contains *Lm*. This view, which was already attributed to Grelling above, is widely accepted by linguists (Jakobson 1981, 1985; Harris 1968, 17, 152, 1988, 34–35, 1991, 274–78; Greimas and Courtés 1979, 226; Droste 1983, *passim*, 1989a, 21, 1989b, 930), but does it stand up to scrutiny?

Before we can begin to answer this question, it must be pointed out that this view that $Lo \supset Lm$ potentially conceals two claims, one about the English metalanguage *stricto sensu*, the other about English metalinguistic sentences at large.

It seems trivially true that English contains the English metalanguage in the narrow sense. Consider examples (2) above and (3).

- (3) *With* is a preposition.

Both are unquestionably English sentences, and, using quotations, they make points about an English sentence and an English word, respectively. All the other words involved are themselves English. So, yes, $Lo \supset Lm$ is true on that conception of the English metalanguage. However, that conception is too narrow to be interesting, because the English metalanguage is a mere subset of English metalinguistic sentences. There are a great many such sentences that are about other languages, real or imagined, as in (4) and (5), about pseudo-words (6) or non-words (7), or even about no particular language at all, as with many statements about languages in general or about language (in the sense of ‘language faculty’), as in (8) and (9):

- (4) Did Galileo really say “**Eppur si muove**”?
 (5) “**Grildrig**” was the name the Brobdingnagians gave Gulliver.

- (6) This is because *wug* [wʌg] was one such pseudoword used by Jean Berko Gleason in her *wug* test 1958 experiments. (Wikipedia)
- (7) It is nonsense to say, ‘*Wbnjnmrtk.*’ (Droste 1983, 687)
- (8) Nouns are typically used to denote objects and people.
- (9) The V-features of an inflectional element disappear when they check V, the NP-features when they check NP. (Chomsky 1993, 30)

All of the above are English sentences, never mind the non-English elements occurring in (4) to (7). Hence they belong to the set of English metalinguistic sentences. And yet none of them talks about English (or English specifically).

This means that the two terms *Lo* and *Lm* do not suffice to characterise the relations (of inclusion or others) we are interested in. Sentences (4) to (9) are part of the natural language English. But English is not their object-language. A dissociation needs to be made between *Lo* and natural language. If we take *Lm* to be the set of English metalinguistic sentences, we see that it comprises English sentences that talk about (i) English, (ii) other (possibly imaginary, or even maybe impossible) languages, (iii) no particular language. Irrespective of whether *Lm* sentences are about (i), (ii), or (iii), they are all sentences of the natural language English. Hence our discussion cannot dispense with a third term, ‘natural language,’ or *Ln* for short.

2.1.1. Droste’s proposal

In the 1980s, the linguist Flip G. Droste made a proposal that built on these three terms. Unsurprisingly, his account looks upon *Lm* as a particular use of *Ln*. What of *Lo*? Just as I did above, Droste warns against equating *Lo* with *Ln*. Instead, he provides a definition of *Lo* via a set of elements of *Lm*, the set of proper names that refer to some linguistic entity (cf. the quotation-names in Section 1.1). Of this set of names, Droste says the following: “the proper names constitute a lexicon which is part of *Lm* or, rather, is essential to the creation of the special use *Lm*” (1983, 696). To which he adds: “And the items of this lexicon refer to entities which, taken together, define the set *Lo* exhaustively. The *Lo* element [e.g. the referent of ‘*Boston*’ in (1)] is an autonym of the *Ln* element but under no condition whatsoever should it be identified with it” (1983, 696–97).

Several remarks are in order. First, in order to dispel confusion, it must be made clear that Droste uses the term *autonym* differently from Carnap, who coined the term, and Rey-Debove, who wrote extensively about it. Whereas they both regard an autonym essentially as a quotation—Rey-Debove’s autonym is understood as a ‘metahomonym’ of its referent,⁸

Carnap's as a name for itself—Droste's autonym is the referent of that name. I will therefore not adopt Droste's confusing use of the term, and will stick to the phrase *Lo element* in the discussion below. Second, it is not obvious why Droste should warn against confusing Lo elements with Ln elements. Granted, not all the referents of quotations are elements of Ln. Quotations may also, as Droste points out, designate 'quasi-expressions' (cf examples (6) and (7) above). It is therefore correct to say that Lo cannot be the same thing as Ln. However, on Droste's account (and on anybody else's), Lo also contains "linguistic elements such as sentences, words, morphemes, etc." (1983, 697). When those are English sentences, words, morphemes, are they not Ln elements? No, says Droste: "*The Lo elements, finally, refer to Ln expressions or to things which are in the complement set of Ln (where Ln is defined as a set of sentences)*" (1983, 697; italics mine). One ends up with the following picture: quotation-names refer to Lo elements, (some of) which in turn refer to Ln elements (while the rest refer to elements from other languages, or to 'quasi-expressions'). Applied to examples (1) and (2) above, this means that the quotations in those sentences refer not to Ln but to Lo elements, say BOSTON and SNOW IS WHITE, which elements in turn refer to the English word *Boston* and the English sentence *Snow is white*.

The postulation of a double layer of reference appears unwarranted. For one thing, it is not economical. For another, it leads to something of a paradox: in using quotation, one does not in fact speak about what one could be expecting and expected to speak about: whenever a quoter intends to say something about an Ln element, and thinks she is, she is in fact talking about an Lo element, which, Droste insists, cannot be the same thing. Access to the intended referents is only gained indirectly, secondarily, as can be seen in the short analysis of (1) and (2) above.

2.2. An amended three-term account

Although Droste's account does not ultimately make the grade, I want to retain three of his crucial insights:

- (i) a third term is needed if one is to be able to define Lo and Lm in the context of natural languages;
- (ii) the so-called 'object-language' is not a language (cf. Droste 1983, 690), but a set of 'quotables' instead. The first claim is uncontroversial. The second—Lo is a set of quotables—is rather more so. As is shown in Section 2.2.2., this assumption results in a substantial mismatch between Lm and Lo;

- (iii) L_m is a particular use of L_n , which I interpret as equivalent to the claim that $L_m \subset L_n$. This claim faces some severe challenges at the lexical level: in particular, it is not evident that quotations, all of which have a reasonable claim to membership of L_m , can be said to belong to L_n too. I propose a novel solution to this problem, which allows me to conclude that L_m does form part of L_n .

The discussion will be broken down into three main sections, each adopting a different angle. In Section 2.2.1., I focus on $L_n \cap L_o$, and explain which elements fall inside or outside this intersection. In Section 2.2.2., I try to state the exact relations between L_o and L_m . Though that question may seem to call for a trivial answer—“ L_o is the set of referents for L_m expressions... by definition!”—things turn out to be more complex after all. Finally, in Section 2.2.3., I pick up all the previous threads and make a proposal compatible with maintaining the claim that $L_m \subset L_n$.

2.2.1. $L_n \cap L_o$

I have said above that I endorsed Droste’s view that L_o is the set of referents of the quotations that can occur in L_m sentences.⁹ I also accept the consequence that L_o is not a language, but a heterogeneous set of suitable referents. I do not, however, agree with Droste that the intersection between L_o and L_n is empty. I argue that $L_n \cap L_o$ contains all the sentences of L_n , and also the whole L_n lexicon, to be understood in a broad sense, as containing words, but also morphemes, and possibly phrases and constructions, depending on one’s theory of the lexicon. Evidence of this is provided by examples like (10) to (13), which quote an English sentence, English words, an English derivational suffix, and an English phrase, respectively.

- (10) “**This is yellow**” is a sentence epitomizing these three categories. (*The Corpus of Historical American English (COHA)*, Davies 2010-)
- (11) “**This**” represents the uncharacterized particular; “**yellow**,” the unparticularized character. (COHA)
- (12) *Vasectomy. It’s the -ectomy that puts me off.* (Barnes 2001, 109)
- (13) “**Control freak**” is the phrase often used to describe Messier. (*The Corpus of Contemporary American English*, Davies 2008-)

Perhaps more difficult to classify are arbitrary fragments of an L_n string. To give but a few examples (with $L_n = \text{English}$): sub-morphemic or non-morphemic strings like *-chine* (as in *machine*) or *-ervation* (as in

preservation, conservation, etc.) (cf. (14)); incomplete supra-morphemic units (*quiet and reserved is seen*), as in (15); strings that are judged unacceptable, as in (16):

- (14) Ever noticed that the **-chine** of *machine* is pronounced just like *sheen*?
- (15) In the sentence *In some cultures being quiet and reserved is seen as a sign of politeness*, the string **quiet and reserved is seen** is not a syntactic constituent.
- (16) It is incorrect to say, **'Him saw her'** (Droste 1983, 687)

Though (14) to (16) are Lm sentences containing quotations of strings made up of 'bits of Ln,' these strings are none the less not part of Ln, and therefore fall outside $Ln \cap Lo$. In the case of (15) and (16), this conclusion rests on the sensible assumption that the referents of *quiet and reserved is seen* and *Him saw her* are these strings taken as a whole, and not each word taken separately. One who adopted the latter view of quotation—Geach (1957) is a rare example—would say the referents of the quotations in (15) and (16) *do* fall within $Ln \cap Lo$.

There are many more elements of Lo that do not belong to Ln. A precise categorisation of these must be underpinned by a thorough discussion of what elements deserve the label 'linguistic.' Before I proceed to do just that, however, I will begin by introducing the various cases that I think should be distinguished:

- First, any morpheme, word, phrase, sentence, etc. of *another* natural language, actual or imaginary, is also part of Lo. In (17), the first quotation refers to a word in imaginary Brobdingnagian, the next two to Latin words.

- (17) She gave me the name of **Grildrig**, which the family took up, and afterwards the whole kingdom. The word imports what the Latins call **nanunculus**, the Italians **homunceletino**, and the English **mannikin**. (Swift 1960, 108)

- Second, $Lo \setminus Ln$ (i.e., Lo minus Ln) contains strings that comply with English phonotactic rules but are not attested in the productions of native speakers. Take *wug* in (6), or creations like *brillig*, *slithy*, *toves*, *gimble*, *outgrabe*, etc. from Lewis Carroll's *Jabberwocky*.
- Third, $Lo \setminus Ln$ contains any arbitrary sub- or supra-morphemic string from any natural language other than English—the non-English counterparts of the strings quoted in (14) to (16).

- Fourth, what might be called ‘non-words’ relative to English, such as *Wbnjnmrtk* in example (7) above. These are different from items like *wug* or *brillig* in the sense that they violate the phonotactic rules of English.
- Fifth, strings that are impossible in any natural language [if these can be devised and quoted].

The classification above depends on our ability to distinguish between what is linguistic, what is pseudo-linguistic, and what is non-linguistic.

2.2.1.1. ‘linguistic’

The term ‘linguistic’ should be defined independently of any particular language: a linguistic item is any string that exists in the lexicon or can be generated by any given grammar. Hence, the quotations of an Italian sentence in (4) and of Latin words in (17) indisputably refer to a linguistic Lo element. One might question whether strings in imaginary ‘languages,’ as are quoted in (5) and (17), also deserve to be called ‘linguistic.’ In the case of Brobdingnagian, the few words mentioned by Swift suggest that it works along very similar principles to English or other Indo-European languages. So, we have no trouble regarding *Grildrig* (or *Glumdalclitch*, *splacknuck*, *Lorbrulgrud*, or *relplum scalcath*) as linguistic entities. But one could imagine ‘communication systems’ that are so different that they do not self-evidently rate as ‘linguistic.’ A case in point is a version of (written) ‘Martian’ considered by Bennett, in which the symbols are not “arranged on a directional line” (Bennett 1988, 413). For example, vertical relations between (word-like) clusters of symbols might be relevant at the same time as horizontal arrangements are; or the order of symbols within a cluster might be a matter of aesthetics, with only the *number* of symbols being relevant to meaning. What Bennett is trying to do with this example is to warn the language scholar against parochialism: maybe languages are possible which have completely different organising principles from those that are known to us. Should we therefore dismiss these as not being languages? I certainly cannot answer this question here. But just the possibility of raising it shows that fixing the limits of what deserves to be called ‘linguistic’ is a tricky affair.¹⁰

2.2.1.2. ‘pseudo-linguistic’

Unlike ‘linguistic,’ ‘pseudo-linguistic’ can only be defined meaningfully relative to a particular language. For the purposes of this paper, the term

will be regarded as synonymous with *pseudo-English*. Psycholinguists call ‘pseudo-words’ those word-like strings that, although not recorded in dictionaries, comply with the phonotactic rules of the language spoken by the subjects of their experiments. As regards higher levels of grammatical organisation, pseudo-phrases, -clauses and -sentences should similarly comply with the morphological and syntactic rules of the language. On such a definition, Lewis Carroll’s *‘Twas brillig, and the slithy toves did gyre and gimble in the wabe* (from his *Jabberwocky*) is a pseudo-English sentence containing six pseudo-English words. By the same token, the poet Henri Michaux’s *Il l’emparouille et l’endosque contre terre; Il le rague et le roupète jusqu’à son drôle* (from his *Qui je fus*) is pseudo-French.

Quotations of pseudo-English sequences are not infrequent:

- (18) “Nothing much, to be honest. Nothing helpful. I was just wondering what “**Feminian**” means.”
 “...?”
 “I wonder if it’s a real geological term, or if Kipling just made it up.”
 (Barnes 1982, 119)
- (19) Beneath the window is a bilingual rubbish bin with a spelling mistake. The top line says PAPIERS (how official the French sounds: ‘Driving licence! Identity card!’ it seems to command). The English translation underneath reads LITTERS. What a difference a single consonant makes. (Barnes 1985, 82–83)

Neither *Feminian* nor *litters* are linguistic items in English: the first is not recorded in any of the major dictionaries I have consulted, and the second, at the present time, can only be used as a noncount noun.¹¹ However, given an expansion of the English lexicon and a not unusual change in grammatical status (a different exploitation of the grammatical potential), both pseudo-words would become actual linguistic items.

Note that it is not unreasonable to assume that all lexical and grammatical innovations begin as pseudo-elements, in the sense of “possible but not yet acceptable.” Some of them retain that status, while others end up being accepted as part of the relevant language. This dynamic dimension of lexicons suggests that the boundary between pseudo-English and actual English is fuzzy. One source of fuzziness is the fact that the object ‘English’ is no more than a useful theoretical fiction. It might perhaps be more accurate to write that, at a certain moment in time, some of what I have called ‘pseudo-English’ strings are genuine members or products of the lexicons or grammars of some idiolects, but still fall

outside too many such lexicons or grammars to be regarded as English in the standard collective sense of the term.

The intersection between pseudo-English (or pseudo-Chinese, etc.) and linguistic items need not be empty. That is because a pseudo-linguistic item with respect to language L_1 might exist as a linguistic item in language L_2 . In language contact, interference is rife: the creations of Belgian students of English (*constatate*, *factures*, *tenniswomen*) are pseudo-English strings with sometimes a linguistic existence elsewhere: *factures* and *tenniswomen* exist in the written system of French.

2.2.1.3. ‘non-linguistic’

What do I mean by ‘non-linguistic’ elements, and why do they matter at all in the current discussion? It is a fact that ‘just about anything’ can be quoted. In (20) and (21), strings of letters are used to quote noises and musical sounds, respectively. In (22), a transcription of a spoken (and gestured) example, some bodily motions (which are not to be confused with lexical items of a sign language) are used to ‘quote’ a particular feeling:

- (20) M pushes the penis on a gilt cherub which is flying up the mirror frame, and the whole edifice slides back with a great “**gzzhhd.**” (*New Statesman*, 20/12/99, 6)
- (21) **PA-PA-PA-PA PUM PUM PUM** went Sir Jack as Woodie, cap under arm, opened the limo door, “**Pum pa-pa-pa-pa pumm pumm pumm**. Recognize it, Woodie?” (Barnes 1999)
- (22) And I’m like [SIGHING AND ADOPTING FACIAL EXPRESSION DENOTING DISAPPOINTMENT].

The quoted elements are not linguistic entities, because none of them is part of a natural language. Here as elsewhere, it is important not to confuse the quotation (which in (20) and (21) resorts to linguistic means—letters, syllables) with the quoted entities. Now, is the quotability of the quoted elements reason enough to include them within Lo ? The answer can only be a yes. That is because I have decided to take on board Droste’s idea that Lo is a set of quotables. Restricting this set to just linguistic quotables would leave us with the question of what to do with the other quotables. Should they be included in a different set, one that would introduce a fourth term into the present discussion? This seems unnecessary, as, on the current picture, non-linguistic and linguistic quotables are already clearly distinguished: while the latter belong to $Ln_i \cap Lo$ (the intersections of Lo with some natural language), the former do not. Besides, as we saw above,

Lo is not a language. So there should be no problem with it also encompassing sounds, noises and body language. (I return to the consequences that this has on the Lm-Lo relation in Section 2.2.2.)

Just as tricky are occurrences such as *Him saw her* in example (16) or Zellig Harris's example of a meaningless string *slept the a the* (1991, 31). They are neither English nor pseudo-English, since they do not comply with the syntactic rules of English. Yet they appear less non-linguistic than the noises, sounds or body language quoted in (20) and (21). Likewise with *Wbnjnmrtk* in example (7). As initially intended by Droste, *Wbnjnmrtk* is not to be read as a transcription of only the consonants of a spoken expression that would also contain vowels, as occurs for example in Arabic script. *Wbnjnmrtk* is to be understood as complete. As such, it seems to defy the phonotactic rules of any natural language: is *Wbnjnmrtk*, for all that, a non-linguistic object? There seems to be no definitive answer to this question. Bennett's warning against theoretical parochialism (1988, 413–14) might suggest that a fourth category is needed, between pseudo-English and non-linguistic, that of 'pseudo-linguistic items with respect to any language.' The rationale would be that there is no theoretical restriction against these strings occurring in Martian, so maybe they are not genuinely non-linguistic. This, however, might put us on a slippery slope to acceptance of just anything as being pseudo-linguistic in this extended sense, since nothing proves that other similar noises, sounds, facial expressions, etc. could not be meaningful parts of a yet-to-be-discovered language. I will refrain from going this way here, but, as my reader can see, this is not a decision that is (or can be) grounded in empirical evidence.

2.2.1.4. *Intermediate recap*

All in all, Lo is a peculiar collection of objects whose core is a linguistic component but whose periphery also includes a variety of pseudo-linguistic and non-linguistic entities. The contents of Lo raise questions to do with the relations between Lm and Lo and with the question whether Lm can be said to be included within Ln. I turn to these issues in the next two sections.

2.2.2. Relations between Lm and Lo

We saw that, in the theory of formalised languages, there were two possible relations between Lm and Lo. Either Lm contains Lo as a part, or Lm contains (exact) translations of all the formulas of Lo. The situation is

radically different when we turn to natural Lm and Lo. To show this, I will temporarily assume the widespread view that quotations are part of the metalexicon, i.e. the set of metalinguistic lexical items, of the quoting language. In Section 2.2.3., I will argue that such a conception is inadequate and must give way to one on which quotations are not part of the metalexicon.

First, on a strict interpretation of the term *metalexicon*, only those quotations that denote genuinely linguistic elements of Lo should be considered for membership. The others, namely quotations of pseudo-linguistic¹² and non-linguistic objects, could not, strictly speaking, be members of a *metalinguistic* lexicon. As a result, many Lo elements have no counterpart in Lm. This is a far cry from the interdependence between Lm and Lo that was found in the theory of formalised languages.

Second, as a consequence of the first point, those sentences that contain quotations of non-linguistic objects are no more part of Lm than are the quotations themselves.

Third, not all elements of the metalexicon have a referent in Lo. Whereas those quotations that are part of the metalexicon refer to an Lo element, metalinguistic common nouns like *preposition* or *clause*, by contrast, do not.

Fourth, turning now to sentences, it can be observed that not all metalinguistic sentences are about an object-language. Obvious counterexamples are provided by statements about languages in general or about language, as in examples (8) and (9).¹³ Does that mean that the determining factor is the distinction between generic and specific sentences? Well, it appears that specificity is a necessary condition for a metalinguistic sentence to be object-language-oriented. However, it is unclear that it is a sufficient condition. Witness an example like (23), which is specific, because it makes a point about a particular linguistic entity (say the root *quote*), but does not quote an element of Lo.

- (23) That root can combine with several derivational morphemes to form new words.

On the definition of Lo adopted above, (23) cannot be object-language-oriented, because it does not contain a quotation. Note, however, that the definition could be amended to include referents of ‘heteronymous’ cases of mention, namely mention by means of an NP that is not iconically related to its referent (see Recanati 2000, 137; De Brabanter 2010a, 378). If that was done, then (23) would be object-language-oriented after all.

Interestingly, object-language-orientation is independent of whether the metalinguistic sentence makes a genuinely linguistic point or not. Compare the next two sentences:

(24) ‘**Chicago**’ rhymes with ‘**cargo**.’

(25) I really love ‘**Chicago**.’

(24) makes a phonological point. (25) merely expresses a personal preference of the speaker’s for a particular word. Yet, since they are both (amongst other things) about an element of the object-language, the name *Chicago*, they are both equally object-language-oriented.

Fifth and last, considered as sets of elements, L_o and L_m show no overlap, provided only the first meta-level is taken into consideration. (See Section 2.3. for more about levels).

2.2.3. $L_m \subset L_n$?

I have assumed above that a language was made up of a lexicon and a set of formation rules generating sentences of that language. I take it that the claim that $L_m \subset L_n$ entails both that the metalexicon is included in the L_n lexicon, and that the set of L_m sentences is a subset of the set of L_n sentences. The second part of the claim is unproblematic. We saw above that metalinguistic sentences like (4), (5), (6) that contain quotations of non-English linguistic material are unquestionably English sentences. We need hardly have any doubt that L_n includes all L_m sentences, since L_n also readily admits apparently more demanding cases like sentences (7) or (20), (21), (22), which contain quotations of pseudo- or even non-linguistic objects.

Where issues arise is in connection with the metalexicon. Lepore (1999) sees problems with any attempt to enumerate the class of quotable items of English. If quotations somehow contain the quotables they refer to, then those problems extend to quotations too. Now there *may be* theories of quotation that do postulate that quotations are, for instance, names or descriptions which are built from their referents—the quoted entities—together with other elements, typically quotation marks. Both Tarski (1983, 156) and Quine (1940, 23) view their quotation-names as denoting the very expressions enclosed in quotation marks. It must be noted, however, that they also regard their quotation-names as atomic expressions (single morphemes, to use a linguist’s term). As such, quotation-names cannot properly contain expressions. So there is an inconsistency in their theories, one which is rarely pointed out. But let us, for the sake of argument, stick with Lepore’s reading of these theories for

a moment. If it turns out that there is an infinite number of quotables, then there must be an infinite number of quotations too. And if quotations are part of the metalexicon—hence also of the lexicon—of a natural language, then that lexicon is itself infinite. Most theories of language agree that languages are infinite sets of sentences built from a *finite* lexicon using a finite number of formation (i.e. morphological and syntactic) rules. Would an infinite number of quotations jeopardise this standard picture? There are several possible answers to this question.¹⁴

The first consists in denying that there is an infinite number of quotations. However, we saw above that L_0 is a heterogeneous set of things, linguistic or not, to which there are no obvious limits. Note that the set of quotations must be assumed to be infinitely large even if the non-linguistic component of L_0 is dismissed as irrelevant and only the linguistic component is considered: given that *sentences* can be quoted or mentioned, and given that there is an infinite set of sentences in every L_n , the number of quotable linguistic objects is infinite, and so is the number of quotations.

The second response consists in denying that quotations are part of the metalexicon, and hence of the lexicon of L_n . Quotations, on such a picture, are part of a ‘lexicon’ that is not language-specific. Several writers (e.g. Rey-Debove 1978, 138–39; Wreen 1989, 366) have made suggestions of this sort regarding proper names, arguing that they are cross-linguistic items that lie outside the linguistic competence of speakers of one specific language. It is easy to see how this could be extended to quotations: just as, say, the name *Fidel Castro* does not belong to any particular language, the quotations ‘*Fidel Castro*’ or ‘*Eppur si muove*’ do not either. Quotations, then, would belong to a sort of pan-linguistic reservoir that vastly exceeds the specific L_n lexicon: they would be every bit as much part of French, Swahili and Chinese as they are of English. The question is whether we are ready to say that all the languages of the world share a substantial part of their lexicons.

Rey-Debove defends a moderate variant of this position, arguing that the metalexicon of French contains only those quotations that are metahomonyms of French words, i.e. a finite number (Rey-Debove 1978, 29; she does not say what to do with quotations of French morphemes, phrases, sentences). Quotations that do not refer to French words fall outside the French metalexicon. This way, Rey-Debove avoids any runaway inflation of the French lexicon. Although this allows her to maintain that $L_m \subset L_n$, it seems a rather arbitrary criterion.

A third answer consists in saying that an infinite lexicon is only an issue if it cannot be specified recursively. Mark Richard (1986) undertakes

to show that the quotational component of the metalexicon can be generated recursively using a finite set of primitives (letters, punctuation signs, spaces, mathematical symbols, and a few more elements). These items provide the input to two rules of concatenation that output quotations that are understood as names of their linguistic referents:

- (A) For any expression e , lq followed by e followed by rq is a term.
 - (B) For any expression e , lq followed by e followed by rq denotes e .
- (Richard 1986, 398)

In these formulas, e stands for any combination of primitive items, lq is the name for the left quote mark, and rq for the right quote mark. Note that (A) and (B) seem to define two notions of specificity, one for quotations as forms, the other for quotations as lexemes. If Richard is right, that is, if his two rules are sufficient to ensure the specification of the whole class of, say, English quotations, then the infinite numbers postulated by his type of nominal theory of quotation eventually turn out to be no obstacle to giving a linguistic description of languages that possess quotations-as-names.

Yet, there is at least one writer who finds fault with Richard's solution. Ernest Lepore (1999) argues that Richard's attempt to finitely generate the infinite set of quotations fails. However, says Lepore, there is a way out of this serious difficulty. All that takes is to recognise that whatever objects are quoted do *not* belong to the syntactic and semantic structure of the sentence containing the quotation. Instead, only a demonstrative pronoun, contributed by the quotation marks, occurs in the sentence. As for the quoted material, it is located in the context of utterance, not in the sentence. This, in essence, is the theory of quotation advocated by Donald Davidson (1979).

The reason why Richard's attempt is not successful is, says Lepore, because of the unboundedness of quotability, which cannot be systematically predicted by 'alphabets,' however numerous (e.g. Greek, Cyrillic, Zapf Dingbats, etc.). Consider the following example:

- (26) Gillian marks up the newspaper every morning. She has a red pen and puts ☆s by stories she thinks I might find interesting or amusing.
- (Barnes 2001, 187)

One could nevertheless retort that more items could still be added to the set of primitive terms without it becoming infinite. But Lepore remarks that any squiggle, whatever its shape—and I would add any sound, noise, gesture—is quotable, suggesting that no finite number of primitives will

ever suffice to generate all of them. Hence, the impossibility of specifying the lexicon with finite means.

Lepore is certainly right that Richard's proposed solution would not succeed in recursively specifying all the quotations that can occur in English sentences. However, this limitation may appear to be only tangential to our current concerns: the reason why recursive specificity is jeopardised is because there can be quotations of 'just any squiggle' (or noise, sound, etc.). If, on the other hand, we are concerned with only the (quotational component of the) metalexicon proper, i.e. with quotations that mention linguistic objects, it is reasonable to assume that recursive specificity can be achieved along the lines proposed by Richard, at least if one also identifies a set of primitives that can handle spoken quotations.

Should that satisfy us? The answer is that it should not. Underlying Richard's proposal is a theory of quotation along Tarskian lines, with quotations understood as names, which proves unable to account for a large subset of quotations. Those are the quotations that (i) syntactically do not occur as complements of a head, and (ii) semantically do not function as singular terms. Recanati (2001) has called them 'open.' (Some examples are given in Section 2.4.3., under the heading 'hybrid.')

The theory endorsed by Richard is, therefore, empirically inadequate, and preference must be given to a type of approach that can be extended fruitfully to all quotations, whether they have a referent or not. 'Depiction Theories' (see De Brabanter 2017) satisfy this requirement. Building upon the crucial insights of Clark and Gerrig (1990) and Recanati (2001), these theories view quotation as essentially *not* a linguistic phenomenon. Understanding a quotation, as opposed to an ordinary word, phrase or sentence, is not a matter of knowing a convention between a form and a meaning. Using popular Peircean terminology, quotations can be described as 'iconic' communicative acts. In other words, they belong to a mode of communication that is essentially different from that of natural languages (which are essentially 'symbolic').

Now, in spite of their essentially non-linguistic nature, quotations can, under certain conditions, function as constituents of a sentence. When that happens the iconic act is recruited (Recanati's term) as a subject, a direct object, a modifier, etc., and the quotation is said to be 'closed.' Closed quotations have this property that the token enclosed in quotation marks is semantically and syntactically 'inert,' that is, its internal structure—if it has one—remains segregated from that of the quoting sentence (cf. Davidson 1979, 37; Recanati 2001, 651).¹⁵ As a consequence, individual quotations, say '*Chicago*' as distinct from '*cargo*,' are not part of the

sentences they occur in, and there is therefore no justification for including them in the metalexicon.

On the Depiction account advocated by Recanati, recruitment can be understood as a function that turns any iconic input into an element of Lm (and therefore of Ln). This operation has no inflationary consequences, because what gets added to the lexicon (or, perhaps more broadly, to the language system) is not each quotation taken separately, but the ‘recruitment function,’ to be broken down into a very limited set of sub-functions that output NPs, Ns, AdjPs (or modifiers), and Vs, as illustrated below:

- (24) [repeated] as NP: ‘Chicago’ rhymes with ‘cargo.’
- (27) as N: All the “*Thou shalt not’s*” of the Bible (Jespersen 1946, 73)
- (28) as AdjP: quite the ‘I’ve got to be organized, get 100%, be the best’ person (Pascual 2014, 62)
- (29) as V: — No pal you are the one lying. Saving the planet?! Don’t talk such utter nonsense [*sic*].
— Firstly Don’t **pal** me, i’m not and never will be your pal [...].
(<http://www.didcot.com/forum/?read=18084>; an exchange on an internet forum)

Each individual quotation, *qua* non-linguistic communicative act, remains outside the metalexicon. Only the small set of sub-functions enriches Ln. This way, any worries about an inflationary lexicon dissolve.

The proposal made here also has a couple of attractive consequences. To begin with, remember that I said at the beginning of this section that Lm sentences containing quotations of strings from other languages, and sentences with quotations of non-linguistic objects are none the less English sentences. Though I regarded (and regard) this as unquestionable, because in accordance with native speakers’ intuitions, my initial claim begged the question *why* these were English sentences. If, at some level, quotations are part of the semantic and syntactic structure of the quoting sentence, then the Englishness of sentences (7) or (20), (21), (22) is unexpected. On the picture just proposed, however, there can be no puzzlement. The quotations themselves are not in the quoting sentences: only the output of a function that turns them into English constituents is. The outcome is sentences that contain nothing but English lexical items, making it no wonder that they are judged to be English.

Another potentially attractive consequence of my proposal is its impact on a point made in 2.2.2. There it was said that quotations of non-linguistic elements (and, consequently, sentences containing such quotations) fell outside Lm, resulting in a mismatch between Lm and Lo

elements, and in an awkward split between at least two sets of quotations. On the present proposal, the distinction between quotations of linguistic, pseudo-linguistic and non-linguistic material is made irrelevant at the lexical level: there are no quotations as such in the metalexicon.

At sentence level, the situation is different. The recruitment function, though it belongs to L_n , is most likely not a metalinguistic operator, precisely because it accepts ‘just about anything’ as input. As a result, its presence in a sentence (or the presence of its output) does not suffice to make that sentence metalinguistic. Does this mean that we have lost the ability to distinguish between those quoting sentences that are metalinguistic and those that are not? No, one can still use a criterion based on whether the referent of the recruited quotation (the L_o element) is linguistic or not. The difficult cases, on this proposal, will be the same as on the other proposals, notably sentences that quote pseudo-English (pseudo-linguistic) objects, and sentences that contain quotations of sub- or supra-morphemic units (and no other quotations and/or meta-words), as in (30), however unlikely they may appear:

(30) I really like *-ervation*.

The present proposal certainly does not solve all the outstanding issues, but it does not generate any significant new issues. I conclude that it is preferable to the other existing accounts, for the reasons just given and, just as importantly, because the theory of quotation that underlies it is the only one with a chance to account satisfactorily for the empirical facts of quotation (cf. De Brabanter 2017).

Whatever the issues that remain unresolved, we are now in a position to provide a positive answer to the question posed at the beginning of this section: $L_m \subset L_n$! This conclusion, however, rests on grounds that are very different from those offered by previous accounts. Here, essentially, what has happened is that L_m has been radically slimmed down. As a result, all the elements whose membership of L_n raised issues have been placed outside the metalexicon, not as part of an ad hoc manoeuvre, but based on a correct apprehension of the nature of quotation.

To conclude, Section 2.2. has brought us a lot closer to elucidating the relations between L_n , L_m and L_o . But have these relations been clarified enough that they can now be represented in a diagram? The answer is no. There is one task that must be accomplished before that can be done: we need to get clear about language levels.

2.3. Language levels

A neat hierarchy of language-levels is not a requisite in the case of natural languages. For one thing, there is no need to avoid semantic paradoxes. It is an aspect of the versatility of ordinary languages that they are able to generate ambiguous or contradictory utterances of any kind.

In the theory of formal languages, we saw that it was always possible, should the logician wish to formalise a metalanguage of level n , to construct an $n+1$ language for the description of the semantics (and/or the syntax) of that L_0 . Thus, an L_{n+1} is used to describe an L_n . In the theory of natural languages, the situation, as expected, is messier. Still, something like a hierarchy of languages can be made out in connection with certain aspects of natural language. A ready illustration of this is the fact that quotation is iterable, a property recognised by several writers (Rey-Debove 1978, 42–45, 114; Saka 1998, 119–20). Droste (1989b, 931; also 1989a, 20) establishes this iterability by means of the following pair of examples:

- (31) *John* is a proper name.
 (32) '*John is a proper name*' is a correct English sentence.

Whereas (31) is a sentence of the first-order metalanguage L_{m_1} —it can be uttered to state something about the personal name *John*—, (32) is a sentence of the second-order metalanguage L_{m_2} —it can be uttered to state something about the metalinguistic object '*John is a proper name*'. This could be continued ad infinitum.

The iterability of quotation is not the only sign of a hierarchy of languages in L_n . Another one can be found among meta-words (Rey-Debove 1978, 44). Most meta-words (*say, noun, verb*) belong to the first-order metalanguage L_{m_1} . A few meta-words are words of L_{m_2} , such as *autonym, metalanguage, metalinguistic, or quotation* (in a narrow ordinary sense which I reject here). These words designate elements of L_{m_1} . Meta-words above that level are few and far between, but *meta-metalanguage* would be part of L_{m_3} .

2.3.1. A diagram of the relations between L_n , L_m and L_0

Now that the applicability of language levels to natural languages has been clarified, we can finally draw a diagram of the relations between L_n , L_m and L_0 , provided we specify a particular L_n and fix a level for L_m and L_0 . In Figure 1-1, L_m is set at level 1 (first-order metalanguage), and L_0 at level 0. It is important to realise that a different diagram would be needed

for any pair of Lm_i and Lo_{i-1} higher in the hierarchy. First-order metalinguistic sentences like (4), (5) or (6), which occur in $L_n \cap L_m$ in Figure 1-1 would ‘move’ to $L_n \cap L_o$ in a diagram representing the relations between L_n , L_m and L_o . The same would be true of all the quotations occurring in the examples above that refer to linguistic objects. That is because a meta-metalanguage would take metalinguistic sentences, meta-words, and (linguistic) quotations as its objects, as in (33) and (34):

- (5) **“Grildrig”** was the name the Brobdingnagians gave Gulliver. [repeated]
- (8) Nouns are typically used to denote objects and people. [repeated]
- (33) In sentence (5), **“Grildrig”** is a quotation of the name *Grildrig*.
- (34) You find the following statement in many grammar books: **“Nouns are typically used to denote objects and people.”**

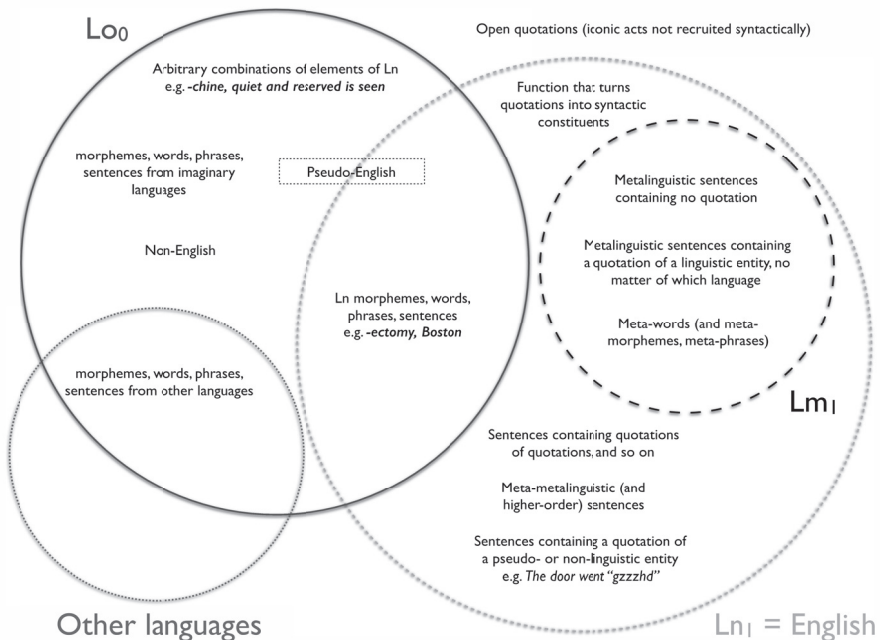


Fig. 1-1. Relations between L_n , L_m and $L_{o_{i-1}}$, for $i = 1$

Therefore, in Figure 1-1, sentences like (33) and (34) are placed not in L_m , but in L_n , since they are incontrovertibly English sentences. Two more

remarks concerning Figure 1-1: The sentences and quotations that refer to an element of Lo_0 that is not linguistic,—which cannot properly be considered metalinguistic—occur in $Ln \setminus Lm$. Pseudo-English—which, as explained in 2.2.1.2. and 2.2.1.3., is different from non-English—here straddles the border between Ln and $Lo \setminus Ln$, reflecting difficulties involved in locating it with any confidence.

2.4. Problems Even With the Three Terms

Sections 2.2. and 2.3. have brought some order to the relations between Ln , Lm and Lo . This section, in contrast, will have a mainly disruptive thrust, as it will point to major complications that upset the relatively tidy picture arrived at at the end of Section 2.3.

There would be no need for these disruptions if the metalinguistic character of sentences and lexical items was an all-or-nothing affair. But it is not. Not all metalinguistic words are maximally metalinguistic; there exist different degrees on a scale of ‘metalinguistic density,’ to use Rey-Debove’s term (1978, 31). In the following sections, I look at several factors that affect this metalinguistic density. In Section 2.4.1., I consider the varying density of lexical items, and in Section 2.4.2., that of metalinguistic sentences. Finally, in Section 2.4.3., I turn to so-called ‘hybrid’ cases (see De Brabanter 2010b), which combine quoting with ordinary use of language.

2.4.1. The metalexicon

The metalexicon was briefly introduced in Section 2.2.2. In what, to this day, remains the most detailed description available, Rey-Debove begins by splitting the Ln lexicon into three main subsets: the first is that of ‘mundane’ words, i.e. items which never signify anything linguistic (e.g. *water*, *whitewashed*, *to vacuum*); the second that of ‘neutral’ words, i.e. items which may or may not signify language, depending on the context in which they occur (e.g. *bound*, *to raise*, *position*, but also *that*, *in*, *it*, *long*, *have*); the third is the metalexicon (Rey-Debove 1978, 26–27). The metalexicon includes two main categories of words: those metalinguistic items that are lexicalised in Ln , like *word* or *say* (‘metalinguistic words stricto sensu’); and those that are created in context, namely quotations. For convenience, I will call the former ‘meta-words,’ and will have little to say on quotations, given that I excluded them from the metalexicon in Section 2.2.3. Meta-words display varying degrees of metalinguistic density, which can be illustrated by subjecting

them, if only for the sake of argument, to a componential analysis. Any word that contains a [+ language] component is a candidate for membership of the metalexicon. However, the degree to which it is metalinguistic will depend upon the position (degree of inclusion) of [+ language] in its definition (Rey-Debove 1978, 31):

parole: simple element of **language** that makes communication possible
parler: to utter simple elements of **language** which make communication possible
parleur: one who utters simple elements of **language** which make communication possible.

The further left the meaning component [+ language] occurs, the more metalinguistic the lexeme. The further right, the less metalinguistic. On this basis, it can be established that *parole* is more metalinguistic than *parler*, which in turn is more metalinguistic than *parleur*. This way, some general trends can be revealed, notably the fact that meta-nouns (such as names of linguistic units: *verb*, *preposition*, *subordinate clause*, etc.) are more metalinguistically dense than meta-verbs. One, however, quickly encounters borderline cases with items such as *parloir* ('parlour'). Is it still metalinguistic?

Two alternatives are envisaged by Rey-Debove. Either the presence of [+ language] somewhere in the definition is enough to classify the word as metalinguistic. This way, there is a cut-off point between meta-words and others, with the meta-words themselves placed along a continuum of density from 'highly metalinguistic' to 'barely metalinguistic.' Or the component [+ language] is not a sufficient condition for metalinguisticity, and some (arbitrary) decision has to be made regarding the degree of inclusion of [+ language] beyond which the lexeme is no longer to be classified as metalinguistic. As far as I can see, Rey-Debove wavers between these two possibilities, neither of which is satisfactory. For convenience, I shall opt for the first one.

In introducing the first source of fuzziness, I have tacitly relied on a fiction according to which every word is monosemous. This helped me to reveal the scale of metalinguistic density (cf. also Rey-Debove 1978, 27). Had polysemy been taken into account from the outset, that scale would not have emerged clearly: *parler*, for instance, has several senses. In the sense 'utter simple elements of language,' it is highly metalinguistic, at least for a verb. But in the sense 'appeal to, attract,' as in *Ce tableau ne me parle pas*, it is hardly metalinguistic at all. Polysemy, therefore, constitutes a second cause of fuzziness between the metalinguistic and mundane poles: it is not as if the lexicon were simply divided into lexemes whose

meaning contains the component [+ language], at whatever level of inclusion, and lexemes that do not at any level. There is a set of polysemous lexemes that include one or more metalinguistic senses, but also one or more that have little or nothing to do with language, for example *proposition*, *coordination*, *passive*, *singular*, *transform*, *generate*. It is unclear where Rey-Debove's 'neutral' sublexicon fits into this picture: is *position* a monosemous word that is compatible with a metalinguistic reading in the appropriate context of use, or is it a polysemous lexeme only one of whose senses is metalinguistic? I am not going to answer this question in this paper. Instead, I shall assume that *senses* of lexemes, rather than lexemes themselves, are metalinguistic (or not).

2.4.2. Metalinguistic sentences

Sentences too vary in the degree to which they are metalinguistic, and we shall see that this variability stems from several sources. But before that, it is necessary to examine how the logically prior differentiation between metalinguistic and non-metalinguistic sentences can be founded. What few sources there are in the literature suggest that such a distinction may be based on two kinds of criteria, pragmatic or formal. Droste has defended the former approach, Rey-Debove the latter.

2.4.2.1. Droste on metalinguisticity

Droste defines metalinguisticity in pragmatic terms. There are no purely linguistic items whose occurrence can, of itself, give rise to a metalinguistic utterance. Meta-words do not as much as come up in Droste's discussion: if a sentence includes meta-words but no quotation (examples (8) and (9) above), it does not rate as metalinguistic. Although this means that Droste (1983, 682) makes the presence of quotation a necessary condition for a sentence to be metalinguistic, that presence is not a sufficient condition. This can be illustrated with these three examples of Droste's (1983, 683):

- (35) 'Your witness,' he said.
- (36) He said, 'Your NitWess.'
- (37) He did not say, 'Your witness,' he said, 'Your NitWess.'

Whereas Rey-Debove, for example, would consider all three metalinguistic, Droste judges that only (37) is clearly so, because "[i]t is the exterior aspect of language, its phonetic structure, which is being discussed" (1983, 683). Though (36) looks similar to (37), Droste hesitates

as to how it should be classified: apparently, the sentence leaves open the possibility that the quotation is not intended as a comment on somebody's pronunciation, but merely, say, as a verbatim rendition, in which case it shouldn't be classified as metalinguistic. Finally, though Droste says nothing explicit about (35), we may surmise that he does not regard it as metalinguistic: elsewhere, he classifies the sentence *Say 'Daddy,'* as uttered by a mother to her very young child, as 'natural' instead of metalinguistic. For Droste, therefore, not every utterance about language is part of the metalanguage; only those involving a quotation and a direct and conscious comment on (the use of) a particular language should properly be termed 'metalinguistic.'

As a consequence, some utterance-tokens of a given sentence may be metalinguistic whereas others are not. So, for instance:

(38) He said, '**Au revoir**'

will rate as metalinguistic if the quotation "refers only to the spoken sound chain," because it was the speaker's intention to "communicate a funny-sounding thing, a kind of greeting he is not familiar with." But it will not if the intention was to "refer to the greeting itself, i.e. to the message implied in the quotation" (1983, 694).

There is no basic problem with founding the metalinguistic vs non-metalinguistic divide on the pragmatic criterion of the speaker's intentions. But, since Droste does endorse the idea of a continuum, how can intentions account for various *degrees* of metalinguisticity?

More and more one wonders whether the parameters 'natural' and 'metalinguistic' are mutually exclusive; here, as with 'grammatical' and 'ungrammatical,' we may have to accept certain degrees of use and 'more or less' standards. (1983, 683)

The polar extremes can easily be told apart: the speaker either wants to qualify an aspect of a natural language or does not. In the first case, the utterance is metalinguistic, in the second it is not. But what of the intermediate degrees? Can you have 'half a mind' to make a comment on (the use of) a language? Droste keeps mum about this, but perhaps what he is thinking of are cases where speakers produce utterances with several simultaneous intentions in mind. Puns might be a case in point. Because they are first and foremost meant to be humorous, puns would be less metalinguistic than examples (31), (32), (37), which are unambiguously and solely about language. I am not sure this is a fair reflection of Droste's position, but it is the only way in which I can make sense of his

“hypothesis that there is a hierarchy of intermediate levels between metalinguistic and natural use” (1983, 685).

2.4.2.2. *Rey-Debove on metalinguisticity*

Rey-Debove’s starting point is different. She takes it that, if a sentence contains a meta-word and/or a quotation, it is to some degree metalinguistic. This position defines a formal criterion for the ‘meta’ vs ‘non-meta’ opposition. Two factors are going to influence judgments of metalinguistic density: (i) if the sentence contains only (a) meta-word(s), the very density of the term(s) in question will be of paramount importance; (ii) in all cases, the grammatical position and function of the meta-word(s) and or quotation(s) is also going to prove significant. In the rest of this section, I set out Rey-Debove’s views, but keep in mind that on the proposal defended here, only quotations of linguistic material make a quoting sentence metalinguistic.

Factor (i) must be dealt with in reference with the continuum identified in the lexicon. Consider the next four sentences:

- (39) Those words are scary.
- (40) That book is scary.
- (41) That writer is scary.
- (42) Your brother is scary.

Applying the criterion of the level of inclusion of the component [+ language] in the definition of the heads of the Subject-NP in these four examples, we find that *words* is more metalinguistically dense than *book*, which in turn is denser than *writer*. As for *brother*, it is a mundane word, displaying no degree of metalinguisticity whatsoever. As a consequence, (42) is not a metalinguistic sentence. By contrast, all of (39) to (41) are (to some degree) metalinguistic, and can be arranged on a scale from most metalinguistic (39) to least (41).

As suggested above, there is a cut-off point between metalinguistic and mundane sentences. However, one may find that the demarcation is not set in the right place, and that a sentence like (41) does not deserve the label ‘metalinguistic’: it is easy to imagine a world in which *That writer* in (41) and *Your brother* in (42) refer to the same person, so why should the former sentence be regarded as (somewhat) metalinguistic, whereas the latter is not? About example (43), Rey-Debove writes that the fact that the meaning of the word *book* includes the component [+ language] does not obviously turn it into a metalinguistic sentence.

- (43) She was dusting the books with a feather duster. (Rey-Debove 1978, 165)

However, ruling that (43) is not a metalinguistic sentence requires an arbitrary decision on a threshold of metalinguisticity. We are back with the second one of the alternatives introduced in Section 2.4.1.

Factor (ii) is examined at some length by Rey-Debove (ibid.: 165–70). To avoid further complications, I investigate metalinguisticity judgements as they apply to clauses rather than sentences. Indeed, within a complex sentence that is not itself ‘about’ language may be embedded an unequivocally metalinguistic clause, as in *I don’t like his matiness, if ‘matiness’ is the right word.*

Let us now consider three basic situations in simple NP-VP structures. The examples are inspired by Rey-Debove’s:

- (44) A linguistic sign has a meaning / *Never* has a meaning.
 (45) Language changes over time / *Smashing* is old-fashioned.
 (46) Albert is talking.

In (44), both the subject and the predicate are highly metalinguistic.¹⁶ In (45), only the subjects are, while the predicates are neutral—they can also be applied to a non-metalinguistic subject, as in *Woollen caps are old-fashioned.* In (46), only the predicate is metalinguistic. Rey-Debove suggests that sentences like (44) are more metalinguistically dense than the other two sorts. But more importantly, perhaps, she stresses that the universe of discourse of the grammatical subject is a decisive factor in determining degree of metalinguisticity. Both (44) and (45) are about language in a way that (46) is not.

This last point has interesting implications for the following pairs of sentences (see Rey-Debove 1978, 167):

- (47) A gardener wrote this poem.
 (48) This poem was written by a gardener
 (49) Certain kinds of seats are called *armchairs*
 (50) *Armchair* designates certain kinds of seats.

Although (47) and (48) are semantically equivalent, in the sense that they entail each other, they are not metalinguistically dense to the same extent, with the second member of the pair being denser as a consequence of the metalinguistic NP being in subject position. By the same token, (50), whose subject is a linguistic quotation, is more metalinguistic than (49), which is primarily about items of furniture.

2.4.3. Hybrids

To my knowledge, Rey-Debove is the only author to have considered the impact of hybrid cases of quotation on metalinguisticity.¹⁷ What I call ‘hybrids’ are strings of words which are both used ordinarily and quoted at the same time. They form a subset of Recanati’s ‘open’ quotations. Thus, in (51), the phrase *telescope of the mind* makes its normal contribution to the syntax and the semantics of the sentence, but it is also quoted.

- (51) Done properly, computer simulation represents a kind of **‘telescope for the mind.’**
(www.nytimes.com/2008/10/01/opinion/01buchanan.html)

In a case like (51), Rey-Debove talks of “weak metalinguistic density” (1978, 254), because the quoted segment also has a mundane employment: it denotes both language and ‘the world.’

Whereas (51) would typically be described as involving an instance of ‘scare quoting,’ an example like (52) contains what Cappelen and Lepore (1997) have called ‘mixed quotation,’ i.e. a string of words that occurs within an indirect report but is quoted at the same time. In mixed quotation, a modicum of metalinguistic density is always contributed by a reporting verb. Therefore, *ceteris paribus*, sentences containing mixed quotation tend to be slightly more metalinguistic than those containing a scare quote, as the latter are often the only metalinguistic ingredient of the sentence in which they occur.

- (52) Alice said that life **“is difficult to understand.”**

Rey-Debove relies on a semantic criterion to rule that (52) is less metalinguistic than (53): in the former case the quoted string is not solely ‘about language,’ whereas in the latter it is.

- (53) Alice said, **“Life is difficult to understand.”**

However, as was already observed in the case of (41) for example, the application of her criteria does not necessarily result in satisfactory classifications. In the case at hand, it is not obvious (though not inconceivable either) that (52) would be judged less metalinguistic than (53). After all, both sentences are chiefly about a person and something that person said.

2.4.4. Bringing the various criteria together

I can see no way of offering a satisfactory assessment of the relative metalinguistic impact of the various factors identified by Rey-Debove, because (i) none of them has a clear-cut application, and (ii) several of these factors are likely to crop up at the same time. For instance, what is one to do with the following pair of sentences?

- (54) She told everyone that Lucy had gone over the top
(55) The ‘**gunman**’ could not remember the name of his target.

Neither is essentially metalinguistic; density is low. But they both display at least a minimal degree of metalinguisticity, (54) because its main verb is a meta-word (the reporting verb *told*) and licenses indirect discourse; (55) because of scare quoting and a metalinguistic direct object (*name*). Yet, when all is said and done, there is no way that these two sentences can be ranked along a scale of metalinguistic density. The best that can be said is that some basic patterns can be distinguished, where metalinguisticity judgments are fairly reliable. This is the case, notably, when the subject of discourse is unequivocally linguistic. When one ventures beyond this elementary situation, however, especially when scare quoting and weakly metalinguistic lexical items are present, judgments become volatile.

Rey-Debove’s fundamental criterion for metalinguisticity—at least one lexical item in the sentence must, at some level of its meaning, contain a component [+ language]—provides us with a way of sorting (variously) metalinguistic sentences out from mundane ones. This criterion, though, often fails to match with intuitions about metalinguisticity (if there can be such a thing). These problems, however, should not obscure the one important lesson that can be drawn from our investigations in Section 2.4.2.: when dealing with natural language, we cannot hope to make the same clear-cut distinction between metalinguistic and mundane sentences as can be made in the theory of formalised languages.

2.5. Comparison with the Main Properties of the Relation in Formal Languages

It is time for a recap of the main properties to have emerged from our investigation of how metalanguage fits within natural language. For ease of comparison, I take up the seven points distinguished in Section 2.4.

- (i) L_0 is not a language. L_m can be regarded as a language, but it is more satisfactory to consider it a *use* of a language. The ‘third man,’ L_n , is a language;
- (ii) L_m is produced largely naturally and is not restricted to talking about one particular object;
- (iii) L_m has its own lexicon. It is possible to list its elements as all those that include a lexical item with the component [+ language] as part of its meaning. This, however, produces odd consequences in connection with meta-words, e.g. that the verb *mail* and the noun *reader* are metalinguistic. The alternative is to accept that there is a continuum from mundane to metalinguistic with no non-arbitrary cut-off point;
- (iv) As a consequence of the previous point, it is often awkward to determine if a component of a sentence belongs to $L_n \setminus L_m$ or L_m ;
- (v) With a difference in levels set to 1—e.g. with L_{m_1} and L_{0_0} —there is no inclusion relation between L_0 and L_m , since $L_{m_1} \cap L_{0_0}$ is empty. On the other hand, it has been shown that $L_m \subset L_n$, though for reasons different from those that are generally advanced;
- (vi) There is no neat hierarchy of languages, though some lexical items and some sentences can be placed at this or that level of a very incomplete hierarchy. Besides, L_0 fits nowhere in the hierarchy, as it is not a language;
- (vii) Because lexical items can display varying degrees of metalinguisticity, and because of hybrids, it is often impossible to determine if a sentence belongs to $L_n \setminus L_m$ or L_m .

Conclusions

I have shown that none of the characteristic features of formalised object-languages and their metalanguages have an exact counterpart in the theory of natural languages. Although talk of ‘metalanguage’ in the latter context may be useful, it is no more than a convenient term for what is one function, amongst others, of natural language. Blackburn (1994, 240) states more squarely that the term is “abused” when applied to any “discourse about other sayings (e.g. the discourse of literary criticism).” As for the ‘object-language,’ it is not even a language, and therefore hardly has any place in linguists’ discussions. In this respect, it is interesting to note that linguists’ writings make massive use of the term *metalinguistic*, but have less use for *metalanguage* and even less for *object-language*.

This paper has not, however, been for nothing. There was a need for a clarification of how terms borrowed from logic *actually* apply to natural languages. The work done towards such clarification has revealed a vast number of complications which are often not even suspected. Two scholars have been exceptions in this respect, Droste and Rey-Debove. Both, especially the second, made crucial, though I believe still incomplete, progress towards a thorough comprehension of the categories and phenomena under scrutiny. It is my hope that this paper has furthered their insights and brought us closer to a fuller picture.

The last point I wish to highlight is the fact that many interesting cases of quotation (or sentences containing quotations) do not fall under the metalanguage proper. This suggests that it may be more useful, and more warranted, to work towards a theory of quotation than towards a theory of natural metalanguage.

CHAPTER TWO

WHAT LINGUISTS DO WHEN THEY WRITE SOMETHING: THE ART OF STENOGRAPHY¹

NICOLAS BALLIER

Introduction

“*Les peintres ne doivent méditer que les brosses à la main*”: Balzac² has it that a painter should only think with a paintbrush in his hands. This chapter tries to describe whatever it is that linguists do when they inscribe linguistic data with their analyses, using graphic signs within their metadiscourse. Linguists may resort to graphic signs, symbols without necessarily explaining what they do with an explicit metadiscourse (like orthoepists in the 18th century) and still indicate intellectual distinctions by means of graphic representation. These material³ inscriptions are an indirect reflection of the theoretical possibility of metadiscourse as such. Even if the use of these graphic signs does not correspond to a formal set of algebraic notations serving as a formal metalanguage *per se*, I would like to make the claim that linguists do have grounds for using a whole set of graphic signs to elaborate theories and discourses on the substance of language. I am not claiming that there is such a thing as a formal metalanguage, but forms of metadiscourse do exist. I claim that linguistic analysis only makes sense if use is made of material representations of language, in the sense of inscriptions materializing conceptions of language. I propose to delineate some properties of these inscriptions, both as a phonologist and as a linguist exploring other domains. I have called this whole apparatus ‘stenography,’ not only because it literally means ‘short hand for,’ but also because it strikes me that the French linguist Jean-Claude Milner uses the term ‘*sténogramme*’ in his major book about the epistemology of linguistics (Milner 1989) to refer to linguistic ‘Gordian knots.’ Therefore, ‘*sténogramme*’ refers both to a problematic concept (Milner recurrently refers to problematic issues with the phrase ‘*un sténogramme de problèmes*’), and sounds like a good candidate for a

minimal unit in linguistics—almost a ‘stenographeme.’ In other words, talking about language without a formal metalanguage is a problem, and a whole range of graphic signs can be seen as a possible solution. Resorting to graphs, arrows, indices, or schemata is a metalinguistic praxis. As it is the case with language, they are likely to be described as individual units (stenographemes) and as a sort of a system (stenography). At least, this is a very tempting analogy for phonologists, who deal with graphic representations (graphemes), phonological representations (phonemes), and phonetic realisations (phones and allophones) as well as their representations (transcriptions). It seems to me that similar theoretical questions attend the articulation of our metalinguistic reasoning through representations and graphic signs. I call ‘stenography’ the whole apparatus of stenographemes: signs, conventions, symbols, used by linguists to represent their conceptions of, or stance on a particular aspect of language. I argue that stenography has its own theoretical status and properties. Some of these properties are limited to one theoretical framework, i.e. when a stenographeme is part and parcel of the theory or conceptual apparatus. In other cases, stenographemes can be more widely adopted in a field and are more likely to be considered conventions, usually because they materially embody a conceptual distinction.

I have defined *stenography* as a set of stenographemes,⁴ just like language is a set of signs. There are multiple kinds of stenographemes, depending on the linguistic properties that linguists aim to show. Additions to texts, figures, any kind of signalling devices (indices, curly letters, bracketing, small caps, typographical modifications) should not be seen as evidence of our metalinguistic powerlessness but as examples of our creativity when designating linguistic properties. Stenographemes are used to pinpoint linguistic phenomena: for example, indices can be used to denote coreferentiality. The subscript *i* is the materialization of the semantic connection between antecedents and pronouns in the following example: *a man_i came in. I saw him_i.* In this simple case, the stenographeme points to a semantic property. There are several kinds of stenographemes used in linguistic analysis. First among them is the use of italics, used to refer to what is called “mentions” or “citational forms.” They are not just conventions, or rather, conventions are only a subset of possible stenographemes. Here, the italics for the quoted form signal a different semantic status, and a potential autonymic interpretation. The analysis of language is pluriosemiotic: it presupposes not just words but a graphic apparatus surrounding them. This is what the concept of stenography tries to capture, including the complex schemata devised by linguists to represent syntactic dependence, phonological structures of

syllables, morphological structures, and more complex concepts. There is an undeniable heterogeneity in stenography, which is another way of saying that *stenography* is not only the whole set of possible graphic representations, but the very faculty of highlighting linguistic properties with graphic signs. The chapter will end with a tentative typology that shows the need for diverse semiotic regimes. The more general aim of this chapter is to show that stenography is a way to categorize. Depending on the types of categories or the levels of representation targeted by the stenographeme, we have several semiotic regimes; various ways of categorizing linguistic properties can exist simultaneously in more complex systems. In a nutshell, stenography is the graphic mark of a categorization procedure.

As a science, linguistics suffers from a lack of consistency regarding notational conventions, a weakness that mathematics can hardly be accused of. Although my former math mentor (who would rightly cringe at being mentioned here) made it a point that explanatory discourse was the key to sound mathematical practices, many would agree that a clear difference between mathematics and linguistics is that a single blackboard can contain a whole demonstration, understandable to any mathematician. I am not sure linguists from diverse backgrounds would necessarily understand each other, should they exchange blackboards. The IPA may be said to have tried to answer the challenge of a set of shared conventional notations. It will come as no surprise that some of my arguments about the need to use graphic signs stem from the current practice in phonetics and phonology to deal with what can only be partial representations of speech. We know only too well how imperfect our current system of representations is when tackling the inherently elusive and variable nature of speech. Many studies have made this clear (see, among others, Ochs 1979; Ballier 2004; Delais-Roussarie, Post and Portes 2006). To some extent, phonologists have got rid of the problem by simplifying speech data to the extreme. I claim that the problem emerged at a specific moment in the history of linguistics. Following Auroux's (1994) analysis of the history of linguistic ideas, I will mostly focus on the second revolution of 'grammatisation,' and more specifically, on what I call a watershed moment in phonological thought (Pouillon, forthcoming): transcriptions of speech in eighteenth-century English pronouncing dictionaries. These conflicting notations, which were the first step in the invention of a phonological representation of speech, incorporated grapheme-to-phoneme systems and forms of syllable division (see Ballier and Pouillon 2015; Ballier, Beal and Pouillon 2016).

In other words, these questions echo the paradigm established by Sylvain Auroux (1994), who divides the history of linguistics into a series of technological revolutions. The first revolution saw the invention of writing. The second one is the result of the creation of grammar and dictionaries, and more or less corresponds to the eighteenth century in the West (the age of the Encyclopaedia). We are currently witnessing the third one, with the complex interaction of (huge) corpora, tools, computer science (not to say data science) and the Internet. Some would be tempted to say that extreme computational approaches to linguistics hail the end of notational conventions and metalinguistic issues. After all, huge datasets, crammed with numbers, will speak for themselves, and number crunchers can do without complex linguistic representations. I beg to differ. It seems to me that the third revolution of grammatisation implies specific stenographic procedures. Even with user-friendly interfaces, the analysis of data and metadata in integrated tools such as R (R Core Studio 2016; Ballier, forthcoming), the application of built-in tokenisation and Natural Language Processing (NLP) driven techniques (in other words, computerization of linguistic data) does not put an end to these questions.

My aim is to show that stenography, the system of metalinguistic representations entrenched in notational procedures, is part and parcel of this system of conceptualisation *via* notation and *qua* notation. This chapter is organized as follows: section 1 presents some examples of theoretical constructs entrenched in stenography, proposed within the ‘enunciative’ frameworks (i.e. linguistic theories in the wake of Benveniste) which aim to deal with the linguistics of English (Ballier 1997). Three figures will illustrate a semantic version of stenography. Investigating these specific features of graphic symbols and schemata can be seen as analysing the very locus of linguistic theories. Stenography might be called the creativity of the linguist (just like Deleuze describes the philosopher as having craftsmanship with his concepts, similar to a creative craftsman or ‘artisan’); I take it to be at the heart of the discipline that reflects on linguistic theories. Section 2 examines the reconceptualization of speech by eighteenth-century orthoepists (Ballier, Beal, and Pouillon 2016) and exemplifies the stenography of pronunciation. Section 3 examines properties of stenographic representations of the third revolution of grammatisation.

1. From graphic representations of metalanguages (theoretical language) to a multiplicity of graphic practices

This section is a reconstruction of the theoretical problems raised, among others, by Frege (1879) when he devised his conceptual writing, as an attempt to formalise the logic of predicates by means of graphic signs. Among other things, this kind of formal analysis ran into trouble with the many paradoxes of recursive citations. When quoting linguists (sometimes within quotations), it becomes almost impossible to abide by a common set of rules for *prima facie* quotations, italics from the authors, and various notational conventions (italics, quotes, small capitals, bold characters...). It becomes a challenge for the would-be analyst to master all these layers of representation, or even to try to make sense of successive embedded quotes with a unified set of conventions. This is what we would have hoped to find in linguists' discourse: consistent terminology with everything in its place. But in the corpus of linguistic analyses that can be examined, the typographical choices made to highlight authors' concepts are fraught with contradictory and irreconcilable practices. Because linguistic analysis is often about discourse, analysing theories means producing another discourse on existing texts, and therefore, having to deal with a *millefeuilles* of jarring notational inscriptions. Rather than treating this state of affairs as an impediment to the analysis, in retrospect, perhaps it should be treated as the very condition of the analysis. Linguists interested in linguistic theories should tackle linguistic inscriptions. In some cases, linguistic communities adopted comparable conventions. That turned out to be true for the linguists I investigated, with a convention emerging that markers should be capitalised when under discussion.⁵

1.1. Stenographemes: short of conceptual writing

A perfect tool to analyse language has been a long-lasting philosophical desire. Here is how Frege phrased it in his Preface to his *Begriffsschrift*:

The course I took was first to seek to reduce the concept of ordering in a series to that of *logical* consequence, in order then to progress to the concept of number. So that nothing intuitive could intrude here unnoticed, everything had to depend on the chain of inference being free of gaps. In striving to fulfil this requirement in the strictest way, I found an obstacle in the inadequacy of language: however cumbersome the expressions that arose, the more complicated the relations became, the less the precision was attained that my purpose demanded. Out of this need came the idea of

the present *Begriffsschrift*. (Frege 1879, *Begriffsschrift*, translated by Michael Beaney. Page III of the Preface in the original edition)

As the translator explains in his introduction, *Begriffsschrift* literally means ‘concept-script,’ and the term has also been translated as ‘conceptual notation’ (C. Besson makes the same point about ‘conceptual writing’ in her French edition of Frege’s works published in 1999 by Vrin). The most notorious aspect of this idea of conceptual writing is the logical notation of quantifiers that has been used ever since. Nevertheless, human language has linguistic markers other than quantifiers, so that this one-to-one mapping of words to graphic signs has never been completely possible. In other words, we cannot reduce language to logical symbols, even if we can represent a predicate.

Linguists actually like to build grammatical representations that aspire to correspond to the philosophical notion of the *predicate*. The French Ministry of Education recently created an uproar by reintroducing the concept of *predicate* in official instructions for teaching and textbooks (the 2015 Bulletin officiel).⁶ The debate was muddled by the fact that the concept also exists in philosophy, where it differs from its standard meaning in linguistics. The sad truth of the matter is that the two concepts have entirely different scopes: the philosophical *predicate* applies to semantics, whereas in linguistics, it is a syntactic unit.

Philosophically, one could say that the typology of judgments hinges on the contribution of the predicates, which is tantamount to saying that certain properties are associated with a subject. The philosophical division is roughly between subject (taken here as the focus of attention) and predicate (whatever properties are ascribed to the subject). Kant distinguishes analytical judgments from synthetic judgments. The former only describe properties of the subject, whereas the latter are judgments where the predicate attributes new properties to a subject that are not intrinsic to it. Of course, commentators were likely to be confused since *subject* is potentially ambiguous: it is a grammatical category (shall we say what is usually on the left of a verb in a canonical sentence in English) as well as a common way to refer to a question (as in *the subject of our talk*). Linguists, on the other hand, mostly distinguish predicate and subject. The linguistic predicate is typically summed up as encompassing the verb and all its complements. When examples are given in the public debates which have been taking place in French media, this gives rise to even more confusion since certain complements are compulsory, while others are not. There is room for ambiguity but also for analysis of these two traditions. It is a wonder that enunciative linguists have not more closely studied the possible connections between linguistic predicates and philosophical

predicates in the guise of what they call '*le jugement de l'énonciateur*' (the speaker's stance).

The closest thing to an analysis along these lines is Laurent Danon-Boileau's (1987) interesting 'correlation of facts' between epistemic and root interpretations of the modal auxiliary *will* along these lines. Table 2-1 sums up my reading of his analysis: he makes the point that the kind of judgment (synthetic or analytical) predicated by the utterance varies according to the interpretation of the utterance (specific or generic) and according to the semantic status of the grammatical subject. I believe the correlation he has in mind consists in the simultaneous shift in the semantic roles of the grammatical subject *John* and the judgment of the utterer. In the specific interpretation, the judgment is synthetic, and bears on the predicate, which adds new, separate information. In the generic one, the judgment is analytical, and bears on the subject. The predicate spells out properties of the grammatical subject, which is the focus of an evaluation (*that's just like him*), the experiencer of a property assigned to it rather than an agent. In the generic interpretation, the grammatical subject is the semantic focus and is attributed a property. I interpret this distinction within an ontology where the synthetic judgment describes an event, whereas the analytical judgment predicates a property.

interpretation	utterance	modality	judgment	subject case	scope	ontology
specific	John will do these things.	epistemic	synthetic	agent	predicate	event
generic	John will do these things.	non-epistemic	analytic	experiencer	subject	property

Table 2-1. Laurent Danon-Boileau's (1987) 'correlation of facts'

A crucial point in the analysis of modal auxiliaries within the enunciative tradition is the emphasis on the role of the modal. Henri Adamczewski (1977) had a specific way of representing the modal auxiliary as above the '*noeud prédicatif*' (predicative node). This is handy: his notation (see Figure 2-1) corresponds to a linguistic opposition between the *modus* and the *dictum*. Charles Bally (1955) revived this notion, borrowed from the Middle Ages, between what is said (*dictum*) and how it is 'modalized' (*modus*). As a general extension to this *modus/dictum* analysis, French enunciative linguists came with the idea of the 'predicative relation,' which is the complex structure of the grammatical subject and the predicate. This complex conceptual structure is a good representation of 'modalization.'

The linguistic conceptualisation assumed by the ‘predicative node’ (another recurring expression among enunciative linguists) has often been used to present variations on this *modus/dictum* analysis.⁷ The ‘predicative relation’ is represented with a pared down notation, comprising only subject and predicate with a non-finite verb form:

John / do these things (*John will do these things*).
 He/arrive on time (*he could have arrived on time*)

The notation of the predicative relation was used by, among others, Henri Adamczewski, who used a drawing in one of his earlier papers to account for some specific properties of markers.

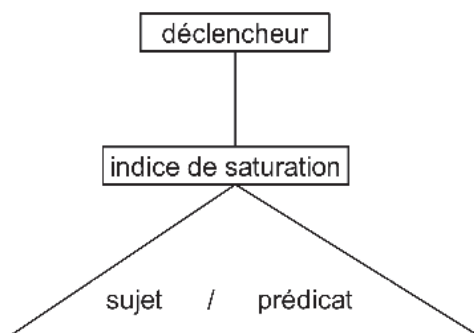


Fig. 2-1. The predicative relation and some of its possible translations.

Figure 2-1 sums up the apparatus proposed by Adamczewski. In this version, contextual information appears under the guise of what he calls the ‘trigger’ (*déclencheur*, see Ballier 2008). The *modus* element is what he calls the ‘saturation index’ (*l’indice de saturation*), *saturation* being a term borrowed from Frege (Ballier 1997). The base of the triangle corresponds to the predicative relation. Some enunciative linguists have adopted minor variants in their notational conventions. For instance, Adamczewski’s disciples use small capitals and angled brackets: <SUBJECT / PREDICATE>; nevertheless, they accord a similar conceptual status to the predicative relation. I maintain that there is a stenography of the analysis of the subject/predicate relationship, and that it can be translated variously into other linguistic approaches or theories. In the case of predicative relations, translatability comes easily, as the predicative relation is a conventionally accepted concept. Our next section

presents two more radically different metalinguistic representations, which can nevertheless be compared, as they are theoretical constructs; I hold graphic representations to be conceptual visualisations that testify to linguists' attempts to capture variation in meaning.

I have suggested (Ballier 1997) that Guillaume's theory could partially be analysed by investigating one of his crucial graphic representations: the radical binary tensor (*le tenseur binaire radical*). I made a comparable claim about Antoine Culioli's theory, basing my analysis on his representation of *la came* (Culioli 1968). How can a single word (marker) be invested with different kinds of meaning according to context? I would like to describe how graphic elaborations were created to capture meaning variability along graphic clines/lines in Guillaume's and Culioli's representations.

1.3. Stenographemes and theoretical frameworks

The circle in Figure 2-2 corresponds to a topological representation suggested in (Culioli 1968) in a paper adumbrating what he believes to be required for language modelling. This is a topological structure proposed by Culioli in some of his papers to account for the variation in meaning of certain markers. Over time, his use of this topological representation has been variable (see Ballier 1997 for details). It surfaces in various papers; he has applied it to the analysis of several linguistic markers, among others the use of French *il* (which stands for both *it* and *he*). The diagram, called '*la came*,' is meant to represent the possible iterations of the meanings of *it*. In a footnote, Culioli explains that the term was suggested by French psychologist François Bresson, that it serves to refer to a category, and that some of its properties are well-known to mathematicians.

The a^{-1} versus $-a$ indications are meant to show that the quasi-circular path is not on a single horizontal plane, but is a 3D representation where the start and end point are not on the same plane. Point a^{-1} is actually exactly above point $-a$; the structure is that of a spiral whose end point corresponds to the beginning. The abstract $-a$ vs. a vs. a^{-1} suggests that the range of possible meanings⁸ might even include contradictory interpretations. Because Culioli postulates core invariants in meaning, he needs to represent different, sometimes opposite meanings. The $-a$ and the a^{-1} are on the same level but not on the same plane: this is what defines his representation, which he borrowed from mathematical topology. In other words, this schematic representation is meant to cover the whole range of interpretations (which Culioli makes a point of calling 'values') that can be attributed to several markers. He has applied this kind of analysis to

French *il*. Figure 2-2 is taken from the 1968 paper where he first uses the concept. The paper tries to account for the fact that French has some uses of *il* which can be translated as *he/it* but also has a whole range of possible interpretations. Below, I briefly comment the *came* as a stenographeme to explain how this representation is used to model the series of possible interpretations and uses of *il*, and the relationships between them. I would contend that Culioli's range of interpretations is a re-reading of Frege's *Wertverlauf*

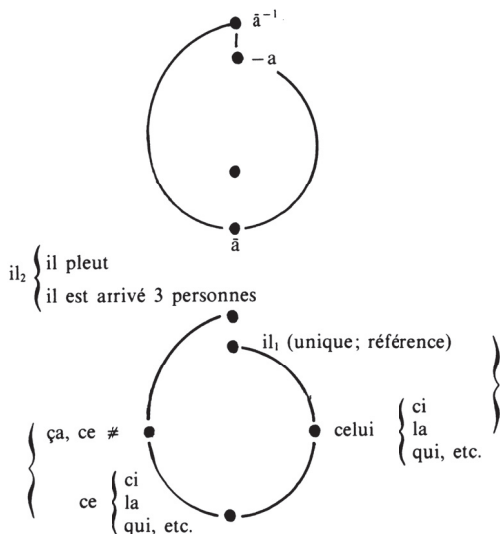


Fig. 2-2. La *came* (after Culioli 1968)

Culioli claims that for reasons of space, he cannot elaborate on the reasoning, which is only sketched in two footnotes. Admittedly, Culioli also explains that the concept is to be at the core of another paper, which was never published. The figure is meant to formalise the ambivalence of the marker *il* (*he/it*) and to account for two sets of linguistic observations. A) In certain cases, *il* is replaced by *ça*, especially in cases where *ça* resumes generic subjects (*les chats, ça griffe*, cats will scratch). B) The semantic reference of *il* may range from definite (noted as il_1) to non-specific (noted as il_2). Culioli makes the point in a footnote that *il* is morphologically masculine but semantically neither masculine nor feminine. The schema circumscribes the range of possible uses of *il* and *ce*, showing how some references can be definite (il_1) or beyond any

specific or generic uses (il_2). In *Il_2 est arrivé trois personnes*, the pronoun corresponds to a plural interpretation, for *il pleut* (*it's raining*), the meteorological *it* does not have any specific reference. On top of the illustration, in the inside of the circle, il_1 has a unique single reference (not unlike *he*). Moving to the downside part of the figure comes the interpretation *the one that* (right), opposite to this interpretation is left hand side *this* or *that*. On top⁹ of initial il_1 comes the so-called impersonal il_2 (which in French corresponds to uses of meteorological *it* and potential uses where *il* stands for a formal subject which can be followed by a plural: *il est arrivé trois personnes*).

Culioli insists that the diagram is “not a toy” nor an intuitive illustration but a tool with formal uses. This graphical representation is an abstract figure trying to account for the whole range of possible meanings.¹⁰ His abstract representation of meaning tries to cater for the possible range of interpretations of a given marker. His language game consists in accounting for the whole range of what he calls ‘semantic values.’ The ultimate aim is to offer what he calls an ‘invariant,’ an abstract representation that accounts for all the possible interpretations of a given marker. I regard this stenographic notation as an example of how linguists’ inventiveness can produce graphic representations of metadiscursivity. I suspect Culioli would claim that this is nothing but a concept imported from mathematics.¹¹ Culioli is very sparing in his theoretical references, but there is an undeniable Fregean flavour to this conceptualisation of ‘the range of (semantic) values.’ I maintain that this figure, the *came*, is an item of his theory and that it nicely exemplifies how a theoretical construct is embedded in a graphic representation. This is what I call ‘stenography.’ Ideally, a linguistic theory should be able to list all the stenographemes that are part and parcel of a given theory. Unfortunately, in spite of the translation into English of one of his seminars¹² and of the publication of his collected papers,¹³ Culioli has never written a single volume spelling out his theory,¹⁴ and how various graphic (and/or) topological concepts could be articulated/connected.

For my second example, the centrality of the stenographeme in the theory is beyond dispute as Gustave Guillaume has made it clear that the ‘tenseur binaire radical’ (binary radical tensor) is central to his theory. He applied it as early as 1919, to the investigation of the ‘problem of the article and its solution in French’ (Guillaume 1919). Figure 2-3 is an application of his concept to English put forward by one of his disciples, André Joly. Here, the whole range of possible interpretations of the articles ‘*a*’ and ‘*the*’ is taken care of by the two contradictory moments represented by the two arrows. The binary radical tensor aims at

accounting for two sets of somewhat contradictory interpretations. The first one moves from generic interpretations to specific interpretations (the arrow from the left to the centre) and accounts for the fact that ‘a’ can be used to determine a noun which may have a generic interpretation (left) to a specific construal/interpretation (middle of the figure). The second arrow or the second range of interpretations covers the various uses of the definite article ‘the,’ which can be specific (centre of the figure) or generic. In Gustave Guillaume’s views, the process is ordered, the top horizontal arrow corresponds to the inscription in the mental/cognitive conception of time, what Guillaume calls ‘operative time’ (*le temps opératif*). I maintain that this figure represents the whole set of possible interpretations from generic to specific uses of ‘a’ and, conversely, the whole set of possible interpretations specific to generic uses of ‘the.’ In other words, this conceptual drawing, this stenographeme, is a convenient representation of the various degrees of genericity and specificity assumed by the referent of the sequence article + noun. There are other figures in Gustave Guillaume’s theory, but the ‘binary radical tensor’ is a prominent candidate for the graphical representations that try to account for the ‘invariant,’ the abstract unity of meaning posited behind every possible use of a linguistic marker.¹⁵

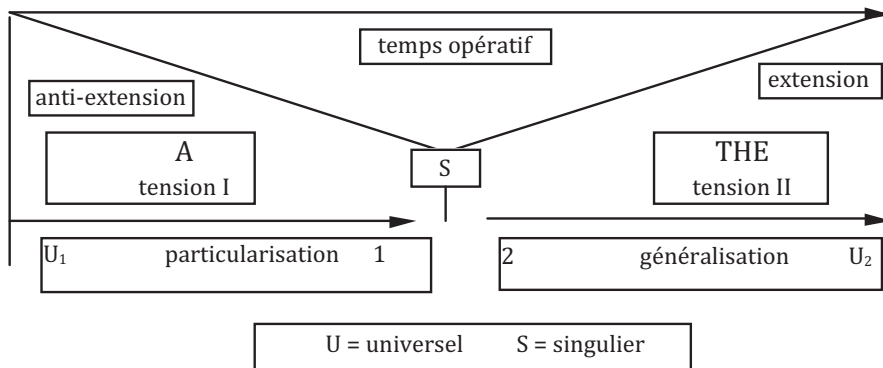


Fig. 2-3. the radical binary tensor (after Joly and O’Kelly 1990)¹⁶:

This two-dimensional graph has been proposed for the analysis of ‘the system of the article’ and is the scheme proposed for the solution to the problem (the range of possible interpretations assumed by the markers within the system).¹⁷

In this section, I have alluded to an ancient dream: the *Begriffsschrift*, a perfect conceptual writing system, as if a set of symbols could spring from our minds as universally intelligible expressions of our ideas. The bad news is that we will have to deal with words (and how we use and arrange them) to investigate our brains and ideas, whether we like it or not. We can dabble in notations, but we won't do away with language. We can nevertheless use graphic signs to visualize mental schemata and analyse the corresponding language games expressed in these schemata.¹⁸ Some abstract representations emulating at our mental representations may be proposed. This is what I mean by a system of metalinguistic representations entrenched in its notational procedures. I have illustrated the concept of stenography with complex representations aiming at representing the variability of meanings assumed by a linguistic marker, for example between specific and generic interpretations. They are more complex than other stenographic devices to be presented in section 2, but they provide a good illustration of the linguist's predicament. Because no formal metalanguage can be invented for linguistics, proposing schemata is a sort of re-enactment of the invention of writing, presented by Auroux as the first technology of linguistics. As such, these graphic explanations of linguistics are part of the founding gesture of linguistics.

The examples I have used, Guillaume's radical binary tensor and Culioli's *came*, are in fact arch-stenographemes, in that they epitomise the theories put forth by these authors. Culioli and Guillaume both use other stenographemes in their theoretical works. What is to be made of this kind of representation? I advocate a conception of linguistics as a language game aiming at discovering linguistic properties (such as *animate*, *transitive*, etc.). In this section I have focused on semantic properties. The stenographemes might be seen as a way to try to predict the observations (or as Culioli says, "calculate"), elaborating a graphic model that accounts for the variability of the interpretations (schemata of variability). The stenographemes I have described are visual representations of semantic mechanisms, an attempt to answer the research question: how does one account for all the possible interpretations of the same markers? Stenographemes try to offer semantic correspondence to observations. They are not the observations *per se*; the *designatum* is not the object. This is not a one-to-one relationship between truth and logical values on the one hand, and words on the other, hypothetically deduced from theoretical principles, but rather a figurative representation that maps out parameters of interpretation.

The semiotic power of this *quid pro quo* network of representations has to be reflected in a more general semiotic debate. I propose to re-read

the contributions of eighteenth century orthoepists within Peirce's conceptions of signs. They attempted to reproduce some aspects of the 18th century English pronunciation in their dictionaries. I first comment on the ambiguities of the contemporaries' reception towards spelling and then show how a radical semiotic distinction has required some imagination. Spence's *New Alphabet* as commented in Beal 2016 is a case in point. I then move on to distinguish two different strategies or semiotic regimes orthoepists resorted to when trying to represent pronunciation with letters. I finally suggest applying Charles Sanders Peirce's distinction between icons, indexes and symbols to account for some orthoepists' practices.

2. Orthoepists and the second revolution of grammatisation

Even though orthoepists focused on a specific aspect of language, the phonetics and phonology of spoken English, they tackled theoretical questions that can be extended to other aspects of reflection about language.

2.1. Resistance to symbols and Spence's theoretical stance

I would like to extend the characterization in Beal 2016 of Thomas Spence's ideas for spelling reform and their reception by contemporaries. She quotes a letter to Spence from Charles Hall:

I cannot conceive what should induce you to disguise your work with such a whimsical kind of Spelling, which renders it so difficult to read, that I could more easily read a book in four or five dead or foreign languages, than I could read yours in my native tongue. You say that it is not formed from mere vulgar and uncertain sound, but is systematic. But to acquire a system so as to use it readily requires too much time, for the reading a single work. (Place Add. Ms. 27, 808: 22, cited in Shields 1974, 39, cited in Beal 2016)

As Bourdieu would put it, the ruling class would not give up their social capital, acquired through years of study, and adopt another, simpler, representation of sounds in which they would lose their competitive edge (reading skills). Joan Beal also quotes a letter by one of his contemporaries where another aspect of the contemporary reactions to his work clearly emerges: the misunderstanding of literality.

He also published some works in what he termed the Spensonian dialect, being an attempt to render the orthography of the English language identical with its pronunciation, like the Italian. This orthography was somewhat defective, as he spelled the words according to the Northumbrian idiom, Newcastle-on-Tyne being his birthplace. (Place, *BM Add. Ms. 27, 808: 227*, cited in Shields 1974, 39, cited in Beal 2016)

I suspect that there is a form of graphocentrism here, as readers believed that if the spelling was altered, then the word itself was altered, hence the representation was deemed “defective.” This is not unlike a Platonic prejudice: any change in the graphic representation of sounds would be equated to a change in the very conception of the word. Some misunderstanding of the diacritics is at work here. Contemporaries abided so much by the letters that maybe they failed to see that the changes proposed in Spence’s ‘New Alphabet’¹⁹ did not necessarily imply changes in pronunciation. Representing sounds with an alternative system was mistaken for altering pronunciation. Spence’s contemporaries misunderstood his goal in proposing a new orthographic system: Beal 2016 repeatedly makes the point that it corresponds to a unique bijective spelling-to-sound correspondence; it should be seen as a proto-phonemic system (Pouillon, forthcoming). The conceptualisation of bijectivity did not immediately result in a clear separation of the graphic and proto-phonemic forms. Spence’s dictionary had a hybrid system: representing headwords with traditional orthography but with a stress mark, followed by a specific respelling of the words using his unique alphabet. I would say that his approach, though it did not distinguish strictly between what we would now refer to as orthographic vs. phonological forms, was a stenography materializing his analysis of speech, even though it is more limited in scope than some of his fellow orthoepists’ analyses (Ballier et al. 2016). I am now going to comment on other stenographies: orthoepists have had different practices while representing sounds with letters, which can be divided into two series of representations.

2.2. ‘Sign-posting,’ or not quite re-iterating the letters²⁰

I propose that orthoepists experienced a specific intellectual epiphany, *le moment phonologique* (Ballier et al. 2016) on the technological innovations, Pouillon forthcoming on the reconceptualization of prescriptivism): they gained theoretical awareness based on the technology of writing systems. One may share the suspicion towards a buzzword such as ‘representation,’ nevertheless, one should try to tackle the complexity of the interplay between our ideas, linguistic forms taken as tokens of

signifiers and linguistic forms taken as tokens of referents. Concerning dictionaries, the second revolution of grammatisation consisted in describing the sequence of sounds associated with each headword. This resulted in a conceptual innovation in which some letters were used as tokens of letters in the headword, but then also as tokens of sounds in a separate form after the headword. They are also tokens of a system that tries to match sounds and letters.²¹ This led to a series of pronouncing dictionaries where headwords were repeated, starting with Kenrick's dictionary (1773): a graphic representation would be followed by what orthoepists called a 'respelling.' This reiteration of letters with letters actually had two main semiotic regimes (Ballier 2004a) which I summarise in the following table:

"Signposting" (Pouillon 2016)	"Transcribing"
<p><i>Hĭghnefs</i> From Buchanan 1757</p>	<p>RELIGION, rē-līzh' -ūn From Sheridan 1780</p>
<p>Between 1721 and 1780, orthoepists added font effects, numbers and other diacritics while preserving the integrity of the spelling. The conventional spelling was always conserved, as in this example from Buchanan. Some devices were used to signal specific realisations. The desire to unambiguously indicate the pronunciation of phonological units (independently of their graphic contexts), while at the same keeping the word intact, led to difficulties in presenting the information in a readable way. The orthographic sequence of the word remains, whatever extra typographical information is added.</p>	<p>From (Sheridan, 1762) a wave of orthoepists started to use fully separate and reconceptualised representations after the headword. This implied that the graphic integrity of the word was no longer central to their approach; instead, truly transcribing involved accepting the modification of the spelled word to represent a series of sounds rather than the word itself. As can be seen, the pronunciation of reduced vowels could be indicated by altering the spelling. A stenographic device was used to indicate syllable division (here, a hyphen) and primary stress.</p>
<p>True diacritics were used, such as the macron (which is not unlike the vowel length symbol in the IPA). The "mute" consonants are not italicised. The graphic spelling is preserved, but extra layers of information are added to the graphemes to convey pronunciation.</p>	<p>'False' diacritics (where letter + diacritic constitute a unit, which in itself might be considered a symbol): the graphic signs do not correspond to a supplementary value, but are indicative of a radical difference. The cedilla is a case in point: ç is not 'c+cedilla' but /s/. Changing the vowel is another such strategy.</p>

(Buchanan 1757) employs font effects, breves, macrons and accents. (Kenrick 1773) has accents, font effects, hyphens for syllable division and superscript numbers denoting vowel realizations aligned with the syllables.	Cedilla, e1, e2 for the notation of sounds
The diacritic is understood through self-referentiality to the graphic system itself. There is a certain amount of autonomy in the sign: its meaning is computed by compositionality. Stenography consists in preserving the spelling but in adding indications as to the realisation of the letters.	The meaning is construed as distinctive: it may correspond to a different degree of aperture, a different place or mode of articulation. In that sense, it does not belong to the same semiotic system. It may stem from a long-standing tradition of special letters (ç, é, è). The metalinguistic meaning of the sign is understood as radically different. There is metalinguistic information inherently contained within the diacritic to some extent; for instance, in (Sheridan 1780) superscript 1 always indicates a short vowel, even though e1, a1, etc. are different phonological units.

Table 2-2. A correlation of properties for the various semiotic regimes at stake

When investigating how the stenography is intended to work, in terms of metalinguistic interpretation (i.e., here, the pronunciation of the headword), two semiotic regimes can be pointed out. In “signposting,” the representation has to match standard orthography, to which various devices (macrons, font effects, superscript numbers, etc.) are added to indicate specific pronunciations. A non-bijective set of relations is posited between graphic sequences and phonic realisations. The devices function as signposts, pointing the reader to the different realisations of the sequences (dealing, for instance, with digraphs, or with “mute consonants”). In transcription, the semiotic regime is different. The inventory of signifiers, though it overlaps with the letters of the alphabet, incorporates new symbols and discards others. A sequence of signifiers does not have to abide by spelling rules, but is designed to emulate sounds (as the <dz> for the voiced affricate in the transcription of *religion*).²² New symbols or non-standard combinations of letters can be used: orthography is not the only material mobilised to represent pronunciation.

What complicates the proto-phonological stenography of the second revolution of grammatisation is the fact that both systems make use of diacritics. The problem of representing sounds with letters is not solved by a simple invention of a graphic device materialising a given sound. The two systems make use of diacritics, but the relation to the interpretation of the graphic structure (and the conceptualisation of its integrity or even the right to respell a word) changes between authors. If anything, ‘signposting’ is the preliminary type of stenography, and it is not incompatible with a certain form of bijectivity. Kenrick (see Figure 2-4) has sixteen superscript numbers, plus a symbol for a schwa-like vowel, in his representation, and so manages to be fully bijective for the transcription of vowels. The evolution of stenography does not correspond to linear progress with perfect representational watersheds, but to a complex process.

MA¹¹THEMATI¹³CIAN—MATH¹¹-E-MA¹³-TICI¹⁵-AN

Fig. 2-4 from (Kenrick 1773)

The semiotic shift of that period could be analysed as a gradual emancipation from the Saussurean signifier. ‘Signposting’ is a form of proto-phonological stenography which focuses on maintaining the integrity of the graphemic sequences. ‘Transcribing’ means something else: it is about accepting the possibility of an alternative semiotic representation. ‘Transcribing’ introduces a ternary representation of the sign, with the spelled form, the transcription and the sound represented by the transcription. It looks to me that outright condemnations of metalinguistic schemata proposed by certain orthoepists might result from a strictly Saussurean conception of the sign in the minds of their contemporaries. A ternary conception (Peirce’s ‘trichotomy of signs’) might be more adapted to linguists’s experimentations with metalinguistic discourse.

2.3. A Peircean conceptualisation of stenography

If we follow Peirce’s semiotic typology, it may well be that the history of representing sounds with letters essentially consists of three phases of representations. Applied to the second revolution of grammatisation, graphic signs would have undergone three stages. Letters used to represent sounds were first used as indices, then icons and at last as full-fledged

symbols. Orthoepists mixed various semiotic regimes incorporating indexical, iconic, and symbolical representation to indicate pronunciation. I am suggesting here that we could describe the various types of stenography adopted by 18th century dictionary-makers following C.S. Peirce's trichotomy of signs.

Iconic	Indexical Kenrick	Symbolical Spence
More immediate relation to the object: conventional spelling	"Signposting" (see above)	"Transcribing": a one-to-one mapping of grapheme to phoneme relationship is assumed
First	Secondness to the object	Thirdness to the object, assigned arbitrarily as cultural or societal convention

Table 2-3. The three ages of letter to sounds representations in 18th century (after Peirce)

My interpretation of the 18th-century revolution of grammatisation has Spence as the ultimate system of stenography, a precursor to contemporary systems: IPA phonetic symbols are often considered to be the ultimate stage of the development of transcription. Admittedly, this is not so simple, and there have been voices criticising the Eurocentrism of the IPA, because so many symbols are also indices in European languages. A similar question loomed large over ASCII characters used to represent languages in the early days of the Internet. This is another story, the beginnings of the third revolution of grammatisation.

The point made here is that graphic signs standing for metalinguistic representations need not be in a one-to-one system (like a signifier to a signified); it is very likely that the Saussurean conception of the sign is too simplistic because it is binary. Metadiscursive representations are probably better understood within a ternary representation like the one proposed by Peirce, which gives room to a more complex theory where signs may refer to other signs (*semiosis*), not just to the idea.

3. Some fragments of a theory of semiotic regimes to account for the uses of signs

This section outlines the semiotic properties of stenographemes. I make the point that issues in stenography do not disappear with the third revolution of grammatisation; I then go on to show that a certain variability in stenography is not incompatible with a certain fixity in

meaning. It is actually a necessary condition for most stenographemes to be translatable. My last point sums up the various properties of stenography beyond translatability.

3.1. The third revolution of grammatisation

The coming of age of digital humanities has resulted in change in the processing of linguistic units. The emergence of concordancers has given rise to intermediate units such as clusters or n-grams. An all-encompassing digital processing of language is under way, whether under the guise of the digitization and automatic processing of texts or digitization of speech and its treatments. This involves not just Natural Language Processing (NLP) but also a radical change in the material manifestation of language. Ancient rolls and *incunabula* have been turned into electronic texts. Digital humanities is not just a buzzword, it is a material fact which still needs to be conceptualised. I distinguish the digital processing of languages from the automatic processing of languages. One of the important consequences is that language, as we have known it, can now be stored and analysed as a series of bits, of ones and zeros, which make it possible to use higher level programming languages such as R or Python. I would like to avoid the illusion that a digital (and possibly quantitative) approach to language would mean the end of this type of stenographic issues.

R is both a programming language (raising metadiscursive issues I will not elaborate on), and also a piece of software that takes as input linguistic data and provides various data visualization and statistical analyses. The error would consist in assuming that the advent of the computer age eliminates any stenographic issues. R scripts are not the ultimate recodification of linguistic data but just another chapter in the history of stenographic practices; in other words, it is not the end of history. Again, most scripts are translatable into other programming languages. Resorting to a given scripting language to manipulate linguistic data is not the unique and ultimate metalanguage of languages. It is one practice among others for some quantitative linguists. It has some bearing on the representation of the data under scrutiny. In some cases, the data can be recoded, for example, logical values can be turned into numbers. This is known as ‘dummy coding.’ The favourite format for linguistic data used in R is called ‘data frame.’ It consists of a table including character strings (words, clauses, clusters), numbers, possibly logical values (true/false), and metadata (sex, age, origin, region...). This tabular dataset is a specific representation of linguistic data, not in the sense that data frames are only

used for the investigation of linguistics with R, but because the structure of each dataset reflects the research question. In that sense, datasets and the digitalization of linguistic input partake of a graphic manipulation of linguistic structures, this is why I consider them to be a form of stenography, be it stenography of data science rather than stenography of linguistics. The illusion would consist in claiming that linguistic data is an unmediated representation of language.

One may question the absolute relevance of stenography for computer scripts, suggesting that most stenographic representations will consist of data visualizations. Nevertheless, having to constrain linguistic expressions with specific codes (such as backslashing symbols which have specific values in the programming language) points to another kind of metalinguistic discourse, on the constraints or limitations of the programming language itself.

I suspect that the most accurate way to describe the stenographic apparatus attached to a certain contemporary praxis of computerized linguistic analysis is to describe automatic annotation procedures. The fact that a growing concern for standardization has reached the linguistic community, as evidenced by the Text Encoding Initiative (TEI Consortium 1994) and the increasing popularity of the XML file format, does not mean the end of stenography as the intellectual activity of linguists dealing with linguistic phenomena. If anything, this whole network of common preoccupations shows how central the question of annotation is to linguists. The fact that it is automatic does not mean that it should not be the focus of attention of linguists. Quite the opposite, I would contend. This opens new avenues for research: what a piece of software does with a grammatical category when tagging texts should foster some research about the ontology of the necessary categorizations. It is a pity that the linguistic and analytical investigation of tagsets has (to my knowledge) only resulted in the AMALGAM project, headed by Eric Atwell at Leeds University,²³ which, as early as the mid-nineties, tried to compare the various existing tagsets in terms of their functional viability. Geoff Pullum, shortly after the publication of *The Cambridge Grammar of the English Language* (Huddleston and Pullum 2002), gave talks (Pullum 2006) suggesting that the tagset closest to the linguistic choices of the CGEL was the Penn Treebank (Marcus et al. 1993). An alternative way to think about these contemporary tools is to re-annotate corpora on the basis of the labels required for the analysis.²⁴ In other words, automatic labelling does not put an end to graphic ornamentation of linguistic structures. If anything, the scenery has changed. Countless debates rage about how the transcription of speech actually corresponds to embedded theory: we

transcribe spoken data in direct connection to our research questions (see Ochs 1979 and my introduction). Though annotation might be automatic, it still corresponds to a whole graphic apparatus enclosing syntactic structures and co-indexing grammatical categories. At the surface level, words are labelled according to their grammatical category, which is known as Part of Speech (POS) tagging. On a higher level, clauses and sentences can be delimited and indicated, which is known as parsing. Again, there might be various representations (dependency annotation or Treebank parsing) but the method for dealing with textual structures is still circumscribed by a lattice of graphic notations. The crucial point here is that they can potentially be converted from one to the other (from syntactic tree to dependency analyses, from the Penn Treebank tagset to the CLAWS tagset). Contemporary research on interoperability²⁵ favours the use of pivot formats such as CoNLL (Buchholz and Marsi 2006).

A point has been made about the persistence of stenographic issues in written data (or transcribed spoken data), an analogy which, to a degree, still holds for speech processing: a simplistic vision would suggest that acoustic analysis (such as the automatic extraction of pitch or vowel formants) translates the signal into numbers that can be dealt with independently of any theoretical considerations. Again, I beg to differ. Visualizing speech does not mean the end of the necessary reflection on what we see. Pitch tracking is a complex operation that requires vigilance: micromelodic effects, creaky voices, or improper settings for pitch range may result in massive misinterpretations. In addition, data may be normalized. Linguists may differ as to their representation of rhythm and as to what should be counted. The stenography of phonetics is much more statistically-based, but the emergence of (big) data does not sign the death of stenography. This is now just called 'data visualization.' Quantitative linguists even manage to disagree between themselves as to the most appropriate visualization techniques. The 'bar barplot' movement is a case in point. This movement calls for alternative representations of variability favouring boxplots rather than barplots (even with error bar). Even though the application of my concept of stenography might sound far-fetched, it just reflects on a relative heterogeneity of the relation between the linguistic data and its figuration, here in the very sense of numbers corresponding to acoustic correlates of a signal.

With the graphic representation of sounds, my analogy still holds. Linguists have invented conventions that allow for mutual understanding. The IPA in the digital age first made it under the guise of SAMPA (Wells 1995), an ASCII representation of IPA symbols deserving its name of machine-readable format. This low profile coding of the IPA gave way to

contemporary visualisations of the symbols with Unicode. This is not the end of the story. With the development of databases, linguists (and engineers) came up with alternative representations, specifically designed for machine-readable purposes. For the CELEX database (Baayen et al. 1993) a system was devised which allowed each phoneme (including long vowels and diphthongs, coded with two IPA symbols) to be noted by a single character called ‘glyph.’ Digital treatment of languages does not mean the end of the language game of graphic representations, it may just imply more technicality. The crucial property that emerges from these technological mutations is that stenographic systems need to be mutually intelligible, in other words, translatable. This is the foremost property of stenography.

3.2. Translatability without fetishism

Stenography consists in an ordered set of signs to refer to specific points in the linguistic nomenclature. They may be idiosyncratic or conventionalized, but stenographemes only make sense if they allow some reference to a specific meaning. They should not be taken too literally: the IPA symbols for closing diphthongs were changed at the IPA Kiel convention in 1993. What counts is the possibility of interpreting the meaning of the graphic sign unequivocally. In that sense, stenographemes should be translated, possibly as a whole subset, and should not be considered with what Marx called ‘fetishism.’ If I may say so, we have to allow for a figurative dimension for this literal meaning entrenched in stenographemes. Paradoxically, we have to be rigorous when using stenographemes. In other words, how can you talk about language without securing the proper way to do it? At the risk of sounding as primitive as parietal drawings of our ancestors in prehistoric caves, there is such an art in stenography which can only exist because of rigour.

3.3. Stenography as an ‘art’ with rules

There was debate in the sixties as to whether Semiotics should be regarded as a subpart of Linguistics or vice versa. Proponents of the latter claimed that language was only one system of signs among others, whereas dissenters observed that semiotics was carried out using words, therefore working like a sub-discipline of linguistics. I would like to make the point that there is getting away from this paradoxical structure, and suggest that maybe linguistics does resort to specific signs to achieve a linguistic analysis. Stenography is not over and above (*en surplomb*) the

language object but a real part of the linguistic analysis. What follows is an attempt to characterize some features of stenographemes.

1. Stenographemes point to a proper level of analysis.

Some stenographemes stem from distinction-making practises, they are tools in granularity. The difference in semiotic status should not be held against linguistic analyses but as an integral part of the analysis as such. Notational conventions do what they should: characterize the level of granularity aimed by the analysis.²⁶ They are a precondition for the analysis, more often than not, they correspond to the literal expression of a linguistic notion. My favourite example is the distinction between usage and mention: in the sentence ‘*this* has four letters,’ the italics are compulsory. Any citational form should be in italics to correspond to mention, whereas, in most cases, usage does not require this specific highlighting.

2. Stenographemes are not immutable.

There is a certain historicity in the development of conventions, as evidenced in the treaty about the history of the footnote (Grafton 1994). The point made here is that, nevertheless, semantic distinctions are made visible by typographical inventions. Those may have become conventionalized, but can still be challenged. Phonologists are a good example of how we may flout conventional notations. For instance, they may use symbols flouting the IPA conventions, in order to make a point about the relevance of a given feature. Typically, they may note the long vowel of FLEECE as /i/ (instead of the expected IPA /i:/) to make the point that length is irrelevant, they see the KIT/ FLEECE distinction as a difference in vowel quality (or advanced tongue root). In other words, they are making a statement while twisting the standard IPA convention. Ochs viewed *Transcription as Theory*, but the conflicting representations of syllables or syntactic trees do exactly the same thing. They are making a statement about what they hold to be important in their representation. (For example, in the trees representing syllables, branching onsets and codas may not be represented by several lines, but by a single one). Stenographemes assign granularity to the analysis, they are embedding linguistic conceptions in the graphic figuration.

3. Stenographemes may vary in scope.²⁷

In some cases, they refer to an indexical value, sometimes they indicate the bracketing or the scope of a given sequence.

- # corresponds to a boundary. It points to its own location. It is indexical in Peirce's sense. The location of the interpretation corresponds to the location of the symbol

- ◊ these angled brackets are meant to signal that whatever is between them is a graphic representation of the word, the spelling is the real focus.

- ° means that the following sequence is a potential word. The meaning is not indexical, but is a tag for the interpretation of the following sequence. This is probably the easiest way to decipher a diacritic: it is a metalinguistic signal for the interpretation of the following sequence. It should come as no surprise that stress marks that were noted at the end of the stressed syllables by orthoepists ended up being indicated before the stressed syllable in the IPA.

For linguistics, some symbols are in strict relation to substitution (they stand for what they represent), others should be treated as interpreters: they indicate a certain metalinguistic status (what follows is ungrammatical, a potential word, a narrow transcription, a graphic representation). There may not be specific guidelines as to why this should be the case. In other words, there is a certain amount of arbitrariness for stenographemes.

4. Stenographemes are arbitrary but necessary

I wish to make the same point Benveniste made while commenting on Saussure's conception of signs. The relation between the signifier <arbor> in Latin and the signified /tree/ is arbitrary, as commonly assumed for the Saussurean sign. But the association between the meaning and the word is also necessary, as Benveniste remarked in his famous essay. Applied to stenographemes, this means that there are not immutable, but serve a necessary purpose in pinpointing a specific linguistic meaning or level of analysis.

Conclusion

I have advocated an analysis of the figurative that has to be literal with a vengeance: as Rimbaud put it 'this means what it means, literally and in all its senses.' The interpretation of stenographemes has to be adhered to strictly and systematically, and yet within the context of the general

framework of the linguistic analysis, i.e. within the whole graphic apparatus of a given linguist. In this respect, linguistics is the art of forging/taming notational conventions and of making sure one's linguistic analyses are as explicit as possible in regards to the use of stenography. If linguistics is a discipline involving masters and disciples, one of the main goals is to teach apprentice linguists²⁸ to get these distinctions right. The art of linguistic analysis consists in trying to discipline (graphic) signs when referring to specific aspects of language and of any linguistic analysis. The variability of notational conventions does not signal the end of linguistic analyses, it calls for vigilance and distinctions. Rigour in use does not prevent the plurality of interpretations or cohabitation of homonymic stenographemes. That one text may lead to various interpretations should not be seen as scientific defeat, but may have to do with the *misprision of pragmatics*.²⁹ Similarly, stenographemes may be ambiguous but hopefully can be disambiguated when confronted with the rest of the notational conventions of a linguist. Rigour and daily attention paid to precision is the real order of the day. As the Surrealist Poet Tristan Tsara put it, 'I make it a point to live by all existing conventions, as refusing any convention would just be another away to be conventional.' Some notations get to be conventional and adopted by the linguistic community.³⁰ They correspond to a certain logic of distinction between levels. I claim that there is some dimension of conventionality in some cases that should not be altered, whether idiosyncratic or shared by a linguistic community.

Notational conventions are theory-embedded. Some notational conventions translate nicely, whereas others are terrible when compared to another theoretical framework. We have metadiscourse, the ability to produce discourses about language linguistic structures. We may investigate metalinguistic discourse, without any specific benefit over our 'object.' If we have lost our overhang (*'surplomb'*), what difference is there between a language and a foreign language? Formally, not much. Fair enough, this may suggest that linguistic frameworks might, after all, be regarded as languages of their own.

Up to a point, the question is, 'what is the difference between a stenographic elaboration and a foreign language, if translatability is held to be the most relevant property of stenography?' This raises an excruciatingly difficult question: 'what is the influence of the mother tongue, or the tongue in which we carry out a linguistic analysis on the analysis of language?' The question is much too difficult to attempt a real answer, especially in a chapter, when at least a book would be required. The title of my PhD sums up this ambiguity by addressing the French

enunciative schools for the analysis of English. The fourth chapter shows how some specific features of French, such as an overuse of nominalization (compared to English) and a potential ambiguity between processes and results undermines or characterizes some of the analyses proposed by the French linguists I investigated. Analysing linguistic input with another language provides a form of natural metalinguistic *surplomb*, and it is probably helpful to benefit from this inherent distance. However, my point would be that one has the disadvantages of your benefit: there are some properties of your metadiscursive language that show/can be seen through your analysis. I suspect the real agenda of non-native speakers of English writing in English would consist in pointing out what the English language forces them to do. As Lacan put it, 'language speaks through me,' I am not only speaking my language. In other words, some properties of my metadiscursive language will have some bearings on my representations and on my linguistic analysis.

In an ideal world, we would rejoice to read linguistic analysis in as many languages as possible. Linguistics made tremendous progress when linguists stopped believing that some languages were better than others (some neogrammarians of the 19th century distorted facts to support such debatable ideologies). It would be interesting to remember that English is not necessarily the ultimate vehicle for linguistic analyses as such. I do not necessarily share some of my colleagues' views that the untranslatability of French analyses is such that it does not make sense to try to write papers in English about the French-born enunciative theories. I am sympathetic to Barbara Cassin's dictionary of untranslatable words, where experts explained how connected to the original language the concepts under scrutiny were. Nevertheless, I believe that we should take the risk of losing some of our semantic distinctions in a lingua franca that allows debates with linguists of other mother tongues and theoretical denominations.

There might be a paradox within the two properties I am underlining: we should be able to translate a specific analysis into other languages, but, at the same time, every linguistic analysis is unique, partly because it is expressed in a given natural language. Just like stenographemes might be unique, they only make sense if they can be translated into another system. We do not have a single (logical) metatheoretical tool to describe language; we have a variety of expressive means. In that sense, the diversity of modern languages can be seen as a variety of windows over language. I am reusing a metaphor put forward by Henri Adamczewski. He repeatedly suggested that modern languages had specific markers that gave a certain perspective into the workings of language; he called these markers (such as the auxiliary 'do' in English), 'windows.' I would like to

make the claim that modern languages act as windows to potential semantic distinctions. This may sound reminiscent of the Sapir-Whorf controversy, but there might be some language-bent representations of the world, and I would like to point out that some Polish-born linguists and logicians excel at providing a specific kind of analysis. At the risk of gritting many teeth, I would even claim that we are also dependent on our native language. For instance, Jan Łukasiewicz, Alfred Tarski and Alfred Korzybski produced ground-breaking work in logic, semantics, and metamathematics. Henri Adamczewski and Katarzyna M. Jaszczolt produced very subtle distinction in the fields of modality and *de re/de se* analysis (see Jaszczolt 2013, for instance) and I like to think that this is because in Polish, some specific *de se* distinctions are encoded in the subjunctive. In other words, some linguistic phenomena may be grammaticalized in a given language, therefore, being a (native) speaker of this language sounds like a good way to have access to these semantic distinctions.

We are, thus, indebted to our mother tongue as linguists. My claim would be that Saussure's seminal distinction between *langue* and *parole* is not only different from whatever language vs. speech may express as a distinction, they are the epitome that *langue* as a word encapsulates what French refers to as *langue* (the modern language) and *langage* (the abstract faculty). I have been hoping for a conference or an edited volume inviting linguists to situate their analysis of English on the basis of their mother tongue and their favourite theoretical framework, in order to describe the possible anxiety of influence from the mother tongue in their investigation of English.

I may venture to add that any psychoanalytical equivalent to the analytical cure is some introspection on what our native language brings to the debate. To get some further teeth grinding, I would suggest that linguists should also consider the linguistic framework in which they were raised. Lacan has a specific term to refer to the psychological investment of individuals towards their mother tongue. He calls this *lalangue*. There is a heavily symbolic dimension in this representational language. I have often been struck by the unreasonable tones some linguistic disagreements could lead to.³¹ I believe this is some form of transfer, and *monmodèle* (my model) is probably lurking behind *lalangue*. There is a form of *tyrannical daemon of theory*, where linguists are dependent (hooked?) on their theoretical framework, in other words, the stenography and the conceptual apparatus in which they were born to linguistics. I have tried to suggest that there was nothing necessary in the association of language and a given set of stenographemes. It must have become obvious to the reader that I

profess a form of agnosticism as to linguistic theories, or, rather, that I have a consumer's approach to linguistic theories.³² I believe some are very strong for some specific linguistic issues, more or less just like some languages are good windows on particular linguistic facts. One of the consequences of the impossibility of an abstract formal metalanguage (of the Fregean type) is the proliferation of linguistic metadiscourses and their accompanying stenography. Just as linguistics is an investigation of the complex functioning of a great variety of languages, it may well be the case that semiotic processes (unlimited *semiosis*, as Peirce would have it) articulating language and its stenographic representations should be unbounded. The relation between language units and its linguistic stenography need not be unique, but this stenographic apparatus is required.

This may lead to a paradox. Stenography, or the ability for a linguist to own a metadiscursive system of graphic representation, has a somewhat hybrid status: it is both a consequence of a given discourse on the object and a precondition of its very object. To talk about language, you need a whole apparatus of (hopefully) conventionalized notations. Because language has it in its very structure that you can talk about it using it (this can be seen as the property of reflexivity; some linguists define languages as a system of signs endowed with self-referentiality), linguists need this kind of apparatus. There is in language this inexhaustible property of being potentially commented. Foucault calls this the 'commentary' and sees it as the inescapable destiny of what he calls the 'order of discourse.' Whether commenting on the semantics of discourse or the graphophonemics of speech, linguists need stenography.

CHAPTER THREE

METALINGUISTIC DISCOURSE IN CHOMSKYAN THEORY AND ‘REPRESENTATION’

JEAN PAMIÈS

Preliminary clarifications and aims of our study

Metalinguistic discourse

As understood here, the term ‘metalinguistic discourse’ (in the sense of Pamiès 2015, the relevant definitions of which we reproduce here for ease of reference) will be taken to cover whatever means the theoretician uses to state what he has to say

- (a) about the object he has chosen to study (including specific descriptions, generalisations or explanations offered as analysis),
- (b) about the epistemological framework he has elected to guide his investigation (including chosen specific goals, accepted idealisation, or ontological claims for posited entities and constructs) and
- (c) reflexively, about the concepts and constructs he uses to achieve his goals.

To find or devise the expressive resources needed, such metalinguistic discourse will typically draw on both

- (i) natural language vocabulary and syntax (with if needs be technical jargon vocabulary and phraseology extensions) and
- (ii) an open class of artifactual diagrams, schemas and formulas, including those of artificial languages or formal systems. Whenever a reflexive consideration of type (c) concerns a type (ii) element and is couched in a formulation using a type (ii) item, we shall say that, on the one hand, the element of type (ii) is treated as pertaining to the (artificial) object-language and, on the other hand, that the type (ii) item used in the

reflexive consideration belongs to the (by definition, artificial) meta-language.¹

Chomskyan Theory

The term ‘theory’ (in the sense of Pamiès 2001, 36) will be taken to mean a class of studies the ‘intersection’ of which is a significant set of common (b) choices for the constitution of the object of investigation. The term ‘Chomskyan theory’ will be taken to mean ‘pertaining to a theory adopting Chomsky’s leading (b) assumptions at time t’.

From 1955 to the present, the process of theory change has been so extensive and abrupt—hence the ‘at time t’ in the above formulation—that our choice of the singular (‘theory,’ not ‘theories’) calls for some justification. For instance, it is common practice to distinguish between I ‘First Model Theory,’ II ‘Standard Theory’ (ST), III ‘Extended Standard Theory’ (EST), IV ‘Revised Standard theory’ (REST), V ‘T-Model Theory’ (MT), VI ‘Government and Binding Theory’ [*aka* ‘Principles and parameters Theory’] (GB/P&P), and VII ‘Minimalist Program’ (MP)²—a proliferation of occurrences of the term ‘theory’ (in all but one of the cases) that is hardly suggestive of unicity.

However, we will continue to assume that in spite of sharp (b) inflections and spectacular (i) and (ii) modifications,³ what is referred to with deceptive terminology is properly viewed as seven stages or ‘states’ of one constantly evolving theory, definable in terms of a significant stable core of shared (b) assumptions, centred on the perceived need for (at least) two distinct non-finite levels of representations, in systematic correspondence *via* some mediating explicit apparatus for formal computation (Pamiès 2001, 36–46 and 339).⁴

Aims

Linking with previous work on (formal) representation (Pamiès 2015), the aim of this study is to shed light on what unites (or fails to unite) the motley conglomeration of intended acceptations for the term in such collocations as ‘mental representation,’ ‘cerebral representation,’ ‘theoretical representation,’ ‘symbolic representation,’ ‘semantic representation’ and ‘intentional representation.’

In order to identify what was (or is) at stake in a given inflection or disruption in type (i) use of ‘representation,’ we will focus on two or three turning points in the non-linear development of Chomskyan theory, with greater emphasis on assessing the validity of claimed epistemic/ontic

status for posited entities and constructs than on the detail of historical developments.

Organisation

In section A, we show that, given certain fundamental (b) options, Chomskyan theory (at least until stage VI) is plagued by a form of (overreach) vacuity having resisted a series of type (b) modifications involving the abandonment of the level of Semantic Representation (SR). In section B, we show that, contrary to its own type (b) commitments, Chomskyan theory suffers from another form of (empirical) vacuity; we expose the rhetorics of protean ambiguity which, stemming from a systematically confusing use of ‘representation,’ and extending to a whole network of diversely related terms, permeates the whole metalinguistic discourse of Chomskyan theory; thereby uncovering the deceptively unapparent, even obfuscated incompleteness of an underlying conceptual paradigm which we restore to full symmetry. This critical analysis leads us to argue that, for lack of a hosting theory Tmc [as defined in 31b], Chomskyan theory suffers from a type of architectural (mc-)defectivity [in a sense summarised in note 148 (1)] which in itself (with no need to invoke the aforementioned forms of vacuity) would suffice to explain why (at least until stage VI) Chomskyan theory is not justified in its demand for parity of ontological status with physics. In section C, we show that pleading the mind/brain—hardware/software analogy and inscription of Chomskyan theory in the computer model of the mind does not solve, indeed worsens its mc-defectivity problem. In section D, we show that, given a number of further type (b) contentions, the theory offered for the interface to the Conceptual and Intentional systems suffers from another type of (mCI-)defectivity [in a sense to be made precise in note 148 (4)] which precludes finding a solution to the aforementioned overreach problem. Then we argue that, though certain transcendently inferred pronouncements imply (*via* yet another type of overreach vacuity) biologically necessary empirical vacuity, there is no reason to take them seriously and that it is bad enough that Chomskyan theory should be vacuous on three accounts (*de facto* mc- and IC-defectivity, and just one type of overreach vacuity). Finally, we conclude that Chomskyan theory has become so abstract and removed from observation that it has drifted into unsupported pre-scientific speculation, and will remain so as long as no remedy is not found for its structural defectivity.

For ease of internal cross-reference, isolated as a unit regardless of required length of exposition, each logical step in the argument will be

allotted a boxed number from $\boxed{1}$ to $\boxed{66}$ as we go along. Subsections numbered 1, 2, etc—and “a,” b, c, etc. will be introduced or provided together with subdivisions and liminary and/or provisionally conclusive square bracketed summaries whenever helpful or necessary.

A. The demise of ‘semantic representation’: explicitness, metalanguage and overreach vacuity

[In this section <with background Chomskyan references ranging from stage II to stage VI>, we show (A-1) how certain type (b) and (ii) early defining options of Chomskyan theory yielded (A-2) a form of overreach vacuity conundrum or deadlock which resisted (A-3) later type (b) simplifying and reshaping rectifications involving a renunciation of the over-ambitious initial linguistic level of ‘semantic representation’ and the introduction and establishment of a more restricted level of Logical Form (LF).]

A-1. On a few theoretical features of the early states of Chomskyan theory

[This subsection <with background Chomskyan references ranging from stage II to stage V> recapitulates a few essential points. To satisfy its *sine qua non* requirement that it be explicit ($\boxed{4}$), Chomskyan theory [also known, significantly, as *Generative Grammar*] chooses ($\boxed{3}$) to resort to type (ii) formal systems generating formal representations in order to construct appropriate type (i) definitions for ‘language’ ($\boxed{1}$), ‘grammar’ and ‘structural description’ ($\boxed{2}$)—so that, as soon as the domain of investigation is extended to semantic interpretation ($\boxed{5}$) by a type (b) decision, structural descriptions must comprise semantic representations, and one of the main goals of the theory is to formally specify the sound/meaning correspondence characterising each of the infinitely many sentences generated by each grammar ($\boxed{6}$).]

$\boxed{1}$ A language L is an infinite set of grammatical (*aka* well-formed) sentences:

“*LSLT* is concerned with three fundamental and closely related concepts: language, grammar and structure. A language L is understood to be a set (in general infinite) of finite strings of symbols drawn from a finite “alphabet.” Each such string is a sentence of L ” (Chomsky [1973] 1975, 5).

2 A grammar of L must assign to each sentence of L a structural description:

A grammar of L is a system of rules that specifies the set of sentences of L and assigns to each sentence a structural description. The structural description of a sentence S constitutes, in principle, a full account of the elements of S and their structural organization. By the “*structure of L* ,” then we mean the set of structural descriptions of sentences in L . [...] To use some terminology introduced several years later, we may say that a grammar *weakly generates* a language and *strongly generates* a structure (Chomsky [1973] 1975, 5).

By a *grammar of the language L* I will mean a device of some sort (that is, a set of rules) that provides, at least, a complete specification of an infinite set of grammatical sentences of L and their structural description (Chomsky [1961] 1964, 119–20).

3 In order to achieve 2 in the precise, formal way required to go beyond the vague notions of traditional grammar, appropriate type (i) and (ii) steps must be taken:

As a “general insight,” “the idea that language can be studied as a formal system [...] [has provided] techniques [...] that have made it possible [...] to approach the traditional problems once again” (Chomsky, [1966] 1969, 6–7);

“[by resorting to] “precise formulation” and “provid[ing new] notation for grammatical description” we may “go beyond traditional grammar” and “give [...] a precise account of the units of which the sentence is composed, the manner of their combinations, the formal relations of the sentence to other sentences, and so on” (Chomsky [1961] 1964, 120, 119).

4 [Explicitness requirement]. In sharp contrast with traditional grammar, 2 must be carried out without resort to the intuitions it is meant to account for:

For a grammar to “offer explanations on formal grounds for the linguistic intuitions of the native speaker,” “it must be possible to determine what [this] grammar states about particular sentences without exercise of intuition” (Chomsky [1973] 1975, 63; [1961] 1964, 120).

Thus, since they are “intended for the use of the intelligent reader,” “a good teaching grammar or a standard traditional grammar do not provide an analysis of the qualities of intelligence that the reader brings to bear on the information presented” (Chomsky 1980, 237).

By contrast, “a generative grammar” of a language is “perfectly explicit” in that “it does not rely on the intelligence of the understanding reader but rather provides an explicit analysis of his contribution” (Chomsky 1965, 4).

Later, the same point is constantly re-emphasized: “I have always understood a generative grammar to be nothing more than an explicit grammar” (Chomsky [1995] 1996, 162), “We cannot tacitly presuppose “the intelligence of the reader” or “fail to attend to facts that had been tacitly assigned to the unanalysed “intelligence of the reader.”” “Rather, this is the object of inquiry” (Chomsky 2000, 6, 122).

‘Generative grammar’ is a standard, hallmark designation for what we are calling here ‘Chomskyan theory.’⁵ It is therefore quite revealing of the crucial importance of requirement 4 that ‘explicit’ and the key term ‘generative’ should be held to be so inseparable in meaning by Chomsky that, as he uses them, they potentially stand in a relation of mutual explication⁶:

“explicit (that is, generative) grammars” [taking for granted the other to clarify the one, Chomsky 1965, 42]; “the generative grammar of a particular language (where ‘generative’ means nothing more than ‘explicit’) [...]” [taking for granted the one to clarify the other, Chomsky 1986, 3].

5 [As of 1964/5,]⁷ the grammar must specify the semantic interpretation of each sentence:

A [...] description of a natural language seeks to determine what a fluent speaker knows about the structure of his language that enables him to use and understand [...] any sentence drawn from [its] infinite set of sentences (Katz and Fodor [1963] 1964, 482),⁸

so that “[thanks to its “semantic component”] a “generative grammar [...] determines the semantic interpretation of a sentence [by] relat[ing it *via* “the syntactic component”] “to a certain semantic representation” (Chomsky 1965, 1).⁹

6 Hence, as part of its structural description, each sentence is assigned its semantic representation(s), and the grammar as a whole seeks to systematically relate sound and meaning:

As an “attemp[t] to characterize in an explicit way the intrinsic association of phonetic form and semantic content,” “a (generative) grammar may be said to generate a set of structural descriptions, each of which, ideally,¹⁰ incorporates [...] a semantic interpretation [...] and a phonetic

interpretation,” where “a set of rules that recursively define an infinite set of objects may be said to generate that set”¹¹ (Chomsky [1967] 1972, 126 (note 12 and associated main text).

So that, given the equivalence of “*A* is a recursive set” and “there is an algorithm for recognizing ([/enumerating]) the members of *A*” (Wall 1972, 280), it follows that, at the time of *Aspects*, there is supposed to be¹² “an algorithm for enumerating sentences [and full] structural descriptions” (Chomsky 1965, 202, note 18).¹³

Besides, ‘algorithm’ is commonly defined in terms of ‘procedure,’ as in: “a procedure that is guaranteed to terminate after some finite number of steps, regardless of its input, is known as an algorithm” (Wall 1972, 280). But it is so easy to read the gist of [4] through the classic definition of a ‘procedure’ (in the intuitive, “every day sense of the word”) as “a finite set of instructions that can be executed in a completely mechanical fashion, i.e. without the use of judgement, intuition, or other capacities that “humans, but not computing machines, are generally considered to possess”¹⁴ (Wall 1972, 279–80) that it becomes immediately transparent why, given [3] and [6], ‘explicit’ and ‘generative’ should be so close in meaning.

To summarise then, technical details aside,¹⁵ a fundamental tenet of Chomskyan theory is that

“A grammar of a language [...] can be loosely described as a system of rules that expresses the correspondence between sound and meaning in this language,” in that “the grammar as a whole relates semantic and phonetic interpretations, the association being mediated by the syntactic component” (Chomsky [1969] 1971, 183; [1967] 1972, 125).¹⁶

A-2 On why those features are lethal for the theory

[In this subsection <with background Chomskyan references concentrating on stage III>, we argue that, since [7] no formal representation is legible *per se*, it follows that [8] instructions must be provided to decipher what formal representations are representations of, which [9] engenders a continuous escalation of unintelligible would-be clarifications from meta to meta-meta- etc.—which can only end by recourse to natural language instructions, hence [10], in the particular case of formal semantic representations, an inevitable breach of the explicitness requirement. So that if [11], by a type (i) decision, we use the term ‘vacuous’ for a theory the goals of which are of necessity unattainable, it follows [12] that, as long as it is characterised by [1]-[6], Chomskyan theory is doomed to (overreach) vacuity.]

A fundamental claim (with its corollaries) established at length in Pamiès 2015—to which the present work is a sequel—will provide our starting point(s) here:

7] No formal representation is legible *per se*:

“a would-be formal representation FR reduced to just an orphan *locus tenens* (or ‘place-holder’) is no representation at all, nothing but useless graphic junk” (Pamiès 2015, 6).

To give a striking illustration of 7], Pamiès 2015 concentrates on a number of formal representations such as Figures 1-1, 1-8, 1-11 and 1-12 [pp. 46, 47, 48, 48 respectively], the literate reader has obviously no chance of understanding however hard he strives, unless something is done to allow him to read what it is that they are meant to be representations of [*aka* that they stand for, *aka* that they are place-holders for, or *locus tenens* for].

8] Therefore, instructions must be provided to enable the reader to decipher formal representations:

“In a formalised theory, the notations *per se* are illegible, so that, properly speaking, a formal(ised) representation FR consists of an ordered pair (notations, deciphering instructions for those notations). In our (i) and (ii) terms,

(II) A formal representation FR is an ordered pair (LT_{FR} , DI_{FR})”)” (Pamiès 2015, 11).

“Given the deciphering instructions DI_{FR} , one has access to what the *locus tenens* LT_{FR} stands for—in our (ii) terms couched in function-like notation, one has access to $DI_{FR}(LT_{FR})$. Fully spelled-out then, an FR representation may be held to consist in not just an ordered pair (LT_{FR} , DI_{FR}), but expanded into the derivable ordered triple (LT_{FR} , DI_{FR} , $DI_{FR}(LT_{FR})$)” (idem, 235–36 (note 45)).

As an example of how it thus becomes possible to make sense of *per se* illegible place-holders (and summarising some of the results of Pamiès 2015 [subsection 6.1, 32–36 and 255–267 (notes 145–165)]) we may go back to the small set of *prima facie* enigmatic barbed-wired *locus tenens* of Figures 1-2, 1-8, 1-11, and 1-12, now extended to Figures 1-2 and 1-3 (Pamiès 2015, 46, 47 and 52). With the help of a small number of definitions—for ‘canonical tree’ and, in terms of ‘(sameness of) abstract formal objects,’ for ‘notational equivalence’,¹⁷ and once appropriate DI_{FR} deciphering instructions are provided,¹⁸ it can then be demonstrated¹⁹ that, even though they are “distinct both as tokens (concrete inscriptions) and as types (abstract graphemes and/or graphs)” (idem, *mutatis mutandis*, 238 (note 62)), the formal representations of Figures 1-1, 1-2, 1-3, 1-11 and 1-12, however graphically incongruent they might seem, form an equivalence class of notational variants (of which 1-20 is not a member) that all represent the same abstract canonical tree.²⁰

9 But to end infinite regress from abstruse to recondite specification, (minimally enriched with type (c) mention of the type (ii) constructs in need of decipherment) natural language is ultimately the only intelligible metalanguage available for deciphering instructions.

10 Therefore, in blatant contradiction to 4, the notations (*a fortiori* the formulations) of 3 cannot give access to the semantic intuitions of 5 without resorting to the kind of semantic intuition they are supposed to account for.

For instance, in Katz and Fodor (1963) 1964, 513, the sentence “The man hits the colorful ball,” syntactically analysed as “*The + man + hits + the + colorful + ball* → Sentence” is predicted to be ambiguous four ways and the formal semantic representation given for its fourth reading is

“[Some contextually definite] → (Physical Object) → (Human) → (Adult) → (Male) → (Action) → (Instancy) → (Intensity) → [Strikes with a blow or missile] → [Some contextually definite] → (Physical Object) → (Color) → [[Abounding in contrast or variety of bright colors] [Solid missile for projection by engine of war]].”

But, even without having to object to the need for ultimately natural language, hence circular instructions to decipher such opaque type (ii) devices as round and (embedded) square brackets or (chains of interspersed) occurrences of ‘→,’ the gross notational trick of rewriting all sorts of (inflected) words, phrases and sentences of the object-language in italics [for both round-bracketed “markers” and square bracketed “distinguishers,” with capitalised initial letters for all word components only in the case of semantic markers] relies too heavily on the raw semantic intuitions of the literate reader to be of any use in a would-be explicit representation of meaning. The point is so obvious that David Lewis, with scathing irony, famously dubbed “Markerese” the type of pseudo-formal metalanguage used in this kind of representations of meaning, which he derided as just about as useful as “a translation in” Javanese or “Latin” (Lewis 1972, 169–70).

If by type (i) choice we decide (Pamiès 2001, 33) that

11 a theory is vacuous when it assigns itself goals which can be shown to be necessarily unattainable,²¹

then the unpalatable conclusion follows that

12 a theory abiding by **1-6** is doomed to vacuity.

Chomskyan theory is arguably vacuous in our sense since,²² though it purports to account formally for (aspects of) semantic competence, any formal account it could conceivably offer is doomed to rely on the semantic “intelligence” of the reader.

A-3 On some theoretical distinctions and restricting idealisations that failed to solve the overreach vacuity problem

[In this subsection <with background Chomskyan references ranging from stage II to stage VI>, we show that—even though three type (b) moves were taken to discard **(13)** anything to do with use of language or actual (processes of) comprehension or production *via* a ‘competence’/‘performance’ dichotomy; **(14)**, anything messy, quirky, hazy or exception-laden *via* a ‘core’/‘periphery [and (inclusively) markedness]’ distinction; **(15)** anything not strictly tied to structural aspects of meaning (discourse, reference, word meaning, entailment, synonymy, (appropriateness to) situational context, speaker’s intentions, presupposed common beliefs, rhetorical or stylistic manipulations, etc.) *via* a ‘[linguistic level SR of] semantic representation’/‘L[ogical]F[orm]’ distinction [and associated abandonment of the henceforth linguistic level SR]—(15c), this did not prove enough to solve the overreach vacuity conundrum.]

In an effort to alleviate the threat (and dodge a host of related, more easily acknowledged problems [cf. Pamiès 1986]), first

13 A distinction was introduced²³ between ‘competence’ and ‘performance’ (off with problems of use, in an extended/distended sense):

13a – Competence versus performance

We [...] make a fundamental distinction between *competence* (the speaker-hearer’s knowledge of his language) and *performance* (the actual use of language in concrete situations) (Chomsky 1965, 4).

On the basis of this distinction, Generative Grammar can be more restrictively redefined as “a model of competence,” “a characterization of the intrinsic tacit knowledge or competence that underlies actual performance” (Chomsky 1965, 140), with two joint aims: “correctly describ[ing] its object, namely the linguistics intuitions—the tacit

competence—of the native speaker” (“descriptive adequacy”) and explaining how this competence can be acquired “on the basis of primary linguistic data” (“explanatory adequacy”) (Chomsky 1965, 27, 25).

With this restrictive redefinition of its object,

13b – Performance, the usual acceptance of ‘use’ and idealisation

taking ‘use’ in its straightforward sense, Generative Grammar leaves out of consideration, as pertaining to the (as yet unavailable) “theory of performance” (Chomsky 1965, 10) the semantic import of such “stylistic” “reordering” as “inversion” (idem, 17) and such “deliberate “deviation from rules as stylistic device” (idem, 15), and, more generally, of any such rhetorical manipulations as innuendo, cultivated *double entendre* or tongue-in-cheek effects [we return to the exfiltration/discarding of pragmatics in [15](#)].

But much more importantly,

13c – Performance, the distended acceptance of ‘use’ and idealisation

by surreptitiously conflating ‘use’ and ‘processing,’ Generative Grammar similarly discards all issues of speech production and perception:

a generative grammar is not a model for a speaker or a hearer. It attempts to characterize in the most neutral possible terms the knowledge of the language that provides the basis for actual use of language by a speaker-hearer. When we speak of a grammar as generating a sentence with a certain structural description, we mean simply that the grammar assigns this structural description to the sentence. When we say that a sentence has a certain derivation with respect to a particular generative grammar, we say nothing about how the speaker or hearer might proceed, in some practical or efficient way, to construct such a derivation. These questions belong to the theory of language use—the theory of performance. No doubt, a reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer’s knowledge of the language; but this generative grammar does not, in itself, prescribe the character or functioning of a perceptual model or a model of speech perception (Chomsky 1965, 9).

By the same token, because “grammaticalness belongs to the study of competence” “while” “the study of acceptability belongs to the study of performance” (idem, 11), Generative Grammar no longer has to worry about any of the “many factors” which apart from grammaticalness “determine acceptability” (idem, 10); such as varying degrees of “appropriate[ness]” of “true, false, amusing, unintelligible, meaningless,

trivial, etc” grammatical sentences “on particular occasions” (Chomsky [1973] 1975, 7), degree of inebriation, “memory limitations, intonational and stylistic factors such as ‘iconic’ elements of discourse [...] and so on” (Chomsky 1965, 10).

13d – Performance, the distended acceptance of ‘use’ and empirical immunity.

The reason why 13b is particularly important is that it provides a convenient rationale in support of a demand for substantial empirical exoneration. As reconstructed by Johnson-Laird, the justification it allows could run as follows:

“The results of psychological experiments, like any other facts, are of interest only if they bear on explanatory principles.” They have otherwise “no intrinsic interest—other than as curiosities to students of natural history” and the sheer “accretion of experimental data” is meaningless (Johnson-Laird 1987, 148).

Therefore, the emancipating vindication would go, as a theory of competence, Generative Grammar is indeed “part of cognitive psychology” (Johnson-Laird 1987, 147; Chomsky [1973] 1975, 37). But it is neither interested in the “suicidal” task of “predicting behaviour” (Johnson-Laird 1987, 148), nor, crucially, accountable for/empirically refutable by the “results” of “experimental investigation of [actual] perception and production” (idem, 154), which, though they tend to be the norm in non-Chomskyan psychology, pertain rather to the theory of performance. So that, Q.E.D., Generative Grammar need not be saddled with the harassing strictures (and hazards) of lab protocols and experimentation.²⁴

At any rate, when full account is taken of 13a-c, the net simplifying result is that, in its quest for descriptive adequacy

“a generative grammar” is entitled to “abstrac[t] away from [the] many factors that interweave with tacit competence to determine actual performance” (Chomsky 1966, 75, note 2).

Furthermore,

14 a distinction was introduced²⁵ between ‘core’ and ‘periphery’ (off with untidy data).²⁶

14a – Core *versus* periphery

Technically speaking [within the Principles and Parameters framework of state VI of Chomskyan theory]

A core grammar [...] is determined by fixing the parameters of UG in one or another of permitted ways (Chomsky [1979] 1981, 126).

But “what is actually represented in the mind of an individual” is “a core grammar with a” “large periphery of borrowings, historical residues, inventions” (ibid.), “odd facts, exceptions and quirky” “marked elements and constructions” (Riemsdijk and Williams [1986] 1987, 175; Chomsky [1979] 1981, 126).

14b – Periphery and markedness

In the above, “marked structures,” though part of the ‘periphery,’ may be “related to the theory of core grammar by such devices as relaxing certain conditions of core grammar, processes of analogy in some sense to be made precise, etc.” (idem, 127).

With such “relatively marginal” “marked structure,” “judgments tend to vary, and there appear to be differences in judgment depending on lexical choices.” “In general, this seems to be a rather hazy area and the construction[s] appea[r] to be rather unusual”²⁷ (idem, 144).

14c – Core, periphery, idealisation and self-assertedness.

In terms of this new core/periphery distinction, another simplifying type (b) decision is henceforth that an explanatory theory of competence should concentrate on (the acquisition of) core grammar and ignore the periphery: “we [...] should not want to incorporate [the periphery] within a principled theory of UG” (idem, 126).

If one had to choose between ‘artifact’ and pristine ‘reality,’ since “the reality of what a particular person may have inside his head” is an unsorted mixed bag, one would expect that the real thing is the messy ragbag, and the artefact the theoretical ‘construct ‘core grammar’ postulated to sort it out. But the confidence of Chomsky in his theory is such that, *via* type (i) stretching of the usual acceptance of the terms past breaking point, he does not consider “another point of view” according to which the reality is the artefact as untenable: “what a particular person has inside his head is some kind of artefact resulting from the interplay of many idiosyncratic factors,” while it is the artefact that is “the more

significant reality” (ibid.). We expatiate on issues of realist stance and degrees of epistemic confidence in [22] *infra*.

But above all

[15] A level of ‘Logical Form’ (LF), restricted to “purely linguistic semantic aspects of meaning,” was introduced (off with the huge domain of non-“structural meaning”).

15a – On idealised LF as originally supplementing the pre-existing level of representation SR.

LF was first presented as an intermediate level linking onto fuller semantic representations:

‘logical form’ (LF) I use the latter term to refer to those aspects of semantic representation that are strictly determined by grammar, abstracted from other cognitive systems (Chomsky 1977, 6).

The logical forms [...] are subject to further interpretation by other semantic rules [...] interacting with other cognitivestructures, giving fuller representations of meaning (Chomsky 1975, 105).

“Given the logical forms generated by sentence grammar, further rules may apply. [E.g.] [...] further rules of reference determination [for pronouns] may involve discourse properties [...] in some manner; and they interact with considerations relating to situation, communicative intention, and the like.²⁸” “Other semantic rules apply, interacting with rules belonging to other cognitive structures, to form some fuller representations of “meaning” (in some sense)” (idem, 104, 105).

15b – On idealised LF as supplanting the henceforth extinct former level of representation SR.

Making use of the concept of modularity,²⁹ the level of ‘semantic representation’ was abandoned altogether,³⁰ and only LF remained as a level of representation of (structural) meaning (only), which not only

“abstract[ed] away from [...] pragmatics” and considerations of “presupposed common beliefs, situational context,” intentions of the speaker, “rhetorical force, [or] appropriateness conditions,” but also from “reference,” word meaning and “[*pace* Katz], analyticity, entailment and synonymy” [for the second quotation, Chomsky in Parret ed. 1974, 39; for the others, Riemsdijk and Williams [1986] 1987, 183).

15c – On LF, for all its idealisation, as no less vacuous than SR.

However, in spite of (in this respect) off-the-mark observations by Riemsdijk and Williams,³¹ this reduction of broader semantics to a much narrower level of LF discarding word meaning was not enough to remove all threat, because “the logical language” it “postulate[s]” “as the representation of the “structural meaning” of sentences”[*ibid.*] remained open to charges of vacuity (in the sharpened senses of note 21 *supra*).

For instance (many details aside), at stage VI in the development of Chomskyan theory (LGB) we are told that there are “well-formedness conditions on LF,” that are “specific to natural language and have no counterpart in standard predicate calculus theory”; one of which is the “Specified Subject Condition” SSC, which states that “an anaphor in the domain of a subject must not be free in the smallest S containing that subject (i.e., the S immediately dominating that subject.” Now, since LF-theory is supposed to be “empirical,” on pains of being vacuous₃, it must be liable to empirical support or refutation. In compliance with this requirement, then, we are told (Riemsdijk and Williams, *idem*, 189–191, with a., b., d., e., d.’ our relabeling for 9a, 9b, 10b, 10a (respectively); and c., a.’, b.’ and c.’ our added examples) that SSC “unites the following kinds of examples [with and without NP-movement, respectively:

- a. *John_i was believed [_S Mary to have seen e_i]
- b. *John_i believes [Mary to have seen himself_i]_S”

and applies to

- c. Mary_j believes [e_j to have seen John]_S,

but not to

- d. “[each of us_i] [someone_j [e_j wants PRO_j to see e_i]]”

But to appreciate whether or not those examples comfort the SSC claim, we need, first, to read into a.-d. not only which candidates for the status of sentence are predicted to be ill-formed, i.e.

- a’. John was believed Mary to have seen
- b’. John believes Mary to have seen himself

and which are predicted to be well-formed, i.e.

- c'. Mary believes to have seen John
 d'. Someone wants to see each of us,

but also [to make sense of a.-d.' as a coherent paradigm] what they are claimed to mean as well as what they would supposedly mean if they were not ill-formed. Then, but of course only then would we be in a position to confront those predictions with our native speaker intuitions: if they are in agreement, the empirical claim is comforted, if they clash, it is not (and something, somewhere, somehow needs looking at in the theory).

However, as is particularly obvious with the claim that representation d. (as containing no relevant anaphor, hence not falling under SSC) is an adequate tool to predict the syntactic well-formedness (*aka* grammaticality) of d', (and ignoring the empty use of standard orthographic object-language words as pseudo elucidating formal representation of their meaning), a.-d. are couched in double-Dutch Markerese notation like (co-) indices '*i*' and '*j*' and empty category '*e*' or 'PRO' which are *per se* illegible for the literate reader unless natural language Deciphering Instructions are supplied (e.g. using the wording of SSC), which in turn cannot be understood without "ultimately"³² resorting to native speaker sum total tacit semantic intuitions, i.e., in particular, without resorting to the semantic intuitions about 'structural meaning' which the theory of LF is supposed to account for—in flagrant infringement of explicitness requirement [4], which states that "theories and grammars [...] should be interpretable without recourse to intuition" (Johnson-Laird 1987, 148).

So that (in the terms of note 21 *supra*) it is only by falling into vacuity₁ that a theory of LF may seem to escape vacuity₃.

Of course, the theory has since evolved [for instance, (co-)indexing has been abandoned (cf. Chomsky 2005, 13)], but essentially the same diagnosis of infringement of explicitness requirement [4] can be made at all stages in the development of LF theory. We return to LF and its SEM avatars in C.

[**To summarise**, in this first section we have shown that, in spite of the idealisation from SR to LF, given its type (b) decision to resort to formal tools, ambition to account for meaning assignment, and commitment to an explicitness requirement, Chomskyan theory (up to stage VI) remains plagued with a form of inescapable overreach vacuity due to the unintelligibility *per se* of (the *locus tenens* of) formal representations.]

B. ‘Bifurcation’ revisited: mental, corporeal and theoretical representations, methodological dualism and chronic empirical vacuity

[In this section <essentially ranging over stages III-VI of the theory>, leaving aside unresolved issues of overreach vacuity, we show <in B-2, and *via* disambiguating type (i) notations> that, given a number of type (b) contentions <stated in B-1, and among them, crucially, 16c>: – incidentally, the four-way polysemy of the type (i) term(s) ‘represent(ation)’ is revelatory of a systematically confusing (only partially acknowledged and vindicated) fourfold ambiguity which permeates the whole metalinguistic discourse of Chomskyan theory and rhetorically induces a deceiving impression of self-evidence; – essentially, that the four-acceptation paradigm of this revealing term is incomplete, that when its intrinsic symmetry is restored, a consideration of the missing two pieces in the puzzle is enough to demonstrate, [provided 16c and note 35 are taken seriously, without in any way endorsing the bifurcation thesis], that it is simply because of its mc-defective architecture [as defined in 31c and note 148 (1)], relative to its own type (b) claims and expectations [and because of its chronic empirical insolvency (16d)], that Chomskyan theory cannot (yet) reach parity of ontological status with physics.]

B-1 On the Chomskyan vindication of parity of ontological status with physics: some key background assumptions

[In this subsection, we review the way Chomskyan theory officially endorses the ‘Galilean style’ of scientific investigation (16a-c); conceives of the relations between would-be *res cogitans*, mind, body and brain, inobservable interior mental objects, largely unknown physical mechanisms, and abstract characterisations (17–21); endorses a variety of scientific realism (22); and finally (23), rejects the bifurcation thesis—and we show in passing (16d) that, in infraction with 16a, Chomskyan theory is afflicted by a form of (not inconceivably curable) *de facto* empirical vacuity (or insolvency) due to the chronic unfalsifiability of its major contentions.]

In Rules and Representations 1980, Chomsky:

16 endorses “the Galilean style” of scientific investigation

[At this juncture in the argumentation, once the Galilean style has been defined in terms of idealisation and refutability (16a), we show that

Galilean style idealisation is already at work in [13]-[15] (16b), and that, though Galilean style refutability is indeed officially endorsed by Chomsky (16c), it can arguably (16d) be deemed (provisionally) *de facto* empirically insolvent—*aka*, in the terms of note 21 *supra*, *vacuous*.]

16a – Defining ‘Galilean style.’

“The Galilean style” of scientific investigation³³ [*aka* “methodological naturalism”] is characterised by the “construction of abstract explanatory theories” and “models” “that may involve substantial idealisation”³⁴ (Chomsky 1980, 8, 11, 9; Chomsky 2000, 76), and are in principle permanently subjected to the “test” (Chomsky 1975, 172, 174) of empirical corroboration/refutation [*aka* confirmation/disconfirmation].³⁵

16b – On Galilean style idealisation as already at work in [13]-[15].

Idealisation was already at work in [13], since the less concise, celebrated definition of ‘competence’ (Chomsky 1965, 3) reads as follows:

Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random and characteristic) in applying his knowledge of the language in actual performance,

thus leaving out of consideration many features of the “complex[ity]” of “the real world of overlapping styles and dialects” (Chomsky [1973] 1975, 52, note 66).³⁶

Similarly, Galilean style idealisation was already at work in [14], for essentially the same reason, since, “core grammar [being] an idealization,” “it is hardly to be expected that what are actually called ‘languages’ or ‘dialects’ or even ‘idiolects’ will conform precisely or even perhaps very closely to [...] what I [...] call ‘core grammars.’ This could only happen under idealized conditions that are never realized in fact in the real world of heterogeneous communities” (Chomsky [1979] 1981, 126).

Finally, Galilean Style idealisation was also at work in [15], if only because of the “importan[ce] to the notion of ‘logical form’ (LF) [of] the notion ‘sentence grammar’” [- where] “sentence grammar [*aka* generative grammar aiming for descriptive adequacy] is the theory of sentences as [idealized] objects, [and] not” of “such things” as “their [actual] uses in

larger frameworks such as discourse or logical argument” (Riemsdijk and Williams [1986] 1987, 184).

16c – On Chomskyan theory as officially subjected to Galilean style empirical accountability.

Concerning the Galilean/Popperian refutability requirement, it is clearly doubly endorsed by Chomsky:

It is the case for Chomskyan theory as a theory of competence aiming at descriptive adequacy, since “a grammar determined by a linguistic theory (given data) constitutes a hypothesis concerning the speaker-hearer’s knowledge of his language and it is to be confirmed or disconfirmed in terms of empirical evidence drawn, ultimately, from investigation of the linguistic intuitions of the language-user” (Chomsky [1973] 1975, 37).³⁷

It is also the case for Chomskyan theory as a theory of the acquisition of competence, since “the general theory, now regarded as an explanatory theory, is likewise to be understood as a psychological theory that attempts to characterize the innate human ‘language faculty,’ and that can be tested in terms of its consequences in particular languages” (ibid.).

16d – On Galilean style empirical refutability as *de facto* unfeasible.

However, this twofold endorsement is assorted with two provisos which, given the state of the art, have rather epistemologically upsetting implications.

First, for theories of competence, “it must be borne in mind that the general rules of a grammar are not invalidated by the existence of exceptions. Thus [...] the generalization that relate Manner Adverbials to passivization³⁸ [is not] invalidated by the fact that certain items must be listed, in the lexicon, as conflicting with this generalization [...]. The generalization is [...] invalidated [...] only if a more highly valued grammar³⁹ can be constructed that does not contain it”⁴⁰ (Chomsky 1965, 218, note 28).

Second, for theories of competence acquisition, a given theory of UG “would be refuted by the occurrence of a natural language [a descriptively adequate grammar for which would be] outside the scope of the theoretical parameters (Johson-Laird 1987, 153).

But, as frequently acknowledged by Chomsky, “there is no complete account of the syntax (and *a fortiori* the semantics) of any natural language” (idem, 154).

So that it ultimately follows from the above two provisos and this further premise that for generative grammars (as both descriptive and explanatory accounts), empirical refutability, though officially an epistemological requirement, is *de facto* an impossibility—for chronic want of the required piece(s) of counter-evidence. In other words, Generative Grammar remains vacuous₃ in the sense of note 21 *supra*, with the unpalatable conclusion that empirical accountability is more of an empty promise than a threat for Chomskyan theory.

17 rejects Cartesian dualism (no *res cogitans*),

Though he professes himself to be a continuator and reviver of Cartesianism, and though he is well aware that both creativity and the postulation of a mental substance are indissolubly part and parcel of Cartesian philosophy,⁴¹ Chomsky, in his customary pick-and-choose eclectic way retains the former but rejects the latter.⁴²

We may speak of the abstract study of human intellectual capacities and their functioning as the study of mind, without thereby implying that there is a *res cogitans* as a “second substance” apart from body (Chomsky 1980, 227).

As a consequence, in sharp contrast with staunch dualist views rejecting it as meaningless,⁴³ and *pace* Descartes, the question of the relation between the mental and the cerebral becomes a perfectly legitimate scientific issue, the above quotation leading naturally to “We may also attempt to investigate the physical basis of mind insofar as this is possible” (*ibid.*),⁴⁴ thus opening the way for the next stages in our argumentation, whereby Chomsky

18 claims that “the study of the human mind” may be viewed as “actually being a study of the body—specifically the brain—conducted at a certain level of abstraction” (Chomsky 1980, 31),

19 “posit[s]” “the existence” of “inobservable” “mental interior objects” (Bouveresse, in Parret (ed.) 1974, 307, for the second quoted excerpt); Chomsky 1980, 13, for the others),

20 in conformity with **17**, refuses to consider that positing such objects in any way implies “the existence of entities removed from the physical world” <thus claiming that the interior objects of **19** do not partake of a second substance [*res cogitans*] apart from the body>:

positing “interior mental objects” does not imply the existence of non-physical entities, as I am using these terms (Chomsky 1980, 5, 257, note 21),

[21] <in application of [18]> takes it that, underlying all such “interior mental objects” of [19] as “mental images” and “mental computations” (idem, 14) are “certain physical mechanisms, as yet almost entirely unknown,” for the “properties” of which they help provide “abstract characterization” (idem, 5),

[22] subscribes to a variety of scientific realism:

[there is no reason why we should “reject”] “the normal realist stance of other branches of empirical inquiry” (Chomsky 1980, 78 and, in substance, 203).

[At this juncture in the argumentation, to disentangle the issue, we provide some background definitions and references (22a); then, using type (ii) notations to factor out recurrent (dis)similar features of argument structure (22b), we uncover seemingly erratic epistemic fluctuations and contradictions and make sense of them in terms of the more or less (ir)resistible appeal of the transcendental (22c-g); only then to, finally, manage to get to the gist of the matter (22h) in terms of ‘suspension of disbelief.’]

22a – On defining ‘(varieties of) scientific realism.’

First then, summarizing what the word ‘normal’ is alluding to here, it is commonly assumed that to subscribe to (some variety of) scientific realism is to give (variously qualified) positive answers to all of the following philosophical⁴⁵ questions:

- (1) Does a mind-independent, out-there world exist *per se*?⁴⁶ If so, does it have, not only observable, but also inobservable objective properties⁴⁷ [in which case (either way) there is a fact of the matter⁴⁸];
- (2) Can science conceivably be non-fictitious? Can its statements conceivably be factually true? Can its constructs conceivably correspond to objectively existing features of the out-there world?⁴⁹ [if so, science can be seriously taken to be a worthy attempt at accounting for what the fact of the matter is];

- (3) In the affirmative, are (if not all, at least some) scientific accounts not only conceivably, but actually successful (if not completely, at least by way of a significant step in an ever-closer process of asymptotic approximation) in their attempts at unveiling what (parts of the) world, with its inner workings and properties, really are? [if so (at least in some cases) scientific accounts do unveil what the fact of the matter really is].⁵⁰

22b – On the strategy used to expose Chomsky’s fluctuating epistemic stance.

In the light of the above clarification, Chomsky’s stance appears to have been somewhat unstable, in that his way of qualifying his affirmative answer to (3) seems to have somewhat wavered between various shades of optimism, if not pessimism.

To show this, we shall call on a few of the various ways Chomsky has relentlessly argued for basically the same two claims P on the basis of essentially the same two alleged facts Q .

More precisely, we shall examine the varying ways two sets of claims – $P' = \{p1, p3, p4, p5, p7\}$ ⁵¹ and $P'' = \{p2, p6\}$ ⁵² [each separate set comprising more or less equivalent (re)formulations/elaborations of one and the same theoretical contention]—and two sets of facts— $Q' = \{q1, q4, q5, q6, q7\}$ ⁵³ and $Q'' = \{q2, q3\}$ ⁵⁴ [each separate set comprising (identical or) more or less equivalent (re)formulations/elaborations of one and the same allegedly factual statement]—have been argued by Chomsky to enter into (one-many, many-one) relations of inference.

The instability in point is then clearly apparent in the varying epistemic status claimed for the inferred conclusion P of the argument.⁵⁵

22c – On Chomsky, transcendental ‘proof’ or inference and epistemic over confidence.

In certain cases, P is claimed to be beyond doubt. For instance, by transcendental argumentation,⁵⁶ P' [declined as $p1$] is argued to be the only possible *explanans* (aka sufficient condition)⁵⁷ for Q' [declined as $q1$].⁵⁸ Furthermore, a counterfactual claim of the form $\sim P' \rightarrow \sim Q'$ is invoked to show that, besides being the sole sufficient condition for Q' , P' [declined as $p7$] is also a necessary condition for Q' ⁵⁹ [declined as $q7$].⁶⁰ So that, in what may be reconstructed as a form of transcendental proof,⁶¹ it would emerge that P' is argued to be the sole necessary and sufficient condition for Q' .⁶² In such cases (as again for the avatar $p2$ of P'),⁶³ the

modal qualification for the inferred *P* is typically some near paraphrase of ‘it must be the case that’ *P*.

22d – On Chomsky, inference to the best explanation and well-tempered epistemic confidence.

In other cases, *P* is claimed to be not beyond doubt, but only plausible.⁶⁴ For instance, *p4* [declining *P*] and *p6* [declining *P*] are each argued to be the best explanation for *Q*’ [declined as *q4*⁶⁵ and *q6*,⁶⁶ respectively]. In such cases, the modal qualification for the inferred *P* is typically some near paraphrase of ‘it may be the case that’ *P*.

22e – On Chomsky, ‘muffled transcendental inference,’ and half-hearted renunciation of transcendental over confidence.

Furthermore, and most strikingly, a weirdly convoluted way is found in other cases to force the cohabitation of the usually mutually exclusive presence and absence of doubt. In such cases, for instance that of *P*’ [declined as *p5*⁶⁷ and *p3*⁶⁸], the modal qualification of *P* is typically some near (and of course more elegant) paraphrase of the transparent but clumsy ‘it may be the case that it must be the case that’ *P*.

Confronted with the temptation of relishing the sharp bite into certainty of transcendental proof and/or argumentation, these modal contortions reveal such tensions between an effort to resist the temptation and a certain reluctance to curb it (if not a lurking insistence on keeping in reserve, as it were, the shadow of a potential for indulging in it) that we shall talk in such cases of ‘muffled transcendental’ argument(ation) and inference.⁶⁹

22f – On inference to the best explanation as Chomsky’s well-composed, official stance.

But, for all this modal wavering and intermittent wandering off the epistemological track, Chomsky is of course well aware that, since there may both be more than one logically unrelated⁷⁰ sufficient conditions [aka *explanans*] for one *explicandum*,⁷¹ and more than one logically unrelated necessary conditions [aka *explicanda*] for one *explanans*,⁷² neither abductive, nor transcendental inference⁷³ nor Kantian-like (so-called) transcendental proof,⁷⁴ are logically compelling as appealing as they may be.

Predictably then, in his occasional explicit type (b) metalinguistic comments on attainable modes of investigation, he clearly admits that

even in such crucial cases as “the argument from poverty of stimulus” [inferring, for instance, P from Q], “the argument is of course nondemonstrative. It is what is sometimes called an inference to the best explanation, in this case that what the stimulus lacks is produced by the organism from its inner resources” (Chomsky 1980, 267, note 29 and 36).

Thus, though one may suspect that this is just attitudinising,⁷⁵ and that transcendental modes of reasoning have always provided the fundamental heuristic impulse guiding Chomsky’s research program for better or worse,⁷⁶ the fact remains that Chomsky has officially abjured the overconfident affirmative answers to (3) that he himself has more than once indulged in.

22g – On Chomsky, refutability and bend-over-backwards epistemic scepticism.

But this meandering course from the certain to the plausible to the plausibly certain and back to the merely plausible has not yet completed our peregrinations here since, in other cases, instability verges on sheer paradoxical contradiction, with the way Chomsky sets the epistemic cursor touching on downright scepticism. Thus, he confesses that “in the natural sciences, when a theory is devised in some [...] domain” “we expect that” “even if [it is] on the road to truth,” “the theory is probably false” (Chomsky 1980, 104). Concerning, for instance, the “suppos[ition] [...] that [in substance, 6] is false,” in that “neither FL [=Faculty of Language] nor any other system of the mind/brain involves ‘semantic representations’ [...], the specific knowledge that the child has acquired and uses [being] represented in the mind/brain somehow, but not in the manner developed in studies of natural-language semantics, now cutting a very broad swath,” he admits that “this is not unlikely,” and he even envisages that “current phonetics may also turn out,” not just to fail to be true, but “to be wide off the mark” (Chomsky 2000, 186).

22h – On Chomsky’s scientific realism as of the ‘suspension of (ontological) disbelief variety.

Finally, to reconcile such apparently irreconcilable views, it is crucial to note that in the context of Chomsky’s essentially affirmative answer to (3), “normal” means ‘conventional,’ in that for members of the community of “scientists,” the norm is to conform to “its **conventional** realist assumptions” (Chomsky 1975, 171). In other words, though we all know that scientific theories cannot be (quite) true, one is expected to pretend that they are.⁷⁷ In that sense, at the end of the day, Chomsky’s variety of

scientific realism turns out to be of what we shall call the ‘suspension of disbelief’ variety.⁷⁸

Chomsky furthermore

23 rejects as “entirely unwarranted” “what Hockney (1975) has called the “bifurcation thesis” [*aka* “methodological dualism” (Chomsky 2000, 76)], that is the thesis that theories of meaning, language and much of psychology are faced with a problem of indeterminacy that is qualitatively different in some way from the underdetermination of theory by evidence in the natural sciences” (Chomsky 1980, 16).

23a – The bifurcation thesis sceptical attack on Chomskyan theory.

As made clear in Putnam’s reformulation, the bifurcation thesis stems from the extension to a more general indeterminacy thesis⁷⁹ of Quine’s sceptical views on the radical indeterminacy of translation.⁸⁰

“As Putnam insists, the thesis of “indeterminacy of translation” holds as well in any domain of psychological description. [Quine’s] “main point,” he writes, is that “indeterminacy of translation is equivalent to indeterminacy of the transition from functional organization (in the sense of machine table, or whatever) to psychological description” (Chomsky 1980, 15, quoting Putnam [1976] 1978, 49). From then on, since “there is no fact of the matter,”⁸¹ “there is no sense to the construction of a theory of language and mind that tries to establish that [its mechanisms, for instance] rules of grammar assign [any of its constructs, for example] phrases in one or another way in mental representation” (Quine 1969, as subsumed in Chomsky 1980, 15)—so that, as a theory of mind and mental representation, Chomskyan theory is held to be devoid of ontological value.

To make matters worse, while Quine (1972, quoted in Chomsky 1980, 257 n25) regrets that “this indeterminacy of translation is unsuspected in mentalistic semantics,” Putnam (1976, 50, quoted in Chomsky 1980, 16) charges Chomsky with “ignor[ing] the existence of ... pairs of equivalent descriptions” and [“elsewhere,” quoted *verbatim* and in substance (respectively) in Chomsky 1980, 17 and 23] goes as far as to condemn the fact that “psychological explanations cannot be given “anything like a ‘scientific proof’” and to claim that the “study of psychology requires a uniquely holistic approach rejecting idealization [to any] subdomain.”

23b – Chomsky’s rebuttal of the bifurcation thesis.

Chomsky’s scathing counter-attack (correcting in passing Putnam (Chomsky 1980, 258 n27) for making reference to just “pairs of equivalent descriptions” where it would have been “more to the point” to mention that “there are sets of non-equivalent descriptions compatible with all available evidence”) is to retort [subsuming the essential points made in 16a, 22h and notes 33–35, 77 and 78]—incidentally, that (Chomsky 1980, 257 n25), *pace* Quine, “the advocates of ‘mentalism’” in semantics and elsewhere whom Quine seems to have in mind do not reject the thesis that theories are under-determined by evidence, “but regard it as obvious,” since (Chomsky 1980, 21) if they were not “they would have no interest at all”—more fundamentally, that none of the “natural sciences could withstand such criticism” since “no reason has been advanced for [the] belief” that “the criteria of ‘scientific methodology’ [(with or without idealisation) could] offer ‘scientific proof’ that avoid the underdetermination of theory by evidence” (Chomsky 1980, 17)—and, crucially [referring back (Chomsky 1980, 267 n24) to Chomsky 1975, 179–204], that “beyond” the triviality of common-lot underdetermination, “nothing [qualitatively different in any way] follows [for mentalist theories] and that Quine’s efforts to show otherwise had not only been futile but lead to internal inconsistency.”

Chomsky’s conclusion, then, is that (Chomsky 1980, 164)

“to reject the bifurcation thesis” is simply “to maintain the normal assumptions of rational inquiry, as in the natural sciences,” and therefore to hold (Chomsky 1980, 141) that “it is reasonable to study the human mind and its products in the manner of the natural sciences, rejecting a curious dualism [...] that seems [...] to have considerably less warrant than the metaphysical dualism it succeeded. I have in mind the several variants of the bifurcation thesis and the general wariness over attribution of inner mechanisms of mind [...], and in general, the unwillingness to [...] understand and interpret” “our theories” “in the way [16a] we take for granted [22h] in the natural sciences,” in other words, the refusal to admit that (Chomsky 1980, 71) we “should” “regard the grammar we postulate as a component of the mind, then seek to test this hypothesis in whatever way we can [and] take the normal “realist” stance towards our theoretical constructions.”

To summarise this rejection of Quinean ontological ostracism, Chomsky’s final word is then that, since “no argument at all has been presented and [...] the bifurcation thesis leads to contradiction within Quine’s systems,” “the conclusion that there is [...] “no fact of the matter”

” [...] seems entirely unwarranted, particularly if one is unwilling to draw the same conclusion for physics on the same grounds”⁸² (Chomsky 1980, 16 and 258 n26).

From which we derive what we take to be Chomsky’s challenge:

23c – Chomsky’s challenge.

Granted that non parity of ontological status between Chomskyan theory and physics could only be established by parity of valid argumentative treatment of those two theories; and admitting that Chomsky’s tacit assumption is that no one form of Galilean-abiding argumentation can be such that while respecting parity of treatment, Chomskyan theory falls because of some established flaw whereas physics stands because of its flawlessness on the same account; then, under our reconstruction, Chomsky’s challenge is for any one to disprove this assumption by finding one such flaw and associated form of argumentation.

B-2 Paradigm regained and the rhetorics of ‘representation’: arguing for nonparity of ontological status with physics while rejecting the bifurcation thesis.

[In this subsection, with the help of compact type (i) disambiguating notations, we show (24–27, respectively) that, from context to context, the term ‘represent(ation)’ may have any one of the following four acceptations: ‘Rm’ <mental representation>, ‘Rc’ <corporeal (typically cerebral) incarnation/embodyment in the biological substrate⁸³>, ‘R_(Rm,Rc)’ <for a given Rm, the relation between Rm and its biologically allotted Rc>⁸⁴, and ‘Tm(Rm)’ <theoretical representation of/theoretical construct accounting for Rm>; then we show (28) that the four way polysemy Rm, Rc, R_(Rm,Rc), Tm(Rm) is not confined to the acceptations of just the term(s) ‘represent(ation),’ but nourishes an all-pervasive, systematically entertained (and only partially acknowledged and vindicated) four-way Rm /Rc / R_(Rm,Rc) / Tm(Rm) ambiguity which, by blurring out all questionable junctures, rhetorically fosters an artificial air of self-evidence in the whole metalinguistic discourse of Chomskyan theory; then (29, 30, respectively) we show that, as made transparently obvious in our type (i) notations, the above four-way paradigm is incomplete, lacking ‘Tc (Rc)’ and ‘Tmc (R_(Rm,Rc))’ that are to ‘Rc’ and ‘R_(Rm,Rc)’ (respectively) what ‘Tm(Rm)’ is to ‘Rm’; and finally (31,32), without in the least endorsing the bifurcation thesis but taking 16c and the content of note 35 seriously,

we argue that it is simply because of the absence of (presupposing Tm and Tc) a reasonably advanced, refutable⁸⁵ and empirically corroborated theory Tmc (R_(Rm,Rc)) (compounded with the *de facto* empirical insolvability noted in 16d *supra*) that Chomskyan theory does not bear comparison with physics or even biology.]

In the metalinguistic discourse of Chomskyan theory, then, as amply exemplified at stages IV and V of its development:

24 The type (i) designation for the ‘internal objects’ of **19** is ‘(mental) *representations*’ [with, for ease of (re)formulation and syntax permitting, free variation between ‘represent(ed)’ and ‘representation’]—which, in our disambiguating notations, we shall couch as ‘Rm’ [with ‘R’ for ‘representation,’ and ‘m’ for ‘mental’]:

“To know a language [...] is to be in a certain mental state, which persists as a relatively steady component of transitory mental states”⁸⁶; “to be in such a mental state is to have a certain mental structure consisting of a system of rules and principles that generate and relate mental **representations** of various types” (Chomsky 1980, 48).

“[Given that] to know a language is to be in a certain mental state comprised of a structure of rules and principles,” “I [...] will use the term ‘knows English’ with reference to the appropriate mental structure, quite apart from his capacity to use the internally **represented** knowledge (even in thought) or even to gain access to it” (idem, 51, 52).

“Endowed with” “universal grammar” “the child develops” “a [tacit] grammar that is very rich and complex and that goes well beyond [the primary linguistic data to which he has been exposed]; in particular, a grammar that provides **representations** for sentences that are not related by any useful notion of ‘analogy’ or ‘generalization’ to the evidence available” (Chomsky 1980, 148).

25 The (received [cf., e.g., Whitaker 1971]) type (i) designation for the unknown ‘physical mechanisms’ (**21**)embodying/incarnating the objects of **19** is also (corporeal, typically cerebral) ‘*representation*’ [with the same kind of free variation between ‘represent(ed)’ and ‘representation’]—in our disambiguating notations, ‘Rc’ [with the precisions of note 83 *supra* on the mnemonic use of ‘c’]:

From the point of view I have adopted, universal grammar and the steady state grammar are real. We expect to find them physically **represented** in

the genetic code and the adult brain, respectively, with the properties discovered in our theory of the mind (Chomsky 1980, 82–83).

“There is interesting recent work suggesting that we have” “mental images” “that share fundamental properties with pictorial representations [...] how they are physically **represented** is unknown” (idem, 14).

“Certain factors that govern [...] “universal grammar [...] are somehow **represented** in the genotype” (idem, 91).

Although the language generated is infinite, the grammar itself is finite, **represented** in a finite brain (idem, 221).

26 The type (i) designation for the relation between a given R_m and its biologically allotted R_c [in a sense sharpened in note 81 *supra*] is also ‘*representation*’ [with the same kind of free variation between ‘represent(ed)’ and ‘representation’]—which, in our disambiguating notations, we shall couch as ‘ $R_{(R_m, R_c)}$ ’:

There is some evidence that face recognition $\langle R_m \rangle$ is neurally **represented** in the right hemisphere $\langle R_c \rangle$ and that this neural **representation** $\langle R_{(R_m, R_c)} \rangle$ is delayed until past the time when language is fixed in the left hemisphere (Chomsky 1980, 248);

There is nothing essentially mysterious in the concept of an abstract cognitivestructure, created by an innate faculty of mind [R_m], **represented** [$R_{(R_m, R_c)}$] in some still-unknown way in the brain [R_c], and entering into the system of capacities and dispositions to act and interpret (Chomsky 1975, 23; 1980, 5);

“Dennis (1980, then forthcoming, cf. 264, n 12) presents evidence that the **representation** $\langle R_{(R_m, R_c)} \rangle$ of focus and presupposition $\langle R_m \rangle$ is a left hemisphere [R_c] function, along with the computational aspects of language and aspects of word meaning that involve grammatical structure, whereas referential aspects of meaning are not specialized in this way” (Chomsky 1980, 266, note 24)—where an $R_{(R_m, R_c)}$ “function” of the right hemisphere is to biologically provide an anatomically circumscribed hosting R_c for the mentally defined native speaker’s tacit knowledge of relations of “focus and presupposition.”

27 The type (i) designation for the theoretical constructs by which the ‘inobservable’ objects of **19** are apprehended is also (theoretical) ‘*representation*’ [allowing, again, for the same kind of free variation between ‘represent(ed)’ and ‘representation’]—which, in our disambiguating

notations, we shall couch as ‘Tm (Rm)’ [with ‘T’ for ‘theory’ or ‘theoretical’]:

suppose that we agree that some sort of **representation** of quantificational structure is to be given by the grammar at the level of **representation** of meaning. Does the notation matter? Should the **representation** involve quantifiers or variables, or be in a variable-free notation, or is the question without empirical support? (Chomsky 1980, 63);

a quantifier rule, which we may think of as mapping a syntactic **representation** into a **representation** in standard logical notation (Chomsky 1980, 125);

[for] “the mapping from S-structure to phonetic or logical form [...] empirical issue[s] aris[e] in the choice of a system of **representation**” (Chomsky 1980, 164)

<where the mention of artifactual type (ii) ‘notation[s] and of a “choice” to be made shows that the “representations” in question are theoretical constructs deliberately elaborated by a theorist.>

28 When a broader class of related type (i) terms is examined, exactly the same four-fold polysemy can be shown to nurture a host of, partially acknowledged and vindicated (28a) systematically entertained ambiguities (28b) inducing (28c) a rhetorical effect of deceptive self-evidence in the metalinguistic discourse of Chomskyan theory.

28a – Acknowledgment and vindication of two cases of Rm/Tm(Rm) cultivated ambiguity.

Particularly in the case of ‘grammar’ the Rm/T(Rm) ambiguity is acknowledged, and justified in terms of harmless uses of a potentially ambiguous term in disconnected disambiguating contexts:

We must be careful to distinguish the grammar, regarded as a structure postulated in the mind <Rm> from the linguist’s grammar, which is an explicit articulated theory <Tm(Rm)> that attempts to express precisely the rules and principles of the grammar in the mind of the ideal speaker-hearer. [...] It is common to use the term ‘grammar’ with systematic <Rm/T(Rm)> ambiguity, letting the context determine whether it refers to the internalized grammar or to the linguist’s theory. The practice is unobjectionable[, though it] may lead to confusion unless care is taken (Chomsky 1980, 220).

In a similar vein,

Like grammar, the term Universal Grammar [...] is ambiguous. On the one hand, it refers to the linguist’s account of the notion ‘possible grammar of a human language’ <Tm(Rm)> [...]. On the other hand, [it] can be viewed as” “the general human ability” to acquire natural languages <Rm> (Riemsdijk and Williams, 1987, 4–5).

28b – Much broader, systematically entertained, Rm / Tm(Rm) / Rc / Tc(Rc) ambiguities.

But in fact, what is at stake [nothing to do with disconnected disambiguating contexts] is the unacknowledged and unjustified equation or conflation of a whole range of conceptually mutually exclusive acceptations within one and the same continuous, cohesive discourse or argumentation, without the slightest sign of “care” being taken to avoid the “confusion”:

‘represented’: conflating Rm and Rc, thus by-passing, ignoring or disregarding the Rm/Rc distinction:

represented in the mind and brain (Chomsky 1980, 223).
the grammar, in whatever form it is represented in the mind and brain (Chomsky 1980, 223–24)

‘universal grammar’: equating Rm and Tm(Rm):

the theory of UG <Tm(Rm)> [...] is <equation> an innate property of the human mind <Rm> (Chomsky 1975, 34).

‘S-structure’ and ‘logical form’: equating Rm and Tm(Rm):

empirical considerations provide evidence bearing on quite specific proposals as to the mental representations <Rm> that appear at the level of S-structure and logical form <theoretical constructs> (Chomsky 1980, 165).

‘mental organ’: by relished oxymoron, reifying of Rm as unsubstantiated Rc:

the language faculty [and] [...] other mental faculties (Chomsky 1975, 13)
<unreified Rm>
the language faculty and [...] other mental organs (Chomsky 1980, 44)
<reifying, unsubstantiated Rc>

mature sexual organs (Chomsky 1980, 236) <unreifying, substantiated Rc>,

‘growth of language’ (e.g. Chomsky 1977, 20): reifying of Rm as unsubstantiated Rc:

acquisition of language (Chomsky 1965, 58) <unreified Rm>
 growth of physical systems (Chomsky 1980, 261, note 62) <unreifying, substantiated Rc>

‘trace’: equating Rm, Tm(Rm) and mathematical object (the identity element of an algebra):

[Cutting many corners,] “the sentence ‘what is it easy to do today[?]’ being ascribed the transformationally derived “S-structure” “[_{NP} what] [_S [_{NP} it] [_{VP} is [_{AP} easy [_S NP [_{VP} to do [_{NP} e]]]]] today]], the empty category that I am calling ‘trace’ (namely [_{NP} e]) <a theoretical construct Tm(Rm) introduced at stage IV> is <equation> a real element of mental representation <Rm>,” and we are invited to “think of *e* as the identity element⁸⁷ of the level of representation [of S-structure] regarded as a concatenation algebra with further structure”⁸⁸ (Chomsky 1980, 144, 145, 146, 276, note 6).

‘logical form’: equating Rm and Tm(Rm):

“One might speculate that the familiar quantifier-variable notation would in some sense be more natural for humans than a variable-free notation. [...] The reason would be that, in effect, the familiar notation is “read off of” the logical form that is the mental representation for natural language” (Chomsky 1980, 165) <vanishing of the Rm / Tm (Rm) distinction> <in the above, LF is allegedly a “mental representation” of type Rm; but since that which is liable to be “read off of” is an artifactual type (i) “quantifier-variable notation,” LF is at the same time held to be of type Tm(Rm).>

‘universal grammar’ and ‘schematism’: equating Rm, Tm(Rm), Rc and Tc(Rc).

we can proceed to spell out in specific detail a schematism that characterizes the initial state. Call this schematism “universal grammar.” We may think of universal grammar as, in effect, the genetic program, the schematism that permits the range of possible [instantiations]⁸⁹ that are the possible human languages (Chomsky 1980, 233–34).

To see through the discursive thicket of the above, it may help to know that in certain disambiguating contexts, both ‘universal grammar’ and ‘schematism’ are univocally used to designate mental, type Rm, representations.⁹⁰

<On the one hand, then, the first occurrences of ‘universal grammar’ and the second of ‘schematism’ designate representations of type Rm, just as in the unequivocal case of note 90 *supra*. On the other hand, since “proceed[ing] to spell out in specific detail” is a theoretical task, the first occurrence of ‘schematism’ designates a theoretical construct, a representation of type Tm(Rm). This Tm(Rm) reading, *via* “this,” anaphorically spreads to its second occurrence, *via* “call” [by regressive equation from defined (*definiendum*) to defining (*definiens*)], and to the first occurrence of ‘universal grammar.’ So far, ‘universal grammar’ and ‘schematism’ indiscriminately designate both a type Rm and a type Tm(Rm) type of representation. Furthermore, given its progressive equation [from explicated (*explicandum*, or *analysandum*) to explicating (*explicatum*, or *analysans*)]⁹¹ with “the genetic program,” [i.e. with a subpart of our corporeal make-up, as theorised by (currently held to be adequate) standard biological theory], the second occurrence of ‘universal grammar’ [hence, regressively, also the first, by default accretion of contextually acquired values] indiscriminately designates a representation, not only of type Rm and type Tm(Rm), but also of type Rc and (slightly anticipating) Tc(Rc). Finally, as announced, given its regressive equation [from explicated, back to explicating] with “the genetic program,” the third occurrence of ‘schematism’ [hence, by similarly regressive accretion, also the second and the first], indiscriminately designate the same huddled four types of representation. – In this remarkable piece of discursive obfuscation, judging by the back and forth leapfrog acceptance-gathering acrobatics required to understand what corners are being cut and to what effect, it would seem that “letting the context determine” may sometimes be optimistic for ‘working hard to disentangle muddled up contextual clues’ and “may lead to confusion,” a mild understatement.>

28c – *De facto* (if not intended) rhetorical effect of the associated erosion or blotting out of fundamental distinctions.

In all such cases (pervasively recurrent in protean form throughout the metalinguistic discourse of Chomskyan theory), the common denominator is that the fundamental distinction between what is in need of theorisation (so far, in pre-theoretical terms, Rm’s and Rc’s) and the constructs offered to theorise them is systematically ignored, blurred or blotted out, if not rendered discouragingly difficult to trace.

All questionable junctures being thus made inapparent, it becomes all the more difficult to get a critical grip on what to think of Chomsky's rejection of the bifurcation thesis. The poker-faced assurance with which all this equating and conflating of theorised and theorising is presented as self-evident has for *de facto* rhetorical effect (if not function) to give the reader the impression that Chomsky's confidence in the validity of his theoretical constructs is too obviously justified to deserve discussion.⁹²

But more importantly, our elucidating type (ii) notations make it transparently obvious that the four term terminological/conceptual paradigm identified so far (24-27) is incomplete, lacking (29) 'Tc (Rc)' and (30) 'Tmc (R_(Rm,Rc)')' that are to 'Rc' and 'R_{(Rm,Rc)'}' (respectively) what 'Tm(Rm)' is to 'Rm.'

29 No designation has been coined for the analogue of 27 in the case of Rc—in our disambiguating notations 'Tc(Rc)':

All we have by way of Tc(Rc) is occasional, unintegrated reference to ongoing work in the separate domain of brain studies (cf., for instance Chomsky 1980, 264, note 12); and, crucially,

30 no designation has been coined either for the analogue of 27 in the case of R_(Rm,Rc)—in our disambiguating notations 'Tmc (R_(Rm,Rc)')':

All we have by way of Tmc (R_(Rm,Rc)') are,cautiously formulated allusions to its remote conceivability

– on the 'cerebral' side of 'c' (in the sense of note 83 *supra*), "A scientist [...] might [...] proceed to **investigate** the physical representation of grammars [...] in this way, he would develop a science of [this] human cognitive structur[e] and, perhaps [its] physical basis" (Chomsky 1975, 139, 143),

– on the 'corporeal' side of 'c' (idem), "The 'innateness hypothesis, then can be formulated as follows: Linguistic Theory, the theory of UG [...] is an innate property of the human mind. In principle, we should be able to **account for it** in terms of human biology" (Chomsky 1975, 34).

31 Under specifiable conditions, Chomskyan theory could enjoy parity of ontological status with physics, which already shows that its sorry state of *de facto* empirical vacuity brings no support to bifurcationism.

[At this juncture, we argue that given (31a) a number of type (b) options of Chomskyan theory, and assuming the ontological ‘suspension of disbelief’ conception of scientific realism of 22h, if (31b) a successful Galilean style-abiding theory $T_{mc}(R_{(R_m, R_c)})$ were available that integrated it as its T_m subcomponent, then, for all its idealisation and underdetermination, Chomskyan theory would deserve parity of status with physics. Next (31c) we argue that if, short of integration into a suitable T_{mc} , for all its mc -defectivity, Chomskyan theory could at least provide, by way of successful simulation, a falsifiable and empirically corroborated predictive formal device, then it would not be entirely devoid of empirical content, even though it would no longer be in a position to justify its initial idealisation or efficaciously sustain its ontological claims. Finally, we argue that though (31d) Chomskyan theory is *de facto* doubly empirically insolvent, it would be fallacious (31e) to claim that the resulting type 3 vacuity demonstrates the validity of the bifurcation thesis.]

31a – Rule of thumb recapitulation of relevant tenets, contentions, strategies, expectancies and ambitions of Chomskyan theory.

Of particular relevance here, the fundamental epistemological tenet is that (16a) Generative Grammar, as a particular case of Galilean style scientific investigation, may only resort to substantial idealisation and abstract formal tools if its predictions are submittable to, and borne out by, confrontation with empirical data.

An essential contention is that (17, 20) the mind does not exist as a second substance cut off from the corporeal.

The main strategy (18) is to conceive of “the study of the human mind” as “a study of the body—specifically the brain”—conducted at a certain level of abstraction.”

Since they are held to be “real,” the crucial “expect[ancy]” is that (quoted in 25) such “properties [allegedly] discovered [by] the theory of mind” as those of “universal grammar and the steady state grammar” will actually be “f[ou]nd to be physically represented in the genetic code and the adult brain, respectively.”

Finally, the driving ambition is to obtain for the reality of its constructs the same level of acceptance or suspension of ontological disbelief (22h) as for those of physics.

31b – On a Galilean style-abiding hosting $T_{mc}(R_{(R_m, R_c)})$ theory [if it were available] and its constructs as conceivably fulfilling the expectancies and ambition of 31a without being open to bifurcationist criticism.

An empirically falsifiable theory T_{mc} ($R_{(R_m, R_c)}$) [henceforth, ‘ T_{mc} ’] would provide a predictive, explanatory account of how it is that, given the kind of corporeal substrate that $T_c(R_c)$ [henceforth ‘ T_c ’] claims we have, we should come to have the kind of structured mental representations that $T_m(R_m)$ [henceforth ‘ T_m ’] claims we have.⁹³

If such an overarching theory were available, all its constructs, and in particular (those of) its integrated sub-component T_m , would likely be highly abstract and as divorced from common-sense notions as quirky quarks, black holes and mind-boggling identities in their distinctions of matter and energy, waves and corpuscles. Furthermore, the theoretical reconstruction T_{mc} of the $R_{(R_m, R_c)}$ link between R_m and R_c —whether it be in terms of emergence,⁹⁴ supervenience,⁹⁵ connectionist networks,⁹⁶ canalisation,⁹⁷ software and hardware,⁹⁸ quantum leaps, and/or whatever (radically) new concepts may eventually come to be used⁹⁹—would no doubt be underdetermined by evidence (since such is the common lot for all predictive empirical theories) and sooner or latter bound to be supplanted by some more satisfactory theory.

But, for all this abstractness, weirdness, underdetermination and predictable obsolescence (and as long as no rival, superseding theory is available that would do the same explanatory job without resorting to a subcomponent T_m , i.e. without having to postulate the existence of R_m ’s as distinct from R_c ’s),¹⁰⁰ crucially, if (and only if) such a theory T_{mc} survived the test of sustained attempts at empirical falsification and thereby proved robust enough to be deemed “highly confirmed,” then—for such is the rule of the Galilean game—no indeterminist pseudo excuses could be invoked, and all its constructs, in particular (those of) T_m would have to be “accepted, at least temporarily.”

In other words¹⁰¹ at the relevant time t , such unobvious constructs of Chomskyan theory as LF, SSC, empty category, $[_{NP} e]$, trace, pro, PRO, level architecture, inter level connecting rules, etc. would be granted <ambition fulfilled> the same degree of provisional benevolent ontological credit as the aforementioned seemingly quirky and mind-boggling constructs of physics—though it might take some time before reaching the transparent familiarity of the once abhorrent notion of long distance (gravitational) occult attraction through a vacuum.

Such a theory T_{mc} would demonstrate <expectation fulfilled> that T_m and T_c both fit in an empirically corroborated overall account of the $R_{(R_m, R_c)}$ relation between R_m ’s and R_c ’s.

To complete the achievement, given, by hypothesis, that it would be offering some account of actual processing¹⁰² and that T_m would be successfully mobilised to accomplish that task, T_{mc} would in one clean

sweep: demonstrate that the seminal insight (13a) “competence [...] underlies actual performance” does head in the right direction; provide some support to the view that, since it paid off so well, the initial process-rejecting idealisation to competence (13c) was after all justified; and finally, having withheld empirical falsification, allay the qualms about 13d and bring a final, reassuring answer to Johnson-Laird’s objections (cf. note 24).

A sceptic of any description (instrumentalist, conventionalist, fictionalist etc.) who did not endorse scientific realism would of course be unimpressed by such a theory T_{mc}. But the main point <exit bifurcation> is that there is no way one could be ontologically sceptical about the constructs of T_{mc} without being at the same time ontologically sceptical about those of physics. In that sense, T_{mc}, if it were available, would be immune to bifurcationist criticism.

31c – On what could conceivably be expected from a severed, mc-defective theory T_m standing on its own.

By type (i) decision, we shall use the term ‘structurally’ (*aka* ‘architecturally’) ‘**mc-defective**’ for a theory of mental representation T_m for which no hosting theory T_{mc} (as defined in 31b *supra*) is available [for further detail and perspective, cf. note 148 *infra*]. In the terms of notes 13 and 39 *supra*, such a theory T_m could conceivably provide some formal machinery enumerating all and only the (sound, linguistic meaning) pairs characteristic of the tacit competence of the idealised speaker. And it could conceivably offer some formal and/or conceptual reconstruction of the (primary linguistic data, tacit competence grammar) pairing [*aka* correspondence] characteristic of language acquisition.

Of course, given that there are indefinitely many equivalent ways of computing the same function, and since an isolated T_m would be making no attempt at spelling out what it means for a formal computation to “correspond” to a mental computation (Chomsky 1980, 220), or for “physical mechanisms” to “meet” its specifications (*idem*, 228), sceptics of instrumentalist or conventionalist persuasion would be particularly unimpressed by such an mc-defective theory T_m, and it comes as no surprise that Bouveresse (in Parret ed. 1974, 314) should have thus aired his qualms about the generative enterprise:

The interest of a notion of competence is rather thin if one cannot indicate any research direction which would eventually be able to throw some light on the way this competence is materialized and put into practise. It is true that the [generativists] have repeatedly said that, as linguists, they did not have to give such indications and that the description they propose of the

mechanisms underlying language use is *a priori* compatible with very different manners in which these mechanisms could be realized. It is precisely in these conditions, however, that it is hard to understand the insistence with which they maintain that they have provided the description of something which really deserves to be called the ideal speaker's competence.

Naturally [ignoring issues of overreach vacuity] such a theory T_m unrelated to T_c would not be fully satisfactory. In spite of its *ex hypothesi* success at predicting the pairings which actually obtain, it would offer no guarantee at all that there should be a term-for-term isomorphism between the inner workings of the predicting formal machinery and the inner workings of the corporeal, nor, more radically (unless, precisely, one such theory be proposed) that, as it stands, it should be at all integrable into any falsifiable and empirically corroborated theory T_{mc} of the relation between R_m 's and R_c 's.¹⁰³ However, it would be unfair to claim that the constructs of such a falsifiable and corroborated theory are absolutely devoid of empirical content. Even though all it has to offer is an accurate, but makeshift formal apparatus to, one way or another, formally compute the right function (leaving to others¹⁰⁴ the task of discovering how the underlying corporeal computation is actually performed)—still, it may be argued that, in the perspective it has deserted of a theoretical account of the relation between mental and corporeal representations, such an isolated, mc-defective T_m might at least serve (without having to suggest any explanation) as a valuable condensed reminder of what needs to be explained, thus providing a precious “guide” (Chomsky 1980, 89) for future investigation and, in a round-about way, providing some residual justification for the initial strategy ($\overline{18}$): for instance (Chomsky 1975, 91), “studying the use and understanding of language, we reach certain conclusions about the cognitive structure (grammar) that is being put to use, thus setting a certain problem for the neurologist, whose task it is to discover the mechanisms involved in linguistic competence and performance.”

31d – On the handicaps of Chomskyan theory as it stands at stage VI.

Given the unavailability (in a reasonable state of advancement) of an overarching predictive, falsifiable and empirically corroborated hosting theory T_{mc} that may successfully integrate it as a subcomponent, in the terms of 13a, the expectancies of Chomskyan theory are not yet fulfilled and, in contrast with the theory T_m of 31b, but just as would be the case for the theory T_m in 31c, it is in no position to justify its initial process-

rejecting idealisation to competence or answer the objections of note 24, and is no strong position to allay doubts.

But, in actual contrast with the hypothetical case envisaged in 31c, Chomskyan theory is empirically vacuous. Because, as we have seen in 16d, it is doubly empirically insolvent in its account both of tacit competence and of acquisition (an insolvency stemming from such demands on what is to count as adducible piece of counter-evidence that no empirical falsification is *de facto* feasible); and also because, concerning acquisition, in support of the claim that universal grammar “may provide only finitely many grammars (or perhaps one grammar) associated by the principles of language development with a collection of data sufficient for language acquisition” (Chomsky 1980, 268, note 42), no “translat[ion]” of that claim “into an effective procedure for acquiring a grammar for a language, given a corpus of sentences from that language”¹⁰⁵ is offered (Johson-Laird 1987, 147) to try to counterbalance the fact that no attempt is made to link onto the corporeal and explain how UG is “represented in the genotype” (Chomsky 1980, 91).

And as a result of this empirical unaccountability, Chomskyan theory is much worse off than Tm in 31c. Being totally deprived of empirical support, it cannot provide even residual justification for its initial strategy and, to put it bluntly, is a sitting duck for sceptic fire.

31e – On the ailments of Chomskyan theory as no valid proof of the Bifurcation thesis.

This unfortunate state of affairs, however, cannot be held to demonstrate the radical bifurcation between scientific studies of the mental and prototypical hard-core physics without committing the fallacy of begging the question [aka *petitio principii*]:

The essence of the bifurcationist claim is that the lack of parity of ontological status in the case in point is due to or reveals an unbridgeable difference in nature between Chomskyan theory [henceforth in this paragraph, ‘GG’] and physics. Now, we have seen (31b) that if an overarching, predictive, refutable and empirically corroborated theory Tmc were available which integrated a theory Tm as a subcomponent, then any such theory Tm (and in particular GG) would deserve equality of ontological status with physics. Which entails that, if it were established that it is in the nature of GG to be forever radically uninscribable in any such Tmc whatsoever, the bifurcation theory would be demonstratively established.

But then [leaving aside issues of *de facto* (i.e. contingent, not necessary) empirical vacuity], given that the only established extra premise that we have is that GG is not as yet integrated into any suitable theory T_m; that unintegrable does not follow from not yet integrated; and that no independent proof is offered to establish GG unintegrability, it seems clear that inferring bifurcation from the flaws of GG would be simply taking its unintegrability for granted, thus surreptitiously smuggling in “by assumption as premis[e] of one [...] propositio[n] which [is] identical with (or in a simple fashion equivalent to) the conclusion to be proved”—thus committing the fallacy of “*petitio principii*.”¹⁰⁶

32 It is possible to meet Chomsky’s challenge by showing why Chomskyan theory at stage VI cannot claim parity of ontological status with physics without at all endorsing bifurcationism.

[At this juncture, we shall dwell, first (32a) on the insolvency deplored in 31c, then (32b) on the architectural (*aka* structural) mc-defectivity defined in 31c [and note 148 (1.) *infra*], paying particular attention to Chomskyan counter-argumentation (note 103) and (null) contribution to the bifurcationist cause. And we shall conclude (32c) with our final answer to Chomsky’s challenge and the need to come to terms with historical contingencies.]

32a – On empirical insolvency as an unacceptable infringement of the rules of the Galilean style game.

Playing the Galilean style game by the rules, one may deserve (22h) benevolent ontological credit for one’s theoretical constructs. But to play the game by the rules, one must [16a and note(s) 33 and) 35] propose refutable hypotheses. Therefore, since (16d, taken up in 31d), its claims are *de facto* unfalsifiable, Chomskyan theory, by breaking a fundamental rule, is not worthy of ontological recognition.

Therefore, since, on their side, physicists agree to run the gauntlet of confrontation with experimentally obtained empirical data, one sufficient reason why Chomskyan theory cannot claim parity of status with physics is simply that the latter abides by the refutability requirement while the former does not. Furthermore, since the rules (infringed by the former, respected by the latter) govern every kind of scientific investigation, this rationale for denying parity of ontological status has nothing to do with any kind of ‘bifurcation.’

Riemsdijk and Williams (1987, 183) aptly remark that such T_m constructs as “LF [...] are empirical in nature though [...] quite abstract and removed from the fact.” But that is no excuse for empirical unaccountability, otherwise physics, with its far greater degree of abstraction and remoteness from direct observation would have never managed to attain refutability.

32b – On why an overarching theory is required in the case of Chomskyan theory and not in the case of physics.

At this step in the argumentation, it should be relatively straightforward that (under 16a and 22h) a theory T_m imbedded in a reasonably successful theory T_{mc} deserves ontological recognition, while an insolvent theory T_m on its own does not, since under 16a alone, it does not even qualify as scientific investigation.

But what still needs looking at is why [as left implicit in the first paragraph of 31d] an isolated theory T_m , because of its isolation, is still refused ontological recognition even though it is falsifiable and empirically corroborated. After all, so the objection would go, neither Chomskyan theory nor physics are integrated into a successful overarching theory. Why then should one and not the other be charged with architectural or structural mc-defectivity, and why should this mc-defectivity be sufficient ground to deny ontological recognition for one and no problem at all for the other? Unless some answer is given, it would look as if this were a case of unfair discrimination and the surreptitious back door reentry of bifurcation in disguised form.

The matter is all the more pressing as, since Chomskyan theory has only been shown to be *de facto* insolvent, things could not inconceivably be mended (for instance the over-strict conditions on adducibility invoked in 16d could be made less stringent, and the stage VI amendments alluded to in note 105 *supra* could be deemed a step in the right direction).

One possible answer to such qualms and criticism would run as follows:

On the one hand, given wide-spread naïve contentions and expectations endorsed by type (b) decision in [18] and [25]), there is a pre-theoretically identified X (the corporeal in general, the brain in particular) such that Chomskyan theory is “the study of X at an abstract level,” and such that it is expected that some theory (T_{mc}) will show that the constructs of Chomskyan theory are actually realised (*aka* corporeally represented) in X.

On the other hand, in the absence of any endorsable naive contention or expectation to do with anything like an underlying sub-physical reality, there is no homologue Y of X such that physics could be defined as “the study of Y at an abstract level”¹⁰⁷ or such that it would make sense to expect that some broader theory will show that the constructs of physics are actually realised in Y (for instance, should one try to instantiate Y by physical reality, then the theory supposed to show that the constructs of physics are actually realised in X can only be physics itself).

In other words, it is because of sharp differences in adopted pretheoretical claims and expectations that while everyone has an idea of what X Chomskyan theory might fail to be related to, no one has the slightest intimation of what Y it could be that physics could conceivably be held to fail to relate to. As a result, it makes sense to talk of an overarching theory T_{mc} relating X to Chomskyan theory [and hence to evoke structural mc-defectivity for Chomskyan theory], but it would make no sense to talk of an homologous overarching theory that would relate such an inconceived Y to physics [nor would it make any sense to evoke structural mc-defectivity in the case of physics].

Consequently, the reason why Chomskyan theory, but not physics, may conceivably be reproached with structural mc-defectivity is a matter of freely ratified distinct pretheoretical contentions and expectations [resulting in just one set of theoretical constructs (of T_{phys}) <trivially related to itself by identity> to account for the properties of, say the sun (note 107), and not just one, but two <as yet problematically totally unrelated> sets of theoretical constructs (those of T_m and those of T_c) to account for the properties of, say, the brain], which has nothing to do with unfairly inflicted discrimination (*aka* disguised bifurcationism). And finally, since acknowledged indetermination of T_{mc} and T_m are no obstacle to realist ascription in 31b, it has nothing to do with *stricto sensus* bifurcationism either.

32c – Meeting Chomsky’s challenge and calling a spade a spade.

Taking up Chomsky’s challenge as reconstructed in 23c, without “abandon[ing] scientific rationality when we study humans “above the neck” (metaphorically speaking), becoming mystics in this unique domain, imposing arbitrary stipulations and a priori demands of a sort that would never be contemplated in the sciences, or in other ways departing from normal canons of inquiry” (Chomsky 2000, 76), we found a Galilean-abiding form of argumentation such that, by parity of treatment, Chomskyan theory falls (*de facto* empirical insolvency aside) essentially

because of one crucial flaw (architectural mc-defectivity) whereas physics stands because of its flawlessness on the same account.

The reason why Chomskyan theory is found to be flawed while physics is by no means mysterious or ineffable about a mental quintessential entelechy that it fails to grasp, but is relative to wide-spread naïve contentions and expectations endorsed by free type (b) decision in Chomskyan theory, which have no actual or conceivable equivalent at all to be endorsable in type (b) contentions and expectations of physics.

In the present state of the advancement of learning, in the terms of 31b, the reasonably successful empirically falsifiable theory $T_{mc}(R_{(R_m, R_c)})$ that would convincingly begin to provide a predictive, explanatory account of how it is that, given the kind of corporeal substrate that $T_c(R_c)$ claims that we have, we should come to have the kind of structured mental representations that $T_m(R_m)$ claims that we have is clearly unavailable. As constantly re-asserted by Chomsky himself, for all sorts of reasons, including the fact that “in the study of humans, direct experimental inquiry into physical mechanisms is generally impossible because of the ethics of experimentation” (Chomsky 1980, 226), the rudimentary state of T_c (hence T_{mc}) is such that, in line with the considerations of note 99, beyond an account “in neurological terms,” “it may [even] well be that the relevant elements and principles of brain structure have yet to be discovered” (Chomsky 2000, 25).

But if it is thus a matter of historical contingency (and hardly anybody’s fault) that for lack of such a suitable overarching theory T_{mc} , Chomskyan theory is as yet architecturally mc-defective, the fact remains (drought does not abolish the vital need for water) that as long as this mc-defectivity is not remedied, 22h suspension of ontological disbelief will have to remain an unfulfilled ambition.

Given that the privilege of a realist stance is reserved for “mature,” “well confirmed” and “predictively successful scientific theories” (Psillos 2006a and Salmon 1967, quoted in notes 50 and 35 *supra*, respectively), it might perhaps do no harm at this stage to call a spade a spade: (at least until state VI) Chomskyan theory is so far from qualifying as a mature and predictively successful scientific theory¹⁰⁸ that, unless some decisive progress is made, it is just pointless to carry on demanding parity of ontological status with physics.

[**To summarise**, in this second section, we have shown that the systematically confusing way such terms as ‘representation’ are used in Chomskyan metalinguistic discourse leads to a rationale for not granting Chomskyan theory parity of status with physics that resorts to none of the dubious arguments of bifurcationism, respects the strictures of the Galilean

style of scientific investigation, and hinges on structural or architectural mc-defectivity.]

C. Computer model of the mind, interface and structural mc-defectivity

[In this section, we show (C-1) that Chomskyan theory at stage VII (MP) would seem to presuppose some version of the computer model theory of the mind (33); that one might therefore (34) have the impression that, with this tacit endorsement, the mc-defectivity problem vanishes; but that (35) this impression is just an illusion in that, though possibly made more precise in its formulation, the mc-defectivity problems remains unsolved; and finally (C-2) we show that, given (36) the number of external systems with which the SEM avatar of LF is supposed to interface, the defectivity problem at stage VII not only remains, but is magnified (37).]

C-1 On the mc-defectivity problem as unsolved under the computer model of the mind

33 “The computer model of the mind “ assumes that basically “the mind is the program of the brain and [...] the mechanisms of mind involve the same sorts of computations over representations that occur in computers” and “has guided research in cognitive science since [...] the 1960’s” (Block 1990, 247).

Granted 33 (as strongly suggested in the case of Chomskyan theory by the key use of such terms as ‘interface’ and ‘instructions’) it would seem to follow that, contrary to 32b-c.

34 the mind being to the brain what software is to hardware, Tm should not be bothered with any details of Rc’s, Tc, or Tmc.

What might foster this impression is that given, on the one hand, that the same software utilities can be made to run indifferently on a huge variety of material supports¹⁰⁹—in Chomsky’s terms (1980, 226) “the same program [may be] represented [*aka* “realized”] in devices of very different design and constitution”—the programmer may devise his ‘higher level’ programs without having to pay any attention to ancillary hardware issues, then, on the other hand, it would seem that, in strict

parallel fashion, the mentalistic theoretician may devise his Tm constructs without <exit mc-defectivity> having to pay any attention to ‘hardware’ Tc or Tmc issues.

But the point is that, while in the case of the computer, the existence of the software and some explanation of how it can run on a huge variety of hardware are beyond doubt because they are known¹¹⁰ by conception and construction, the relevance of the software/hardware analogy to the mind/brain interaction (and, crucially, justification for the constructs of Tm) is at best a matter of non-demonstrative inference, if not sheer speculation. As a result of this, [33], hence [34], are no articles of faith, and for their justification

[presupposing the content and using (in single quotes) the notations and terminology of Tanenbaum et al. 2013 (2–4)], in Galilean style, empirically corroborated, admittedly to be sooner or later superseded, etc.,

[35] some reasonably advanced theory Tmc must be supplied to convincingly begin to explain how the computed mental representations <now re-interpreted as some type of software, higher language-like programmed ‘instructions’> that Tm tells us that we have, not only by what ‘compiling’ or ‘interpreting’ operations those mental representations get to be ‘translated’ into some cerebral ‘machine language’ ‘instructions’ to be ‘executed’ by the brain, but also, crucially, how, under some theory Tc of the anatomy, physiology and physico-chemical properties of the brain, ‘below level 0,’¹¹¹ those ‘L0’ ‘machine instructions’ can conceivably be stored, accessed and ‘executed,’ *aka* ‘carried out’ by the brain <now re-interpreted as a form of corporeal hardware support>.

If nothing is offered by way of [35], [33], hence [34], are just hand-waving, pre-scientific speculations, if not trivial metaphorical common-usage extensions of the technical terms.¹¹²

C-2 On the unresolved mc-defectivity problem as magnified under the ‘interface’ approach of the ‘SEM’ avatar of LF

But, within the apparatus of a state of Chomskyan theory broadly characterizable in those terms:

Currently, the best theory is that the initial state of the language faculty incorporates certain general principles of language structure, including phonetic and semantic principles, and that the mature state of competence

is a generative procedure that assigns structural descriptions to expressions and interacts with the motor and perceptual system and other cognitive systems of the mind/brain to yield semantic and phonetic interpretation of utterances (Chomsky 2000, 60),

(leaving PHON aside) the LFs mentioned in [15] are fully rephrased in [33]-terms. They are now called ‘SEMs’ or ‘semantic interfaces’ (Chomsky 2012, 161, 301):

[36] “[An] SEM [...] is a representation, a complex mental event <an Rm> described in theoretical terminology <i.e. by Tm(Rm)s constructs> that in the theory of mind <Tm> is treated as a specific configuration of an interface with other mental” “conceptual and intentional systems” and thus “configures experience,” “vision,” thought, “imagination” “etc.” (Chomsky 2012, 259 and 301, 258, 191, 161).

But then the requirement of [35] (a particular case of 32b-c) now expands dramatically to

[37] for each of the cognitive systems Si “on the other side of SEM” (Chomsky 2012, 161), not only must Tmc be enriched to explain how specific configuration programs may be compiled to ‘send’ and ‘receive’ corporeal ‘machine language’ ‘instructions’ for and from Si to be ‘carried out’ or ‘executed,’ but, for each Si, some theory Tmci need to be provided to convincingly begin to explain how, given its corporeal texture and structure, Si may possibly ‘send,’ or ‘access’ and ‘execute,’ *aka* carry out’ these ‘instructions’—so that, if n is the number of such systems Si with which SEMs are supposed to interface (and there are apparently many), the mc-defectivity problem of Chomskyan theory at stage VII, far from disappearing, is made at least n times worse.

Without [37], such terms as ‘interface,’ ‘configuration,’ ‘system’ or ‘implementation,’ ‘instructions,’ ‘access’ and ‘wired in’ (Chomsky 2000, 174, 128; 1980, 134) are vague, flashy, useless words and any hope of justifiably evacuating anything elsewhere on the other side of SEM to get rid of any annoying problem collapses.

[To summarise, in this third section, we have shown that, should Chomskyan theory be viewed as endorsing the computer model of the mind,¹¹³ then its mc-defectivity problem, instead of being solved, would be made much more intractable.]

D. The naturalistic and internalist approach to meaning and reference: interfacing to the Conceptual/Intentional systems and the spectre of transcendently self-declared empirical bankruptcy

[In this section, we show that under a number of naturalistic and internalist contentions [D-1], a theory of the interface to the Conceptual system [D-3] and a theory of the interface to the Intentional system [D-4] are developed which are each in its own way [48d and 59], respectively] structurally defective (and therefore empirically insolvent). Then we show [D-5a] that given further (transcendental) contentions [D-2], neither of these types of defectivity can ever be remedied. Then, even though [D-5b], if taken seriously, this pronouncement amounts (instead of provisional empirical insolvency) to disastrous transcendently self-declared empirical bankruptcy, we argue (66) that there is no compelling reason to take the pronouncement (hence the resulting bankruptcy) seriously.]

D-1 Assumptions, attitudes and pronouncements having to do with naturalism and internalism

[In this subsection, we show how assumptions about scientific naturalism (38) and internalism (39) lead to the rejection of E-language (40), Platonism (41) and (by neglect, if not disdain) (42) of Meinongian objectivism.]

38 [Arguably as of stage IV,] Chomskyan theory endorses a variety of scientific naturalism claiming that [if apprehended as a non-rigid framework undergoing a continuous, unpredictable process of improvement and innovation], the way the natural sciences proceed (as epitomised by physics) is the only sound way to conduct the investigation of mind and brain:

“A “naturalist approach” to the brain investigates the mental aspects of the world as we do any others, seeking to construct intelligible explanatory theories, with the hope of eventual integration with the “core” natural sciences” (Chomsky [1994] 2000, 76). Thus, by doing away with the “encumbrant ideological baggage that forms part of our intellectual tradition” and agreeing “to study humans as organisms in the natural world, we may “develop a science of human cognitive structures and perhaps their physical basis” (Chomsky 1975, 139, 143).

In this view, “naturalism is taken primarily as a methodological stance, a determination to employ only well-established scientific findings and methods whatever they may be” at a given stage of their “continuously changing” course (Giere 2006, 501). Therefore, though “naturalism claims [...] that the scientific method provides the only sound basis for knowledge of reality” and commonly holds that “all realities” are part and parcel of a “fundamental causal network consist[ing] in chains of physical cause and physical effect produced by the operation of physical forces,” it is out of the question to consider that this “physical nature” shared by all realities has once and for all already “been established” (Campbell 2006, 492). In other words, though it may be granted that “accounts of our minds, our knowledge, our language must in the end be continuous with, and harmonious with the natural sciences” (Chomsky [1994] 2000, 80—after (quoting Dennett), Baldwin 1993, 172), in the spirit of note 99 *supra*, Chomsky insists that it would be “surely unacceptable” to claim that this means that “the theory of mind should be “continuous” and harmonious” with **today’s** physics” (Chomsky [1994] 2000, 81–82, our resort to bold type).

In this naturalistic framework, at stage VII, Chomskyan theory:

39 takes [in part, has taken since stage VI] the internalist turn to I-language multiply ‘interfacing to’ the conceptual and intentional¹¹⁴ systems on the ‘other side’ of SEM.

Where (McGivnay in Chomsky 2012, 154) ‘I’ “stands for “individual, internal and intensional,¹¹⁵ and [McGilvray] add[s], “innate and intrinsic,” in a sense which will become clearer as we go along, particularly when we get **(40–42)** to divergent notions, concepts and theories which, being social, external, extensional and/or non ‘natural’ [*aka* non ‘physical’ in some extended sense of the term] contravene to **38** and/or **39**—and as to the systems on the other side of SEM, cf. the referenced quotations of **36** and **37** *supra*;

40 rejects—and denies (1986, 28–29) having ever¹¹⁶ entertained—the notion of a socially given, non-‘natural’ ‘E[xternal]-language’ as anything but an “epiphenomenon”¹¹⁷:

“[whereas “grammar” and “universal grammar” “are real”], “ ‘language’ [defined] as a pairing of sound and meaning over an infinite domain” “is epiphenomenal. Its ontological status is the same as that of a set of pairs of expressions that rhyme—a set that may also be determined by the grammar” (Chomsky 1980, 82 and 83). “The notion ‘language’ [being thus] derivative

and relatively unimportant[.] not only might we dispense with it, with little loss” (Chomsky 1980, 82, 83, 127), but “it could [even] turn out that there is no intelligible notion of language” (Chomsky 1982, 107).¹¹⁸

By thus rejecting the idea that language should be viewed as a social object (a ‘public language’) pre-existing ‘outside’ the individual as an institution, or a collective, historically hoarded treasure grown so huge that no single person may hope to embrace it all,¹¹⁹ [40] is well in line with the tacit ostracisation of (non-aligned) social sciences in [38], as with his central contention that “what makes us human is not [essentially] society or culture,” but our species-specific genetic endowment (McGilvray in Chomsky 1982, 178);

[41] rejects Platonistic Realism as positing non-‘natural’ external objects.

Contrary to Quine [cf. note 82 *supra*], the variety of scientific naturalism endorsed by Chomsky involves a rejection of Platonism, i.e. “the view” [held to be incompatible with [38]] “that our mathematical theories are descriptions of an abstract mathematical realm, that is a non-physical, non-mental, non spatio-temporal” “and acausal” “aspect of reality” (Pamiès 2015, 242 n. 86, quoting Balaguer 1998; or, providing an essentially equivalent definition for ‘Platonism,’ *aka* ‘platonism,’ *aka* ‘realism ontology,’ Shapiro 2006, 274):

even though “in the case of arithmetics,” Chomsky is willing to concede “at least a certain initial plausibility to a Platonistic view insofar as the truths of arithmetics are what they are, independently of any facts of individual psychology, and we seem to discover these truths somewhat in the way that we discover facts about the physical world” (Chomsky 1986, 32), crucially, “*mathematics in its advanced form is invented,*” and not discovered as “a free gift of natural resources” (McGilvray in Chomsky 2012, 215)¹²⁰,

[42] ignores Meinongian Objectivism, which posits external, non-‘natural’ impossible objects.

Summarily (cf., *verbatim* and/or in substance, Chisholm [1967] 2006, 115, 116), “the two basic theses of Meinong’s theory of objects [...] is that (1) there are objects that do not exist (2) every object that does not exist is yet constituted in some way [has a *Sosein*] and thus [*pace* Russell] can be made the subject of predication,” and its fundamental “doctrine” is that “the *Sosein* (character) of every object is independent of its *Sein* (being).” In particular,¹²¹ Meinong’s theory (which, he notes “might properly be

called objectivism,” since it is “broader than [Platonistic] Realism,”) holds that there are objects, “impossible objects,” which are what they are [they have *Sosein*] without having either existence or being [they have no *Sein*]. “A round square, for example, has a *Sosein*, since it is both round and square; but it is an impossible object, since it has a contradictory *Sosein* that precludes its *Sein*.” “Such objects,” Meinong insists, “have no being at all; they are “homeless objects,” to be found not even in Plato’s heaven.”

This unfashionable theory is not without problems¹²² [for instance, if there **are** objects which have neither existence nor being, what is the nominal meant to correspond to ‘are’? It can neither be ‘existence’ nor ‘being,’ and ‘(having) reality’ might pose problems of internal consistency]. But the main reason why such a theory should be unpalatable for the internalist naturalism of Chomskyan theory is the external nature of its posits:

None of the objects discussed above is created by us, nor does any of them depend in any way on our thinking. Had no one ever thought of the round square, it would still be true *of* the round square that it does not exist; the round square need not be thought in order not to exist.

It comes as no particular surprise, then, that, though one of its unreferenced concepts is used, Meinong’s theory should have passed “[un]discuss[ed]” and unmentioned in the following excerpt:

[It is no easy task] to deal with the fact that fictional terms such as “Pegasus” and descriptions such as “the average Irishman” and “the square circle” make perfectly good sense [...], even though the ‘world’ seems to lack average Irishmen and a winged horse named Pegasus. And square circles are particularly daunting. No one has difficulty using or understanding *The square circle horrifies geometricians*, even though square circles are ‘impossible objects’ (McGilvray in Chomsky 2012, 216);

43 rejects the re-presentational’ approach of Fodor, which tries to save an externalist “word-world” conception of semantics *via* a would-be ‘natural’ causal theory of reference.

“In order to naturalize linguistics and mentalistic phenomena by reducing them to the terms of physical science,” Fodor (1981, 1998, 2008) developed “a causal theory of reference, on which for *n* to refer to *x* is for a causal chain of a special kind to connect *n* to *x*” (Williamson [1996] 2006, 289). He believed he could thus save a “word-world” conception of semantics and “salvage a representationalist theory of the mind by

introducing a view of reference/denotation claimed to depend on natural [*aka* ‘nomological’] laws” (McGilvray in Chomsky 2012, 301, 162 n 2). Less summarily, in his “representational theory of mind,” Fodor psychologizes Frege’s senses¹²³ and calls them “modes of presentations,” or MOPs,” which provide his account of what “a concept is.” In his “externalist” approach, Fodor “believes he can show MOPs are *of* or refer to (their denotations). Essentially, he claims that the MOP for say *water* develops automatically in a person as a result of some kind of causal informational relationship to—or predominantly to—water ‘out there,’ and that this causal relationship also establishes an inverse semantic relation, denotation, so that the water-MOP denotes the property *being water* ‘out there’ (and, he insists, *being H₂O* ‘out there’ too” (idem, 188, quoting Fodor 1998).

In reaction to these efforts to define a concept as a pair consisting of a MOP and some property ‘out there,’ given that—though “there is nothing wrong with holding that MOPs are acquired by some kind of causal relationship; [since] any nativist account holds that concepts develop as a result of some ‘triggering’ relationship[–] there is no reason to believe that a semantic relation of denotation [should be held to] piggybac[k] on the world-head causal triggering relationship”¹²⁴ (McGilvray, in Chomsky 2012, 188), it is argued at length by Chomsky (1986, 20–25) that Fodor’s externalist views are plain wrong and misguided, and his final pronouncement is that “whatever the nature of (such concepts as) HOUSE, or LONDON, ARISTOTLE, or WATER is, [...] it’s just not connected to mind-independent events” (Chomsky 2012, 33).

D-2 Transcendentally inferred contentions

[In this subsection <our type (i) terminology>, we concentrate on transcendentally induced pronouncements: the ‘angel argument’ (44) concludes that [as distinct from provisionally unsolved ‘problems’ in the sense of Chomsky 1991] there must exist mysteries beyond our intellectual capacities; the ‘deadlock argument’ (45) provides one way of identifying some of those mysteries, among which the epitome is (45) the stimulus-free, creative use of language.]

(44) By transcendental inference, the ‘angel argument’ concludes the necessary existences of mysteries beyond our mental capacities.

In what may be reconstructed as a form of counterfactual ‘proof’ [cf., *mutatis mutandis*, 22c and related notes (and for the scare quotes, note 74

supra), with P' , $\sim P'$, Q' and $\sim Q'$ instantiated by 'to have limited mental capacities,' 'to have limitless mental capacities,' 'not to be an angel' and 'to be an angel,' respectively—where an 'angel' is (for non-believers in cherubs) a mock denomination for the Meinongian impossible object 'mind deprived of any organic substrate'—the claim is thus established (Chomsky 2012, 136):

“a person who held” “that we have some kind of capacity to be able to solve every problem we might encounter” “and to pose any question” $\langle \sim P \rangle$ “would be saying” $\langle \rightarrow \rangle$ that “we are somehow angels” $\langle \sim Q \rangle$. But since we are not angels [$\sim Q$], it tacitly follows by *Modus Tollens*¹²⁵ that we have limited capacities $\langle \sim \sim P \rangle$, that is to say P .

In other words, what is allegedly demonstrated here is that due, not only to the limits of our *Homo sapiens* genetic endowment, but because of “the limitations of possible organic development,” there are not only problems for which we will never find a solution, but there are even bounds to be problems which we will never be able to conceive of¹²⁶;

45 by transcendental inference, the ‘longstanding deadlock (or stalemate) argument’ may be used for identifying which problems (that we can at least pose) are beyond our mental capacities:

Whenever a problem X has been actively discussed for a very long time t without any sign of progress, then, by tacit counterfactual proof of the form

If X were not beyond our cognitive capacities $\langle \sim P \rangle$, then $\langle \rightarrow \rangle$ after such a long time of sustained efforts at least some progress would have been made $\langle \sim Q \rangle$. But since no progress has been made during that period $\langle \sim \sim Q \rangle$, it follows by *Modus Tollens* that X is beyond our cognitive capacities ($\sim P$, that is to say P , conclusion of the deadlock argument).

For instance (Chomsky 1966, 95 n 61), given that the discussion by such (near-)contemporaries of Descartes as “Beauzée,” “Cordemoy” and “Herbert of Cherbury” of “the question of how creative thought is possible” $\langle \text{problem } X \rangle$ “was no more satisfactory than any account that we can give today $\langle \text{after a period } t \text{ of more than two centuries} \rangle$ —that is left as complete mystery,” it follows from the deadlock argument that the problem of how creative thought is possible is beyond our cognitive capacities;

[46] the archetypical case of creative use of language and freedom.

The archetypical conclusion transcendently obtained *via* the deadlock argument is most of the time just taken for granted:

Concerning “the question of causation of behaviour, and more broadly, our ability to choose and decide what we do” and such “questions of will and choice” as “accounting for the creative aspect of language use” or “Descartes’s ‘indeterminate action’,”¹²⁷ they “will¹²⁸ remain shrouded in mystery¹²⁹ even if we achieve the fullest imaginable success in a study of the mind of the sort I have been discussing,” since “even the most amazing success in this endeavour, while it might identify the mechanisms of mind, would not answer the question of how they are used” (Chomsky 1980, 46 and 79–80).

D-3 SEM and the interface to the Conceptual system

[In this subsection, we show that [47] Marr’s theory of visual perception sets an inspiring example, but that [48] the Chomskyan theory of SEMs totally fails to attain its standards of predictive rigour and non-defectivity.]

[47] The non-structurally mc-defective theory of visual perception provides an inspiring example of precise, formal specification of ‘(interface) configuration.’

47a – On Marr’s theory as an example of non-structurally mc-defective cognitive theory.

“Along lines pioneered by David Marr (1982)”¹³⁰ and his collaborator Shimon Ullmann (1979) “the theory of visual perception” Tmv “is mostly concerned with operations carried out by the retina or, loosely put, the mapping of retinal images to the visual cortex.” The first reason why this work is of relevance here is that, with its “famous three levels of analysis—computational, algorithmic and implementation,” and given available theoretical accounts Tcv of the anatomy, physiology and corporeal texture Rcv of the organic visual system [particularly as enriched by the seminal contribution of Gehring 2005¹³¹], it is sufficiently advanced that some reasonably satisfactory (falsifiable, empirically corroborated, sooner or later to be superseded, etc.) explanatory account be given of how it is that, given the kind of corporeal substrate that Tcv tells us that we have we should come to have the kind of structured mental

representations that Tmv tells us that we have—thus setting an example for what the missing overarching theory Tmc of $\boxed{31}$ or $\boxed{35}$ would need to be like for Chomskyan theory not to be structurally mc-defective.

47b – On Marr’s theory as an example of precise formalisation of the notion of ‘configuration.’

But of more particular interest for us here is the way one of the constructs of Tmv may be seen to formalise the notions conveyed by ‘configure’ or ‘configuration’ in two of their pre-scientific acceptations:

In its study of “vision” “as a mapping from one representation to another, [...] the initial representation [...] consist[ing] of arrays of image intensity values as detected by the photoreceptors in the retina” (Chomsky 2000, 159, quoting Marr 1982, 31), crucially, an algorithmically derived construct of Tmv is that of “minimal visual experience” [MVS] defined as “a particular assignment to values of coordinates of a retinocentric six-dimensional volume. Each of the points in this volume has a specific set of ‘spatial’ and ‘color’ coordinate values, the spatial coordinates being (visual) depth,¹³² altitude, and azimuth and the color coordinates hue, brightness and saturation.”

This construct of Tmv arguably brings “completely internalist” scientific support (in the particular case of visual perception) for the pre-scientific Kant-inspired¹³³ notion that it is our mind from within and “not the world outside” which shapes [*aka*, in a first sense, ‘configures’] experience and determines “how we perceive things as such-and-such.”

To take the example of ‘green’ and color perception, Marr’s theory shows in substance that one should not properly say that a person “*sense[s] green*” [(as the sense-datum theory¹³⁴ suggests), because Tmv precludes any conception of “the mind as a theater populated by green sensations at which some internal homunculus (or the person, for that matter, stares”]; nor that a person “*sees green*” [(as—cf., *mutatis mutandis*, note 124 *supra*—the re-presentationalist theory of perception suggest)s, because—since “the internal operations” of Tmv “modify and ‘add’ a great deal to the input,” “there is” “no [...] one-to-one matching of color experience and spectral inputs”]; rather, “[a person’s] visual system/mind/brain ‘senses *greenly*” [(which suggested the appellation “adverbial theory of sensation” for Tmv), because, under Tmv, to experience greenness is to apprehend (*aka* configure) the world through the prism of a self-assigned triple array of HBS values] (Chomsky 2000, 159; McGilvray in Chomsky 2012, 257–256; 247, note 1, 256).

And finally, since “the visual system” is a “peripheral, [...] input [...] and output system” which “receives data from the outside and transmits data to the inside” (Chomsky 2012, 69), its output is fed to other systems. But since the MVSs internally generated by the visual system are fully specified six-dimensional formal objects which, as such, are *a priori* liable to further algorithmic treatment, given the need for ‘downstream’ systems to adjust to their specificities in order to be able to use them as input for their internal computations, the well-established six-dimensional format of MVSs could conceivably help in ‘configuring’ (now in a more technical sense) the interface(s) of the visual system to such distinct but related systems as (idem, 257) the “object-[configuration system]” and the “facial-configuration syste[m]” <‘configuration’ rather than ‘recognition’ (compare with the excerpt cited in [26] *supra*), presumably to avoid representational overtones>.

[48] By contrast, the theory of SEM shows no sign of overcoming its overreach problem and remains at the level of naïve theory suggestions about (interface) configuration.

[At this juncture, we show that (48a) the Chomskyan theory of SEMs purports to be a Marr-like ‘adverbial’ theory of concepts and argue that, in cruel contrast with its inspiring model, this account (48b) is pseudo-formal (and still plagued with overreach vacuity), fails (48c) to provide any formalisation of the notion of ‘interface configuration,’ and may only (48d) plead rough weather today but sunshine tomorrow.]

48a – On the theory of SEMs as purporting to be a (partial) ‘adverbial’ account of concepts.

In clear reference to Marr’s theory of visual perception, “like the adverbial account of a color” (cf. 47b) (McGilvray in Chomsky 2012, 259),

SEM’s, [...] construed as complexes of lexically specified innate concepts [cf. note 124 *supra*] do their ‘work’ in an ‘adverbial’ way. They fix—or with other systems contribute to fixing—the ‘hows’ of experience: the various manners in which one can conjecture, understand, imagine and experience. Interpretation is not a matter of searching for the right concept or right description to fit some ready-formed experience, but a matter of ‘making’ the experience, here understood to be participating in a cooperative exercise involving several mental systems, each with its unique form of contribution (idem, 255).

So that (on McGilvray's view, tacitly accepted by Chomsky in the co-authored Chomsky 2012) the theory of SEMs and the theory of MVSs are but two illustrations of "the crucial point [...] that innate conceptual, linguistic, sensory and other forms of internal 'cognitive' machineries partially determine experience in that they—not the world outside—fix how one can see and understand" (idem, 255).

48b – On the actually offered theory of SEM as pseudo-formal and still plagued with overreach vacuity.

As an example of the way the internal cognitivestructures of the mind shape [in a non-technical sense, 'configure'] understanding, Chomsky cites the typical example of how a kid, when told a story about a donkey being turned into stone, had no difficulty following the narrative and spontaneously conceived of the petrified result of the metamorphosis as a trivially mineral avatar of the donkey, with no rupture of 'psychic continuity.' Similarly, as an example of the way the mind projects into the outside world to forcibly perceive things as such-and-such, McGilvray (219 n 3) notes the way "children do often invest their toys with psychic continuity, as well as other properties of living creatures."

But <all square-bracketed page numbers referring to Chomsky 2012> when it comes to the formalised account of these interesting remarks [in cruel contrast with Marr's theory of the MVSs, which is not content with the interesting but unformalised remark that visual perception is a matter of depth, altitude, azimuth, brilliancy, hue and saturation], all we are offered by way of formalisation is that among the "abstract features" (108) that may constitute SEMs is the feature ... 'PSYCHIC CONTINUITY' (203, 219). So that (judging by Chomsky 2012), after fifty years, it would seem that hardly any progress has been made since the heroic 'Markerese' days (cf. [10] and [12] *supra*) and that the old vacuous trick of turning the object language into (pseudo-) formal meta-language by the magic wand of capitalisation is still resorted to [in order to show that this is not just a case of one-off relapse into type [11] overreach vacuity, cf. the similarly unilluminating or question-begging resort to such features as 'ARTIFACT,' 'SHELTER,' 'SUITABLE MATERIALS' (198), 'NATURAL OBJECT' (218), 'INSTITUTION' (203), 'PERSON' (259) and 'POLITY' (202).

48c – SEMs and interfacing: configuring, instructions and hand-waving.

In the non-technical sense of ‘configuring,’ such decapitalised naïve theory concepts as ‘polity,’ ‘psychic continuity’ or ‘artifact’ may be quite insightful as to how we experience the world as such-and-such. But [again, in cruel contrast with Marr’s theory], since no formal apparatus is offered that could effectuate calculations on their capitalised counterpart [and thus serve as a predictive model of the postulated underlying cognitive machinery], any talk (cf. for instance, Chomsky 2012, 191, 188, 300) of the available theoretical construct ‘SEM’ as, in a technical sense of those terms ‘configuring’ the ‘interface’ or sending ‘instructions’ to any other system is just wishful thinking, concept-dropping or mere hand-waving.¹³⁵

Furthermore, back to informal terms, while it seems to make some sense that phonetic features reaching “PHON” should be viewed as sending “instructions” to the “articulatory” system on the other side’ of PHON (Chomsky 2012, 300), it is difficult to grasp (even vaguely) in what sense the features successfully reaching SEM should be viewed as sending ‘instructions’ (as distinct from ‘providing information’) to the conceptual system on the other side of SEM.

And finally, instead of SEMs somehow, in a loose sense, ‘formatting’ or configuring’ the system on the other side, it would rather seem to work the other way round, since it is a minimalist stricture on derivations that (on pains of a ‘crash’), by the time they reach the SEM/CI interface, they should have been weeded of all features ‘illegible’ to the conceptual system.

48d – SEM theory of concepts and empirical insolvency: from deferred payment to transfer of debt and someone else’s (de)fault alibi.

Spinning the yarn of a financial metaphor, under 16a, being empirically accountable and corroborated is a debt on any Galilean style-abiding Theory, and we have seen that, on that account, Chomskyan theory is insolvent. Until stage VI, one available defence for the debtor is to plead that Chomskyan theory is a relatively young discipline and that the situation will improve as further research develops (the deferred payment strategy). From stage VII, a second line of defence may reinforce the first. Since, as we have seen in 48a (Chomsky 2012, op. cit. 259), “understanding” involves “participating in a cooperative exercise involving several mental systems,” which it is the job of other theories to deal with [for instance those of the C-system(s) on the other side of the SEM/CI interface], being accountable for empirically observable facts of (mis)understanding is a collective burden which cannot be incumbent on just Chomskyan theory (the partial transfer of debt strategy). And finally,

it might also be pleaded that the reason why the SEM constructs cannot really be empirically tested to demonstrate their adequacy [we shall say is ‘structurally mC-defective’¹³⁶] is that the theories with which to convincingly dovetail have not been developed yet (the someone else’s (de)fault alibi).

D-4 R-theory and the interface to the Intentional system

[In this subsection, we show that, in its account of (R-)reference and of its relation to the Intentional systems, Chomskyan theory relies (D-4a) on a number of type (b) pro- and anti- assumptions and (D-4b) faces severe difficulties.]

D-4a – On some of the underpinnings of the ‘syntactic’ R-theory of ‘denatured’ reference.

On these issues, Chomsky

49 rejects the traditional word-world view of semantics on the basis of **41** and **43**:

In this traditional, externalist approach, “semantic discussion generally [...] focuses on what traditionally have been called matters of truth (for sentences) and reference (for terms), thus on aspects of [cf. note 114 *supra*] what philosophers call ‘intentionality’” (Chomsky 2012, 251).

Chomsky rejects the Fregean versions of this approach because (cf. **41**) he claims that there are no Platonic entities to which to ‘refer’ and he rejects its re-presentational, Fodorian versions because (cf. **43**) no relation of denotation (*aka* ‘reference’) can be defined as the inverse of a stable physical world-word nomological, causal relation, since there simply is no such relation that the relation of denotation could be the inverse of;

50 considers that it is a good thing to “appropriate much of the formal machinery [...] and insights [...] of formal semantics”¹³⁷ (Chomsky 2012, 217),

and therefore, in order to avoid both of the pitfalls of **41** and **43**,

51 opts for an internalist variant of “Model-theoretic” approach to reference (Chomsky 2012, 230):

In terms of this approach, the reputedly untenable traditional notion of ‘reference’ is theorised in terms of a “relation R” between formal entities

of language and a “postulate[d] [...] domain of mental objects,” so that, being defined as a relation from mental entities to mental entities, the ‘R’ theorisation of ‘reference’ is no longer external, but purely “internal to the theory of mental representations” and,

52 by deliberate resort to such constructs of model-theory (Hodges 2006) as ‘reference’ or ‘truth’ in a model (Chomsky 2012, 229 n 1), obtains only a “denatured version” of “the ‘real’ truth” and “reference” (Chomsky 2012, 207, 230, 208).

For instance, in this type of closed-circuit mode of functioning, one could theorise constraints on co- or disjoint reference in terms of identity or distinction of co-jointly R-allotted mental objects without having the slightest idea of what the relation might be between those postulated mental objects and the ‘outside world.’ Similarly, *via* ‘internalised’ techniques of truth-assignment as functions from predicates to n-uples of members of the postulated domain, one could conceivably obtain a theory of linguistic inference as non-loss of R-truth without having the slightest idea of how truth might alight from the ‘outside world’ onto proposition (veridiction).¹³⁸

D-4b – On some of the difficulties with the ‘syntactic’ R-theory of ‘denatured’ reference.

However, since

53 formal representations *per se* are as devoid of reference as they are of meaning, (cf., *mutatis mutandis*, **8-10** and Pamiès 2001, 334–35),

54 the R-theory of reference suffers from overreach vacuity, for inevitable lack of *per se* referentiality of whatever formal *locus tenens* are offered for its postulated “mental objects.”

And since

55 it would seem that any theory of co- and disjoint reference must somehow accommodate expressions with and without, in some pre-theoretic sense, an existing ‘referent’ on an equal footing:

For instance, “the principles of pronominal reference” which say that in “‘John thinks he is intelligent,’ *he* may refer to John, but not [in] ‘he thinks that John is intelligent’” could be “account[ed for] by a theory of the structural configurations in which a pronoun can acquire its ‘reference’ from an associated name that binds it.” But the problem is that the same principles seem to be at work in such strictly parallel cases as “‘the average man thinks that he is intelligent’ and ‘he thinks that the average man is intelligent,’” though in such cases there would seem to be no “reference” to acquire, since “no one assumes that there is an entity the average man (or John Doe [“introduced as a designation for ‘the average man’”]) to which the pronoun is entitled to refer in one but not the other case” (Chomsky 1986, 44).¹³⁹

And since, furthermore,

56 the R-theory solution to this accommodation problem is to include in its postulated mental objects “quasireferent[s]” which are explicitly denied existence (Chomsky 1986, 79):

To accommodate such expressions as “the average man,” the R-solution is to analyse them as “*r-expressions*, where the term indicates that they function in a quasi referential function, not in the sense of true [external] reference but rather in that they may be taken to denote elements in [the] associated model [the “domain” of **52**],” with “denotation” redefined as “the relation between an r-expression [for instance a pronoun antecedent] and the element or elements to which [in a new technical, internalist sense] it ‘refers,’ or which satisfy it, in the case of a variable”¹⁴⁰ (Chomsky 1986, 79).

As a result

57 the R-theory of reference suffers from apparently insoluble ontological inconsistency:

The denotata postulated for such r-expressions as “the average man” or “John’s lack of talent” are held to be “mental objects” (cf. **51**). Hence,¹⁴¹ by **19** and **22**, they must be deemed to really exist. But at the same time (cf. **56**) they are denied existence. So that, if we label ‘D’ the domain (*aka* model) of **51** and **56**, the “domain D of individuals that serve as values of variables and as denotata” “of names, etc.” (Chomsky 1981, 324), the

domain D is paradoxically populated with existing non-existents, or, if one prefers, non-existing existents.

For internalist reasons [as argued in 42] any Meinongian approach that tries to find a way out of the paradox by working on the *Sein/Sosein* distinction is excluded.¹⁴² Of course (as suggested in Pamiès 2015, 255, n. 144), an extended ‘universe of discourse’ defined as distinct from and encompassing standard ‘domain of discourse’ [idem, 6.3, 29–31—and particularly (XXX)] could provide a haven for those ‘homeless objects.’ But that would involve endorsing not only Platonism, but ‘Plenitudinous Platonism’ (*aka* ‘FTP’) (in the sense of Pamiès 2015, 254–255, n. 143), a totally unacceptable perspective in the light of 41.¹⁴³ So that, no way out of the paradox being in sight, under the type (b) contentions of Chomskyan theory, the R-theory of reference would seem to be caught in hopeless ontological inconsistency.

Furthermore,

58 to offer any perspective of accounting for the ‘intentionality’ or ‘aboutness’ of language, this R-theory “denat[ur]ing both [truth and reference]” (Chomsky 2012, 230) would need to be relayed by the ‘intentional system(s)’ on the other side of the SEM/CI interface:

“th[is] study of the relation of syntactic structures to (models [...]) should be regarded as pure syntax,¹⁴⁴ the study of various mental representations, to be supplemented by a theory” of non-denatured “real semantics,” i.e. “of the relation these mental objects bear to the world or to the world as it is conceived or believed¹⁴⁵ to be” (Chomsky 1986, 45 and 1981, 324),

so that,

59 In the meantime, the SEM (R-)theory of reference will remain (largely) empirically insolvent.

Just as in the case of 48d, since referring involves participating in a cooperative exercise involving several mental systems, which it is the job of other theories to deal with, being accountable for empirically observable facts of (mis)referring is a collective burden which cannot be incumbent on just Chomskyan theory (again, allowing for the transfer of debt strategy). And finally, it might also be pleaded that the reason why the SEM R-constructs cannot really be empirically tested to demonstrate their adequacy [we shall say it is ‘structurally mI-defective’¹⁴⁶] is that the theories with which to convincingly dovetail [for instance those of the I-

system(s) on the other side of the SEM/CI interface] have not been developed yet (again, the someone else's (de)fault alibi).

D-5 SEM, R-theory and defectivity: empirical insolventy, transfer of debt and the spectre of transcendently self-proclaimed bankruptcy

[In this subsection, we show that (D-5a) 'real' understanding and referring being both held ([60], [61]) to be aspects of creative use of language, should be viewed ([62]) as beyond our species-bound intellectual capabilities if the pronouncement of [46] is taken seriously; then (D-5b) we show ([63]-[65]) that the impact of [62] on Chomskyan theory would be disastrous, but argue ([66]) that there is no compelling reason to take [46], hence) [66] (or its would-be impact) seriously.]

D-5a – On Chomskyan pronouncements on 'real semantics' understanding and referring.

Given that

[60] 'real' understanding is a matter of creative use of language:

[Assuming the argumentative content, concepts and terminology of [48] *supra*] we have seen in substance that “in any case in which a SEM and its conceptual ‘information’ [...] is used, that is when it plays a partial role in interpretation or understanding, multiple systems come into play.” But as a result of this, “the concepts or clusters of semantic information that are expressed [as features] at SEM can receive multiple applications skewed to serve various human interests” (McGilvray in Chomsky 2012, 161). So that the way “things” are actually understood and interpreted results from such “a massive interaction effect” that in order to obtain a “determinate, scientific theory of ‘what happens’ on the other side of SEM,” one would need to find the key to some “central cognitive system, [a] device that [would] giv[e] humans cognitive flexibility by taking contributions from various systems and integrating them.” That is, one would need a theory capable of “describing and in some sense explaining how a ‘massively modular’ mind manages to integrate its components and produce coordinated actions, with special focus on the role of language.” In other words, one would need to elucidate (one fundamental aspect of) the mystery of the creative use of language (cf. [46]) whereby “humans [have]

the ability to speculate and wonder, take their thoughts to any situation at any time, fantasize, engage in all sorts of thought, and so on” (McGilvray in Chomsky 2012, 162 and 161),

61 ‘real’ referring is also a matter of creative use of language:

[Assuming the argumentative content, concepts and terminology of **48** and note 124 *supra*]. When speakers “use [...] a mode of presentation” (*aka* MOP) [associated with this or that expression, and ‘expressed’ by this or that (cluster of) features forming the SEM of that expression], the way they “apply” it “often [...] has nothing to do with whatever distal entities [triggered] the mode of presentation’s acquisition” (219). In fact, by ‘projection,’ they constitute at will what they take to fall under the MOP, and are under no *diktat* of any outside impingement. In that sense, “referring or denoting is something that people do” (188) “and do freely” [208], stimulus-free, “a form of (free) human action—not some kind of ‘natural relation as Fodor wants to believe” (188). So that, just as ‘real semantics’ understanding, ‘real semantics’ referring is a product of the creative use’ of language” <all pages references to McGilvray in Chomsky 2012>.

As a consequence of **60** and **61**, under **44** and **46**, by transcendental argument,

62 the workings of ‘real semantics’ referring and understanding are beyond our species-bound intellectual capacities.

The inferred conclusion is stated with usual type 22c-f variation in degree of epistemic confidence, from the “they will forever remain shrouded in mystery” of **46**, *via* its ‘muffled’ softening in note 128, down to the ‘probably,’ ‘I doubt,’ ‘appears to be’ and ‘likely impossible I suspect’ of:

“interpretation [*aka* understanding] is probably beyond the reach of science”; “for reasons that go back to the creative aspect of language use, I doubt that these [issues of ‘real’ understanding] can be addressed in a scientific way”; “‘real’ reference” or “referring” is a “matter of” “free” “human uses of terms and sentences” and, as “a form of human action[,] appears to be out of reach of science”; “it is not clear what to do about truth if one wants to keep anything like ‘real’ truth (likely impossible, I suspect)” (McGilvray in Chomsky 2012, 219 n 3; 162; 230, 217 and 250; 230),

and Chomsky spells out (cf. [45] *supra*) a reactualised version (at $t + 23$ years) of the longstanding deadlock argument leading on to [62]:

“General issues of intentionality, including those of language use, cannot reasonably be assumed to fall within naturalistic inquiry.” Just as they did at the time of “Cartesian dualism, the scientific hypothesis¹⁴⁷ that sought to capture, in particular, the apparent fact that normal language lies beyond the bounds of any possible machine, [...] they still seem to pose a complete mystery. They are, for example, unaffected by the transition from the complex artefacts that intrigued the Cartesians to today’s computers, and the brain sciences shed little light on them” (Chomsky 1995b, 27).

D-5b – On the would-be impact of these transcendental pronouncements.

[At this juncture, to measure the impact of [62], we shall first consider <in [63] and [64]> what the situation would be like under [48] and D-4b, but without D-2 and D-5a (hence, crucially, without [62]); then, on that basis, we shall argue <in [65]> that if the conclusion of [62] were taken seriously and fully endorsed, then the unwitting impact on Chomskyan theory would be disastrous; and finally <in [66]> we shall explain why these pronouncements should not be taken seriously.]

If neither [62] nor D-2 and D-5a are endorsed, then

[63] If a theory of interfacing to the CI systems just happens to be contingently unavailable, then, in its internalist approach to reference and meaning, Chomskyan theory just shows further signs of *de facto* empirical insolvency.

Briefly recapitulating, we have seen [in 13a] that a key justification offered for the idealisation to competence is that “competence underlies performance [*aka* use of language].” So that, under 32a, and using the metaphors of [59], it is a ‘debt’ to Chomskyan theory that it be empirically corroborated by a theory of language use successfully integrating its constructs. We have seen that if by ‘use’ ‘processing’ [one of its dubiously conflated acceptations, cf. 13c] is understood, then Chomskyan theory is seen [cf. 32a] to be still insolvent [mc-defective] for lack of any available successful hosting theory. What [63] adds is that if by ‘use’ ‘use *stricto sensus*’ [its undistended acceptance] is meant, then Chomskyan theory is seen to be as yet insolvent [both structurally mC- and mI- defective, hence, for short, ‘structurally mCI-defective’¹⁴⁸] on another account, for, *mutatis mutandis*, the same reason.

Finally, we have seen (cited in [4]) that a central goal of Chomskyan theory is “to offer explanations on formal grounds for the linguistic intuitions of the native speaker,” particularly (cited in [6]) on “the correspondence between sound and meaning in the[ir] language,” and that [cf. note 32 *supra*] “there is no way to avoid [...] the assumption that the speaker-hearer’s intuition is the ultimate standard that determines the accuracy of any proposed such formal explanation.” But the only intuitions that can be elicited from people have to do with raw tokens of ‘real’ (mis)understanding or (mis)referring. So that, in the case envisaged in [63], as concluded in [59] and 48d, the significance of these intuitions for the validity of theoretical claims about a subsystem contributing for a small part only to the advent of those intuitions cannot be discerned unless (i.e. until) a theory of performance is available to sort out the tangle of co-contributing system—which, in the meantime, compounds the empirical debt of Chomskyan theory.

In the eventuality envisaged in [63], then, on the one hand, the empirical insolvency problem is not to be taken lightly. In the present-day stage in the advancement of learning, it is provisionally impossible to empirically corroborate the adequacy of any of its theoretical constructs offered to formally account for the sound-meaning correspondence, and it is provisionally impossible to empirically corroborate the validity of its key idealisation to competence. So that, because of this major contravention to 16a and lack of empirical falsifiability and support, it is provisionally impossible for Chomskyan theory to qualify as a Galilean style-abiding science at all, let alone on a par with physics.

But at least, on the other hand, the metaphorically expressed strategies of [59] and 48d are available: the debt will be paid later (deferred payment), it will be someone else’s job to pay (transfer of debt), the blame cannot be on us if it has not been done yet (the someone else’s (de)fault alibi), and there is some residual hope for the future: should someone (else) foot the bill one day, the ‘provisional’ shackles might then fall off.

Furthermore,

[64] if a theory of the interfacing to the CI systems is *de facto* unavailable, then it might not be totally inconceivable that the unintelligibility *per se* of (the *locus tenens* of) formal representations should cease to pose an overreach vacuity problem for Chomskyan theory.

[Assuming the argumentative content, concepts and terminology of D-3 and D-4b, and elaborating on note 135 *supra*], as outlined in D-4b and [48], in the absence of any mention of relations to the corporeal, a non CI-

defective theory of SEMs and R-reference is just a sophisticated simulation of type 31c. As such, it is not absolutely devoid of empirical content, but, as it happens, it is still plagued (cf. 48b and 54) by overreach, type 1 vacuity (cf. 11, 12 and note 21 *supra*).

However, if—somehow, in a presumably remote future—the mc-defectivity, not only of Chomskyan theory, but also of the theories of the systems on the other side of the SEM/CI interface could be remedied (a possibility no longer *a priori* precluded as soon as the transcendental pronouncements of D-2 are lifted), then things might be quite different.

[Closely following the template of 47] to sharpen the vague suggestions of 48], a Marr-like adverbial theory Tadv of concepts could then actually become available, with its three interrelated levels of computational, algorithmic and implementation. Anterior to the advent of such a non mc-defective theory, given a word *x* (more generally, an expression), its conceptual import *X* is theorised as MOP*x*, where MOP*x* is a particular SEM, SEM*x*, consisting in a package of *n* features *Fx*1, ..., *Fx**n*.

But once successful non-defective Tadv theorisation is available, Tadv (*X*) [i.e. Tadv (MOP*x*), i.e. Tadv (*Fx*1, ..., *Fx**n*)] could, not inconceivably, be an algorithmically derived [and cerebrally implemented] construct defined as a particular assignment to values of coordinates of a conceptual *n*-dimensional conceptual space (or matrix). Under such a theory, instead of improperly saying, in pre-theoretical terms, that ‘*Y* falls under *X*’ [where *Y* may be just about anything on which attention comes to be focussed], one should more properly say that ‘(on a certain occasion,) the mind/brain of the speaker conceives *X*-ly of *Y*’—by which it is meant that, under Tadv, to conceive of *Y* as *X* it is to apprehend *Y* through the prism of a self-assigned *n*-tuple array of Tadv (*X*) values.¹⁴⁹

By thus assuming the impossibility of by-passing the problem of underlying processes, this alternative, algorithmic mode of theorisation might arguably be held to provide “explanations on formal grounds for the linguistic intuitions of the native speaker” without having to resort to a supposedly expliciting metalanguage itself in need of explication, so that the unsolvable (vacuity₁) overreach problem no longer arises.¹⁵⁰

[The point of 64] is not to recommend this hasty sketch of what a theory *T*_m could be like as anywhere near the mark, unproblematic, or particularly promising (if at all plausible), but simply to underline the fact that, assuming the pronouncements of D-2 to be ill-founded, there is at least the faint glimmer of a hope that a quiet way out of the vacuity₁ overreach problem that has plagued Chomskyan theory for a good half century can be found if a theory of processing is built into the theory of competence.]

By contrast, then

65 taking seriously and fully endorsing the pronouncements of D-2, D-5a and **62** would amount to transcendentally self-declared disastrous bankruptcy.

65a – Why ‘bankruptcy.’

We have argued at length that—continuing to harp on the financial metaphor of 48d [taken up in **59** and pursued in **63** and **64**—it is a ‘debt’ on Chomskyan theory as a Galilean-style enterprise that it be(come) empirically refutable and corroborated [cf. 16a and note 35 *supra*]; and we have argued at length [as recapitulated in **63**] that it will remain insolvent on that account as long as its mc- and m-CI structural defectivity [cf. 32a and (subsumed in **63**), 48d and **59**] will remain unremedied. But if **62** is endorsed, then, by alleged biological necessity, Chomskyan theory will forever remain insolvent, and neither the deferred payment nor the transfer of debt strategies can work: from paying on the never-never to no one ever paying, that is called bankruptcy.

65b – Why ‘amounts to,’ and not just ‘self-proclaimed’

Chomskyans would seem to take issues of empirical accountability too lightly to show much concern about historically contingent or biologically necessary insolvency. For instance, Chomsky pays lip service to the fact that “when we develop a theory about our thinking, about our computations, internal processing and so on [...] it’s going to have to be translated into some terms that are neurologically realizable” (Chomsky 2012, 9) but he does not appear to worry about the fact that today’s state of the art shows every sign of mc-defectivity.

Similarly, we have seen [in **61** and **62**] that the reason why McGilvray states that “interpretation [*aka* understanding] is probably beyond the reach of science” is that (being a matter of creative use of language) the interfacing to CI systems is beyond our cognitive reach. But—since, by the same token, any abstract theory positing constructs meant to interface to CI systems is deemed forever structurally defective, hence forever empirically insolvent—when he adds that “the potential contributions of various cognitive systems are not [beyond the reach of science]” (McGilvray in Chomsky 2012, 219 n 3), it sounds in context as if the touchstone to separate what does not pertain to science from what does could be reduced to just the difference between (respectively) having

nothing to say [on issues of causation of creative behaviour] and having something to say [e.g. about competence]. In other words, what would seem to be overlooked here is that what really matters is the difference between having something to say that cannot be empirically refuted (“storytelling,” Chomsky 2012, 128) and having something to propose that, being falsifiable, may be empirically corroborated [scientific].

In a nutshell then, what Chomskyans would rather ignore [and will never themselves proclaim] is that on matters of empirical insolvency, at the end of the day, if no one can foot the bill, bankruptcy looms.¹⁵¹

65c – Why ‘disastrous’

By uniformly replacing each occurrence of ‘it is provisionally impossible to’ by an occurrence of ‘by biological necessity, it will forever be impossible to’ (with trivial pruning and adjustment) in the last paragraph but one of [64]—and with exactly the same argumentative underpinnings—it is possible to measure the consequences of [62] for Chomskyan theory: under [62], by biological necessity, it will forever be impossible to empirically corroborate the adequacy of any of its theoretical constructs offered to formally account for the sound-meaning correspondence, by biological necessity, it will forever be impossible to empirically corroborate the validity of its key idealisation to competence. So that, because of this inescapable contravention to 16a and insurmountable lack of empirical falsifiability and support, by biological necessity, it will forever be impossible for Chomskyan theory to qualify as a Galilean style-abiding science at all, let alone on a par with physics.

So, Chomskyan theory will never be a science, its goals will forever remain unattained, its idealisations unjustified, its expectations unfulfilled, and the generative enterprise is something like an alchemic quest that cannot even dream of anything like posthumous nuclear transmutation rehabilitation.

The impact of [62] is so disastrous that one barely dares mention this further consequence that, by way of perspectives for hopefully finding a way out of the insoluble vacuity problem posed by the unintelligibility *per se* of (the *locus tenens* of) formal representations, Chomskyan theory is now plagued with yet another form of overreach vacuity (due to the genetic limits of our faculties),¹⁵² a much more severe handicap than mere *de facto* insolvency, since, under [62], the prerequisites for empirical accountability are forever beyond our species-bound intellectual capacities.

However,

66 given that such pronouncements as 62 are logically inconclusive and that the Chomskyan themselves do not seem for a second ready to envisage or acknowledge the disastrous impact of 65c, it would seem that the bankruptcy of 65a is just a ‘spectre,’ a figment of Chomsky’s transcendental inclinations not to be taken seriously.

We have argued that though (cf. notes 73 and 74) transcendental inferences are logically non-compelling (*aka* fallacious), Chomsky finds it difficult (cf. 22e-f and note 69) to keep a compelling urge to jump to transcendental conclusions in check. But, given that (though in a somewhat incoherent way), the pronouncements of D-2, D-5a and 62 seem to be left to run their own parallel course—as if they had no impact on Chomskyan research—we shall take them to be ‘unmuffled’ Cassandra-style vaticinations that may conceivably be shrugged at, but should not be viewed as counting against the validity of Chomskyan theory and its constructs.

[**To summarise**, leaving aside our financial metaphor(s) and using the typology, terminology and notations of notes 21 and 151 *supra*, in this fourth and last section, we have argued that, at stage VII, Chomskyan theory still suffers from overreach vacuity_{1A} and, because of its extreme idealisation and mc- and CI-defectivity, (if not from overreach vacuity_{1B}), from *de facto* empirical vacuity₃—and therefore does not yet satisfy the defining criteria of Galilean-style scientific explanation.]

Conclusion

To conclude, the critical study of the protean use of ‘representation’ in Chomskyan metalinguistic discourse has provided a valuable lead into the arcane of Chomskyan theory,¹⁵³ which, we have argued, suffers from severe problems of vacuity, and in particular of [crucially, defectivity-induced] empirical insolvency. Of course, it must be acknowledged that success in meeting the strict demands of Popperian falsifiability and corroboration is not the only “credential” for a theory, and that, for instance, “theories get extra credence by entailing novel predictions—that is, predictions such that information about the predicted phenomenon was not previously known and not used in the construction of the theory” (Psillos 2006b, 577–78). But even on that account [at stage VII] the Minimalist *Program* is much too sketchy¹⁵⁴ to be deemed at all empirically supported. To epitomise the acuteness of the problem, the state of the art answer to the mCI-defectivity problem outlined in 48d and 59

supra, all Chomsky is in a position to offer by way of suggestions on how to handle issues of interfacing to the CI systems is (Chomsky 2000, 187):

“At most, I-linguistics is committed to [...]

- a. When X understands the word W, X makes use of its properties, and
- b. The properties might include I-sound and I-meaning and, if so, the latter play a part in determining what X refers to in using W.

Beyond that, the chips fall where they may”—in other words, as Marmin would say, [cf. note 110 *supra*] beyond that, no one has “the foggiest” idea.

The reason why such issues of (mc- as well as mCI-) defectivity are so crucial is that—if idealisation is viewed as distillation and an ‘essence’ as what is left after the distilling process—the object of Chomskyan theory is at least quintessential in that [cf. note 4 *supra*] it has been successively abstracted away from use and processing, from periphery, from external social or cultural determinants, from third factor type general constraints (on organic growth and computation), and from ‘real’ understanding and referring. As a result of this massive process of cumulative idealisation and shrinking of the domain, Chomskyan theory has come to be so removed and cut off from observation that until it can be shown to dovetail with successful theories of the fleshing out of its posited arch-abstractions [thus allowing for empirical testing and corroboration], it will look as if all we have at stage VII is a minimalist ‘story,’ if not an ontological tale of an ideon world of ‘perfect’ design.

With his detachment on issues of falsification and corroboration, Chomsky sometimes fosters the impression, that, by some transcendental anointment of the (default) alibi [cf. 48a and 59], since no one can be blamed, by a kind of reversed methodological dualism, it is as if, while physics must run the gauntlet of risky confrontation with (experimental) empirical data, Chomskyan theory could be exonerated.

But of course, such self-proclaimed all-clear is untenable, and unless some way is found to restore or ensure empirical accountability, at a stage when—every reason having been invoked to justify giving up descriptive work, and rule in advance of objection that any conceivably attainable empirical evidence is too insignificant (*aka* not sufficiently abstract) to count as a counter-example—its contentions remain unsupported, its idealisations unjustified, and its expectations unfulfilled, claiming parity of ontological status with that of physics for the constructs of Chomskyan theory is not asking for benevolent suspension of disbelief. It is asking for faith¹⁵⁵—which may be charismatically obtained, but has no place in science.

In a nutshell, what this work tends to show is that over a period of more than half a century, the generative enterprise, which started with a flourish, has drifted into—part heuristically fascinating, part chimeric—pre-scientific speculation.

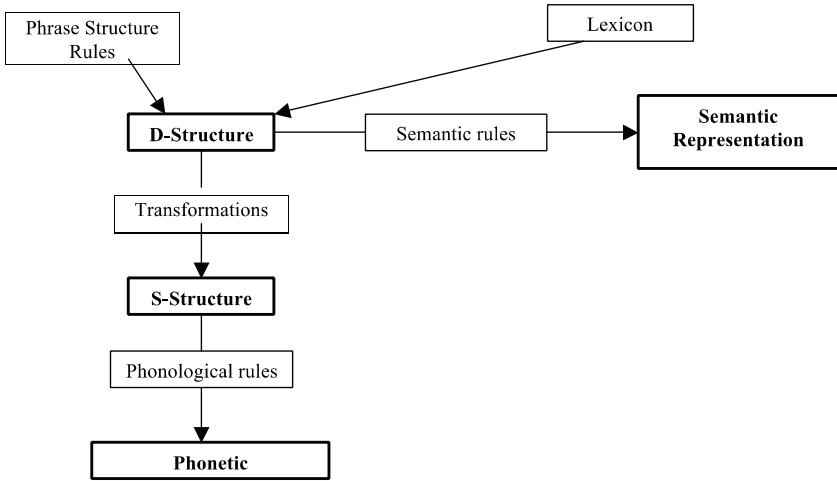


Fig. 3-1. “Standard Theory model of grammar”
 Adapted from Riemsdijk and Williams ([1986] 1987, 172).

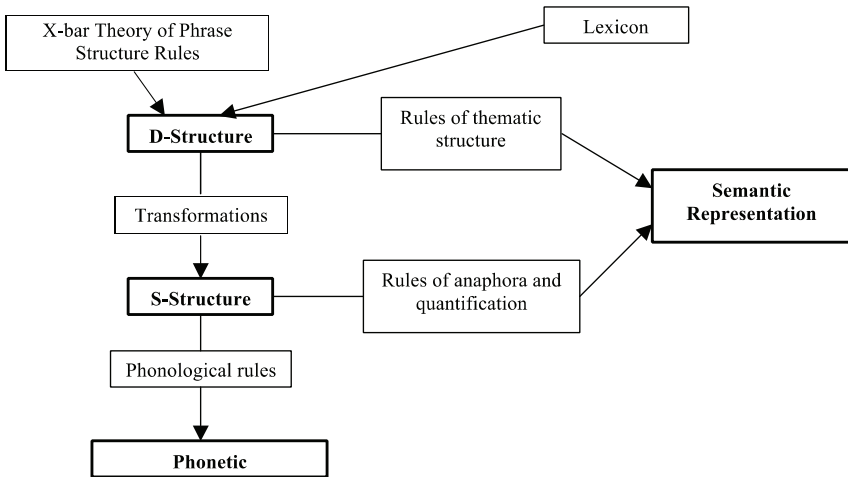


Fig. 3-2. “Extended Standard Theory model of grammar”
 Adapted from Riemsdijk and Williams ([1986] 1987, 172).

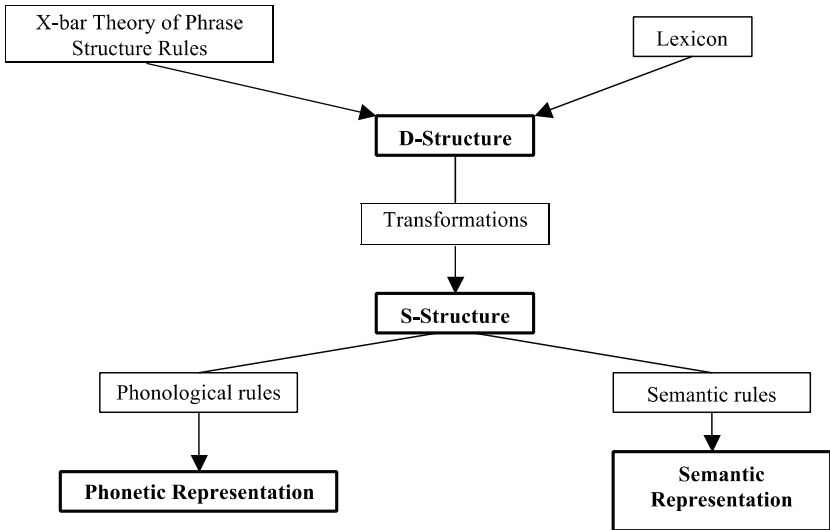


Fig. 3-3. “Revised Extended Standard Theory model of grammar”
Adapted from Riemsdijk and Williams ([1986] 1987, 172).

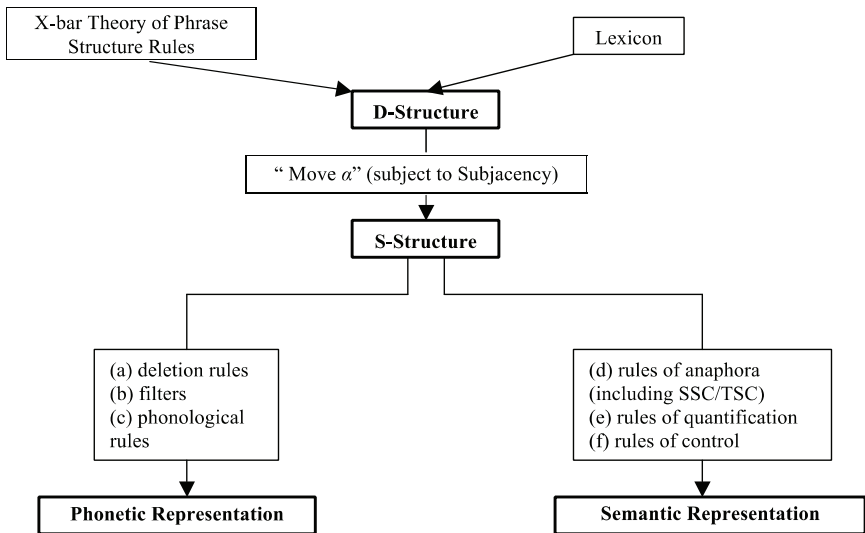


Fig. 3-4. “T-model of grammar”
Adapted from Riemsdijk and Williams ([1986] 1987, 172).

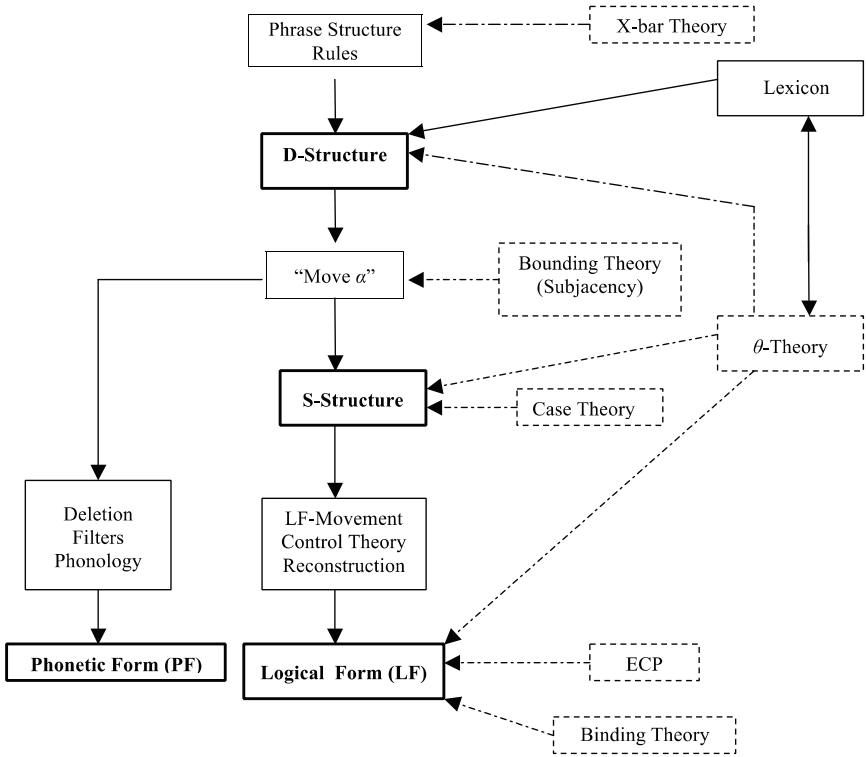


Fig. 3-5. “The organization of the modules of Government-Binding Theory.” Adapted from Riemsdijk and Williams (1986) 1987, 310.

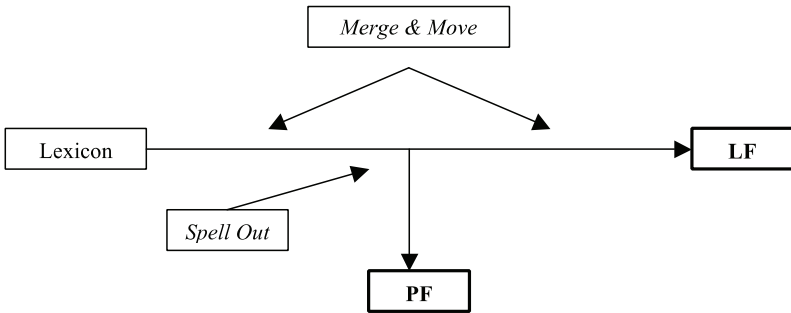


Fig. 3-6. The Minimalist model. Adapted from Abraham et al eds. (1996, 5).

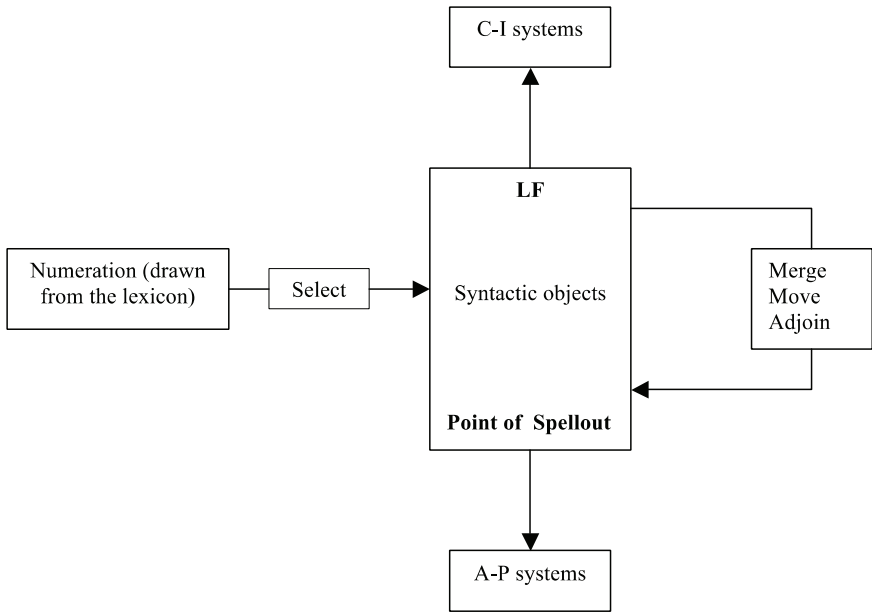


Fig. 3-7. The Minimalist model
Adapted from Adger ([2003] 2004, 146).

PART II

LINGUISTICS AND COGNITION

CHAPTER FOUR

COGNITIVE LINGUISTICS AS ONE OF THE COGNITIVE SCIENCES: A QUESTION OF TERMINOLOGY

KATARZYNA KWAPISZ-OSADNIK

The aim of this chapter is to review the fundamental notions that are embedded in the already-rich history of cognitive linguistics, and which are rooted, on the one hand, in various disciplines such as philosophy, psychology, mathematics and ethnology, and on the other, in the variety of approaches and methodologies found in the study of linguistics itself. This heterogeneity is one reason why in cognitive linguistics, which is often taken to be interdisciplinary, these notions can give rise to misunderstandings and confusion, leading in turn to the use of a terminological jargon that lacks precision. I am well aware that this subject cannot be fully covered in the modest proposal of this chapter and, indeed, could serve as the theme of an entire monograph (and, we may note, there have already been several attempts to systematize these notions of cognitive sciences, and notably of linguistics, in the form of glossaries).¹ Nevertheless, it still seems worthwhile to review the terminological questions in cognitive linguistics in light of the interest that this field currently attracts and in light of the constraints in international exchanges of ideas, which continue to lead to divergences in terminological interpretations. In this chapter, therefore, I will concentrate upon the notions most often utilized in cognitive linguistics, such as category, prototype, semantic invariant, stereotype, schema, imagery, conceptualization, metaphor and metonymy. A review of these terms can clarify their definitional range in relation not only to the disciplines from which they have been derived and in which they function, but also in relation to traditions in the practice of linguistics (for example in the US, in France and in Poland). And therefore the individual term will be the guiding theme of each section, and reflections on the phenomena that determine speech events will be centered on it. While fully aware that this work will not be exhaustive and can give rise to

many doubts and questions, I hope all the same that my observations may shed new light on long-standing views, show how ideas, research topics and approaches have mutually influenced and complemented each other, and stimulate reflection on the question of whether we are dealing with interdisciplinary terminological confusion or instead with a coherent methodology.

Notions – Reference-points in cognitive linguistics

1. Category and categorization

Categorization is a fundamental adaptive conduct by which we “cut out” physical and social reality. Its cognitive function is the creation of categories (objects, individuals etc.) necessary for transition from continuous to discreet (*Vocabulaire de sciences cognitives* 1998, 66, my translation).

These two notions have taken on a new life following the work in psychology of E. Rosch and her collaborators (cf. Rosch 1973a, 1978, Rosch and Mervis 1975). We may distinguish three phases in the evolution of the notion of category: the first is due to Aristotle, who defined it as the general term for the various modes that he identified as being ten in number (substance, quantity, quality, relation, place, time, position, possession, activity, and passivity) (cf. Tricot 1936). On the one hand this definition constitutes the point of departure for the classic definition of category, which has remained in current use up to the present day, and on the other hand it has inspired philosophical (and linguistic) reflection on the nature of universals, whether these be considered in terms of predicates (cf. Karolak 1984, 2007), primitive words (cf. Wierzbicka 1972, 1999), archetypes (cf. Desclés 1990, 2003, 2010), or simple cognitive domains (cf. Langacker 1987, 1995, 2008). In the second phase, the term category has been taken to refer to a set of objects or of concepts having the same properties, an understanding which gave rise to referential conceptions in philosophy and in logic (cf. Kant, Husserl, Mill, Frege, Russell).² Moreover, this understanding of the category is likewise often attributed to Aristotle. By contrast, the non-referential conceptions propose to define the category according to the syntactic terms of phrase, noun, and functor. The syntactic category would thus be a class of expressions which are reciprocally substitutable, therefore remaining within the same grammar. This has given rise to categorial grammars (cf. Ajdukiewicz, Carnap, Montague).³ In linguistics, the notion of the category has become deeply rooted: we may distinguish the syntactic categories in terms of

operator or functor, semantic categories in terms of lexemes or predicates, and linguistic categories in terms of unities that have features in common and that can figure in the same syntactic environment. In psychology, the category developed into an ambiguous notion, becoming open, blurred, graduated, and determined by geo- and socio-cultural factors. Following numerous studies on categorization, beginning in the nineteen-seventies in the psychology of development (cf. Piaget 1937, 2005) and cognitive psychology (cf. Rosch 1975b, Dubois 1983, 2000, Bronckart 2002), scientists came to distinguish scientific categories from natural categories, those that reconciled the classical vision with the cognitive vision, and this admission of the existence of natural categories stimulated an interest in the notions of prototype, of invariant, of stereotype, of scheme, and of metaphor and metonymy.

2. Prototype and semantic invariant

To speak of a prototype at all is simply a convenient grammatical fiction; what is really referred to are judgements of degree of prototypicality (Rosch 1978, 40).

The semantic analysis is the place for prototypes, but also for invariants—the one does not exclude the other (Wierzbicka 1999, 27).

According to Langacker (1987, 1995), categorization is carried out either according to prototype or according to scheme. Categorization by prototype consists in seeking the similarities of a categorized element to the central element of the category, which possesses most features proper to that category. Grzegorzczkova (1998) proposes these definitions according to prototype: firstly the extensional definition, according to which the prototype is the most typical representative of a category around which the other members of the same category are organized; secondly the intensional definition, according to which the prototype is the collection of features typical of a category, and thirdly, which is a variant of the second, the semantic definition, where the prototype constitutes the semantic center that gives access to metaphorical and metonymic extensions. The hesitations of Rosch (1978), to whom we owe the notion, and the considerations of Lakoff (1982) and Kleiber (1990) have led the prototype to be defined in terms of usage, that is to say at the level of expression. This line of inquiry ends with the definition of the prototype proposed by Desclés and Banyś (1997), which is as follows: the prototype is a form or a value of a category which is intuitively the most frequently employed by the users of a given language. This definition seems the most pertinent one

to me, though on the one hand it raises doubts about the typicality of the features (cf. Rastier 1987, Jackendoff 1983, Wierzbicka 1999), and on the other hand it does not hinder reflection about the existence of invariants (cf. Provôt et al. 2010, Wierzbicka 1999, Banyś 2000, Hickmann 2009, Kwapisz-Osadnik 2009), and, furthermore, it raises the question of linguistic norms. As to the typicality of features, the problem is to determine which features are typical: inherent, necessary, essential, stereotypical, contextual, etc.? When it comes to the notion of the invariant, this coexists with that of the prototype, since, as Wierzbicka points out, in the definition of the concepts it must be borne in mind that the consequence, the prototype and the semantic invariant are not mutually exclusive. It is the same if we are considering the prototype in terms of its use. Desclés (1997) proposes three definitions of the semantic invariant: firstly the common denominator, that is, a semantic value common to all the forms of a given category; secondly the abstract formula that is compatible with all the forms and values of a category; and thirdly the prototypical value that presupposes a general tendency. This means that every conceptual and linguistic category possesses a prototypical form or value and a semantic invariant which delimits the functioning of such categories.⁴ Attempts to clear up the ambiguities of use due to context and polysemy are being made in the field of computer-assisted translation: to disambiguate the word would mean to find a single sense on the base of a morphosyntactic context⁵ (cf. Banyś 2005). The operation would permit one to establish the so-called linguistic norm and, in consequence, to designate prototypical uses, these referring to frequency of use (cf. Langacker 2003, 2008) or to preferentiality (cf. Banyś 2000, 2005).

3. Stereotype and linguistic vision of the world (world view)

The only feeling that anyone can have about an event he does not experience is the feeling aroused by his mental image of that event (Lippman 1922, 43).

The diversity of languages is not a diversity of signs and sounds but a diversity of views of the world (Von Humboldt, quoted in Pütz and Verspoor 2000).

Even if the origins of the notion of stereotype are sociological (cf. Lippman 1922, Putnam 1975), in linguistics this notion has provoked considerable debate over its relation to the prototype and on the question of semantic features. This notion has become a crucial one in the language

sciences, above all in the framework of ethnolinguistic and sociolinguistic studies, in which a great deal of use is made of the notion of a linguistic vision of the world, the origins of which can already be found in Aristotle (*loci communes*). Chlebda (1998, 32) demonstrates differences as defined in the notion of stereotype on linguistic grounds, and this subsequently results in the distinction of firstly, stereotype as a reproducible morphological compound word (a lingual stereotype), secondly, stereotype as a specific mental construct (a mental stereotype), and thirdly, stereotype as a mental construct rooted in consciousness by a linguistic sign (a lingua-mental stereotype). As a compound word, a stereotype has three variants, namely topic, formula and idioms. A topic is constituted by stabilized word combinations, which do not have an embedded formal feature, e.g. *foreigners do the hardest jobs*. The formulas function as formal word combinations, e.g. *as mean as a Scot*. Meanwhile, idioms are stabilized word combinations (formulas), which have ceased to provide a clear semantic motivation for language users. In other words, it is transparent or opaque interpretations of idioms, e.g. *to eff and blind* (cf. Tokarski 1998, 125). A stereotype in a mental form is decoded global knowledge in the minds of a given society, which may occur in the form of notions, judgements or images. A lingua-mental stereotype functions in the form of linguistic signs, which occur at the moment of perception of a part of reality, e.g. *while seeing a cat*, we visualise its mental image, which results in the initiation of relevant formulas, such as, for instance ‘not to buy a pig in a poke.’ A lingua-mental stereotype corresponds to a given fixed world perception, which is a feature both of units and social groups. The role of stereotypes which act as the elements of a given world model, and which simplify cognitive processes and allow for quicker orientation in reality, explains the huge interest in this phenomenon within contemporary linguistics, hence inclined towards the interdisciplinary approach. In cognitive linguistics, the word stereotype refers to “a representation of a subjectively determined object; it includes both descriptive and evaluative features, these features being the result of an interpretation of reality in the framework of the cognitive models that are socially determined” (Bartmiński 1998, 64). This means that the stereotype is a set of features that need be neither true nor essential for determining membership in a category, but, being based upon social and cultural factors, they form an integral part of the general knowledge about the category.

As for the linguistic vision (or image, or representation) of the world, this notion would correspond both to a conceptual structure which is consolidated and confirmed within the system of a given language (cf.

Grzegorzyczkowa 1999) as well as to the lexical structure while being taken together with the grammar, namely that which permits the reconstruction of the modes of perceiving and comprehending the world employed by the users of a given language. The idea of a linguistic view of the world is nowadays assigned to Von Humboldt (1836), who stated that languages are means to comprehend different views of the world. However, many philosophers have pointed to the relation between a language and a world vision, proposing a somewhat different approach to the problem; for example, for Von Humboldt there is contained in every natural language its own particular worldview (“so liege in jeder Sprache eine eigenthümliche Weltansicht” 1907, 60), while for Herder a language underscores the spirit of the nation (“Each nation has its own reservoir of thoughts, which became signs, this reservoir is its language : it is a reservoir, which centuries contributed to—it is a reservoir of thoughts of the entire nation” (1877 in Anusiewicz 1999, my translation). Interest in studying the linguistic view of the world reappeared together with the need to describe the dying cultures of different American Indian tribes in the USA. Since then it has become an integral part of ethnolinguistic studies, and the famous Whorf-Sapir hypothesis has become the starting point for creating such research fields, as for instance cultural linguistics or anthropological linguistics. In 1929 Sapir wrote that “Human beings do not live in the objective world alone, nor alone in the world of social activity as ordinarily understood, but are very much at the mercy of the particular language which has become the medium of expression for their society. It is quite an illusion to imagine that one adjusts to reality essentially without the use of language and that language is merely an incidental means of solving specific problems of communication or reflection” (1929, 209). According to Whorf “...users of markedly different grammars are pointed by their grammars toward different types of observations and different evaluations of externally similar acts of observation, and hence are not equivalent as observers but must arrive at somewhat different views of the world” (1956, 221). Contemporary Polish researchers analysing the linguistic view of the world suggest the following definitions of this phenomenon (1999): Anusiewicz states that the linguistic view of the world constitutes the conclusion and the juxtaposition of daily experiences and norms, values, ways of evaluation as well as concepts and juxtapositions towards reality, approved by a communicative community; for Grzegorzyczkowa the linguistic view of the world is a notion structure implemented in a system of a given language, that is in its grammatical and lexical properties of utterances. Tokarski treats the linguistic view of the world as “a set of patterns included in

grammatical units and lexical structures, demonstrating different ways of viewing and comprehending the world” (1999, 366). According to Bartmiński, “The linguistic image of the world is a current interpretation of reality from the point of view of a typical user of a given language; it takes account of his mentality and corresponds to the totality of his judgements and needs” (2006, 14). His judgements are established in the grammar, in the lexis, in the clichés and in prior judgements; that is to say, are implied by the linguistic forms that are established at the level of social knowledge, myths and rites (2006, 12). Often with these types of studies it seems difficult to classify them linguistically.⁶ Interest in the notion of stereotype and in geo-cultural factors (cf. Berlin and Kay 1969, Geertz 1973) has led to a revival of the idea of linguistic relativism, but in this instance we are speaking of cultural relativism, which was opposed to the idea of cognitive universalism formulated in accordance with the psychological studies of Rosch. Cultural relativism is based on the proposition that conceptualization, categorization and (linguistic) symbolization depend on the culture that pertains to an individual who experiences the world, who interprets it, who acquires knowledge and who expresses him or herself in a given language. By contrast, cognitive universalism is based on the belief that corporality, sensory contact with reality and cognitive processes, which are essentially common to all human beings, contribute to the formation and organization of knowledge (cf. Kwapisz-Osadnik 2013).

4. Scheme

This formal and pure condition of sensibility, to which the conception of the understanding is restricted in its employment, we shall name the schema of the conception of the understanding, and the procedure of the understanding with these schemata we shall call the schematism of the pure understanding (Kant, Critique of Pure Reason, second part, chapter one).

In psychology and cognitive science, a schema (plural schemata or schemas) describes an organized pattern of thought or behavior that organizes categories of information and the relationships among them (Di Maggio 1997, 271).

The notion of scheme became implanted in psychology during the period of research into the representations of consciousness, and from the the nineteen twenties onwards it became an object of analysis in psychology, in the information sciences and in linguistics (cf. Scaruffi

1991). In psychology, the notion of scheme converges with that of the mental representation, which generalizes or concretizes the individual experiences (cf. Selz 1924, Bartlett 1932). Head wrote that “for this combined standard, against which all subsequent changes of posture are measured before they enter consciousness, we propose the word ‘schema.’ By means of perpetual alterations in position we are always building up a postural model of ourselves which constantly changes. Every new posture of movement is recorded on this plastic schema and the activity of the cortex brings every fresh group of sensations evoked by altered posture into relation with it. Immediate postural recognition follows as soon as the recognition is complete” (1920, 605–606). The schematization of knowledge evokes Quillian’s (1968) idea of semantic networks, which was quickly applied to the field of Artificial Intelligence. As Desclés has written, “semantic networks constitute a class of structures of input that are easily representable in a computer. They are used for this reason in Artificial Intelligence as means of representation of knowledge” (1987, 58). In linguistics, schematic conceptions are numerous, and the notions employed in the field of linguistic research are as follows. Firstly there is model (cf. Lakoff and his idealized cognitive model: “That we organize our knowledge by means of structures called idealized cognitive models or ICMs, and that category structures and prototype effects are by-products of that organization” (1987, 68), then comes domain (cf. Culioli proposes the idea of a notional domain: “I am going to introduce the term of *notional domain*. We are going to visit rational entities: my abstract occurrences (i.e. that I can conceptualize, imagining, roughly speaking, what can be called a transition to the class) of a typed notion are going to constitute the notional domain. If I return to my example, *dog*, I have an idea of what a dog is and I may say to myself: ‘Now that, that isn’t a dog’” (1995, 46). Then we have Langacker who speaks of the cognitive or the experiential domain: “a coherent area of conceptualisation relative to which semantic units may be characterized” (1987, 488). Then comes schema (cf. Desclés and his semantic-cognitive schema: “a coherent system of meaning of relations” (1990, 245), and framework (cf. Pottier employs the terms conceptual schema or conceptual framework, he distinguishes *visuème*, *idea*, *noème* and *schema* : “Mental schema, as cognitive studies have repeatedly shown, are an essential element of the semantic path which leads to polysemy. As I proposed in formal studies, looking for more abstract representations [...]” (2008, 94). There is also frame (cf. Minsky: “A frame is an artificial intelligence data structure used to divide knowledge into substructures by representing” “stereotyped situations” (1974), and Fillmore (1985): “Frames, in this sense, play an

important role in how people perceive, remember, and reason about their experiences, how they form assumptions about the background and possible concomitants of those experiences, and even how one's own life experiences can or should be enacted. Cognitive frames are usually expressed as 'slot-filler representations,' structures of interconnected roles together with constraints on the possible or likely fillers of those roles" (Fillmore et al. 2009, 314). We also have graph (cf. Sowa 1984 and his conceptual graphs that form a semantic network: "Conceptual graphs (CGs) are a system of logic based on the existential graphs of Peirce and the semantic networks of artificial intelligence. They express meaning in a form that is logically precise, humanly readable, and computationally tractable. With a direct mapping to language, conceptual graphs serve as an intermediate language for translating computer-oriented formalisms to and from natural languages" (1998, 287), and script or scenario (cf. Schank and Abelson: "A script is a temporally-ordered schema; it describes a reader's knowledge of stereotypical goal-oriented event sequences that define a well-known situation" (1977, 422)). These notions are applied to different levels of analysis: precognitive, cognitive, prelinguistic (cf. Jacob 1992), semantic-cognitive and semantic. Langacker speaks of schema and schematization as one of the main mental operations that participate in categorization: a person categorizes according to a scheme, that is to say, by concretizing a concept in relation to another which is more schematic. Lakoff and Johnson use the notion of schema in examining the phenomena that determine conceptualization: preconceptual schemata (or image schemata) are the structures that correspond to our principal experiences of the world. Johnson wrote that "image schemata operate at a level of mental organization that falls between abstract propositional structures, on the one hand, and particular concrete images, on the other" (1998, 29). They are by nature imaginative, repetitive, dynamic, global and relatively stable in the sense that they form part of the conceptual network. They are stored in the memory and become activated every time the individual recognizes and identifies a fragment of reality that recalls them. In this context, Desclés proposes the notion of cognitive archetype, which is the basis of cognitive representations, these being the constituents of semantic-cognitive primitives (cf. "[...] The Applicative and Cognitive Grammar. In this theory different levels of representation are defined: primitives which are the basic components of the description of meaning, semantico-cognitive schemes which are organizations of semantic primitives and which represent a given acceptance of a lexical item in a given context, cognitive archetypes which represent the structure of the different acceptations of a lexical item, and semantico-cognitive

fields which represent the similarities of meaning of groups of lexical items,” Desclés et al. 1998, 29). Rastier defines primitives as “constructions of our interactions that are general, dynamic and recurrent in our experience” (1993, 174). Cognitive representations engender in turn the linguistic representations that are codified in a given language. The notion of primitive is linked to the search for universals in the fields of philosophy (cf. Plato, Ockham, Descartes, Kant, Armstrong),⁷ of logic (cf. Kant, Frege, Mill, Russell),⁸ of linguistics (cf. Wierzbicka, Karolak, Desclés, Lakoff), and of psychology (cf. Rosch). Among the many questions related to the existence of universal concepts there is also that which concerns their representation. Are they symbols, the consequence of which is the schematic representation of complex concepts? Or rather are they linguistic unities, which are present in all natural languages and which serve to form the definitions of complex concepts? For example according to Wierzbicka, semantic primitives are “the elements which can be used to define the meaning of words (or any other meanings) [and] cannot be defined themselves; rather, they must be accepted as ‘indefinibilia,’ that is, as semantic primes, in terms of which all complex meanings can be coherently represented” (1996, 10). In turn Karolak defines primitives as “a conceptual category that is universal in character. It is necessary to distinguish between aspect as a semantic category and its lexical and grammatical exponents. Of necessity, the exponents are idiomatic in nature: languages differ in the way they express aspect formally” (Karakol 2008, 125).

The role of graphic representations, diagrams, and images in the explanation of the relation between experience of the world, processing of data, formulation of knowledge and beliefs about the world, and language, is incontestable in modern linguistics, for it refers to what is preconceptual and in a sense universal (biological), in order to describe the construction and expression of contents on the level of the speech act. The diversity of the many research approaches addressed above may lead to a certain amount of confusion, and even resistance. However, if we consider that the guiding term of this section is the schema and its various kinds (model, image, construct), then in this context the many references we have made to the different phenomena and lines of research on this subject which we have presented in a schematic or iconic form, would seem to be fully justified. The visualization of linguistic phenomena appeals to man’s intellectual faculties, notably to that of imagery, and it is starting from this new perspective that we will try to examine the way the categories of language function.

5. Imagery and conceptualization

The schema is not an image, because the image is a product of the reproductive imagination, while the schema of sensible concepts (also of figures in space) is a product of the pure a priori capacity to imagine... (Eco 1999, § 2.5, my translation).

It is to Langacker that we owe the notion of imagery in linguistics, although it had been employed earlier in psychology in the area of memory research. Initially this notion was treated with caution, and one spoke more commonly of model or of mental representation (cf. Shepard 1962, Shepard et al. 1971, Kosslyn 1980, 2005, Johnson-Laird 1983, Pylyshyn 1981, 2003). Johnson-Laird distinguishes imagining from sentence representation and mental model: “[...] and in principle at least three distinct sorts of mental representations could exist: – propositional representations which are strings of syntactically structured symbols in a mental language. – images, which are two-dimensional visualizable icons typically of an object or scene from a particular point of view. – mental models, which are also iconic as far as possible, but which can contain elements, such as negation, that are not visualizable” (Johnson-Laird and Byrne 1991, Newell 1990). They can also represent three-dimensional objects and scenes (as in Shepard’s studies of mental rotation [...])” (www.mentalmodels.princeton.edu). Observations on perception and the processing of information brought about a distinction between similes and schemata (cf. Denis 1989, Darras 1998), according to which similes are copies of reality, and refer to the faithful reproduction of that which we perceive, and schemata are abstract representations, either general or specific, which are based upon an interpretation of reality. Meanwhile, Langacker states that “By imagery, I do not mean sensory images à la Shepard (1978) or Kosslyn (1980), though sensory images—as one type of conceptualization—are quite important for semantic analysis. I refer instead to our manifest capacity to ‘structure’ or ‘construe’ the content of a domain in alternate ways” (1986, 6). For Langacker, the operation of imagery is identified in conceptualization. This means that these two notions refer to mental processes which consist in the construction of the scene at different levels, as follows: the degree of precision, the base, the focalization, the perspective, and the profiling. Among Polish cognitive researchers, profiling is one of the most commonly used research tools. According to Pottier, “notre VOULOIR DIRE est d’abord un imaginaire où la vue tient une place essentielle” (2000, 5). That is to say that the imagery would be a mnemonic activity permitting the representation of a situation to which is ascribed a semantic content. Pottier is in some ways a

precursor of cognitive linguistics: since the 1960s he has stressed the role of the perception of memory, of imagery and of conceptual schemes in the treatment of linguistic data (1962, 1995, 2012).

Grzegorzczkova (1998, 13) underlines the similarity between profiling and the notions of frame and script, because as she explains “they comprise as if two sides of the same process: profiling cuts out certain elements as exposed ones (placed to the foreground) within an experience field, however an interpretation frame and scene serve as the essential background for comprehending the notions exposed in the process of profiling.” Bartmiński (1993, 1998) understands profiling as creating variants of a given notion, and therefore, unlike Langacker in this interpretation, profiling occurs on already exposed notions and is based on uncovering different facets of a given notion.

Imagery is effected in a given situation by the activation of knowledge which is organized in categories and is pertaining to various linguistic resources, among which are memory, planning, problem-solving, general knowledge, short- and long-term goals, and knowledge of current, social, cultural and linguistic contexts (cf. Langacker 2003, 42). This indicates that conceptualization is a process which is cognitive and linguistic at the same time, meaning that the construction of the scene is equivalent to the construction of the sense (according to Langacker “Semantics is conceptualization”). In the subject literature the process of conceptualisation is identified with a process of categorization, as a result these two notions are interchangeably used in many publications, which may lead to a conflict of terminology. In fact, it is often difficult to establish a boundary between the processes, especially in the case of adults using their stored knowledge, including linguistic knowledge, because at the moment of conceptualisation adults simultaneously categorize the conceptualized fragments of reality, that is they construe the content of a perceived scene thanks to the recognition and naming of objects and relations belonging to a given scene. That is why conceptualisation is a process of creating perceived and categorized contents of reality. Both processes occur based on global knowledge, including linguistic knowledge, stored in the form of memory resources. It follows from this that each time information is processed it is transformed into a speech act which is semantically marked both at the lexical level and at the grammatical level. In this context to speak of cognitive semantics means to speak of cognitive linguistics “tout court,” because any construction on the cognitive level (conceptualization) is actualized simultaneously on the semantic level (on the lexical and grammatical planes).

As in the preceding section, here it is just as difficult to separate out the specific research problem, because the term ‘conceptualization’ logically leads to consideration of its dimensions, including its profile, as well as to consideration of the conceptualization which consists in constructing an image having a semantic content.

6. Metaphor and metonymy

The word metaphor has come to mean a cross-domain mapping in the conceptual system. The term metaphorical expression refers to a linguistic expression (a word, phrase, or sentence) that is the surface realization of such a cross-domain mapping (this is what the word metaphor referred to in the old theory) (Lakoff 1992, 6).

Metonymy is a cognitive phenomenon (Panther and Thornburg 2010, quoted in www.oxfordhandbooks.com).

These two notions have already been studied in detail in the field of cognitive linguistics due to the work of Lakoff and Johnson (1980, 1998), even if theirs is not the first proposal to consider metaphor and metonymy as figures of thought (cf. Black 1962, 1979, Reddy 1979). Black suggests a specific grammar metaphor, introducing the distinction between focus and metaphor frame. Focus is a word, which begins to function metaphorically, meanwhile the rest of the sentence constitutes its frame. In addition, he states that “We can think of a metaphor as such a screen and the system of ‘associated commonplaces’ of the focal word as the network of lines upon the screen. We can say that the principal subject is ‘seen through’ the metaphorical expression, or, if we prefer, that the principal subject is ‘projected upon’ the field of the subsidiary subject” (1962, 41). Reddy introduces the conduit metaphor into communications research. This suggests that language is understood (conceptualized) in terms of a conduit, by means of which the transfer of content takes place. Content is conveyed in containers, that is in linguistic expressions. In turn, Layoff and Johnson not only propose an exact description of the process of metaphorization as a general categorical tool, but enrich this knowledge with a special typology of metaphors (spatial metaphor, ontological metaphor, and structural metaphor). While this issue attracts considerable interest on the part of researchers, in this section it is limited to the summary statement, which posits that in cognitive linguistics, these are the fundamental operations in the processing of information in terms of categorization: the individual employs them when confronted with experiences that do not have a physical dimension, in order to construct

cognitive models of objects and perceived phenomena. Often it is impossible to separate them, insofar as metaphor and metonymy overlap. It is for this reason that Goosens (1990) introduces the notion of *metaphonymy*, to stress the fact that they are two poles of the same phenomenon: “Although in principle metaphor and metonymy are distinct cognitive processes, it appears to be the case that the two may not be mutually exclusive. They may be found in combination in actual natural language expressions [...] I would like to assign *metaphonymy* the status of a mere cover term which should help to increase our awareness of the fact that metaphor and metonymy can be intertwined” (1990, 323). Semiotic research goes even further in proposing the notion of the multi-modal metaphor: “I will define multimodal metaphors as metaphors in which target, source, and/or mappable features are represented or suggested by at least two different sign systems (one of which may be language) or modes of perception. Multimodal metaphors can be delineated best by first describing pictorial metaphors, the type of non-verbal metaphor that has attracted most scholarly attention” (Forceville 2008, 4). While remaining conceptual, the multi-modal is not limited to being expressed in language, but can be expressed in other codes as well—gestural, visual, auditory, and motor—and often these codes are mixed, for example in advertising and in animated drawings. The conceptual character of the metaphor and the metonymy is a matter of debate: does it refer to actual categorization, or rather to a language process that is based on already-established categories? To put it another way, is it a case of comparison or of substitution (cf. Kockelman 2010)? However that might be, at the present time there is no doubt that metaphor and metonymy are conceptual and/or linguistic operations that depend upon many contexts: geo-socio-cultural, discursive (women vs. men), corporeal (right-handers vs. left-handers), and cognitive (ideology, religion, general knowledge (cf. Kövecses)).⁹

7. Levels of linguistic analysis, cognitive semantics

There are six traditional levels on the way from sound to meaning: Phonetics: Speech as a physical process. Phonology: Systems of linguistic sound structure. Morphology: The structure of words. Syntax: The structure of phrases and sentences. Semantics: The meaning of words and phrases. Pragmatics: How speakers and writers use language to communicate

(<http://www.ling.upenn.edu/courses/ling001/approaches.html>).

Here it is evident that the synchronic viewpoint predominates, for it is the true and only reality to the community of speakers. The same is true of the linguist: if he takes the diachronic perspective, he no longer observes language but rather a series of events that modify it (Saussure 1959, 90).

Cognitive Semantics takes the relationship between meaning and mind as its central concern (Cognitive semantics, www.brill.com).

The question concerning the structure of language leads in essence to consideration about how to establish, on the one hand, the boundaries between disciplines (what is the linguist supposed to test? what belongs to the structure of language?) and, on the other, the boundaries between elements belonging to the structure of language (are the phenomena to be analyzed phonological, morphological, syntactic, or semantic?). Hence in the field of linguistics the areas of specialized research disciplines are often being renegotiated—when one speaks for example of morphosyntax, semantic analysis encroaching on areas of morphology and semantics, it turns out to be difficult to define the meaning without the context, and efforts to create semantic theories on the bases of true conditions have not yielded the anticipated results. Therefore from the cognitive perspective as well, language remains a structure, but constitutes a continuum, which as a result leads to the elimination of the boundaries between traditionally formed levels. Langacker postulates three types of linguistic structures, namely phonological, semantic and symbolic structures, the latter ones occur as a result of combining phonological and semantic structures. It means that each linguistic unit is semantically full and is realized phonetically in a specific speech act. That is why the division into phonological, morphological, syntactic and lexical units ceases to be relevant. The issue of the synchronic and diachronic analysis is treated similarly—each linguistic structure may be historically and currently justified.

Cognitive conceptions also cause opinions to be divided on the question of the identity of the cognitive and conceptual levels, even if we univocally stress the cognitive origin of the functioning of language. For example, for Desclés (1990, 2005) there are cognitive, genotypic and phenotypic levels, and linguistic analysis ought to take into account the relations between the mental representations on these three levels. The cognitive level furnishes the representations founded on cognitive archetypes, the genotypic level gives rise to conceptual representations on the basis of predicative universal schemata, and finally the phenotypic level deals with the linguistic representations formed on the basis of semantic-cognitive schemes native to a given language. In his theory of

cognitive semantics Jackendoff (1983, 33) insists on the existence of a single level of mental representation, called the conceptual structure, on which linguistic, sensory and motor information is compatible (“Conceptual structure is not a part of language *per se*—it is a part of thought. It is the locus for the understanding of linguistic utterances in context, incorporating pragmatic considerations and ‘world knowledge’; it is cognitive structure in terms of which reasoning and planning take place” (Jackendoff 2002, 123). According to Fauconnier and Turner (the theory of mental spaces (1984) and later the theory of the blend (1996)), the operation of conceptual integration leads to the elaboration of thought in terms of a conceptual blending which forms at the same time a conceptual and linguistic structure (“Blending is an operation that takes place over conceptual integrations networks. Conceptual integration networks often involve many mental spaces. Blending can occur at many different sites in the network. A blended space can have multiple input spaces. Blending is a dynamic process that can happen repeatedly in the same network” (Fauconnier and Turner 1998, 138). In Polish and Italian literature on the subject, the term ‘amalgam’ often functions alongside the term ‘blend,’ which creates the impression of a certain terminological redundancy.

The operations of imagery and of conceptual integration, applicative and cognitive grammar and semantico-cognitive grammar, and the other conceptions of the processing of developed data in the field of cognitive sciences, notably in cognitive linguistics, are attempts to examine and to describe the human faculty for constructing meaning. This means that every actively enunciative activity of conceptual or semantico-cognitive representations pertains to different notional categories encoded in the given language. According to Culioli (1999, 9), “the notion is a bundle of physical-cultural properties that we apprehend in the course of our enunciative activity of producing and comprehending messages.”

The thesis according to which every linguistic unit is semantically full opens up a debate on linguistic norms. For those in favour of the semanticity of all phrasal constructions, every deviation at the lexical and grammatical level makes it manifestly clear that information is being processed, and this depends upon many factors and linguistic resources. In other words, every use of language is considered correct, because it corresponds to some form of conceptualization. For the sceptics, certain uses can only be explained by frequency of usage, that is to say, by the preference for employing one unit or construction more often than another in a given context. This means that there are also asemanic uses: Kardela (1990) speaks of parasitic gaps, as in the use of the preposition “to” in the phrase, “He forgot to telephone to me.” This leads us to think of the

semantically based grammar of Karolak (1984, 2007), who apart from semantically full terms recognizes syntactic operators that are devoid of meaning. The most frequent usages in a given context are considered to be prototypical, even if they may not correspond to linguistic norms imposed by the textbook.

Conclusions

Just as it was difficult to analyze the principal notions in cognitive linguistics, so it is likewise difficult to formulate some concluding remarks to end this study.

1. The idea of analyzing linguistic phenomena in different contexts is not a new one. Even Plato, and after him Condillac and Rousseau, had already stressed the role of the social context¹⁰; the cultural context turned out to be a determining factor in the ethnological studies of Malinowski ([1945] 1961), Sapir ([1929] 1962), Whorf ([1936] 1956); by contrast it was the anthro-biological context that emerged in the work of Pinker ([2002] 2005, [1994] 2013). The biological context was the basis for positivist theories, and the work of Broca and Wernicke laid the foundation for the neurosciences. Locke ([1689] 1955) and Hobbes ([1691] 2005) spoke of the context of usage, and finally it was the psychological context that became prominent in mental and behavioral conceptions of language. In consequence, in linguistics we can observe the rise of pragmatics (already a decade earlier in France, as can be seen in the work of Meillet, Bally and Benveniste, where enunciative theories make their appearance) and the development of applied linguistics, including psycholinguistics, sociolinguistics, and ethnolinguistics (which today in France form part of the language sciences).
2. The interdisciplinary character of cognitive linguistics results from the conception of language itself: we recall that in this field language is considered to pertain to cognitive resources, and this means that an analysis of language phenomena ought to take place in the context of the numerous cognitive operations that are activated at the moment of enunciation, or in Langacker's terminology, during the speech event.
3. The linguistic units constitute part of our knowledge and this means that they undergo the same operations of information processing as the other forms of our knowledge about the world.

4. The examination of linguistic phenomena is thus holistic, which is to say that the construction of an enunciation, and its semantic and pragmatic values, results from the processing of data at the phonological, morphosyntactic, and discursive (language) levels, but we must equally take into consideration the role of the other linguistic resources that participate in the speech event, such as memory, decision-making, the faculty of recognizing the social context, problem-solving, general knowledge, planning, etc. (cf. Weil-Barais 1993).
5. This interdisciplinary vision of cognitive linguistics, and global insight into language phenomena, should lead us to think of the epistemological origin of the sciences: at the beginning there was only philosophy, but today there is the project of cognitive science, even if one speaks of cognitive sciences (cf. Gardner 1993). Cognitive linguistics has become the central point of these researches (in the United States the term 'cognitive sciences' is used interchangeably with cognitive linguistics), since it is language that proves to be the most direct and natural source of knowledge about human beings in all their intellectual, psycho-affective and behavioral complexity.

CHAPTER FIVE

PROTOTYPE THEORY:
ON THE ORIGINS OF A THEORETICAL
PATCHWORK AND ITS TRANSFER
TO LINGUISTICS¹

JEAN-MICHEL FORTIS

Introduction

Surprisingly, in spite of the existence of numerous textbooks on cognitive linguistics and of Lakoff's monograph on categorization (Lakoff 1987), a thorough history of prototype theory has still to be written. As is often the case, the more distant or foreign the sources, the more obliterated they have been, not counting the oversights of a self-serving historiography. What follows does not pretend to be a full account of this complex history, but is, hopefully, a modest step forward.

Our study will begin with the research on color categorization which was initiated by Lenneberg in the 1950s. Lenneberg's importance resides in the fact that he put in place the philosophy behind the new methodology which would underlie an experimental approach to the question of linguistic relativity. He also directly anticipated some of the tenets of prototype theory. Although his research was conducted at first in a relativist spirit, the experimental approach he inspired was to culminate, perhaps unexpectedly, in the universalist theory of Berlin and Kay.

This about-turn ends the first part of this chapter and provides the context for Rosch's debut and her foray into color categorization. It will be shown how Rosch extended Berlin and Kay's notion of focal colors to good forms, and subsumed both under the name of *prototype*, a concept whose origins will be briefly examined. We will then come to the heart of the matter: the complex theoretical patchwork which Rosch progressively

built up in order to deal with “semantic categories,” that is linguistically identified items grouped into categories such as ‘fruit,’ ‘bird’ or ‘sport.’ In the course of the discussion, various issues will be discussed, notably Rosch’s acontextual conception of typicality. By way of conclusion, a synthetic view of this theoretical patchwork is provided. This ends our second part.

In the third part, we turn to the transfer of prototype theory to linguistics. At this point, a number of questions need to be considered: Why did linguists feel the need to import prototype theory into their own field? In what context and for what purpose? Since it was a rather impoverished version of this theory that was applied to linguistics, we shall see in what ways this theory was simplified, and why.

We shall now turn to the issue which provided the decisive impetus, linguistic relativity and its experimental approach in the field of color categorization.

My deepest thanks to Nick Riemer and an anonymous reviewer, who reviewed this chapter and spent much effort in bringing my text closer to prototypical English. Parts of sections 2.8., 2.9., 3.3., 3.4. have been previously published in a blog post (Fortis 2015b).

1. Linguistic relativity and color categorization: From Lenneberg to Berlin and Kay

1.1. On the context of Lenneberg’s approach

There are several reasons why we should turn to Lenneberg in the context of the present discussion. Firstly, Lenneberg inspired the line of research which directly led to Rosch’s first studies on color categorization. Secondly, in addition to mapping out a research program and contributing protocols for pursuing it, Lenneberg introduced notions which directly anticipate prototype theory. His own evolution personifies a universalist-cognitive turn which, as we shall see, ushers in a perspective that is the backdrop to prototype theory. Finally, giving him due recognition will right an injustice: Lenneberg is not mentioned once in Lakoff’s monograph (Lakoff 1987).

We may describe Lenneberg’s scientific agenda in the 1950s as the development of an experimental approach to linguistic relativity. Both circumstantial factors and elements of a more perennial nature may explain Lenneberg’s concern. On the circumstantial side, there was the publication of some of Whorf’s papers by Trager (Whorf 1952), and the holding of an important conference on meaning and the relation of

language to culture (Hojjer 1954), in the wake of this publication. The proceedings show that it was mainly aimed at finding an appropriate way to handle this question, by establishing possible evidence to show how thought is dependent on language, and how cultural attitudes and thought processes are dependent on semantic structures underlying linguistic patterns. Closest to Whorf's orientation was perhaps Hojjer's study on the consonance between some facts of Navaho grammar and Navaho religious conceptions (*consonance* is Whorf's term; it should be remembered that Whorf refrains from saying that linguistic structures act on thought patterns and culture in a deterministic way; cf. Whorf 1956, 154). The overall impression left by these proceedings is that of widely dispersed efforts leaving open the question of the very methodology one had to adopt in tackling this frustrating question.

To the German-born Lenneberg, Whorf's principle of relativity echoed a more perennial interest of German thinking, the role of language in the formation of national spirits, in conceptual development and the evolution of human knowledge. The Herderian-Humboldtian line was still familiar to Boas (German-born too), who was responsible for professionalizing American anthropological linguistics and thus was widely responsible for introducing this theme into American circles. In Lenneberg's case, the most important German influence was that of a thinker who, so to speak, summed up centuries of reflection on the status of language in the constitution and evolution of knowledge. This thinker was Cassirer (Lenneberg 1955).

It is by discussing Whorf and Cassirer in particular, and by opposing their way of handling the issue of relativity, that Lenneberg was led to define his own methodology. To understand why Lenneberg found it necessary to take a different course, we should first see what objections he had as regards his predecessors.

1.2. Lenneberg's objections to Cassirer and Whorf

In Lenneberg's eyes, Cassirer and Whorf are among those "scholars who treat language as if it were a direct manifestation of the speakers' *Weltanschauung*" (Lenneberg and Roberts 1956, 1). There are two major problems with this view. Firstly, the *Weltanschauung* approach seeks what is idiosyncratic in a culture by correlating it with features of the language under consideration. But this has been done, says Lenneberg, without a clear *tertium comparationis*: if we wish to compare the implications of linguistic variation, we need a common ground, that is, a domain which we can be certain is referred to by linguistic forms in all of the languages

under consideration. In the absence of a common referential domain serving as *tertium comparationis*, research on *Weltanschauungen*, like the one conducted on Navaho by Hoijer, has essentially consisted of single-case studies and its conclusions have not been generalizable (Lenneberg 1953). Secondly, Lenneberg insists that we cannot reach a conclusion concerning the cognitive make-up of the speakers of a language from its semantic or morphosyntactic specificities; this is simply because forms and their meanings underdetermine what is being conveyed in a message, and therefore, the thought-content of utterances:

Morphemes and their meaning are regarded more appropriately as mnemotechnical pegs of a whole situation which is brought into consciousness by the statement as a whole. The general meaning of morphemes is probably of lesser importance in cognition than the SUM OF ASSOCIATIONS bound up with the complete utterance, or even with individual morphemes or groups of morphemes. (Lenneberg 1953, 466)

For Lenneberg, that is precisely what fundamentally vitiates Cassirer's and Whorf's method of literal translation, a method they practice all too often when paraphrasing "primitive" languages, with the idea of magnifying their oddity (Lenneberg 1953, 1955). When, for example, Cassirer says that in a language of Sudan speakers express the notion that the subject is in the process of acting by saying 'I am on the inside of walking, I am the belly of walking,' doesn't he presuppose that the literal meaning is what is being conveyed, that it is the thought-content? For Cassirer, this kind of linguistic evidence is enough to show that spatial and temporal (or here, aspectual) relations are not yet differentiated in some "primitive" states of conceptualization.

The same objection applies to Whorf's literal translations, of which Lenneberg cites the following passage:

We might isolate something in nature by saying 'It is a dripping spring.' Apache erects the statement on a verb *ga*: 'be white (including clear, uncolored, and so on).' With the prefix *nō-*, the meaning of downward motion enters: 'whiteness moves downward.' Then *tó*, meaning both 'water' and 'spring' is prefixed. The result corresponds to our 'dripping spring,' but synthetically it is 'as water, or springs, whiteness moves downward.' (Whorf 1956, 241)

Now, Lenneberg argues, if we paraphrased English in the same manner and glossed any sentence by giving the general semantic value of its morphemes, taking no heed of the situation at hand, English would not fail to sound as foreign and strange as Apache (Lenneberg 1953, 464–5).

Again, what is meant and thought need not be what is expressed literally. What is meant might be the same in English and Apache.

Clearly, Lenneberg rejects what Whorf erected into a *principle* (not as a hypothesis), i.e. that the “literal” semantic value of morphosyntactic forms and lexical items is part and parcel of what makes up the thought-content of an expression, and therefore that the thought-content is relative to these forms (Whorf 1956, 212–3). As Lucy notes:

Lenneberg’s “approach essentially dismisses the question at the very core of the debate, that is, exactly how important are differences in semantic and syntactic structure? Is the *same thing* really being said?” (Lucy 1992, 133).

To put it differently, Lenneberg’s contention is that this principle is in fact a hypothesis whose validity we need to ascertain by a method that is not circular, i.e. does not appeal, in particular, to linguistic facts or to literal translation and general meanings abstracted from the situation.

1.3. Lenneberg’s reformulation of the principle of relativity

In Lenneberg’s view, a method that is not circular should relate linguistic forms and structures not to other linguistic forms (such as paraphrases) but to nonlinguistic, observable behavior. A proper reformulation of Whorf’s principle of relativity should therefore take the form of a hypothesis based on an influence of language on thought and behavior. In Lenneberg’s terms:

Does the structure of a given language affect the thoughts (or thought potential), the memory, the perception, the learning ability of those who speak that language? (Lenneberg 1953, 463).

Success would mean that

a “linguistic condition C is functionally related to non-linguistic behavior K; we accept the hypothesis if K is observed to change with C” (Lenneberg and Roberts 1956, 5).

Now, Whorf had cited evidence at least once for such an influence of language on behavior, namely in his famous example of an explosion caused by the lexical ambiguity of *empty* (Whorf 1956, 135). Because *empty* could mean ‘filled with material considered insignificant’ (such as vapor, or rubbish etc.) as well as ‘void, inert,’ one could describe a drum

filled with explosive vapor as *empty* in a sense conforming to the *first* pattern, and act carelessly in accordance with the *second* meaning. Hence the explosion.

This example failed to convince Lenneberg. It might be objected, says Lenneberg, that

English is capable of distinguishing between a drum filled with an explosive vapor, one that contains only air, and one which is void of any matter. This very sentence is my evidence. The person who caused the fire could have replaced the word *empty* by *filled with explosive vapor*. His failing to do so (as well as his careless behavior) points to a lack of experience with explosive vapors, perhaps complete ignorance of their existence. (Lenneberg 1953, 464)

In other words, because English has the *potential* of distinguishing the two meanings of *empty*, we cannot know if the *habitual* connection between these two meanings was at fault. In effect, then, Lenneberg denies the relevance of Whorf's emphasis on regular linguistic patterns and "habitual thought." Whenever English has a potential for describing a situation in alternative ways, we cannot be sure which construal, habitual or not, is forced upon the speaker. Lenneberg's conclusion is that we should look for a domain where a speaker's potential speech behavior is constrained by the rules of his or her language and the lexical availabilities of that language. In Lenneberg's parlance, we should leave out the *message* level, i.e. those aspects of linguistic structure which involve potential and open expressive capacities, in favor of elements relating to (quasi-)obligatory *codification*.

To sum up the requirements adduced so far by Lenneberg, a well-grounded treatment of the problem should establish a referential domain shared by the languages under comparison, relate the linguistic description of this domain to nonlinguistic behavior, and choose a domain such that its linguistic description is constrained by the expressive capacities of the respective languages. It is now time to turn to the methodology which Lenneberg put in place and which, in his eyes, satisfied all these requirements.

1.4. Lenneberg's methodology

Lenneberg's choice of a referential domain was that of *surface colors*, a domain which Whorf had never touched upon, at least in his published papers, excepting two allusions which are not relevant to the linguistic relativity issue (Leavitt 2011, 172–173). This domain had several

advantages: it was assumed that colors were referents shared in all languages, while their designation was subject to important cross-linguistic variation. Further, from an experimental point of view, colors were a relatively simple material that could be strictly parameterized along three dimensions (hue, brightness and saturation). Researchers could avail themselves of standardized sheets carefully controlled for these parameters (the Munsell charts), so that colors submitted to subjects could be *evenly* spaced, in such a way that sampled the color continuum without distorting it.

Colors are of course a textbook example for the claim that languages arbitrarily segment reality (a metaphor which resonates in Western thought since Plato). And when Lenneberg settled for colors, he was not, even at the time, striking a new chord. Discussion in a nominalist vein on the linguistic grouping of colors could be found in Boas, Cassirer and especially the neo-Humboldtian Weisgerber, whom Lenneberg was acquainted with. It is important to note that the spirit which inspired Lenneberg's first phase of research was relativist, and that he expected to demonstrate an influence of linguistic categories on nonlinguistic behavior. Apparently, Lenneberg was unaware of the 19th research on color sensitivity and its relation to language, and was therefore unaware of perspectives which could be both naturalistic (i.e. attributing differences in nonlinguistic behavior to physiological causes) and non-universalist (Saunders 2007).

The nonlinguistic behavior chosen by Lenneberg and his coworkers was a recognition test (Brown and Lenneberg 1954; Lenneberg and Roberts 1956). Their basic prediction was as follows: colors whose names are readily accessible should be recognized better than colors with designations more difficult to retrieve. The accessibility of color names was measured through an index called "codability." Roughly, highly codable colors were those with shorter names, which were named the fastest, and on whose names there was highest intersubjective and intrasubjective agreement. The connection between name accessibility and length had been inspired by Zipf (1935), who had established a negative correlation between word length and frequency in a number of languages. Since frequency reflected the number of perceptual discriminations made with a given name, it was presumed that discriminations made with frequent names would be more easily retrieved. The results confirmed the hypothesis that recognition was positively correlated with codability.

At this stage, three points are worth noting. First, Lenneberg presupposed that color names tag "elementary sensations," and that they constituted a basic "language of experience" (Lenneberg and Roberts

1956). However, it is not clear that color as defined by hue, saturation and brightness has the kind of universality which Lenneberg attributes to it; surface reflectance and textures may be crucial factors, for example in Hanunóo, where features translatable as ‘wet,’ ‘shiny’ etc. reflect an important dimension of the referents of “color” names (Conklin 1955; Lucy 1992). From a phenomenological point of view, many attributes (texture, glossiness, fluctuation, transparency etc.) may be perceived in unity with color in the restricted sense (Beck 1972), a fact which casts doubt on the status of Lenneberg’s “elementary sensations,” since, as *sensations*, they are presumed to be phenomenologically real. Second, probably with the intention of maximizing the effect of codability, Brown and Lenneberg instructed their subjects to pick out the best examples of the most frequent color names (*red, orange, yellow, green, blue, purple, pink, and brown*); these samples formed part of the material that subjects had to recognize among distractors. In Rosch’s terms, they chose prototypes, without assuming, however, that typicality as such would facilitate recognition. Rather, colors were assumed to be typical *because* language had made them salient.

Finally, a relativist conclusion was drawn from an experiment which was “intra-cultural” (in Lenneberg’s own terms). Again, to tease apart the factors of codability and perceptive saliency, it would have been deemed crucial to turn to a language different from English. This was not, however, the motivation of the next experiment, as we shall see.

1.5. The “inter-cultural” experiment and the structure of categories

By turning to a different culture and a different language, Lenneberg and Roberts (1956) hoped to further confirm, not to challenge, their hypothesis of an influence of codability on recognition. This new experiment was in fact a replication of the previous protocol, this time with two groups, English speakers and Zuni from New Mexico.

Since Zuni conflates in a single category colors that would be described as *yellow* and *orange*, it was expected that they would fare less well than English speakers for these colors in the recognition test. This expectation was fully borne out. Indeed, not a single monolingual Zuni recognized correctly either orange or yellow samples, whereas their foci scored highest for English speakers in the same test (Lenneberg and Roberts 1956).

Importantly, in order to compare English and Zuni color categories, Lenneberg and Roberts introduced a procedure promised to a bright future

(Saunders 1992): placing a transparent acetate sheet directly on a Munsell chart, they asked subjects to draw a line along those samples that could be referred to by a given color name. The degree of intersubjective agreement on what counts as the referent of a color name could thereby be directly visualized on the chart. Colors for which agreement was highest were termed *foci*. They were surrounded by a transition zone, composed of colors ranging from “fair unanimity” to samples reaching only 50% agreement, with possible overlaps between categories. These data afforded a multi-dimensional comparison of categories across languages and within a given language. In English, for example, the category ‘red’ essentially consisted of foci, while ‘blue’ had two foci and a wide transition zone. Categories could thus be compared in terms of their number of foci, their size and density, the width of their transition zone, and the symmetry of their naming probability gradients with respect to the foci.

This investigation into the profiles of color categories culminated in a report summarizing Lenneberg’s doctoral research (1957). Remarkably, Lenneberg was not content with plotting the profiles of categories. In addition, he directly anticipated Rosch’s protocol by asking subjects to rate the degree to which some colors would be likely to be called *brown*, *green*, *blue*, and *rose*. In effect, this task could be interpreted as delivering typicality ratings, and Lenneberg’s instructions, speaking of an “ideal example” of brown, or of the “brownest brown,” invited just this interpretation.

I believe the closeness to Rosch’s ideas hardly needs to be emphasized. When Lenneberg claims that “concepts [= groupings of referents] are best characterized as areas of waxing and waning typicality on a stimulus continuum” (1957, 2), with transition zones, blurred boundaries and inter-category overlaps, he is laying the groundwork on which Rosch will elaborate her own notion of prototype. However, in contrast to Rosch, the whole endeavor was taken to support linguistic relativism.

1.6. Context and the “language of experience”

Even before Lenneberg and his coworkers had published all their results, an experiment conducted by Burnham and Clark (1955) directly contradicted them. Using a recognition task and unsaturated colors, Burnham and Clark had found out that colors without a definite name (“innominate”) were *better* remembered than highly codable colors. Lenneberg (1961) pointed out that the contradiction could be resolved by taking into account the experimental design of Burnham and Clark. In their protocol, subjects had to recognize a previously presented sample in a

display where colors were disposed on a rotating disk and visible one by one through an aperture. Now, because this array included many greens and blues, and these could not be distinguished at a glance, naming was of no help to subjects. Lenneberg (1961) surmised that in this context subjects had used innominate regions as “anchoring points” delimiting greens and blues, with the result that innominate colors had taken on a distinctive feature, while the highly codable tended to be assimilated in memory.

The divergence between Lenneberg’s experiments and that of Burnham and Clark raises an important problem: recognition is highly dependent on task context. Furthermore, as Lenneberg and Roberts noted, naming is context-dependent too: “In a context where only three colors have to be distinguished, we might call something red which in another context would be called dusty rose or pale purplish red” (Lenneberg and Roberts 1956, 48). They were, therefore, perfectly aware of this difficulty, but they thought they had circumvented it. First, their array included foci, which were supposed to “trigger” a basic name like *green*; second, before each informant named individual colors, he was shown the extent of the *entire sample* of colors to be named, and this entire sample was assumed to be an accurate projection from the whole spectrum. In other words, naming was regarded as context-dependent, except when the subject could inspect at a glance a display which was an evenly spaced sample of saturated colors of the entire spectrum. This proviso shows that Lenneberg sought to isolate a “code” that would be immune from contextual effects.

This is confirmed by the way Brown (1976) and Lenneberg (1971 [1967]) handled the results of another experiment performed by Lantz and Steffle (1964). Both the latter experiment, and a subsequent one (Steffle et al. 1966) set out to resolve the above contradiction between the results of Lenneberg’s experiments and those of Burnham and Clark. Lantz and Steffle had proposed a new index, *communication accuracy*, which measured the degree to which the description of a color by an “encoder” would enable a “decoder” to find the corresponding sample. It was hypothesized that subjects, when memorizing a color, would, as it were, communicate with themselves “using the brain as a channel” (Lantz and Steffle 1964). Communication accuracy turned out to be the best index to predict recognition, whatever the display used. Nevertheless, Brown and Lenneberg questioned its relevance for understanding cognition, in a somewhat contorted way. Brown said that the index was exclusively “psychological” and not “cultural” insofar as it measured individual abilities. Without refuting Lantz and Steffle, Lenneberg argued that attention should be redirected toward the cognitive preconditions of

reference, a study which he had started to carry out in his research on the cognitive functioning of congenitally deaf children. In the following years, it seems partly due to Chomsky's and perhaps Cassirer's influence, Lenneberg would embrace what he termed "neo-Kantian" views, a perspective according to which concepts (including linguistic concepts) reflect the structure that orders "impinging physical stimuli in a predetermined and species-specific way" (1962, 105). His 1967 book, *Biological Foundations of Language*, was to recapitulate this newly-oriented research in a masterly way. Lenneberg's own evolution would in fact follow a more global universalist trend.

To sum up, by relegating contextual effects to distortions brought about by "improper" displays, or to individual abilities of the speech act participants, or to an inessential aspect of the cognitive functioning of language, Brown and Lenneberg had in fact taken a decisive step toward the separation of a structural or semantic level of categorization, supposedly in correspondence with "elementary sensations." This paved the way for the naturalization of a range of linguistic discriminations. In this respect, Berlin and Kay, and Rosch too, as will be shown, were to follow suit.

1.7. Away from relativism: Berlin and Kay (1969) on *Basic Color Terms*

By Berlin and Kay's own admission, what provided the impetus for their cross-linguistic investigation was an intuition: "our feeling was that color words translate too easily among various pairs of unrelated languages for the extreme relativity thesis [= the total arbitrariness of the segmentation of the color space] to be valid" (Berlin and Kay 1969, 2). What followed this intuition was a vast inquiry bearing on the fundamental, in their words, "basic" color terms of 98 languages. For twenty languages of various stocks, Berlin and Kay simply applied the elicitation procedure introduced by Lenneberg and Roberts, with the same material (Munsell charts with 329 color chips at the maximal level of saturation). Note, however, that they were interested in collecting lexical inventories, not in correlating naming with nonlinguistic behavior. Unlike Lenneberg, they did not discuss the task- or context-dependency of results elicited with different displays. For the 78 remaining languages, Berlin and Kay resorted to second-hand, sometimes outdated, data. First-hand data were often collected from informers having a command of English (a factor which, in Lenneberg and Roberts' study, had influenced the results).

Lenneberg had focused his attention on highly codable colors; terms for highly codable colors formed a restricted set, but one that was justified on theoretical grounds. In Berlin and Kay's study, the criteria for identifying basic terms seemed to be designed, to a large extent, to make cross-linguistic comparison manageable, especially by avoiding all terms felt as "specialized" (like, for example, terms referring to the colors of cattle), whatever their importance in a culture. The main criteria were that a basic term should be monolexemic, should not be a subordinate (unlike *scarlet*: 'kind of red'), nor restricted to a domain (cf. *blond*), and should be easily elicited. For example, in Dugum Dani (New Guinea), the language to be studied by Rosch, the term for 'yellow' was filtered out, apparently on the grounds that the reference study (Heider [1965] 1970) assigned it to few contexts of use, which meant it was not general enough; the term for 'bright red' got eliminated for an unknown reason. As a consequence of this filtering procedure, the Dani basic term system was reduced to two terms, corresponding to the first stage of the above hierarchy (Berlin and Kay 1969, 46–7). One might well suspect that the data were recoded in order to fit the initial intuitive hypothesis of widespread comparability, and this is indeed the gist of Hickerson's devastating critique (Hickerson 1971). Finally, in spite of their linguistic purview, Berlin and Kay provided absolutely no information on the linguistic functioning of their basic terms, not even of a minimal kind, such as their part of speech.

The crucial findings were substantive universals. Berlin and Kay claimed that basic term systems could be arranged along a hierarchy going from least discriminating languages to more discriminating ones. At each step of the hierarchy, the discriminations were found to be the same, so that languages with a two-term system all had terms for 'black' and 'white,' to which languages with three terms added a term for 'red' etc. The progressive enrichment of the basic term lexicon was summarized in the following "rule":

White / black < red < green *or* yellow < green *and* yellow < blue < brown
< purple, pink, orange, gray.

The rule is to be read as: " $a < b$ signifies that a is present in every language in which b is present and also in some language in which b is not present" (Berlin and Kay 1969, 4). It might also be read as saying that if a language has b then it has a , and as such the "rule" is reminiscent of Greenbergian implicational universals of the type "if the pronominal object follows the verb, so does the nominal object" (Greenberg 1966a).

Lastly, it was shown that the foci, that is the chips rated by subjects as the best examples of their category, were roughly stable across languages

(Berlin and Kay 1969, 7–10). This was taken to support the view that color categorization involved “pan-human perceptual universals” (Berlin and Kay 1969, 109), perhaps of a biological origin, in the way that Chomsky (1965) and Lenneberg (1967) had surmised for other aspects of language.

Since languages at the top of the hierarchy are characteristic of industrialized people, while languages of the first three stages are spoken in technologically impoverished cultures, Berlin and Kay drew the conclusion that their hierarchy corresponded to an evolutionary sequence which, Berlin says elsewhere, leads from “primitive” levels of economic and technological development to “the more civilized nations of the world” (sic) (Berlin 1970, 29). A conclusion of this kind, which would have been anathema in the time of Boas, who strongly opposed evolutionist ideas, aligned well with new forms of evolutionism. Sahlin and Service (1960, 9), for example, saw the cultural evolution of mankind as part of a grand scheme, a march in the direction of an ever-increasing exploitation of physical resources, and declared that culture in terms of “continuing the life process, appropriates free energy and builds it into an organization for survival, and like life, culture moves to maximize the amount of energy exploitation” (Sahlin and Service 1960, 9). From this perspective, cultures which had developed color technologies in textiles, paints etc., thus exploiting more fully the physical resources available, could be assigned to a more advanced evolutionary stage. Like Chomskyan universalism, neo-evolutionism betrayed a certain weariness of dry empiricism and an urge to engage in speculations on cognitive and cultural universals. In spite of the flaws mentioned above, a favorable environment and the seduction exerted by findings with universal scope secured the success of the book and, on the whole, earned it positive reviews (Saunders, 1992). Additional support for focal colors also came from the neurophysiology of color vision, in particular from Kay and McDaniel (1978) who wanted to modelize the differentiation of color categories in terms of Zadeh’s fuzzy set theory. Their model was based on the insight that neural responses to a range of wavelengths could be considered as forming a fuzzy set; however, this first model was still speculative.

The initial research of Berlin and Kay was to launch a program which, to this day, is still unfinished, and has considerably evolved to meet the difficulties it has encountered (Jraissati 2009). These subsequent developments, posterior to the stage at which Berlin and Kay influenced Rosch, need not concern us here. We shall only note that Berlin and Kay did not succeed in putting the final universalist nail in the relativist coffin: the debate on color categorization is still ongoing (Deutscher 2010, for an

entertaining account). The participants in this debate, however, often fail to challenge some of the fundamental assumptions of Berlin and Kay. Dubois (e.g. in Dubois and Cance 2009) and Saunders (e.g. 1992) are among the few who have questioned their acontextual, non-intersubjective, denotational perspective, their protocols and material, and the limitation of the color concept to an artefactual display of surface colors. Finally, although the case of color categorization is certainly paradigmatic, it is but one of the issues currently debated in studies on the interactions of language and cognition (see Everett, 2013, for an overview of current research).

2. Prototype theory

2.1. Rosch: the beginnings

The initial stage of prototype theory may be seen as the resultant of several circumstances: firstly the publication of *Basic Color Terms* shortly before; secondly a student with a background in philosophy, Eleanor Rosch, working at Harvard under the supervision of Roger Brown, Lenneberg's collaborator, and gaining acquaintance with various strands of research on categorization, in the self-same university where Jerome Bruner was also practicing (we shall come back to Bruner's influence); and thirdly her trip to New Guinea, with the opportunity of verifying Berlin and Kay's conclusion on focal colors. A few biographical notes would seem to be called for here (cf. Rosch 1999b).

First of all Rosch studied philosophy as far as the master's level. After a master's thesis on Wittgenstein's *Philosophical Investigations*, she moved to Harvard where she undertook doctoral research on child development under the supervision of Roger Brown (the ex-collaborator of Lenneberg). She then left the U.S. and accompanied her husband, the anthropologist Karl Heider (son of the Viennese psychologist Fritz Heider), to New Guinea, where he had been doing field study on the Dani people (cf. above the reference to his dissertation in Berlin and Kay 1969). Rosch's initial plan was to study mother-child interactions. Before leaving, however, the Heiders met Ekman, who was working on the universal expressions of "basic" emotions, and encouraged them to apply his research to the Dani, which Rosch did not do (Macfarlane 2007; Ekman 1971). This is, to all intents and purposes, a telling sign: the dominant mindset was definitely universalist.

Rosch's first study on categorization (Rosch Heider 1971a) was an "intra-cultural" experiment in which she attempted to verify the perceptual

and cognitive salience of focal colors (in Berlin and Kay's sense) for children: Would children point at focal colors when given a color name? Would they perform better with focal colors in a memory task? The answers to both questions were positive, but a linguistic bias of the sort Lenneberg had hypothesized could not be ruled out: children may simply have reacted to the best examples of English color names. At this juncture, the Dani took on a crucial role. Since it had been "shown" by Berlin and Kay that their language had only two basic terms for 'black' and 'white,' any cognitive privilege accruing to chromatic focal colors would demonstrate that their salience could not be of a linguistic origin. It should be pointed out that this reduction of Dani color terms to stage 1 of Berlin and Kay's hierarchy rides roughshod over the linguistic facts. It is doubtful that 'black' and 'white' appropriately capture the meanings of these terms, which Rosch herself sometimes glosses as 'for dark and cold colors' vs 'for light and warm colors' (Rosch Heider and Olivier 1972). In fact, brightness was found to be a decisive feature of Dani color categorization (Rosch Heider 1972b). Furthermore, as already noted by Berlin and Kay, by Karl Heider ([1965] 1970, 49, 175, 289), and by Rosch herself (1972b), the Dani lexicon for colors is definitely not reducible to these two terms and the motivation for filtering out the other terms was not clearly articulated. Here is what Rosch herself had to say about this embarrassing fact:

A term for 'red' was used by twenty (50 per cent) of the informants. Two 'synonyms' appeared to be available to designate this colour; Dani who used the less frequent term *boksu* claimed that it was the 'foreigner's' word for the more frequent indigenous term *pimut*. [...] *Bodli*, the term for 'yellow,' was used by eighteen (45 per cent) of the informants; *juaiegen* the term for 'blue,' by eleven (28 per cent). All three terms were used by only nine (23 per cent) informants (Rosch 1972b, 451).

When the "unexpected" fact that "Dani chromatic colour terms exist" (ibid.: 456) was established, research on the dubious "basic" two-term system was already under way. More preposterously, by reading Rosch, we do not learn *anything* about Dani grammar, nor get the slightest hint as to how color "terms" are supposed to function, syntactically or in discourse, not mentioning their cultural significance. "Terms" act as mere tags for universally shared referents, a conception which was already in evidence, though in a less caricatural way, in Lenneberg's work.

2.2. Rosch on color categorization: cross-linguistic studies

Her experimental investigation began with a two-pronged attack: cross-linguistic studies on the relation between color naming and color memory (Rosch Heider and Olivier 1972, Rosch 1972a), and an attempt at establishing, for Dani speakers, the “natural” character of categories centred on focal colors (Rosch 1973a). It is in the course of extending the latter approach from colors to shapes that Rosch was to introduce her notion of prototype. But let us examine the cross-linguistic studies first.

In the first cross-linguistic study, Rosch and Olivier reasoned that in a Whorfian perspective, the performances of subjects in a recognition task should match, if not totally at least to a significant extent, their linguistic segmentation of the color space. Furthermore, speakers of different languages (in the present case, English and Dani) should exhibit different patterns of errors in the recognition task. What the authors observed ran counter to these hypotheses, insofar as the recognition performances of Dani and English speakers were more similar to each other than their naming and recognition patterns. There was, incidentally, a “relativist effect” when a different measuring technique was used: naming and recognition patterns were significantly closer than other pairwise comparisons.

The second cross-linguistic study (Rosch 1972a) was based on two samples of languages of six different stocks. It was shown first that subjects reliably identified focal colors (= “the best examples” of basic terms) as the most saturated samples, whatever their native tongue, and, on the other hand, it was shown that focal colors were the “most codable” ones, where codability was measured by the length of the name and response latency. An additional experiment demonstrated that Dani and American subjects remembered focal colors better.

There is an impressive stability in what speakers of different languages and cultures understand as being the “best examples” of color terms. However, deriving codability from perceptual salience was probably a hasty move. Visibly, Rosch, unlike pre-*Basic Terms* research, was not concerned with intersubjective agreement nor did she try to cope with the problem of contextual effects (see above). The reader may be reminded that the index of *communication accuracy*, which took both concerns into account, had been found to be the best predictor of recognition patterns. Lucy is therefore quite justified in saying that “Heider [= Rosch] ignored the most powerful available measure of codability and did not show that her own alternative account of the earlier results would also work for different arrays” (Lucy 1992, 182).

2.3. “Natural” categories

The reasoning behind the notion of “natural categories” was the idea that perceptually focal items should serve, so to speak, as central anchoring points for the formation of categories. Perceptual salience being a natural fact, categories organized around focal items should form naturally. Testing this hypothesis would involve comparing natural categories, that is, in Rosch’s view, actual categories, with artificially constructed categories formed in violation of this “natural” principle of the centrality of focal items. This is what Rosch set out to accomplish in her study (Rosch 1973a), this time introducing, in addition to colors, new material involving regular geometrical forms (square, circle, equilateral triangle) and forms obtained from them by various modifications.

The formation of categories was, as it were, simulated by having subjects learn two main types of categories. Firstly there were categories formed around a naturally good example (focal colors and regular shapes), and secondly categories unnaturally formed because they were so constructed that focal colors and regular shapes were not central members, i.e. they resembled other members to a lesser degree than these members resembled each other. The prediction was, of course, that the Dani would learn natural categories more easily and with less errors. The results confirmed this prediction.

As a term covering both focal colors and regular shapes, all hypothesized to be naturally salient, Rosch used for the first time the term *prototype*. The term was not new in psychology. It might be of interest to retrace its history and see how Rosch gave it a new meaning.

2.4. The notion of *prototype* before Rosch

So far as I can ascertain, *prototype* was introduced into psychology by Attneave (1957) as an equivalent of *schema*. In cognitive research, the term *schema* has been put to many uses, and sometimes stretched to the point of referring to any data structure underlying a generic concept (Rumelhart 1980). The confusion is compounded by the fact that *schema* is strongly associated, in cognitive linguistics, with a non-propositional structure which bears more affinity to Piaget’s sensorimotor schemata than to anything else. An influential exponent of the latter view is Johnson (1987), who sets out to demonstrate the relevance of sensorimotor structures across different levels of thinking, from concrete to abstract thought. There is no doubt that *schema* rings a Piagetian bell, but it is unclear whether Piaget’s ideas are connected with the history of the

schema-as-prototype notion. Since, in this analysis, we are first and foremost concerned with this particular historical strand, we will leave out Piaget and focus our attention on what can be safely said about the theoretical background of the *schema-as-prototype* question.

Schema refers to a concept whose distant origin is Herbartian psychology. Roughly speaking, it is a cognitive structure (in Herbart, a mass of associated representations, or *aperceptive mass*) to which incoming representations are assimilated and with respect to which they are differentiated. On the evolution of this notion after Herbart, we can follow and expand somewhat on the historical leads cursorily mentioned by Attneave himself.

The term was used by Head to designate the *postural schema*, or to quote Head and Holmes (1911, 186), “this combined standard, against which all subsequent changes of posture are measured before they enter consciousness.” The postural schema, by assimilating new postures, is itself retroactively modified, thus forming a continuously evolving bodily reference frame for the self.

In his famous book on the process of remembering, the British psychologist Bartlett (1932) adopts the term, finding it suitable to render the dynamic and reconstructive character of the memorization process:

Schema, says Bartlett, refers to an active organisation of past reactions, or of past experiences, which must always be supposed to be operating in any well-adapted organic response. That is, whenever there is any order or regularity of behaviour, a particular response is possible only because it is related to other similar responses which have been serially organised, yet which operate, not simply as individual members coming one after another, but as a unitary mass [note the Herbartian term]. (1932, 201)

In an almost “clinical” way, Bartlett provides detailed analyses of reconstructions exemplifying the assimilative role of past experience, for example in memorizing and retelling a story at different time intervals.

The notion of schema is somewhat simplified by Woodworth (1938) who, after reviewing a number of studies on memory for form, draws the conclusion that new configurations are typically remembered as modifications of simpler, familiar and unambiguous preexisting forms. In Woodworth’s terms, new material is typically assimilated in the form of a “schema with correction.” This catchword is not lost on Hebb ([1949] 2002), who refers back to Woodworth in order to emphasize the fact that new material is more easily learned if it is similar with or does not deviate much from previously acquired knowledge or skill. This brings us up to Attneave.

In American psychology, at a time when behaviorist strictures could still be felt, a schema was a suspect entity. For a behaviorist, what is learnt is what one has been explicitly exposed to, not a ghostly reconstruction of past instances. However, Attneave thought that the existence of schemas could be established indirectly, through their capacity to facilitate learning. To this end, Attneave (1957) had subjects learn name-form pairings under two separate conditions. In one group, his subjects, in a pre-training session, were exposed to a form which was not in the training series, but had served to generate it, through various modifications; and there was a control group, with subjects who had not undergone the pre-training session with generating schemas. Since a generating schema is like a template from which alternate versions are derived, Attneave also called it a *prototype*. The prediction that being exposed to prototypes would facilitate learning was confirmed.

In papers which pursued Attneave's line and capitalized on research done in the same vein, Posner and his coauthors (1967, 1968) showed that subjects learned to categorize artificial visual patterns more easily if these were obtained from prototypes, especially if they were formed from prototypes by a relatively small amount of distortion. Furthermore, prototypes of a category were recognized as belonging to that category in a shorter time than the exemplars subjects had actually seen. Finally, subjects proved sensitive to the variability of items in a category; thus, subjects who had been trained using items of low distortion had more trouble categorizing items of a higher level of distortion. The general lesson to be drawn from this line of research was that prototypes could be extracted from forms sharing some similarities, and that subjects could implicitly store a measure of the degree of resemblance characterizing items of a category.

Presumably the introduction of new experimental material, namely visual forms, was for Rosch an invitation to establish a parallel with the literature on artificial prototypes. However, calling focal colors *prototypes* was a bold move, for it is not clear in what way focal colors could instantiate the central tendency of a class of colors. Furthermore, claiming that prototypes are naturally salient is not obviously reconcilable with the role that experiments on artificial categories assigned to them, for this claim would seem to imply that, in a natural environment, salience overrides the extraction of similarities as a principle of category formation.

2.5. “Semantic categories”

As early as 1971 (cf. Lakoff 1973a), Rosch had suggested that categories like ‘birds’ or ‘vegetable,’ which she simply regarded as groupings of referents, could be amenable to the same treatment as colors (Rosch Heider 1971b). In other words, it was hypothesized that members of a so-called “semantic category,” such as the various species of birds, could be ranked according to their degree of typicality. In this initial stage of prototype theory, this issue was conflated with the question of knowing whether subjects perceived different degrees of category *membership*. It was therefore assumed that typicality ratings would determine the degree to which items belong to a category, an idea which was in all likelihood directly inspired from the case of colors.

The notion of a “semantic category” had been in the air for some time. There was first a strand of research on “semantic memory,” more specifically on decision procedures used for assigning an item to a category and for verifying features of this item. Semantic networks had been developed for that purpose but had soon run into problems, so that more flexible procedures had to be devised (Hampton 2016; for criticism, Rastier 1991). The notion of “semantic category” was also a by-product of research on free recall. It had been noted that subjects, when recalling lists of words, tended to produce clusters of associated words, even when these words had been split up during presentation (Jenkins and Russell 1952). The role of categorical organization, that is, of taxonomical relations between words received especial attention from psychologists like Bousfield, Tulving and their coworkers. Bousfield (1953), for example, observed that subjects tended to recall words in clusters of the same category, even if they had been presented in scrambled order. In this perspective, it was instructive to see the relation of these categorical clusters to frequency norms obtained by asking subjects to list items belonging to various categories. Such norms had been compiled by Cohen, Bousfield and Whitmarsh (1957), but the supply was scanty and the demand so high that Battig and Montague set out to collect data on a larger scale, for 56 categories (Battig and Montague 1969). In their instructions, they asked subjects to list items included in a category that was provided by the experimenter, and gave an example of a “correct” response (‘seafood’ > *lobster, shrimp, clam, oyster, herring* etc.). The responses obtained were classified by order of frequency.

For her own experiment, Rosch extracted 8 categories from the lists compiled by Battig and Montague (*fruit, science, sport, bird, vehicle, crime, disease, vegetable*), and in each list selected 6 instances of different frequencies. As an illustration, for ‘fruit,’ the list was (by order of

frequency): *apple, plum, pineapple, strawberry, fig, olive*. Then, she asked subjects to rate the typicality of these instances, with instructions so explicit that they may be described as “Roschian categories for dummies.” It is worth quoting these instructions at length:

This study has to do with what we have in mind when we use words which refer to categories. Let’s take the word “red” as an example. Close your eyes and imagine a true red. Now imagine an orangish red... imagine a purple red. Although you might still name the orange-red or the purple-red with the term “red,” they are not as good examples of red (as clear cases of what red refers to) as the clear “true” red. In short, some reds are redder than others. The same is true for other kinds of categories. Think of dogs. You all have some notion of what a “real dog,” a “doggy dog” is. To me a retriever or a German shepherd is a very doggy dog while a Pekinese is a less doggy dog. Notice that this kind of judgment has nothing to do with how well you like the thing; you can like a purple red better than a true red but still recognize that the color you like is not a true red. You may prefer to own a Pekinese without thinking that it is the breed that best represents what people mean by dogginess. On this form you are asked to judge how good an example of a category various instances of the category are. The first category is “fruit.” On the left side of the page are six different kinds of fruit; the first is “apple.” To the right of apple are seven blanks; the blank closest to apple is to be checked if an apple is a good example of your idea or image of a fruit. The blank to the extreme right is to be checked if apple fits very poorly with your idea or image of a fruit. The other blanks represent the range in between a very good and very poor fit [...] Mark one and only blank for “apple” etc. Don’t worry about why you feel something is or isn’t a good example of the category (and don’t worry whether it’s just you or people in general who feel that way) — just mark it the way you see it. (Rosch 1973b, 131–2)

Rank of goodness was found to be highly correlated with frequency. Would this result not be a clue that typicality boils down to frequency, at least in this experimental framework? Rosch thought not, and she was apparently confident that her subjects were not simply estimating frequency. Indeed, were her instructions not explicit enough to prevent her subjects from going off the rails? Still, one may wonder if her confidence was not, at this stage, a matter of faith. It should be added, however, that further research has not supported the idea that typicality boils down to frequency. For example, the frequency and typicality norms compiled by Uyeda and Mandler (1980) have not proved to be highly correlated, and Hampton (1997) has shown that, for “semantic” categories, the effect of frequency can be dissociated from the contribution of typicality, especially in tasks which emphasize speed of categorization.

2.6. What is a prototype?

Is there any commonality in the various kinds of prototypes which have been examined so far? Functionally, a prototype may be envisaged as that with respect to which an item is judged as belonging to a category. This first unifying notion underlies Rosch's study on *cognitive reference points* (Rosch 1975a), a notion which was inspired by the Gestalt psychologist Max Wertheimer (1938).

Rosch reasoned that if items were to be categorized as a function of their proximity to a prototype, an asymmetry should be observed: a prototype should instantiate *y*, i.e. function as the standard of comparison, in a judgment like: 'x is almost / virtually / essentially, etc. y.' In her protocol, subjects were instructed to place *physical instantiations* of colors, forms (lines at various angles) and numbers into this linguistic context. It was also expected that subjects would estimate the physical distance of an item to a prototype as shorter when the prototype was stationary and therefore functioned as an anchoring point, by contrast with the situation where the prototype was mobile and the other item stationary. To a large extent, Rosch's expectations were confirmed.

The first test, on hedges, was inspired by Lakoff's paper on hedges (Lakoff 1973a), but Rosch significantly altered Lakoff's perspective. Although Lakoff considered, like Rosch, that categories were inherently fuzzy, and that hedges served to increase or restrict fuzziness, he also thought that hedges acted like catalysts with respect to different kinds of attributes. For example, some hedges, like *technically*, targeted criterial attributes, i.e. features sufficient to confer category membership, others (like *strictly speaking*) targeted definitional attributes, while others targeted neither of these (like *regular* in *Harry is a regular fish*; 'swims well' is not a criterial attribute, i.e. does not confer category membership).

Now Rosch, by and large, ignored this function of hedges. She did not consider that hedges served to adjust the dimension with respect to which two items would be compared. On the contrary, her experimental design reflected the idea that comparandum and comparans (standard) were statuses independent of context, therefore once a prototype, always a prototype. Yet, in some cases, the comparans was *not* the prototype. For *almost*, the comparans was typically the most saturated color, even if not of a prototypical hue, and for numbers the comparans was the highest number. Apparently, *almost* targeted a *quantitative* dimension of comparison. If, therefore, the comparans was a reference point, and the reference point a prototype, then the prototype varied as a function of the dimension of comparison. However, that was not the way Rosch thought

of it. In short, Rosch conceived of typicality as a phenomenon that *must* be acontextual.

2.7. A probabilistic and pragmatist theory of categorization

At times, it seems that Rosch both recognizes the existence, for “semantic categories,” of a distinction between criterial and non-criterial attributes, and yet denies its relevance for typicality judgments. She says for instance:

Subcategories which are identical with respect to criterial attributes may differ in the extent to which they represent the core meaning of the category — for example, the distinction between wild and domestic is irrelevant to the formal definition of “bird” since both birds and nonbirds may be either wild or domestic; however, wild birds are judged more central to the bird category than domestic ones. (Rosch 1973b, 141–2)

If typicality covaries with degree of membership, this means that items which possess criterial attributes, hence whose membership degree = 1, but which are not good examples of a category, have a membership degree < 1. One way to escape this contradiction will be to establish a distinction between *identification procedures*, which may make use of non-criterial attributes and are subject to typicality effects, and judgments bearing on the core meaning of concepts, which may be based on criterial attributes (Smith, Medin and Rips 1984). At any rate, the correlation between typicality and degree of membership had to be abandoned (Armstrong et al. 1983; Kleiber 1990).

Rosch avoids this contradiction by conflating all attributes, criterial and non-criterial, into a single class, that of probabilistic cues. Before Rosch, a similar conception underlay some statements made by Bruner et al. (1956, 47):

A bird has wings and bill and feathers and characteristic legs. But the whole ensemble of features is not necessary for making the correct identification of the creature as a bird. If it has wings and feathers, the bill and legs are highly predictable. In coding or categorizing the environment, one builds up an expectancy of all of these features being present together. It is this unitary conception that has the configurational or Gestalt property of “birdness.” [...] When the conception is well enough established, it takes on the property of being able to serve as a discriminable and seemingly irreducible attribute of its own. One can array things in the degree of their birdlikeness in much the same way as one can array lengths, a presumably less complex attribute.

This probabilistic conception of categorization, which Bruner probably owes to Egon Brunswik (Fortis 2010), has several advantages: category membership is not an all-or-none affair, for cues afford inferences of various degrees of probability; cues are not singly used, but afford inferences about other properties that an item is expected to have; and finally, the interpredictability of attributes paves the way for an explanation of the formation of categories.

Briefly put, categories are created because forming bundles of interpredictable attributes is *useful*. In Rosch's own words:

a category is most useful when, by knowing the category to which a thing belongs, the organism, thereby, knows as many attributes of the thing as possible. Segmentation of the same domain would be progressively less useful the fewer the properties of things predictable from knowing the category. (Rosch 1975c, 197)

This conception is very close to the one defended by Bruner, for example in a passage where it is associated with Peirce:

...the categorial placement of the object leads to appropriate consequences in terms of later behavior directed toward the perceived object: it appears as an apple, and indeed it keeps the doctor away if consumed once a day. Let it be said that philosophers, and notably the pragmatist C. S. Peirce, have been urging such a view for more years than psychologists have taken their urgings seriously. The meaning of a proposition, as Peirce noted in his famous essay on the pragmatic theory of meaning, is the set of hypothetical statements one can make about attributes or consequences related to that proposition. [...] The meaning of a thing, thus, is the placement of an object in a network of hypothetical inference concerning its other observable properties, its effects, and so on. (Bruner 1957, 126)

Perhaps through Bruner, we may discern in Rosch a distant echo of American pragmatism.

2.8. Family resemblance

Different items are not necessarily identified as members of a category by the same set of potential cues. We cannot see penguins flying, and therefore do not confer the property 'can fly' to them, yet we categorize them as birds because they are sufficiently like other birds. Rosch took this fact on board by saying that categories with members of unequal typicality have a *family resemblance* structure. The notion was borrowed from Wittgenstein and introduced by Rosch in the following terms:

This principle was first suggested in philosophy; Wittgenstein (1953) argued that the referents of a word need not have common elements in order for the word to be understood and used in the normal functioning of language. He suggested that, rather, a family resemblance might be what linked the various referents of a word. A family resemblance relationship consists of a set of items of the form AB, BC, CD, DE. That is, each item has at least one, and probably several, elements in common with one or more items, but no, or few, elements are common to all items. (Rosch and Mervis 1975, 574–5)

Wittgenstein was not the first to propose this “principle,” contrary to what Rosch says (for a genealogy, see Goeres 2000; Fortis 2015b). As pointed out in Baker’s and Hacker’s commentary (2005), family resemblance was intended by Wittgenstein as an antidote to his first attempts at defining the essence of terms like ‘proposition,’ or ‘language’ (see also Krüger 1994).

The §§66-67 of Wittgenstein’s *Philosophical Investigations* are the locus classicus. Here is an abridged reminder of them:

66. Consider for example the proceedings that we call “games.” I mean board-games, card-games, ball-games, Olympic games, and so on. What is common to them all? Don’t say: “There *must* be something common, or they would not be called ‘games’ ” — but *look and see* whether there is anything common to them all. [...]

And the result of this examination is: we see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail.

67. I can think of no better expression to characterize these similarities than “family resemblances” [*Familienähnlichkeiten*]; for the various resemblances between members of a family: build, features, colour of eyes, gait, temperament, etc., etc. overlap and criss-cross in the same way. — And I shall say: ‘games’ form a family. ([1953] 2006, 27–28)

Games being heterogeneous, language games are too; on this account, there is nothing common to all language games in virtue of which they are so called. As a result, says Wittgenstein,

what we call “sentence” and “language” has not the formal unity that I imagined [i.e. in the *Tractatus*], but is the family of structures more or less related to one another. ([1953] 2006, §108, 40)

Wittgenstein does not appeal to a notion of prototype, although it was sometimes felt that, lest a concept lose its cohesiveness, he should have made room for it (e.g. Simon 1969, who argues that univocal categories

are held together by *paradigms*, i.e. members comprising all the attributes of the category). Whereas Wittgenstein seems to imply that the various kinds of games do not have anything which is common to them all, Rosch leaves the door open to the idea that members of a family resemblance structure do share features. However, how we should interpret this concession is not clear: is a common feature supposed to be a necessary one? All cats wag their tail, but is wagging one's tail a necessary attribute of cats? Furthermore, we are in the dark as to the relation of typicality, hence membership gradience, to family resemblance. Categories with degrees of typicality may be such that they possess features which are common to all members of a category and jointly sufficient to assign category membership to any member. Take for example the category 'bird': the conjunction of 'lays eggs' and 'has a beak' is sufficient to assign an organism to the category 'bird,' yet Rosch would certainly say that being subject to typicality effects, this category has family resemblance structure. Or perhaps does she intend to claim that family resemblance comes in degrees, with some categories being closer to classical categories, like 'bird,' while others are more like the prototypical category of 'games'? This question raises an issue which Rosch did not deal with: categories with typicality effects, especially as they were interpreted in cognitive semantics, form an heterogeneous lot, and typicality need not correlate with family resemblance nor with other characteristics often associated with these categories (Geeraerts 1989).

2.9. Complexive classes

The reference to Wittgenstein's family resemblance should not eclipse a line of inquiry, which Rosch mentions in passing, and whose influence, therefore, is difficult to assess. I shall dwell on it briefly, since this work does not seem to be well known. I am alluding here to the following passage (Rosch and Mervis 1975, 602):

The principle of family resemblances in adult categories casts a new perspective on children's classifications. Young children have been shown to classify objects or pictures by means of complexive classes, that is, classes in which items are related to each other by attributes not shared by all members of the class. (Bruner, Olver, and Greenfield 1966; Vygotsky 1962)

It is probable that Rosch, a former student of Harvard, where she wrote a dissertation on child psychology, had first-hand acquaintance with complexive classes through the work of Bruner. Now, Bruner himself had

borrowed the notion from Vygotsky, which is duly acknowledged in Rosch's references above.

The closest origin of complexive classes is the developmental theory of Heinz Werner, whom Vygotsky quotes on several occasions (1962; [1934] 1988). For Werner (1933), complex states are undifferentiated psychological contents or acts which are grasped as total units. The most primitive complex states are intuitive groupings which give rise to collections organized according to Gestalt-like principles. Further, connections between objects are initially context-dependent, so that features linking objects can hardly be abstracted from the *hic et nunc* situation. Being subject to circumstances and merged into complex psychological units, these linking features cannot stabilize a word's meaning, with the consequence that the child's verbal concepts do not have the character of generic concepts subsuming clearly defined instances. Conceptual development requires that holistic and situation-dependent states be progressively differentiated into recurring features. Importantly, complex thinking is an inferior form of cognitive functioning, which is the hallmark of so-called *Naturvölker*, children and subjects suffering from mental disorder (especially schizophrenia, agnosia and aphasia). Werner's insistence on the feeble capacity of "primitive" people for abstraction, and his willingness to confirm his own prejudices with a perfunctory use of the literature are an unpleasant aspect of the book.

Like in Werner's theory, in Vygotsky's account complexive classes are characteristic of a stage in the cognitive development of the child. During this stage, objects are grouped together through attributes that may vary from one pair of associated items to the next. Classes thus formed may have various structures: they may be built around a nucleus, i.e. a central instance sharing at least one attribute with every member; they may comprise objects that are functionally related (like 'fork' and 'plate'), or made up of elements chained together like the links of an associative chain, or even be unified by features that are themselves somewhat vague or diffuse. Since complexive classes are not based on consistently applied features, they are not yet "concepts." Features which are criterial of a concept must be consistently singled out and, as it were, stabilized for concept formation to take off. Such stabilization is made possible, says Vygotsky, through language; and it is thanks to this verbal *instrumentation* (as Bruner was later to put it), in interaction with adults, that the child raises itself above this stage of erratic categorization. Remarkably, speaking of words as they are used by children when referring to complexive classes, Vygotsky says they are akin to *family names*, insofar as they connect objects by similarity without there being consistency in the

features establishing this similarity. Lastly, for Vygotsky, lexical semantic change is typically complexive, since it is generally the case that new meanings link up with older ones through features of a contingent and unpredictable nature.

In pursuing Vygotsky's ideas on complexive classes and the role of language, Bruner is faithful to his Russian precursor. His typology of complexive classes is close to that of Vygotsky (Bruner 1964; Bruner et al. 1966).

For Rosch, complexive classes do not characterize an inferior stage of cognitive development. The family resemblance structure of semantic categories and their very cohesiveness ensure that Rosch's complexive classes are not collections of straggling members contingently brought together under a name. In short, though they have a comparable structure, they are more tightly organized than Vygotsky's complexive classes. And Rosch's concern is not to explain how we get from complexive classes to bona fide concepts with clearcut boundaries. Finally, for Rosch, there is an important exception to the prominence of complexive classes in children: on a certain mid-level of categorization (the *basic level*; cf. infra 2.12), the sorting by children is not complexive and reflects "natural" discontinuities (Rosch et al. 1976; Rosch and Mervis 1977).

2.10. Cohesiveness and typicality: the *cue validity index*

Categories whose members may be connected by long chains of resemblance are not like contingently formed complexive classes insofar as prototypes hold them together through shared similarities. They thereby retain a degree of cohesiveness which also ensures their distinctiveness, as we shall now see.

Rosch and Mervis (1975) suggest that centrality (i.e. prototypicality) be a function of the degree to which any given item resembles members of a category. On the other hand, central members should, in their category, be maximally different from items belonging to alternate categories. They should, therefore, have high distinctive features. In the psychological literature, an index had been proposed which was intended to measure the degree to which a cue was distinctive or diagnostic of a category. Roughly, this measure expressed the degree to which a cue was associated with one particular category rather than with others:

The validity of a cue is defined in terms of its total frequency within a category and its proportional frequency in that category relative to contrasting categories. (Rosch and Mervis 1975, 575)

This index had been introduced in Brunswik's probabilistic theory under the name of *cue validity*, and transmitted to Rosch via the studies of Reed (1972) and Beach (1964). The distinctiveness of a member could then be reformulated in terms of the sum of the cue validities of its attributes (Rosch and Mervis 1975). Note this way of defining a prototype by a member having the highest total cue validity was not obvious at all. Reed (1972), for example, *opposed* categorizing by estimating the distance of an item to a prototype versus categorizing it by means of cue validity. What seems consistent to Rosch is to attribute distinctiveness to prototypes, hence the possible reformulation in terms of cue validity. This may reflect both her initial arch-example (focal colors) and an idea we shall see being applied shortly, namely that categories mirror discontinuities in the environment. Furthermore, cues with high validity being highly predictive of a category, their utility is also high since they carry inferences about other attributes of the category. This goes along with the idea that categorization works efficiently, especially on the prototype level.

To confirm the idea that total cue validity is high for central members, Rosch and Mervis had subjects list attributes of items at different degrees of typicality. They then computed the relative amount of overlap between the members of a category. They also needed an estimate of the degree to which members, especially central members, overlapped with items belonging to "contrasting categories." This proved a much more delicate matter. What are the contrasting categories of 'weapon' or 'vegetable'? Asking subjects the question "if X is not Y, what is it?" failed to produce consistent responses or produced "creative answers" that were not deemed reasonable (Rosch and Mervis 1975, 584).

To circumvent this problem, Rosch and Mervis used *artificial* categories, in fact sequences of letters, e.g. PHMQB or XPHMQ, in which each letter was intended to correspond to one attribute. It was demonstrated that, as has been hypothesized, categories whose members were relatively more dissimilar from other members of other categories were the most easily learned. However, the relevance of these artificial categories for an account of "semantic categories" may be questioned. Artificial material of the kind used by Rosch is devoid of semantics; furthermore, inter-item similarities and dissimilarities are visually perceived, and the number of attributes per item is, by stipulation, determinate. Nothing of the sort holds for the "semantic categories" these letter-chains are supposed to stand for.

Let me indicate just one crucial problem, which is the context-dependence of category assignment. In *caviar is in increasing short supply*

because of intensive fishing, context assigns *caviar* to, say, the category of sea products which are in danger of going extinct. In this context, the contrastive category may therefore be ‘sea products which are still in abundant supply.’ In a context where differences in socio-economic status are being hinted at, *caviar* might be contrasted with ‘items associated with poverty,’ such as cheap housing, clothes, food etc. vs *castle*, *Champagne*, *Rolls-Royce* and so on (Rastier 1987, 53–54). If context plays a part in determining the categories an item may contrast with, we can understand why contrastive categories could not be agreed upon in Rosch and Mervis’ experiment: context was simply lacking. When Rosch does deal with contextual matters, this is done in a way to corroborate her view on the context-independence of a category’s internal structure. She observes for example that items close to the prototype, like *sparrows*, are better substitutable with *birds*, than items like *turkeys* in a context like *Twenty or so birds perch on the telephone wires outside my window and twitter in the morning* (Rosch 1975c, 190–1; 1978). But, as noted by Rastier (1991, 196), if, when tested on typicality, subjects had just extracted prototypes from implicit stereotyped contexts, one could hardly use stereotyped contexts as independent evidence for the identification of prototypes.

2.11. Correlational structure

We have already seen that categories are “naturally” formed insofar as they correspond to bundles of interpredictable attributes. It is only a short but dangerous step to claiming that bundles of attributes are *given in the environment*, at least for categories Rosch describes as “concrete,” without, however, being particularly explicit as to the scope of the term (are *sports*, or *crimes* “concrete”? Rosch 1977, 37). This claim is most clearly voiced when Rosch reformulates her views on category formation in information-theoretic terms.

In information-theoretic parlance, interpredictable attributes could be described as *redundant*, a point that had already been made by Brunswik about cues. Thus, Brunswik (1955) had emphasized that having multiple cues for one single attribute could ensure that inferences about the environment would be safer and more stable. Now, in a psychological experiment, redundancy could be given a precise definition by using stimuli with an artificially controlled number of dimensions and values.

This line of research was pursued by Garner (1962, 1974), whose notion of *correlational structure* was directly borrowed by Rosch. Garner’s perspective can be best understood from an example.

Take for instance stimuli constructed with 4 dimensions of two values each. In Garner's example, the 4 relevant dimensions are: the shape of the figure (square / circular); the side of the opening (left or right); the orientation of the inside line (horizontal or vertical); the shape of the inside line (straight or squiggly). The set composed of all the equiprobable combinations is a *total set*.

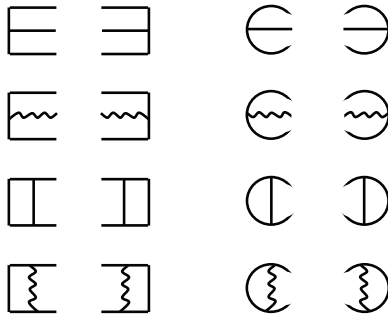


Fig. 5-1. Total set constructed out of stimuli varying on 4 dimensions, of two values each. (Garner 1974, 6-7)

Suppose we choose, out of this set, the first and last columns and leave out the remaining stimuli (fig. 2).

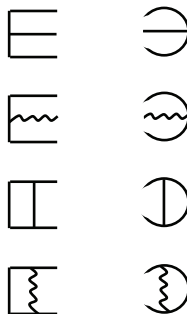


Fig. 5-2. A redundant subset of the preceding total set (Garner 1974, 7)

We can observe that two values are correlated: whenever an element has a square shape, its opening is on the right side, and whenever an element is circular, its opening is to the left. Suppressing one of these dimensions (shape or side of the opening) would not alter the fact that all the elements of our 8-member set are different. In other words, the

correlational structure of this set implies that one of its dimensions is *redundant*.

Garner hypothesizes that, with artificial stimuli, subjects may tend to classify stimuli into groups which maximize inter-item similarities and maximize inter-group dissimilarities (Garner 1974, 98). *Ceteris paribus*, then, stimuli with correlated values might tend to fall in the same group. How closely Rosch follows Garner on this point needs no emphasis.

A conception of attributes according to which they are real properties of the environment is a *realist* one. In that sense, by adopting Garner's correlational structure, Rosch appears to make a plea for realism. However, it is not obvious that attributes can be enumerated independently from the way languages segment them. Suppose, for instance, that the attributes listed for 'saw' may also characterize 'knife' (cf. Rosch et al. 1976). In a realist perspective, we should refrain from saying that 'knife' and 'saw' share an attribute in virtue of the fact that our language uses the same designation for the relevant parts. Perhaps we should say, instead, that this attribute is the same insofar as it performs similar functions, but that it has distinct shapes and is associated with slightly different motor routines in each case. But what about a door *handle*? Does it perform the same general function? Is it the same attribute? Since in French *poignée* is preferred to *manche* when referring to a door handle, should we say that *manche*, being more distinctive when referring to cutting implements, has a higher cue validity for cutting implements than *handle*? In brief, two issues needed to be clarified before we could adopt a realist conception of correlational structure, but unfortunately they remained unclarified. The issues in question were the role of language in segmenting attributes, and, if language is not deemed relevant for that purpose, the criteria we should use in identifying interactional attributes (such as motor routines).

2.12. Taxonomical levels

The claim that the mind, as it were, resonates to the correlational structure of the world implies that categories of "concrete" objects cannot be arbitrary (Rosch 1978). This consequence concurs with the general purview of prototype theory: the search for universal principles of categorization which would be founded on "natural" facts, whether in the world out there, or in cognitive processes.

A crucial aspect of "semantic categories" has not been accounted for yet, and this relates to taxonomical organization. Again, in the perspective of Rosch, this organization should be based on universal principles, and these principles should be articulated with the claim that categories reflect

the way the mind resonates to the correlational structure of the world. Since research on universal principles of taxonomical organization was already being done by Berlin, it was natural to turn to him for leads on how to treat this issue.

In a series of studies on folk biotaxonomies, Berlin had suggested that folk classifications followed an evolutionary sequence, going from a level corresponding to the genus (see the table below for an example) and moving toward the specific and life form levels. The other ranks are secondary developments, the intermediate level being unstable, and the kingdom (or *unique beginner* in Berlin 1972) often being absent or a late offspring.

<i>taxa</i>	<i>examples</i>
kingdom	plant
life form	tree
intermediate	evergreen
generic	pine
specific	whitepine
varietal	Western whitepine

Taxonomical ranks (in bold, most frequently found taxa; Berlin 1978)

The prominence of the genus is attested by the sheer number of distinctions made on this level, its typically monolexemic designations, and the fact that names of genera typically enter into the designation of more abstract or more specific groupings. Furthermore, by contrast, other levels form small contrastive sets (Berlin 1972; Berlin et al. 1973). Just like for color terms, evolution toward greater differentiation is attributed to an increased mastery of natural resources, i.e. to the domestication and cultivation of varietal forms (Berlin 1972, 72). On the other hand, greater abstraction typically obtains at a relatively low rank, that of the life form. Names for life forms and specific-level items often result from, respectively, a genus or a species acquiring a sort of paragon status and transferring or borrowing their name from the genus. It would be tempting to see in such “archetypes” an equivalent of Roschian prototypes, were it not for the fact that in Berlin’s initial account no insistence was placed on their psychological salience. Berlin was well-aware of the history of the genus concept, but the established tradition (e.g. in botany) of characterizing the genus as a configurational category, grasped, as it were, in a single glance, was granted significance at a later stage (Berlin 1992). Quite possibly, this significance was recognized after Rosch proposed a

psychological explanation for the privileged status of the genus (Berlin 1978).

The central idea on which Rosch bases her explanation is stated for the first time in the following terms:

The generic level is the unit at which there is the greatest gain in shape correlation when moving from the next higher (more abstract) classification. For example, the shapes of “chairs” are probably far more highly correlated than the shapes of all objects classifiable as “furniture”; however, relatively little gain in correlation is achieved when we go to the next more concrete level, to armchairs, rocking chairs, and the like. [...] And it may be that the generic level is the most abstract level of classification which can be economically coded in cognition for it may be just the average shape which serves as the prototype. (Rosch 1975c, 201)

Thus, the generic level corresponds to a perceptual discontinuity and to a global shape. In a long paper where Rosch and her coworkers set out to confirm this intuition experimentally (Rosch et al. 1976), this generic level was rebaptized “basic level,” and discontinuity was added to the property of high contrastivity: *chairs* contrast more with *tables* than *rocking chairs*, on the inferior level, contrast with *armchairs*. We now know that both properties can be reformulated by saying that items on the basic level have high cue validity, in fact cue validity is highest for the basic level for the following reason: “Superordinate categories have lower cue validity than basic because they have fewer common attributes within the category; subordinate categories have lower cue validity than basic because they share attributes with contrasting subordinate categories (e.g., kitchen chair shares most of its attributes with living room chair)” (Rosch and Mervis 1975, 586–7). Since this property also characterizes prototypes, the same principle accounts for the formation of prototypes as well as the basic level.

Worthy of note is the fact that in the same paper, a new kind of attribute was introduced: motor movements made to objects. Integrating potential actions into the perception of objects was of course not a novelty. In American psychology, Tolman (1933) had suggested that objects be understood as elements in a field of means-ends relations, and therefore their utility and potential for action be placed on a par with sensory attributes, and, as it were, co-perceived with them. Such features he called *manipulanda* and *utilitanda*. A more distant reference was Lewin’s notion of *Aufforderung*, which Gibson (1966) translated as *affordance*. Similarly, Brown (1965) considered that some categories were marked off by distinctive actions, moreover that distinctive actions characterized a level

of abstraction which had a privileged status in acquisition (Brown 1965). Brown (1958) had also hypothesized that objects were most commonly named at their level of usual utility, e.g. that a *spoon* was so named rather than being called *a piece of silverware* or this *particular ill-washed restaurant spoon* because its distinctive use required just this degree of precision. More generally, it was proposed that the child's vocabulary developed out of this "middle level of abstraction." In short, some pragmatist considerations came into the picture again, and furnished a source of Rosch's notion of a basic level (as noted by Lakoff 1987, 31–2).

We cannot go into the details of the experiments by which Rosch and her collaborators sought to confirm their views on the basic level. What must be noted is that discontinuities in attribute clusters were observed at the superordinate level for biological categories, but they were observed at the (hypothesized) basic level for artefacts: thus, *chair* was at the most inclusive distinctive level for the category 'furniture,' but for a biological category like 'tree,' this level corresponded to 'tree,' not to 'oak' (Rosch et al. 1976, 431; Kleiber 1990, 79–83). For Berlin, it should be recalled, the generic level was 'oak,' not 'tree' (= *life form*). This discrepancy may simply be a reflection of the fact that Berlin's data did not come from urban communities, and thus reflected a higher degree of expertise for biological categories (this is the hypothesis favored by Rosch et al. 1976, 432). Alternatively, the *taxonomically* privileged genus may differ from the *psychological* basic level, presumably for cultural reasons. The matter is further complicated by the observation that for a biological category like 'quadruped,' *dog* has a claim for being at the basic level. Furthermore, linguistic tests which would target a privileged level of categorization (that is, the basic level), give mixed results for some instances. For example, as noted by Kleiber (1994, 246–7), *bird* does not seem to be a good anaphor in:

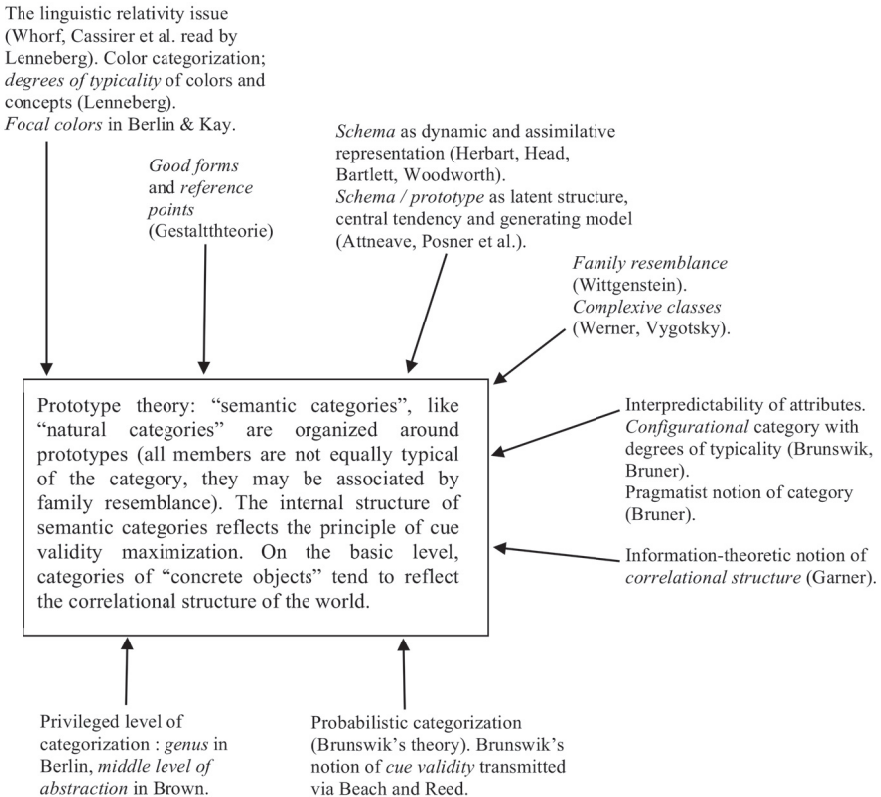
? *Paul has got a hen in his barnyard. Peter has a bird too but that is a turkey.*

A hen would therefore more likely be categorized as a *hen*; the case might be different for *dog*, since a *doberman* would seem to be so named only in specific contexts. Thus, it is not clear that taxonomically privileged rank, default linguistic level of categorization, and psychologically salient level are all equivalent. Finally, although Rosch can now make a clear case for why basic level categories of concrete objects exist, namely because they mirror discontinuities and correlational structures, Rosch does not explain why and how subordinate and superordinate categories are created. In the restricted field of biotaxonomies, especially botanical classifications,

Berlin offers some clues, but, on this point, his account mixes psychological and cultural considerations.

2.13. A summary

It is now time to take stock and sum up this rather complex history. Over the span of about 20 years, we have seen research on linguistic relativity move away from a relativist spirit and turn to universalist views, Rosch’s theory certainly being the most ambitious work accomplished in this latter perspective. An impressive aspect of Rosch’s theory is that it integrated into a conceptual patchwork many notions which had been put forward before and now seemed to fall into place. By way of recapitulation, Rosch’s multiple sources and their integration are summed up in the figure that follows:



3. The transfer to linguistics

3.1. Fuzzy grammar

As far as I know, prototype theory first manifested its influence within the field of linguistics in Lakoff's study on hedges (Lakoff 1973a); more precisely, we should speak of an interaction, since Lakoff, as we saw above (in 2.6), inspired some of the tests used by Rosch in her experiments on cognitive reference points.

Lakoff was very much on the watch for novelties, and after Rosch presented preliminary results on typicality judgements for semantic categories (in 1971), he saw an opportunity to bring together three unconnected lines of inquiry: that of Rosch, the notion of "degree word" in linguistics, which had just been worked on by Bolinger (1972), and Zadeh's theory of fuzzy sets (Zadeh 1965). Lakoff's main interest was in degree of membership, and the way in which hedges or intensifiers modify membership in a category. An intensifier like *very*, for example, may be described as shifting the values of entities which satisfy the 'very Adj' value to notches of the scale higher than those appropriate for the 'Adj' value (see Bolinger 1972, 17, for this kind of description). For example, a person who is *very tall* should be taller than a person who is said to be *tall* (Lakoff 1973a, 471). Other hedges or intensifiers listed by Lakoff were: *kind of*, *loosely speaking*, *more or less*, *sowewhat*, *par excellence*, *in essence*, *largely*, *a true / veritable X*, etc.

For Lakoff, hedges were revelatory in two ways. First, they reflected the fact that speakers made distinctions in typicality, or degree of membership. For example, the following sentences were intended to show that *chicken*, though undoubtedly a member of the category 'bird,' is not highly typical of its class (Lakoff 1973a, 473):

A chicken is a bird par excellence. [false]

In essence, a chicken is a bird. [true]

In a manner of speaking, a chicken is a bird. [nonsense, would presuppose that chickens are not really birds]

Second, hedges, as was previously mentioned, target different kinds of attributes. For example, Lakoff pointed out that *technically* was acceptable only when modifying a definitional attribute, whereas *regular* could be used with non-members with characteristics like those of typical members (cf. *Harry is a regular bachelor*, acceptable if Harry is married but is flighty and feels unbound by his marital responsibilities).

Both aspects, Lakoff argued, could be handled with fuzzy set theory. Fuzzy logic demonstrated the feasibility of a formal approach to sets whose members have degrees of membership varying from 0 to 1. Furthermore, the theory could model the different kinds of attributes targeted by hedges by letting any predicate be a vector composed of the values of this predicate for each kind of attribute.

For Lakoff, hedges were but one facet of fuzziness in grammar. At the time he published *Hedges*, he was actively collaborating with John Ross on gradience phenomena in grammar, and one of his numerous attempts at a grammatical model bore the name of *fuzzy grammar* (Lakoff 1973b). Ross (1973) had submitted the view that syntactic categories were “squishes” i.e. that being N, V, S etc. was not an all-or-nothing affair but a matter of degree of membership. Syntactic tests would establish to what extent a category was sentential or “nouny.” According to Ross, the “Nouniness Squish” corresponded to the following hierarchy:

That S > for...to... > Questions > Gerundives > Possessive -Ing (Max's giving the letters) > Action Nominal (Max's giving of the letters) > Derived Nominal (Max's gift of the letters) > Noun.

The extraction test, for example, was shown to be worse if extraction was made out of very nouny constituents:

*I wonder who he resented (it) that I went steady with.
?*I wonder who he resented my careless examination of.*

Thus, prototype theory first served in a project of wider scope, fuzzy grammar. Fuzzy grammar was itself one stage in the various theoretical attempts which went by the name of *generative semantics*, of which Lakoff and Ross were protagonists, and was therefore an episode in Lakoff's relentless calling into question of the Chomskyan understanding of generative grammar.

3.2. On the role of prototype theory in the constitution of cognitive linguistics

Elsewhere, I have tried to describe the circumstances and the motivations which led a few generative linguists (Lakoff, Langacker and Talmy) to split from syntax-centred generative grammar. From the dissident current known as generative semantics, or from sympathizers, comes the first battalion of cognitive linguists (Fortis 2012, 2015a). In this theoretical split, it is of interest to note that prototype theory was for

Lakoff, according to his own testimonial, of crucial importance. Given what we have said in the preceding section, it is clear that Lakoff did not perceive, at first, any incompatibility between prototype theory and formal approaches. Things radically changed when Lakoff progressively drifted away from generative grammar, and started to ponder on a new type of linguistics which would be “humanistic” and open to the experience of the world (Lakoff 1974, 1977). In short, revelation came from the notion of basic level:

When I first heard Rosch present her results on basic-level categorization, I was thrown almost into a state of shock. They contradicted the world-view that I was brought up to accept as if no other could possibly exist. (Lakoff 1982, 83)

This spurious world-view is what Lakoff characterizes as “objectivist,” which in an early formulation was the idea that

all psychological factors — perception, mental images, human purposes etc. — are ruled out. The world is assumed to be made up of objects with inherent properties and fixed relationships among them at any instant. (Lakoff 1982, 11)

This may come as a surprise, since when accounting for the prominence of the basic level in cognition, Rosch, as we have seen above, appealed to a *realist* conception of the mind-world relation. And in fact, says Lakoff later, “at the basic level of physical experience, many of the principles of objectivism appear to work well” (Lakoff 1987, 270). Apparently, what Lakoff retained from the basic level was a combination of aspects: the relevance of expertise for explaining variations in the level viewed as most salient, the Gestalt-like nature of basic level items, and the importance of interactional motor routines in Rosch’s description (Lakoff 1982). These were psychological factors which had been missing from semantics as Lakoff understood it before his cognitive conversion, and in his cultural background “semantics meant logic—there was no other technically viable approach to semantics” (Lakoff, in Huck and Goldsmith 1995, 107).

Most importantly, Roschian categories were appropriated by Lakoff for a grand scheme: the deconstruction of the so-called “classical” view of concepts and its attendant metaphysics, objectivism, both concurring in a “logical” view of the mind-world relation. In this “logical” view, propositions, hence predicates, reflect states of affairs and properties independently of the mind and must have determinate reference and truth-

conditions when correctly used (Lakoff 1987, 167). Though Lakoff conflates them, it is highly questionable that a truth-conditional conception of meaning necessarily goes hand in hand with a realist view of states of affairs and properties, but we shall not go into that problem now (Haser 2005). Similarly, the anti-logical and anti-formalist trends of cognitive linguistics are not always based on a thorough consideration of what modern logic purports to be, as noted by Desclés (1994). But let us go back to the so-called “classical” view and its strategic role.

The “classical” view holds that concepts should be definable by necessary and sufficient conditions, or, in other words, that things which fall within a concept share common features which are jointly sufficient to uniquely identify a kind. This classical view, explains Lakoff, was first challenged in the 20th century, the earliest challenge being by Wittgenstein and Austin (Lakoff 1987, ch. 2). Lakoff is more sketchy about the history of objectivism and its advocates, though he does refer to Aristotle, Frege and Putnam (Lakoff 1982, 1987, 168–169). Rosch (1987, 1999a) gives us a few clues, which point to the importance of common features in classical definitions: Plato’s universals, Aristotle’s definitions, and the British empiricists. Indeed, Plato’s universals require that commonality be found in all things that have a given property (see e.g. *Eutyphro* 5d); Aristotle explicitly states that attributes confined to a genus and used in defining a species should be jointly coextensive with this species, and it is clear from his examples that these attributes are common to things of the same kind (*Post. An.* II.13); and Locke’s process of abstraction implies that a general name like *man* be given to particulars that partake in “some common agreements” (*Essay* III.iii.7). It seems, therefore, that much of the significance of Roschian categories came from their being counterposed to the latter views. Being given a philosophical import of the greatest weight, this “new” conception of categorization helped establish cognitive linguistics as part of a global paradigm, that is a theoretical approach with ramifications in philosophy, psychology, and even at times neurophysiology. By mirroring Chomsky’s ambitions, cognitive linguistics could appear as a rival capable of challenging formalist and generativist approaches.

3.3. An old new conception

At the beginning of *Women, fire and dangerous things* (1987, 18), we see Lakoff praising Austin ([1940] 1961) for having “prefigured much of contemporary cognitive semantics,” especially on account of Austin’s discussion of the polysemy of *healthy*. Let me reproduce Austin’s text as quoted by Lakoff:

The adjective ‘healthy’: when I talk of a healthy body, and again of a healthy complexion, of healthy exercise: the word is not just being used *equivocally*... there is what we may call a *primary nuclear sense* of ‘healthy’: the sense in which ‘healthy’ is used of a healthy body: I call this nuclear because it is ‘contained as a part’ in the other two senses which may be set out as ‘productive of healthy bodies’ and ‘resulting from a healthy body’... Now are we content to say that the exercise, the complexion, and the body are all called ‘healthy’ because they are similar? Such a remark cannot fail to be misleading. Why make it?

In Lakoff’s view, Austin’s *primary nuclear sense* would be a precursor of the notion of prototypical meaning. Readers acquainted with the philosophical tradition will have recognized in *healthy* the example Aristotle cites when he introduces a form of polysemy which he calls *pros hen* (i.e. ‘said with respect to one <thing>’). The example occurs in a famous passage of the *Metaphysics* (Γ2, 1003a33), in the context of a discussion of the polysemy of ‘be,’ which, were it to be a case of genuine equivocality, would undermine the very enterprise of metaphysics. Now, Austin does acknowledge his debt in the above passage (Austin 1961, 71) but Lakoff took care to suppress the mention of Aristotle in Austin’s text, perhaps because it would not have squared well with claiming the novelty of his own views.

For reasons that need not detain us here, in the case of ‘be’ and ‘healthy,’ tradition has often spoken of *analogy pros hen*, or even of *homonymy pros hen* (e.g. Porphyry in Sorabji 2005, 234–5) although Aristotle just speaks of *pros hen kai mian phusin legesthai* (‘be said relatively to one thing and one nature’), and certainly not of analogy nor homonymy. It is interesting to note that when Brentano (1816, 96) discusses this form of “analogy” (his word), he finds it apt to say that terms like *healthy* behave, with respect to what they refer to, like family names (*Familiennamen*); in other words, languages are often unspecific and designate by one and the same “name” certain things whose family resemblance stems from the fact that they are all related to a fundamental meaning. Plainly, if Austin is a precursor of the notions of prototype and family resemblance, and even of cognitive semantics at large, then Aristotle is a precursor too. The claim that cognitive semantics goes against the philosophical tradition “from Aristotle to the later work of Wittgenstein” (Lakoff 1987, 6) should therefore be taken with a grain of salt.

3.4. Semantics

The first American cognitive linguists, who sought to enter fields left open by generative linguistics, from which they were disaffiliated, were, to a certain extent, cut off from the tradition of lexical semantics, still very much alive for example in Ullmann's book, *The Principles of Semantics* (1951). In other words, shortly before the advent of generative linguistics. They found that prototype theory was a handy tool for engaging in this line of research and especially for dealing with polysemy (on this transition, see Kleiber 1990). This appropriation of prototype theory was, however, partial. Prototype theory was essentially reduced to two tenets: one is there are basic meanings, and the second is meanings of polysemous words are linked by family resemblance. Thus, almost all studies (except Nunberg 1978) failed to take into account cue validity, and consequently tended to neglect the contrastive dimension of categories. To this day, the dominant approach is therefore semasiological. Further, there was no attempt at correlating central meanings with typicality ratings, and experimental validation was more than scarce.

In this new perspective, for reasons discussed below, whatever meaning is regarded as basic by the linguist, it is considered as prototypical (Kleiber 1990, 168), with the consequence that basicness and Roschian typicality may come to diverge. This is so in accounts which posit, in addition to prototypes, abstract meanings subsuming the senses derived from a prototype (Langacker 1987, and Langacker's student, Lindner 1981), and meanings linking otherwise unconnected networks of senses (Lindner 1981). In these accounts, abstract and linking meanings have the best claim to being those members most similar to the other members, hence, perhaps, Roschian prototypes (but note the issue is not discussed, and cue validity drops out of the picture); however, they are not considered as prototypes, due to the fact they are not primary, i.e. underived, meanings. Incidentally, in Langacker's framework, abstract meanings were needed, in particular for grammatical reasons: verbs, for instance, were analyzed as propositions saturated with abstract nominal meanings (Langacker 1979, 1981).

In lexical semantics, the first detailed applications of this modified version of prototype theory are Coleman and Kay's study of the verb *lie* (Coleman and Kay 1981), Brugman's monograph on *over* (Brugman 1981), later expanded and systematized in Lakoff (1987), and Lindner's analysis of the particles *out* and *up* (Lindner 1981). Since Brugman's and Lindner's studies were the first fruits of a productive line of research on adpositions and particles, we should say a few words about them.

First, Brugman's and Lindner's studies were *synchronic* analyses. They did not attempt to justify the primacy of their central meanings by going through a diachronic analysis of their evolution. Second, they used diagrammatic representations for their central spatial meanings and for the network of senses connected with these central meanings. Semantic networks were not a radical innovation, and it is interesting to note that Darmesteter (1887) used this kind of representation in the *diachronic* analysis of semantic evolution. Nunberg (1978) read Darmesteter, and since we know Lakoff read Nunberg, there is a possibility that Lakoff got acquainted with these pre-cognitive diagrams through Nunberg. To the best of my knowledge, the first diachronic analysis in which the notion of prototype appeared was due to Geeraerts (1983). In this sophisticated study, Geeraerts (1983), unlike Lindner and Brugman, assigned prototypical status to meanings which were not primary but had a particular salience as a result of being multiply connected with other senses (hence as a result of being more "central").

An important hallmark of Brugman's and Lindner's analyses is their localism: prototypical meanings are spatial, and other meanings are derived from them either by suppression or modification of spatial features, or by metaphorization. For example, Lindner claimed that in *The professor singled him out for criticism*, the use of the particle is motivated by a metaphorical extension from one of the prototypical meanings of *out* (essentially, 'extraction from within a place'; Lindner 1981, 103–4). We cannot fail to observe that this localist perspective converged with the empiricist views defended around the same time by Lakoff and Johnson in *Metaphors We Live By* (1980), and according to which "we typically conceptualize the nonphysical *in terms of* the physical" (Lakoff and Johnson 1980, 59).

Localism is, of course, not the exclusive property of cognitive semantics; it is, in fact, a traditional view of which the first instance I have been able to identify is Aristotle's analysis of the Greek preposition *en* in *Physics* (209a15sq). However, though Aristotle's description was localist, since he regarded the relation of spatial containment as primary, his localism was not justified on cognitive grounds. Rather, space is primary because, says Aristotle, "that without which nothing else can exist but which can exist without anything else is primary" (*Physics* 209a1–2). *Cognitive* localism seems to coincide with the gathering momentum of empiricism, that is to say, in the 17th century. More circumstantially, Locke's drawing attention to the role of "particles" (conjunctions and prepositions) in reasoning, together with scattered observations by various authors such as Scaliger, prompted a new interest in the semantics of

“particles.” A remarkable example is the analysis of prepositions and cases proposed by Leibniz in two opuscles in which he offered a semantic description very much in the spirit of cognitive linguistics (Leibniz 1685–1686, 1687–1688). Later work may be cited as well, such as James Harris’ *Hermes* (1765), localist theories of cases in the German-speaking world and elsewhere (Fortis 2014), and generally the work done in (post-)empiricist and more or less speculative etymology. In retrospect, then, we see that the notion of prototype served to reawaken a well-established tradition. In this respect, it has fully succeeded.

This resurrection was, however, a less trivial achievement than the existence of this tradition may suggest. It had to deal with the fact that particles and prepositions, being especially complex in English grammar, are, at least in part, idiomatically used. It also had to deal with the fact that regular patterns of use, if indeed observed at all, are not fully productive, for reasons that are unclear. There is also the fact that the various uses of particles and prepositions seem too variegated to warrant a semantic analysis. Authors like Bolinger (1971) helped cognitive linguists to take up the challenge of finding semantic motivations behind these seemingly incoherent patterns of use. But this was done by abandoning the project of generating acceptable patterns, and by emphasizing the fact that linguistic patterns exhibit a whole range of degrees of productivity. In this respect, we may contrast Langacker and Lindner with Nagy (1974), whose analysis of the metaphorical extensions of *up/down* (and other expressions of verticality) still evinced the generative concern of the time, that of finding a principled account for productivity and its limitations.

3.5. Other applications

As is testified by Lakoff’s bestseller (1987), the bulk of the work inspired by prototype theory involved semantic matters. This was rather unsurprising since prototype theory had been extended for the purpose of handling “semantic categories,” and in so doing, had participated in a global semantic change which encompassed linguistics and psychology. Perhaps less importantly, the notion of prototype also found applications in phonology, first in Jaeger’s dissertation (1980), where she followed in Rosch’s steps in having subjects learn categories of sounds (like [+anterior] or [+sonorant]) and having them judge the attributes and centrality of various sounds with respect to these categories. The outcome was a description of overarching “featural categories” like [+sonorant] in which these categories were decomposed into intuitive, phenomenological attributes, and into members of different degrees of centrality. Obviously,

this approach was a direct counterpart to the original, Roschian version of prototype theory (for a short presentation, Jaeger and Ohala 1984). On the other hand, aspects of another study by Nathan (Nathan 1986) may be viewed in parallel with the post-Roschian, semantic appropriation of prototype theory, insofar as *phonemes* themselves were described as *categories* of allophones of various centrality, in the same way that polysemous lexical units were treated as sets of form-meaning pairs. Further, since in Nathan's view centrality resulted from an interplay of natural constraints and functional (including semantic) factors, his perspective linked up with natural phonology and functional linguistics.

Another application of the notion of prototype came about from a theoretical orientation which shared with fuzzy grammar its central tenet, namely, that membership in syntactic categories, like *noun* and *verb*, is gradient. Such a view was defended by Hopper and Thompson (1984), in a paper which deserves mention for several reasons. Firstly its orientation was typological and functional, much in the spirit of Givón (1979 and other studies), and it was therefore a precursor in hybridizing cognitive notions with this line of research. Furthermore, gradience phenomena were neither formally apprehended (through syntactic tests, as in fuzzy grammar) nor, as will be the case in Langacker's cognitive grammar, correlated with fundamental forms of conceptualization. In Hopper's and Thompson's paper, prototypical nouns and verbs were associated with semantic functions (essentially, designation of objects and events), but these functions were considered as derivative of discourse functions, in other words, introducing participants for future referenciation in the discourse under consideration and reporting on events in the scope of assertion. Nounhood and verbhood, therefore, went by degrees, and these degrees were indexed by the capacity of nouns and verbs to receive the full range of morphological markings and engage in the syntactic behavior of prototypical members of their category.

That a typical member of a category should exhibit a wider range of morpho-syntactic marking and behavior had already been stated, though in somewhat different terms, by Greenberg (1966b), who spoke of *markedness* hierarchies (*unmarked* / *marked* was a dichotomy imported from phonology). Croft (1990), a student of Greenberg, was in a favorable position to combine Greenbergian ideas with the functional approach we have just touched upon. Indeed, his new synthesis incorporated elements of functional linguistics, such as, for instance, the correlation between discourse functions and syntactic categories (but note that some functional considerations were already at play in Greenberg's work). In this way, markedness hierarchies could have their foundations in functional and

cognitive motivations, and these considerations were considered as having high explanatory value.

To end this short and incomplete review, a few words need to be said about an application which, just like in semantics, was instrumental in reawakening interest in earlier views. In short, this application concerns analogical productivity. Basically, linguistic productivity is analogical if one or more specific forms establish a pattern which subserves the creation of novel forms, the regularization of deviant forms, or simply the formation of regular forms. This age-old notion (which goes back to Varro) was notably revived in the times of the neogrammarians, who had found it handy in accounting for forms which contravened phonetic laws. But in the American linguistics of the 1960s-1970s, analogical productivity had been delegitimized by generative rules and the quest for maximal generalizations. However, generativism could not fail to leave a gap in the empirical coverage of linguistic theory, namely, all phenomena deemed as weakly productive, be they irregular or idiomatic. The past tenses of English irregular verbs furnished good examples of weakly productive patterns and were seized upon by Bybee and her coauthors (following Zager 1980) in an attack on Chomskyan rules. Firstly, they pointed out that subsets of irregular verbs were weakly regular, in the sense that members of a subset were not strictly formed alike, hence subsets had a family resemblance structure. Secondly, members of a subset, they argued, could be ranked according to their similarity to a prototypical case. Further, verbs were not formed by derivation from a base form, but by analogy with a specific pattern (Bybee and Slobin 1982; Bybee and Moder 1983). Finally, the case-study served to contest the primacy of derivational rules.

The prototype of a subset was identified through its influence in the formation of artificial past tense forms, and from error patterns. For example, it was argued that [...æŋ(k)]_{verb / past} defined the prototype of verbs with a past tense in /æ/, on the grounds that errors were more likely to be in the direction of the prototype when a verb had a form close to it. Thus, *sing* and *drink* were good exemplars of the subset {*begin*, *drink*, *sing*, *swim*...}, whereas *begin* and *swim* were attributed peripheral status. Importantly, because productivity was thought to proceed first and foremost from unanalyzed forms, extensible schemas were claimed to underlie the production of regular forms as well, at least in the initial stages of language learning. In later work, Bybee would in fact defend a “single mechanism” view of morphological processing, or in other words an analogical model in which analyzable forms derived by rules are but an extreme point on a continuum of analogical formations of increasing

abstraction (Bybee 2010). On a wider scale, extensible schemas built from prototypes would become part and parcel of a reorientation of linguistic theory toward bottom-up and frequency based productivity, a global move which Langacker, with others, popularized under the name of “usage-based” grammar (Fortis 2011).

Conclusion

After chronicling this rather intricate history, we should now take stock of its general outline, and present a bird’s eye view of it.

Prototype theory was to a large extent incubated at Harvard University, and followed on from two major lines of research: studies on color categorization, initiated by Lenneberg, and investigation into the strategies used by subjects in categorizing various entities, under Bruner’s guidance. When Rosch took up the subject of categorization, Lenneberg’s initial relativism had already given way to a universalist movement whose effects were perceptible in linguistics and anthropology. In this context, “natural” focal colors and good forms acquired paradigmatic status, with the consequence that, when Rosch’s attention turned to “semantic categories,” these were envisaged as groupings of items with peaks of typicality. At a second stage, semantic categories were discretized into attributes. The problem became that of characterizing the “internal structure” of a category whose members were not placed on a continuum of variation. This is where family resemblance came on the scene. Since family resemblance could not ensure the cohesion and distinctivity of categories, Rosch introduced a principle of inter-categorical contrast, measured by her index of *cue validity*. Distinctivity and contrast also played a role in the formation and categories, and most notably, the establishment of a privileged level of categorization, the basic level.

We have seen how Rosch borrowed ideas from her intellectual environment, and gave coherence to them. Her synthesis was certainly impressive, in spite of problems posed by her acontextual view of semantic categories. Most importantly, it came at a time when American linguistics was opening up new fields, and so redirected part of its attention to pragmatic and semantic matters. In these circumstances, Rosch’s treatment of semantic categories could legitimize and kindle a new interest in lexical semantics. Further, prototype theory was appropriated to handle issues which were, for mainstream generative linguistics, blind spots: gradience phenomena, weak productivity, analogical formation, to which we should add inchoate applications to phonological categories.

As noted above, the notion of prototype was profoundly altered during the transfer of its application to linguistics. In semantics, this notion was reduced to two tenets: polysemous forms have basic meanings, and their meanings are connected by family resemblance. Hardly any attempt was made to obtain typicality ratings from experimental protocols, nor to examine inter-categorical contrastivity. Moreover, it was not clear in what ways basic meanings corresponded to the definition of a Roschian prototype, nor which kind of meaning, primary, central or schematic, best corresponded to a Roschian prototype. Finally, once prototype theory was imported, linguists showed no interest in its evolution, in alternative theories of concepts in psychology (immediately after Rosch, see Smith and Medin 1981), or in theoretical issues posed, for example, by the problem of precisely defining a criterion for category membership (Hampton 1995).

In lexical semantics, as well as in analogical formation, we may marvel at the fact that linguists found it necessary to turn to the Roschian machinery to resurrect fields and notions which had been on the horizon for some time. Reintroducing ideas of yore fulfilled a strategic goal, that of finding a niche for practitioners who had broken away from generative linguistics. Old and venerable ideas are well-suited for pursuing this aim: their distant but unmistakable familiarity lends them credence and makes them easier to adopt. Hopefully, this paper will have shown that this historical twist is best understood by considering the context of the period, and the use of prototype theory in pursuing agendas elaborated in functional-cognitive linguistics.

CHAPTER SIX

“THAT IS CONJECTURE”: ON ENGLISH ASSERTIVE SHELL NOUNS

CARLA VERGARO

Introduction

This study focuses upon the relationship between illocutions and the lexicon, in particular, illocutions and illocutionary nouns in their function as shell nouns (Schmid 2000).¹ Examples (1–4) of assertive nouns with their respective constructional patterns, extracted from the Corpus of Contemporary American English (henceforth COCA), are cases in point.

- (1) A Pakistani forester present countered this advice with the **assertion** that “there are no small farmers in Pakistan.” [ACAD 1994]
N-that
- (2) She said, matter-of-factly, “My daughter was paid to go to prison.” # Cynthia stopped writing and looked at Brixton. “That’s an unusual **allegation**, Mrs Watkins,” he said. [FIC 2011]
Pro-BE-N
- (3) “Then several hundred feet later you suddenly accelerated, lost control of the car, and went off the road.” “Your **conjecture** is that I accelerated about the same time I dialed nine-one-one?” [FIC 1994]
N-BE-that
- (4) “Under Bush tax revenues were at a record level.” “You can’t make that **argument**.” [SPOK 2011]
Det-N

The shell noun is in bold, and it characterizes the action performed in uttering some content. The underlined part is the propositional content it characterizes. Therefore, these nouns are all indicators of how the content

is to be taken, respectively, an assertion, an allegation, a conjecture and an argument.

Schmid's studies (1997, 2000, 2001, 2007) are the best known on the subject of shell nouns, particularly as far as English is concerned. Schmid (2000) defines shell nouns as "an open-ended functionally defined class of abstract nouns that have, to varying degrees, the potential for being used as conceptual shells for complex, proposition-like pieces of information." (Schmid 2000, 4). Nouns, therefore, are not shell nouns because of some inherent property; they have the potential to be used as shell nouns and some of them have this potential more than others.

Shell nouns derive their potential from the very fact that they are nouns, which entails that they have the potential for reifying and hypostatizing chunks of experience as integrated conceptual gestalts. The crucial semantic prerequisite for the capacity of nouns to function as shell nouns is the existence of a gap in their semantic structure. This gap is not only a precondition for shellnounhood, but it also controls the activation of shell nouns and shell contents. (Schmid 2000, 377)

Illocutionary shell nouns are metalinguistic in nature. Metarepresentation² is their main function. As is well known, metarepresentation involves a higher-order representation with a lower-order representation embedded inside it. The higher-order representation is generally an utterance (or a thought). Indeed, the referents shell nouns metarepresent are higher-order entities, namely utterance-acts. More precisely, when the reporting speaker in the current discourse situation uses an illocutionary shell noun, she is conceptualizing the pragmatic action of another speaker in the original discourse situation in a specific way, characterizing it as an assertion, a guess, a supposition, etc., and attributing to the speech act of the original speaker all the components of the illocutionary force, or the script behind that specific speech act. Therefore, the reporting process implies that all these components are attributed to the original speaker in the resource situation and are coded as belonging to her. However, it is entirely up to the reporting speaker in the current discourse situation how she wishes to characterize a given utterance. Whether the characterization is a true reflection of the communicative intentions of the original speaker cannot be verified—unless the current speaker is identical with the original speaker, as, for example, in "my guess is that ..."—and thus has to remain an open question.

From the morphological point of view, in general, most, though not all illocutionary shell nouns, are deverbal abstract nouns derived from speech act verbs.³ As such, they fall into the category of *nomina actionis*. More

specifically, they are a sub-group of *nomina actionis* in that the action they name or refer to is the specific illocutionary force of the speech act verb they come from. The topic of *nomina actionis* has been widely studied in linguistics (see, among others, Hopper and Thompson 1985; Bierwisch 1990; Gaeta 2002), and is considered complex because it involves the transcategorization from a grammatical category (the Verb) to another grammatical category (the Noun). The main function of deverbal nominalization is of a syntactic nature, i.e. that of operating—by reification of the predicate—a recategorization. The feature of reification, refers to the fact that the predicate is conceptualized as denoting an object and, as such, it can participate in the properties generally ascribed to nouns, such as, for example, the possibility of being pluralized. Moreover, there is a loss of illocutionary force, which is a gradual process consisting mainly of (i) the loss of deictic properties (e.g. tense markers), and (ii) the backgrounding of the actants.

In addition to belonging to the wide category of *nomina actionis*, from the semantic point of view, illocutionary shell nouns are a subset of linguistic shell nouns. They share the property of referring to a verbal action—conceptualized as a ‘thing’—that the speaker performs when addressing someone with an intention that her utterance should count as *F*-ing, namely as having the illocutionary force of doing the act named by the verb the shell noun is related to. As such, illocutionary nouns, whether or not in their function as shell nouns, convey concepts of communication because they are used to refer to acts of verbal communication.

The present paper reports the results of the analysis carried out on a group of assertive shell nouns. This type of nouns has been chosen because no prior research has focused on a fine-grained investigation of them.⁴ Moreover, assertive shell nouns are particularly important because the speech acts they name are especially salient in discourse genres that represent the human-universal norm, and they predominate in terms of frequency (Givón 1990, 779; Green 2013, 387).

Although it is clear that the categorial aspects of noun meaning have to be represented in a different way from the components of the meaning of speech acts, if one conceives of meaning as conceptualization of knowledge, the construal of the noun evokes the components of the speech act it names or refers to, in this case assertive speech acts. Searle’s characterization of assertive speech acts is that “The point or purpose of the members of the assertive class is to commit the speaker (in varying degrees) to something being the case, to the truth of the expressed proposition.” (Searle 1979, 12). The psychological state expressed by an assertive is Belief (that *p*). However, as Green (2013, 403) clearly explains,

this is a graded commitment, as if there were a cline of assertiveness, with some members showing more assertoric commitment—association of belief, truth and knowledge—than others.

The present study purports to answer the following research question: if, as it is, assertive shell nouns are used to characterize utterances as any type of assertive speech act, i.e. to refer to acts of asserting, guessing, conjecturing, etc., and if the construal of such nouns corresponds to the components of the illocutionary force of an assertive speech act, then this must be embodied in the noun behavioral profile, i.e. in the complementation patterns the nouns occur with—“the compatibility of certain kinds of nominals with certain kinds of containers” (Vendler 1967, 127)—, as this emerges in their occurrence in reporting or denoting and thereby characterizing speakers’ utterance acts in a specific discourse situation as acts of *F*-ing.

Results show that (i) constructional possibilities are part of the semantic meaning of the noun, and (ii) there is a correlation between semantic similarity and distributional similarity.

1. Theoretical Framework

The theoretical framework underlying this research consists of a combination of selected insights from cognitive linguistics, especially the prototype-based view of semantics. The empirical-conceptual approach (Verschuereen 1985, 1987) supplements such insights.

In cognitively inspired approaches to language, meaning comprises both content and construal, i.e. the conceptualization of experience (Croft and Cruse 2004). Conceptualization can be studied indirectly via language, due to the close relationship between linguistic and conceptual structure. Indeed, construal manifests itself at the level of grammatical and lexical items alike. The consequence is that structure and experience are related: syntactic structure reflects semantic structure and the semantic structure corresponding to a syntactic construction represents a conceptualization of experience. Both grammatical constructions and lexical elements are meaningful units, the only difference between them residing in the higher level of specificity of lexical items compared to the more schematic character of grammatical units. Therefore, the meaning of lexical items and that of grammatical units need to be compatible in order to be integrated and yield felicitous syntagmatic combinations. Syntactic behavior can thus have cue validity in the analysis of meaning categorization.

Categorization, i.e. the ability to create classes to classify experience, is one of the fundamental qualities of human cognition (see, for example, Langacker 1991; Taylor 2003). As is well known, Rosch (1973 and later work) introduced a prototype approach to categorization. In Prototype Theory entities are categorized on the basis of their attributes. However, these attributes are not the binary constructs of the classical Aristotelian view: “In categorizing an entity, it is not a question of ascertaining whether the entity possesses this attribute or not, but how closely the dimensions of the entity approximate to the optimum value.” (Taylor 2003, 44). Categorization is thus graded in nature, and prototypes serve as cognitive reference points for the categorization of not-so-clear instances. A prototype is generally taken to be a generalization or abstraction of some general tendencies. It has to meet specific criteria. For example, a prototype (i) maximizes the number of attributes shared by members of the category; (ii) minimizes the number of attributes shared with members of other categories (Taylor 2003); (iii) is used to define the other terms in the lexical domain through explicitation (Faber and Mairal Usón 1999), i.e. its definition is included in the definition of the other members of the lexical domain.

There is a level of categorization that is linguistically and cognitively more salient than others. This is the basic level of categorization. The notion of a basic level is closely related to the prototype structure of categories. Indeed, there is an interplay between the two: “The basic level has to do with what things are called. [...] Prototypes have to do with what words refer to.” (Taylor 2000, 53). The basic level, thus, captures the onomasiological salience of a term, and the prototype its semasiological salience.

The approach that complements the insights briefly presented above is the empirical-conceptual approach to verbal communication.⁵ This is based on the idea that there is a link between cognition and language use, a unity between concepts and practices: social actors conceptualize their own practices and, therefore, the character of those practices must be partly determined by how they are perceived. In this sense, action is always interpreted action. Indeed, as Verschueren (1985, 20) puts it, if there is a relationship between words and concepts, it is possible to learn something about the conceptual space associated with certain practices by examining the words and expressions that participants in the action have at their disposal to talk about those practices. These observable data are key to making sense of the semantic dimensions along which illocutionary shell nouns vary, and to their subsequent categorization.

2. Methodology

The list of assertive nouns used for this study includes the following types: *affirmation*, *allegation*, *argument*, *assertion*, *claim*, *conjecture*, *contention*, *guess*, *hint*, *presumption*, *statement*, *suggestion*, *supposition*. The nouns under scrutiny belong to a wider corpus of assertive nouns comprising 198 nouns, developed by the author. The complete list of illocutionary nouns belonging to the corpus takes speech act verbs *qua* illocutionary verbs as a starting point, and develops on the consultation/comparison of previously published works on speech act verbs (Austin [1962] 1975; Bach and Harnish 1979; Verschueren 1980; Leech 1983; Searle and Vanderveken 1985; Wierzbicka 1987). For those illocutionary nouns that may not have been included because they are not deverbal nouns, all the synonyms found in Word-Net synsets⁶ were added. The nouns selected for the pilot study designate the speech acts discussed by Green (2013) as exemplary members of the assertive family. The speech acts that do not appear in Green (2013), i.e. *affirmation*, *allegation*, *argumentation*, *claim*, *hint*, *suggestion* and *statement*, have been included to provide more data for the analysis, and have been selected because they all appear in the synsets of the other nouns.

For each noun in the list, two hundred randomly sampled tokens were extracted from the COCA and analyzed, yielding a total of 2,600 examples. Given that some nouns were sometimes indeterminate with respect to the question of whether or not they involved verbal communication, co-textual and contextual clues were used to filter out non-illocutionary uses. For example, in (5) there is an indication of direct quotation.

- (5) [...] the salesman jokes, “You guys must be reactionaries or something.” hardly realizing the irony of his **supposition**. [ACAD 2000]

And, in example (6), although there is no direct quotation, it is likely that Fry’s criticism was put into words at some time, but the occurrence of a linguistic action cannot be guaranteed. This is fairly common with nouns such as *conjecture*, *guess*, *presumption* and *supposition*.

- (6) Freud’s written views on art angered and disturbed a good many people, among them, famously, the distinguished British art critic Roger Fry. His **supposition** was that Freud did not understand the basic elements of esthetic pleasure. [MAG 1990]

When it was not possible to filter out non-illocutionary uses in a clear-cut way, it was decided to follow Vanparys’ (1996) rationale in including indeterminate cases in the data for analysis, because it is still useful to consider what they would mean if they were used as illocutionary nouns.

The procedure followed in the research involves two steps: core analysis, and additional analysis. The core analysis was carried out with the aim of checking whether the nouns were used in their shell noun function, and, if so, what their behavioral profile was.

I then added an additional analysis to this core analysis, namely the analysis of the type of determiners—markers of reference, definiteness, and deixis—that precede the noun. This analysis was intended to check to what degree the source of the utterance-act is backgrounded, defocalized or even deleted, thus providing additional information about the vantage point. The codification of the additional analysis was done using the following notation representing a scale from personal to impersonal, subjective to objective:

Type 1	1 st PERS. POSS.	My guess
Type 2	2 nd and 3 rd PERS. POSS, GENITIVE	Your assertion, his/her/their affirmation, John’s suggestion...
Type 3	<i>a, the, this, that, those, these, any, some, every, each, other, another, such a/an...</i>	The claim, any supposition...
Type 4	Ø	That is Ø conjecture

Table 6-1. Codification of determiners

The methodology used for data analysis involves descriptive as well as exploratory statistics. For descriptive statistics, reliance scores⁷ were calculated, and a chi-square test added. Reliance is a syntagmatic measure that accounts for the combinations of nouns with types of patterns. Reliance scores refer to the relative frequency of tokens of a noun type in a construction vis-à-vis tokens of the same noun in other constructions, and thus capture the degree to which a particular noun relies, or depends, on a pattern for its occurrence. A chi-square test is used to check whether the differences between the constructional patterns the nouns rely on for their occurrence are significant,⁸ and thus whether there is a significant difference in their syntactic behavior.

As for exploratory statistics, a hierarchical cluster analysis was applied to the data to complement the descriptive part. This technique organizes large sets of data into clusters or groups such that the members of one

group are very similar to each other and at the same time very dissimilar from members of other groups. In this way it provides a visually intuitive representation of the data that emerge from descriptive statistics. The results are organized in a dendrogram, i.e. a tree diagram that illustrates the arrangement of the clusters produced by hierarchical clustering. Since the choice of distance measure and the amalgamation/linkage algorithm—the two basic metrics on which clusters are based—may influence the clustering results, two analyses were carried out using two combinations of distance measure and the amalgamation algorithm, viz., a Euclidean and a Manhattan or City-block cluster analysis. It must be emphasized that, even if cluster analyses allow for an objective identification of groups, subjective decisions must nonetheless be made to decide what exactly the dendrogram looks like, and what it is that the dendrogram reflects.

3. Analysis

3.1. Descriptive statistics

3.1.1. Analysis of determiners

Table 6-2 shows the results of the analysis carried out on the type of determiners that precede the noun.

Noun	Type 1	Type 2	Type 3	Type 4
affirmation	0.83%	16.53%	77.69%	4.96%
allegation	0.71%	9.22%	87.94%	2.13%
argument	0.78%	15.63%	82.03%	1.56%
assertion	2.42%	44.35%	53.23%	0.00%
claim	1.00%	36.00%	60.00%	3.00%
conjecture	5.17%	13.79%	59.48%	21.55%
contention	6.82%	44.32%	48.86%	0.00%
guess	61.73%	14.81%	23.46%	0.00%
hint	0.00%	4.08%	73.47%	22.45%
presumption	1.06%	5.32%	93.62%	0.00%
statement	1.92%	21.15%	76.92%	0.00%
suggestion	6.32%	15.79%	77.89%	0.00%
supposition	5.23%	6.98%	85.47%	2.33%

Table 6-2. Determiner analysis

In most cases, Type 3 is used, with a peak of 93.62% of occurrences with *presumption*. This suggests that, in general, the speaker who reports an utterance using one of the assertive shell nouns in the list with this type tends to present the propositional content in a depersonalized, objective way. The deletion of the utterance source facilitates the presentation of personal beliefs as factual information. Thus, in example (7), the impression is that many people share the claim.

- (7) Spurlock wanted to taste the **claim** that eating fast food is making America too fat. [NEWS 2004]

Type 2 occurs especially with *assertion* and *contention*, accounting in both nouns for about half of the data.

- (8) As egregious was **his assertion** that the town of Hebron is essentially an Arab town. [ACAD 2008]

Type 1 is rare. It never reaches 10% of the occurrences, with the notable exception of *guess* (61.73%) for which this represents the most frequent type found in the data. In the case of Type 1 usage, the situation is reversed compared to Type 3, in that here the deictic origin of the utterance overlaps with the speaker in the current discourse situation, and the propositional content is presented as something that is subjective.

- (9) How do you fall without gravity? **My guess** is that they seemed to be moving around pretty normally. [FIC 2007]

Lastly, Type 4 is not common at all, but it occurs rather frequently with *conjecture* and *hint*.

- (10) I intimated at the top of the show that maybe this underscores that al Qaeda is back, and bigger than ever before. That they are regaining strength. Is that **conjecture** on my part? What do we make of what happened in Amman? [SPOK 2006]

Table 6-3 reports the summary of the noun behavioral profile. Of all the constructions the nouns in the list occur with, Det-N, N-that, N-BE that, and Pro-BE-N represent 81.3% of all the occurrences of these nouns as shell nouns, and, therefore, reliance scores concerning these constructions will be discussed in detail.

Noun	Det-N	N-that	N-zero that	N-BE-that	N-BE-zero that	Pro-BE-N	Existential C	Wh clause-BE-that	N-wh clause	Pro-BE-(det) N	others	all shell noun use	Tot.
affirmation	38%	40%	2%	2%	0%	0%	9%	0%	4%	2%	3%	47	100%
allegation	50%	25%	3%	6%	1%	5%	7%	1%	0%	0%	2%	140	100%
argument	38%	33%	5%	4%	2%	11%	2%	0%	2%	0%	3%	123	100%
assertion	31%	63%	2%	2%	0%	1%	0%	0%	0%	0%	1%	124	100%
claim	51%	31%	8%	2%	0%	2%	0%	1%	0%	0%	5%	100	100%
conjecture	33%	14%	2%	3%	3%	11%	6%	10%	2%	12%	4%	116	100%
contention	13%	74%	0%	10%	0%	2%	1%	0%	0%	0%	0%	87	100%
guess	1%	2%	0%	46%	27%	11%	0%	7%	0%	0%	6%	81	100%
hint	20%	22%	2%	0%	0%	0%	14%	0%	14%	0%	28%	49	100%
presumption	18%	47%	0%	14%	1%	2%	15%	0%	0%	0%	3%	94	100%
statement	54%	16%	6%	1%	2%	6%	0%	1%	2%	0%	12%	103	100%
suggestion	12%	54%	3%	3%	2%	5%	8%	1%	0%	0%	12%	95	100%
supposition	35%	38%	1%	8%	1%	11%	1%	0%	1%	1%	3%	170	100%

Table 6-3. Noun behavioral profile

3.1.2. Det-N

The pattern Det-N phrase is encountered across all types of shell nouns (Schmid 2000), not just those belonging to the illocutionary type. It signals anaphoric and cataphoric uses, and thus the cohesive function shell nouns perform in providing referential continuity within texts. Example (11) exemplifies this pattern.

- (11) As a result, young Igbo women commonly leave unfaithful lovers, and use the threat of doing so to curtail their boyfriends’ potential unfaithfulness. While I know of no quantitative data that can support the **claim**, it is my observation that men who were courting potential wives were more likely to be faithful. [ACAD 2010]

Figure 6-1 shows the relative frequency of the nouns under analysis with regard to this pattern. With the exception of *guess*, which relies significantly less than the other nouns on this construction, no important statistical differences emerge in the reliance on this pattern. Indeed, there is little statistical difference between *affirmation* and the neighboring noun *allegation*, and the same holds for *hint* and *assertion*. For the sake of the present argument, it is however important to notice that the corpus occurrences of *statement* and *claim* rely more than the other nouns on the pattern.

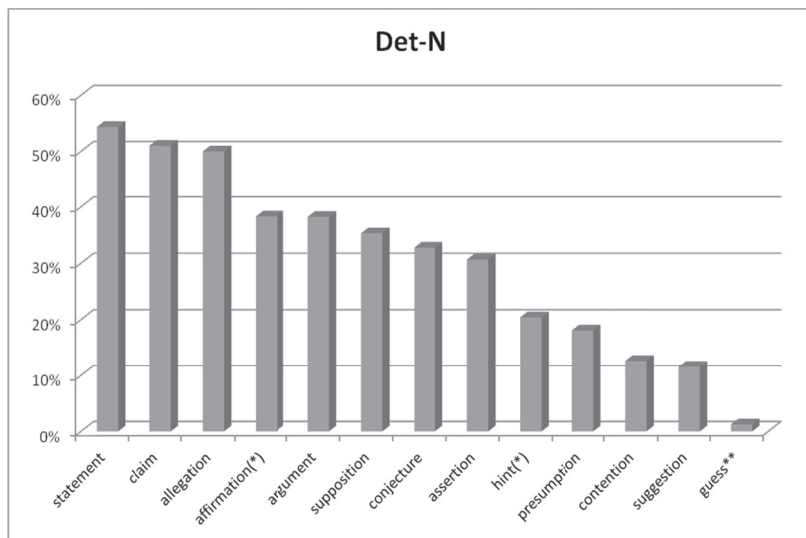


Fig. 6-1. Det-N pattern

3.1.3. N-that clause

Much more interesting from the conceptual point of view is the construction N-that reported in Figure 6-2, namely the pattern in which the assertive shell noun in the matrix clause is followed by a *that*-clause. Example (12) exemplifies this pattern.

- (12) Bald conjecture, as indicated by the word ‘perhaps,’ does little to lessen the central **allegation** here that natives on that memorable day more than two centuries ago exhibited deviousness. [ACAD 2007]

This construction seems to be the best predictor of cluster solutions, in that, whereas all other constructions, with the exception of Det-N, are unsaturated, i.e. they yield empty categories, N-that is always saturated in the corpus.

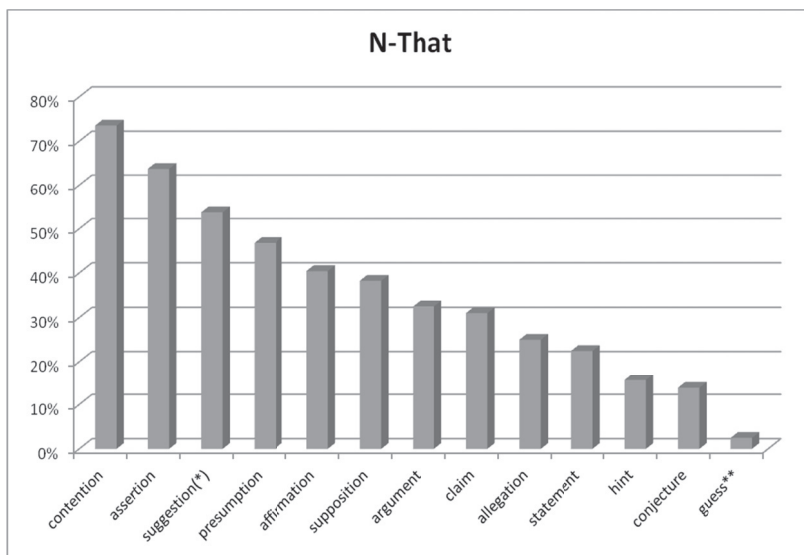


Fig. 6-2. N-that pattern

In the literature, studies on the complementation pattern *that*-clause are numerous and varied, and a comprehensive analysis of all of them goes beyond the scope of this paper. Therefore, I will give a very selective overview of some studies that may help us to understand this type of complementation.

The factual component of verbs that take *that*-clauses is underlined by Quirk et al. (1985, 1180), and supported by Wierzbicka (1988), which is still the most exhaustive study on English complementation. In her radically semantic approach, aimed at showing that the differences between various types of complementation can be explained in terms of meaning, Wierzbicka discusses different complementation patterns. In the case of *that*-clauses, she underlines the association of this type of complement with knowledge. More precisely, she says that, whereas, for example, *to*-infinitive clauses always imply the elements of ‘thinking,’ ‘wanting,’ ‘opinion’ and ‘future time,’ *that* complements are acceptable in those kinds of sentences where a component of the frame ‘know’ can be reconstructed. However, the type of knowledge she talks about is not ‘personal’ knowledge. She defines it as ‘public’ knowledge, i.e. something that is generally knowable, i.e. ‘one can know this,’ and this implies an objective, factual perspective on what is said: “THAT complements introduce an objective, impersonal, ‘one can know’ perspective.” (Wierzbicka 1988, 164). She adds that this would explain the use of *that*-complementation with assertive verbs such as *assume*, *presume*, *expect*, etc., which can be regarded as semantic derivatives of ‘know,’ that is as verbs which in their semantic structure refer to knowing (whether in the affirmative or in the negative). Indeed, Wierzbicka’s claims are not corpus-based, but are confirmed in Vanparys’ (1996) corpus-based study on English illocutionary verbs, in which the objective, informative aspect—contrasted with the binding aspect of *to*-infinitive, i.e. the commitment to a future course of action encoded by this structure—seems to be the main reason for the occurrence of assertive verbs with *that*-clauses.

Frajzyngier and Jespersen (1991) discuss the association between *that*-clauses and the *de dicto* domain, i.e. propositions that have a metalinguistic function, underlining the link between the construction and the notion of truth and actual states of affairs, in contrast to infinitival clauses that refer to potential, not actual states of affairs.

Langacker (1991) deals with complementation in English from the point of view of cognitive linguistics. For the purpose of this study, what Langacker adds to Wierzbicka is the observation that the complementizer *that* used in *that*-clauses imposes an atemporal construal on the clause it combines with and, in so doing, serves to objectify the proposition expressed. All the definitions given so far share the association of the construction *that*-clause with truth, knowledge and objectivity.

Reliance scores show that *contention* and *assertion* rank first with no statistical difference between the two nouns. Let us repeat for the sake of

argument that the characterization of assertive acts by Searle is that “The point or purpose of the members of the assertive class is to commit the speaker (in varying degrees) to something’s being the case, to the truth of the expressed proposition.” (Searle 1979, 12). Indeed, the psychological state expressed by an assertive is Belief (that *p*). Of course, the degree of belief or commitment may approach or even reach zero, as in the case of lies, but along the true-false dimension that characterizes assertives, *assertion* shows more commitment than the others in the group under analysis, and *guess* and *conjecture* the least commitment of all. As for the distribution of *that*-clauses with *contention*, one has to remember that the noun characterizes assertions as acts of defending and safeguarding one’s own position, in which the content is strongly asserted.

As for *suggestion* and *presumption*, one has to consider that philosophers have observed that asserting imposes a kind of responsibility on the speakers to produce an assertion or to rely on it for some further assertion. However, the normative requirement of justification does not generalize to every assertive speech act: for instance, the speaker is not committed to having good evidence when she suggests something. As Kissine (2008, 2010) states, canceling one’s commitment to having sufficient justifications does not prevent one from being committed to the truth of the communicated content. Indeed, the speaker may remain committed to *p* being true with respect to what she takes to be true at the utterance time only. Nouns such as *presumption* and *suggestion* characterize assertive speech acts in a way that makes it mutually manifest to speaker and hearer that the speaker does not know for sure whether the content *p* is true, but that it has to be taken as such for the sake of the argument, and revision and reassessment may follow closely.

Walton (1993) basically says the same things when he explains the difference between *assertion*, *presumption* and *supposition*. As a speech act, presumption is half way between assertion and supposition. Presumption essentially means that the proponent of the proposition in question does not have the burden of proof, only a burden requirement to disprove contrary evidence, should it arise in the future sequence of a dialogue. Thus, a presumption stays in place for a certain number of moves in a dialogue, but for neither party is it a permanent or non-retractable commitment that must stay in place for the duration of the dialogue. A presumption operates to give the argument some provisional basis for going ahead, even in the absence of firm premises known to be true.

- (13) Courts that review convictions have to start with a **presumption** that the verdict was correct. [NEWS 2011]

What mostly differentiates *presumption* and *suggestion* from *argument*, *supposition*, *allegation*, *affirmation*, *claim* and *statement*, i.e. the nouns that are under the same node in the cluster analysis, is the requirement of justification.

- (14) Animals cannot have beliefs and desires. This latter assertion is founded on the **argument** that in order to have beliefs and desires one must have language. [ACAD 1999]

Instead, the main difference between the sub-cluster *argument*, *supposition*, *affirmation*, and *allegation*, and the sub-cluster *claim* and *statement*, is that between more argumentative assertions and more informative assertions (Leech 1983), i.e. assertions that are being used with the aim of informing versus assertions that are being used with the aim of persuading the other discourse participants, of justifying, defending or arguing for a position in a conversation.

Hint is a suggestion or clue as to what the reality might be. It could be construed as an allusion in lieu of an explicit statement. Usually a hint is something that A says to B as a way of leading A to the truth, so it is a partial revelation.

- (15) She was speaking in response to Gopal’s **hint** earlier that only over time and through living together could people get to know each other. [FIC 1997]

Conjecture characterizes a speech act in which one weakly asserts that *p* while presupposing that one has at least some slight evidence for *p*, and, as a consequence, it is one of the nouns that relies less on the *that*-clause construction.

- (16) “Evidently you do not regard yourself as a member of the Yao gentry.” Helsse laughed. “More tactful might be the **conjecture** that I enjoy what I am doing.” [FIC 1993]

Lastly, *guess* is the noun that relies less than all the others on this construction for its occurrence, and the chi-square test shows that there is a statistical difference between the occurrence of *guess* with *that*-clauses and the occurrence of this construction with all the other nouns in the corpus. Indeed, if a conjecture is a very weak assertion, a guess can just be “an unfounded stab in the dark.” (Searle and Vanderveken 1985, 188).

- (17) With impeccable logic and barely concealed scorn, Hook noted that his “guess that either your name had been forged or that you had signed in carelessness” must have been an error. [ACAD 1996]

3.1.4. N-BE-that

Figure 6-3 shows the results for the construction N-BE-that. Example (18) exemplifies this construction.

- (18) It was all amicable, as far as I could tell. My **guess** is that Uncle Karl just didn’t want to work as hard as Dad. [FIC 1993]

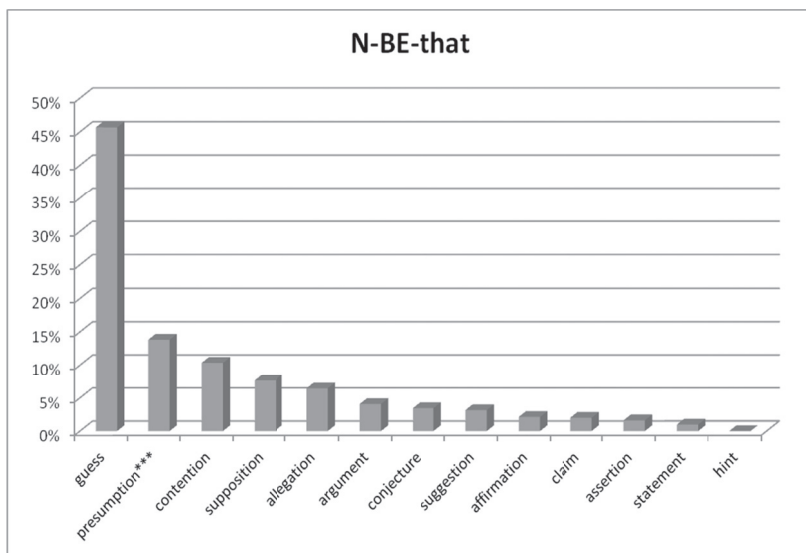


Fig. 6-3. N-BE-that pattern

In the case of N-BE-that, the distribution is somehow reversed. *Guess* is the only noun that relies significantly on this construction. And, in the majority of cases, as the analysis of the determiners points out, *guess* tokens are those in which one does not have the default situation of a speaker who, in an actual discourse situation characterizes a fragment of speech of a speaker in a resource situation as an act of *F-ing*. Instead, one has identity of these two roles. The second noun in the list is *presumption* but, as can be seen from the chi-square analysis, the difference in the reliance on this construction between the two nouns is highly significant,

which means that that construction occurs with *presumption* but the noun is not attracted to it. The same holds true for all the other nouns in the corpus, and *hint* shows no occurrences with this construction.

Caffi and Janney (1994) include the N-BE-that construction among their so-called *evidentiality devices* which include all choices that regulate the truth value of what is expressed. The function of these devices is to reduce the commitment to a proposition, in the case of the constructions under analysis, to the proposition encoded in the *that*-clause. They are common with weak assertives, i.e. those speech acts that have low assertoric commitment, and tend to occur with first-person determiners, most frequently the possessive *my*. They are basically used to underline the subjectivity/tentativeness of propositions.

Moreover, the construction also has a focusing function (cf. Schmid 2001) in that it directs the attention to the information given in the *that*-clause in which one finds the peak of prominence. This has the consequence that the noun in topical position is presented as old information, as something that is shared knowledge. As a result, as Schmid (2001) shows, the presuppositions triggered suggest beliefs and expectations that may lie outside the domains of knowledge and truth.

3.1.5. Pro-BE-N

Figure 6-4 reports the results of compiled reliance for the pattern Pro-BE-N, exemplified in example (19).

- (19) Those factors may be combining to create this highly volatile environment for discovering prices, he said. But for now, that is pure **conjecture** on my part. [NEWS 2008]

As Schmid (2000, 309) rightly points out, this is the pattern with the most conspicuous characterizing potential. It highlights the characterization operated by the noun. So it makes sense that one finds it with *conjecture*, which is an attitudinal noun. *Assertion*, which is neutral as far as the attitudinal component is concerned, is at the opposite end of the ranking.

Uses of this type are clear examples of unmarked distribution of topic and focus. In example (19) the noun *conjecture* makes up the focus of the clause that contains the new information. On the other hand, the leftmost noun phrase of the clause—the pro-form functioning as subject—must be regarded as referring to activated information. This means that the antecedent shell content is represented in short term memory, but does not constitute the current center of attention (Gundel et al. 1993, 278).

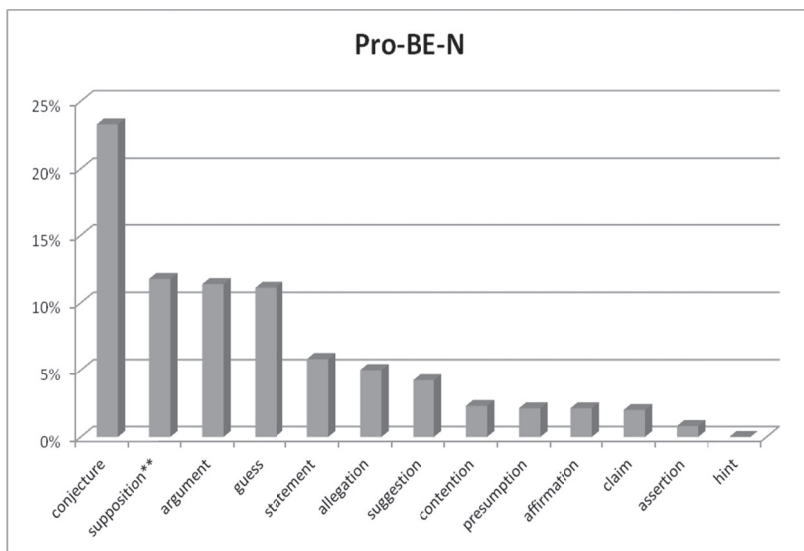


Fig. 6-4. Pro-BE-N pattern

In quite a significant number of cases, in this type of construction there is an AdjP as a premodifier that reinforces the axiological aspect already encoded by the noun. So, for example, in the case of *allegation*, what the AdjPs found in this pattern share from the semantic point of view would seem to be a component of [ANOMALY] accompanied by a negative connotation, as can be seen in adjectives such as *damning*, *shocking*, *noxious*, *phony*, *outlandish*, *ugly*, *false*, *wrong*, *extraordinary*, *cheating* and similar. They all seem to put extra focus on the information. The occurrence of this type of premodifying AdjP, i.e. AdjP containing a head adjective with such semantic traits, might be also due to the fact that corpus data on *allegation* most often belong to the spoken register in COCA.

- (20) Five weeks before the Deepwater Horizon exploded on April 20th, BP and Halliburton knew something was potentially very wrong. That's the damning **allegation** from government investigators, who outline a pattern of failed safety tests and non-communication. [SPOK 2010]

3.2. Cluster analysis

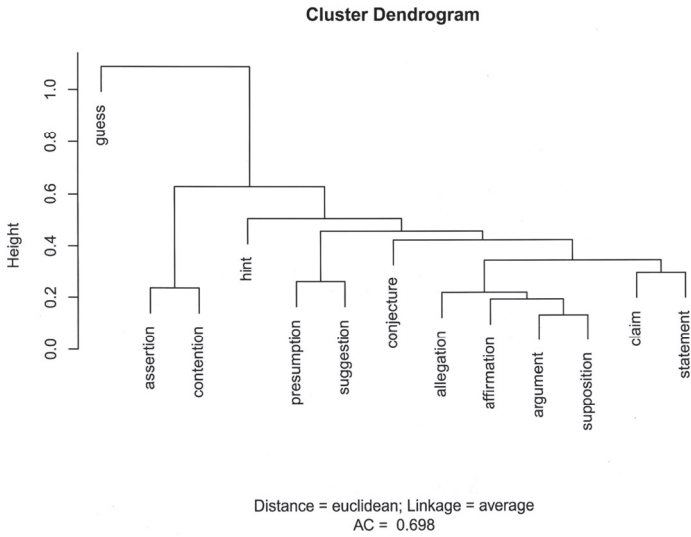


Fig. 6-5. Euclidean cluster analysis

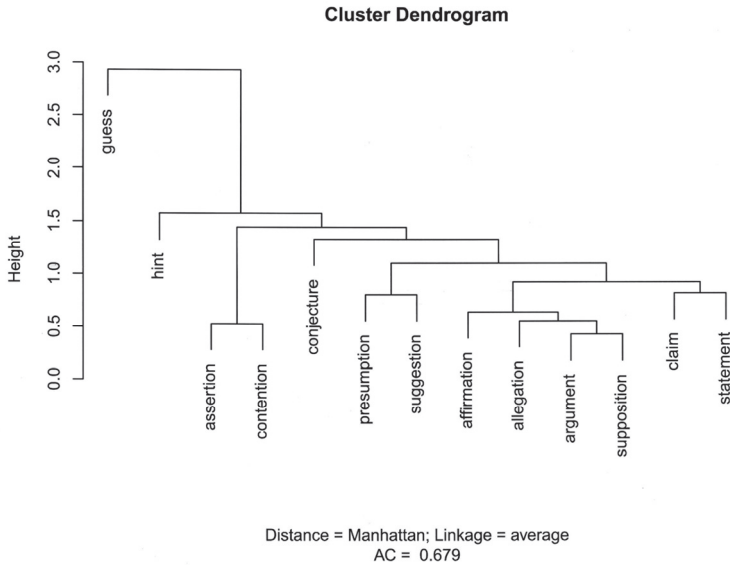


Fig. 6-6. Manhattan cluster analysis

Figure 6-5 and 6-6 show the results of the cluster analysis using the Euclidean and Manhattan distance measure. As can be seen, the only difference lies in the way in which *hint* and *conjecture* are clustered, but no difference can be seen as far as all the other nouns in the group are concerned. Therefore, the Manhattan distance being more precise,⁹ the dendrogram that results from this metric will be discussed. As is well known, there is no single variable that, on its own, succeeds in assigning nouns to a cluster. Though other positions might be defensible, I will argue that three main blocks emerge from the cluster analysis.

First of all, the big cluster of [*argument, supposition*] clusters together first, then it clusters with [*allegation*] and later with [*affirmation*]. This sub-cluster then amalgamates with the sub-cluster [*claim, statement*]. The two sub-clusters then cluster with the sub-cluster [*presumption, suggestion*]. In cluster analytical terms, this means that [*argument, supposition, allegation, affirmation, claim, statement*] present similarities within the group and dissimilarities with [*presumption, suggestion*]. Moreover, they all present similarities in contrast with [*conjecture*], which is the last to amalgamate. The vertical lines that link all the nodes in this rather big cluster are quite short, whereas the length of the vertical line that links it with [*assertion, contention*] is rather long. [*assertion, contention*] amalgamate early and so are very similar, but the height at which they cluster with [*argument, supposition, allegation, affirmation, claim, statement, presumption, suggestion, conjecture*] means that this cluster is very independent from the other large one. At a very short distance [*hint*] links to all the nouns clustered so far, which suggests that they present similarities within the group and dissimilarities with [*hint*]. Lastly, [*guess*] is added at a considerable distance from all the nouns in the group, as shown by the length of the vertical line. These final clustering steps suggest that *hint* and *guess* are the nouns that present the greatest dissimilarities with the rest of the group. This is especially true for *guess*, which is the last to amalgamate. In summary, the distribution of the noun clusters consists of one big group represented by [*argument, supposition, allegation, affirmation, claim, statement, presumption, suggestion, conjecture*], the cluster [*assertion, contention*], and the two outliers [*hint*] and [*guess*].

4. Discussion

In this section I discuss the variations the group of assertive nouns under study encode in the characterization of an utterance as having a specific *F*-ing, and I then motivate the structure of the category of nouns

that conceptualize the speech act of asserting in English as revealed by the analysis. The graphic representations are reported in Figure 6-7.

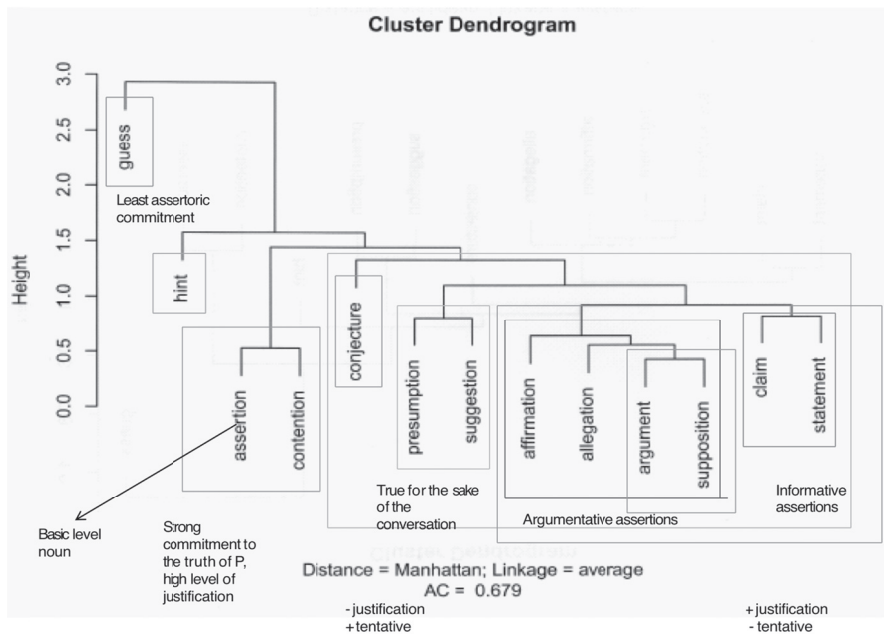


Fig. 6-7. Representation of assertive shell nouns

If one starts with the two sub-clusters of the rightmost node, [*claim*, *statement*] and [[[*argument*, *supposition*] *allegation*] *affirmation*]] can be characterized on the cline \pm informative— \pm argumentative (Leech 1983). *Claim* and *statement* are more neutral in their denotation, as well as being the ones that occur most with Det-N, though they occur a lot with N-that as well. *Argument*, *supposition*, *allegation*, and *affirmation* are less neutral. Indeed, in the case of *allegation*, the axiological parameter is especially relevant. *Claim* and *statement* are also the nouns in which occurrence in N-BE-that and Pro-BE-that constructions is less frequent. It is true that *claim* has to do with ‘taking a stand,’ but not as much as *affirmation*. Affirming is usually opposed to denying and *affirmation*, like *argument*, conceptualizes a defensive speech act. In the case of *allegation*, the speech act conceptualized does not carry the burden of proof that weighs upon the other nouns in this group (in *supposition* the content is

presented as true for the purposes of the argument). However, even if it is weaker than the others as far as the assertoric commitment is concerned, the utterance-act it conceptualizes nonetheless has some truth, and the speaker is ready to prove it and, if proven, which it is not the case at the moment of speaking, there may be consequences.

Moving to the cluster [*presumption, suggestion*], the level of tentativeness increases, and these nouns are instead characterized by the dimension ‘not sufficient justification,’ at the time of the utterance. Indeed, it seems that the main feature that distinguishes the nouns belonging to these two groups is the doubt or trust in the truth of the assertion. Following Leech (1983), the main difference is that between tentative versus confident assertions.

[*conjecture*] is the most tentative of the nouns belonging to this block. It has, like *allegation*, an axiological component. *Conjecture* is also the most complex noun in terms of behavioral profile. The syntactic structures it occurs with are numerous and varied.

[*assertion, contention*] show the highest commitment to the truth of the proposition. *Assertion* occurs only in the most frequent syntactic structures that are present in the behavioral profile of all the nouns in the corpus, whereas *contention* also occurs in the existential construction. *Contention* is also rare as a shell noun. It counts only 87 occurrences of shell noun usage out of the 200 examples extracted from the corpus for each noun.

[*hint*] is rather dissimilar from all the other nouns. It could be construed as an allusion in lieu of an explicit statement. It is the only noun in which there is a complete backgrounding of the speaker, as shown by the fact that it never occurs with the first type of determiners, but has a high percentage of use of Type 4. It counts only 49 tokens of shell noun usage in the corpus and half of them rely on Det-N for their occurrence.

Lastly, [*guess*] shows the least assertoric commitment—association of belief, truth and knowledge. It ranks last in the N-that complementation pattern and first in the N-BE-that. The subjective and tentative component associated with the noun is clearly expressed in the predominance of Type 1 determiners.

Of the nouns belonging to the corpus under study, *assertion* qualifies as the most prototypical assertive noun, namely as the best example of the category that includes all the nouns under study. As already stated, a prototype (i) maximizes the number of attributes shared by members of the category; (ii) minimizes the number of attributes shared with members of other categories; (iii) is used to define the other terms in the lexical domain through explicitation, i.e. its definition is included in the definition of the other members of the lexical domain.

As for (i), *assertion* occurs only in the most recurrent constructions found for the assertive nouns under study—as shown in Table 6-4,¹⁰ and shows the second highest reliance score with the pattern N-that which is the one that characterizes this type of nouns.

Construction	%
Det-N	19%
N-BE-that	1.0%
N-that	39.5%
N-zero that	2.0%
Pro-BE-N	0.5%
x	38.0%
Total	100%

Table 6-4. Behavioral profile of *assertion*

It is not possible in this article to look at criterion (ii) in detail. However, if one considers that in Schmid (2000), what distinguishes, among the others, commissive and directive nouns from assertive nouns is the generalized use of *to*-infinitive with commissive and directive nouns, this construction never shows up with *assertion*, whereas it does occur in the corpus with *argument*, *claim*, *presumption* and *suggestion*. Moreover, the fact that all the other nouns show a behavioral profile with a syntactic complexity that is higher than that of *assertion* provides an additional argument to the centrality of *assertion*.

Lastly, it is true that *assertion* is used in the definition of all the other nouns belonging to the corpus. *Assertion* is also the noun linked to the verb ‘assert,’ which is the primitive assertive, and which names the illocutionary force of assertions (Searle and Vanderveken 1985; Vanderveken 1990).

Conclusion

This paper has reported a fine-grained study of a group of illocutionary nouns belonging to the group of assertive nouns, in their function as shell nouns. The behavioral profiles of the nouns have been investigated using descriptive and exploratory statistics. From the descriptive point of view results show that (i) the complementation patterns found for the nouns under investigation are more highly articulated than the patterns found in Schmid (2000); (ii) the constructions in which they occur are linked to the

semantics of the noun, and (iii) there is a correlation between semantic similarity and distributional similarity.

From the theoretical point of view—though, obviously, further data are necessary—the results emerging from the study seem to lend support to the view that complementation and complement selection are semantic (see Givón 1990; Dik 1997), thus highlighting the need for a more fine-grained analysis of the onomasiological organization of the lexicon to explain the combinatorial properties on the syntagmatic axis.

At the same time, the results on linguistic patterns lend support to the statements made in more philosophical frameworks about the commitment to belief, truth, and knowledge that defines assertive speech acts, thus showing the potential for descriptive English research to be applied across disciplinary boundaries.

CHAPTER SEVEN

INFERENCE PROCESSES EXPRESSED
BY LANGUAGES:
DEDUCTION OF A PROBABLE
CONSEQUENT VS. ABDUCTION^{*}

JEAN-PIERRE DESCLES
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1. Three inference processes

Many authors, in particular in studies of evidentiality, call upon the notion of inference, without however specifying the type of inference used. And yet all inference processes are far from being identical. Peirce (1966)¹ distinguishes three inference processes: Deduction, Induction, and Abduction.

Modus ponens is the prototypical rule of deduction:

$$\begin{array}{l} p \\ p \Rightarrow q \\ \hline q \end{array}$$

Induction is based on correlations of observed cases; through abstraction, it is generalized by means of a conditional relation between facts (thereby establishing a law of inference);

Abduction (or retroduction) is based on facts (observed or known) and the law of inference (conditional relation or material implication) across propositions; it states the plausibility of a hypothesis:

q
 p => q

 p is plausible

Let us take a simple example to illustrate these three processes. *Deduction (D)* leads to a true statement (*This bird is black*) based on two premises declared true (All crows are black and This bird is a crow). *Induction (I)* generalizes through a law (*Tous les corbeaux sont noirs* ‘All crows are black’) based on a correlation between observed facts (*Les corbeaux qui ont été observés sont tous noirs* ‘The crows observed are all black’) from a sample considered representative and sufficiently large. *Abduction (A)*, also called ‘retroduction’ or ‘hypothesis’ by Peirce, leads to a plausible hypothesis (*Il est plausible que cet oiseau soit un corbeau* ‘It is plausible that this bird is a crow’) based on attested facts (*Cet oiseau qui a la taille d’un corbeau est noir* ‘This bird which is the size of a crow is black’) and common knowledge (*Il est bien connu que tous les corbeaux sont noirs* ‘It is well known that crows are black’ and *Les corbeaux sont nombreux dans cette région* ‘The crows are numerous in this region’).

2. Abductive reasoning

To better highlight the nature of abduction and its role as a plausible hypothesis in the linguistic expression of some types of reasoning (whether in daily life or in science), we will first specify some of its characteristics (Desclés 1996, 2000). We will also show that in some languages, grammatical units and constructions can signal a “certain disengagement on the speaker’s part.” Indeed, by using these units, the speaker simply states a plausible hypothesis and not an assertion (which would fully commit the speaker), or the probable conclusion of reasoning.

The mathematician Pólya ([1965] 1989, 105; [1958] 2008, 181) gives, as an example of abductive reasoning, the discovery of the first island in the New World, San Salvador. While Christopher Columbus and his crew were sailing west on an unknown ocean: the increasing presence of birds in the sky indicated the proximity of land. This clue, linked to the shared knowledge of the seamen (“close to land, one often sees birds”) encouraged the navigators to continue sailing in the same direction.

Let us take the example of the so-called conjunctural conditional in French:

- (1) *Il serait donc parti* “So, he must have left // He must have left, then.”

This sentence expresses a plausible hypothesis ‘H’ when the speaker has noted a clue ‘q’ (for example “his car is no longer in the parking lot”); the speaker can clarify, following a request or, if need be, by recalling the shared knowledge ‘p => q’ (“If he has left, then his car will not be in the parking lot”). Basing himself on observed facts, through abductive reasoning, the speaker formulates a hypothesis which he deems plausible.² Nothing prevents other hypotheses from being called into play to explain the facts observed.

The semantics of such statements implies some lack of commitment on the part of the speaker who puts forward a hypothesis while accepting in advance the possibility that it could be contradicted. This would not be the case with the assertive sentence *Il est parti* ‘He left.’ The dialog in example (2) compares the statement of a plausible hypothesis by an investigating judge (*Alors, il serait là*. “So, that’s where he must be”) to an assertion expressed by the judge in the present indicative (*Il est là* “He is there”):

- (2) – *Vous croyez... vous croyez... répétait M. Filleul.*
 – *Si je crois ! s’écria le jeune homme [Beautrelet]. Tenez, rien que ce petit fait: sous quelles initiales ces gens-là correspondent-ils entre eux ? A. L. N., c’est-à-dire la première lettre du nom d’Arsène, la première et la dernière du nom de Lupin.*
 – *Ah ! fit Ganimard, rien ne vous échappe. Vous êtes un rude type, et le vieux Ganimard met bas les armes.*
Beautrelet rougit de plaisir et serra la main que lui tendait l’inspecteur. Les trois hommes s’étaient rapprochés du balcon, et leur regard s’étendait sur le champ des ruines. M. Filleul murmura :
 – *Alors, il serait là.*
 – *Il est là, dit Beautrelet, d’une voix sourde.*

- [– You believe... you believe... repeated Mr. Filleul.
 – If I believe it! exclaimed the young man [Beautrelet]. Look, just this small fact: what initials did these people use to communicate with each other? A. L. N., that is, the first letter of the name Arsène, the first and last letters of the name Lupin.
 – Ah! said Ganimard, nothing escapes you. You’re a tough guy, and old Ganimard lays down his weapons.
 Beautrelet blushed with pleasure and shook the inspector’s extended hand. The three men had moved closer to the balcony, and they viewed the wasteland. Mr. Filleul murmured:
 – So, that’s where he would be.
 – He is there, said Beautrelet softly.]

(Adapted from M. Leblanc, *L’Aiguille creuse* as quoted by Provôt and Desclés 2012)

In *reasoning by abduction*, one must distinguish between statement of the hypothesis ‘H’ and the abductive inference itself. Abductive inference serves to argue the plausibility of ‘H’ since, on one hand, the fact ‘q’ was observed, and on the other, ‘H’ being explicitly linked to the fact ‘q,’ the latter becomes a clue of the plausibility of ‘H’. In general, the plausibility of a hypothesis ‘H’ is reinforced by noting a set of concordant clues ‘q₁,’ ‘q₂,’ ... ‘q_n’. The various stages in *abductive reasoning* are thus:

- 1) observation of a bundle of concordant—facts (or facts already admitted):
‘q₁’ & ‘q₂,’ ... & ‘q_n’;
- 2) formulation of a hypothesis ‘H’ to “explain” the facts;
- 3) reference to a relation (often a conditional relation between propositions) between the hypothesis and the observed facts, within a given theoretical framework or depending on “common knowledge”;
- 4) justification of the plausibility of the hypothesis ‘H’ by *abductive inference*:
 - (a) q₁ & q₂ ... & q_n
 - (b) H => q₁ & q₂ & ... & q_n
 -
 - (c) H is plausible
- 5) statement of the plausible hypothesis H.

Abductive reasoning often calls upon a succession of conditional relations between the hypothesis ‘H’ and its various direct and indirect consequents (within the theoretical framework); whence complexification of (b):

$$(b') [H \Rightarrow C_1 \& C_2 \& \dots C_p] \& [C_1 \& C_2 \& \dots C_p \Rightarrow q_1 \& q_2 \& \dots \& q_n]$$

3. Some illustrative examples of abduction in linguistics

Abduction is often misunderstood not only by epistemologists and philosophers of science, but also by linguists. One example is the critical review in Deutscher (2002) of an analysis by Andersen (1973) who explicitly refers to abduction in order to explain diachronic changes. It is often stated that abduction has no true specificity among modes of reasoning. It is then reduced to being included in the simple distinction deduction/induction. Indeed, it is obviously too simple to directly infer that “this black bird is a crow” from the simple argument “all crows are black.” On several occasions, as noted by Fann (1970) and Deutscher (2002), Peirce revisits the debate concerning the validity of abductive reasoning both in daily life and in science. For Peirce, abductive inference

is not logical reasoning, as the latter must always lead to “truth” based on “truth.” The hypothesis posited by abduction is not probable but merely plausible. Moreover, for it to be seriously entertained, it must generally be contextualized. In abduction, the most important act, as stressed by Peirce, is formulating a new hypothesis which is not always evident. Independently of Peirce, Pólya highlighted the fundamental role of abduction, which he calls “plausible reasoning,” in mathematical discovery. Yet despite the many examples analyzed, abduction is often reduced to a variant of induction, as in the case in Deutscher (2002, 476) “Inference to the best explanation.”

Let us recall a few past examples of abductive reasoning which led to major breakthroughs in linguistics (Desclés 1996, 2000). In his famous *Mémoire sur le système primitif des voyelles dans les langues indo-européennes*, presented in Leipzig in 1879, the young Saussure posited the hypothesis that “there is a given phoneme—in fact a glide—in Indo-European.” The plausibility of this hypothesis was supported by the laws of sound change formulated in Comparative Grammar. These laws made it possible to relate a hypothetical (non attested) phoneme to X facts observed in various Indo-European languages (Sanskrit, Greek, Latin, Germanic languages, etc.).³ It is well known that this phoneme was identified in Hittite 40 years later by Kuryłowicz, thus empirically proving Saussure’s hypothesis. The deciphering of the writing system of the Ancient Egyptian hieroglyphs is another remarkable example of abductive discovery. Champollion was able to justify his plausible hypothesis after a succession of more and more refined hypotheses (and the refutation of wrong hypotheses),⁴ and after having had access to new documents (the Huyot documents). Champollion decided to apply the writing system used for the names of Greek kings to the names of sovereigns of the High Egyptian Empire (a plausible hypothesis). By this abductive gesture of applying his hypothesis to new names, he discovered the triple writing system, at once phonetic, ideographic and symbolic, of Egyptian hieroglyphs.

From a more contemporary perspective, let us mention the abstract reference locator (noted ‘ $\underline{\epsilon}$ ’ and termed ‘underlined epsilon’) introduced by Culioli (1990, 72–80). It defines an abstract relation $[Y \underline{\epsilon} X]$ between a localized entity ‘Y’ and a more determined entity ‘X,’ with specific mathematical properties. This innovation was consequently used in numerous linguistic analyses.⁵ This abstract reference locator is a sort of language plausible hypothesis (Culioli and Desclés 1981, 1982; Desclés 1987). This hypothesis is strengthened by explicit relations which may be established between, on one hand, this abstract and general relation $[Y \underline{\epsilon}$

X], or its converse, and, on the other, the various meanings taken on by occurrences of the units ‘be’ and ‘have’ (or their equivalents in the Indo-European languages) and the various units expressing these meanings in other, non Indo-European, languages. In Chinese for example, there are three verbal units *shì*, *zài* and *yǒu* which are analogous to ‘be’ and ‘have,’ although their distribution does not match what is found in English.⁶ Let us take another example: the notion of ‘genotype language’ introduced by Shaumyan (1987) aimed to formulate and describe, using various types of operators applied to operands, the main voices (accusative, ergative, active/inactive, passive, middle, impersonal...) which are expressed and construed in a variety of ways in the different ‘phenotype languages’ (English, French, Russian, Bulgarian, Chinese, Malagasy...). The abstract genotype language, in contrast to the various phenotype languages which realise it, strives to formulate language invariants X as plausible general hypotheses, progressively implemented through an abductive process rather than an inductive one which would proceed through simple generalizations.⁷

4. Refutation of a plausible hypothesis

As stressed above (and Peirce particularly insists on this point), abductive reasoning leads to plausible hypotheses but not to probable conclusions. Using probability presupposes comparing the measure of a hypothesis’s probability to that of competing hypotheses. In contrast, stating that a hypothesis is plausible leaves the field open for competing explanations (often yet unknown). Inference through abduction simply makes it possible to justify the choice of a hypothesis based on concordant facts which render it increasingly plausible. When hypothesis ‘H’ is retained as a plausible “explanation” for the facts observed, this means that other hypotheses ‘H₁,’ ‘H₂,’ ... could also potentially be invoked to “explain,” at least as well, the very same facts as well as all other already observed facts which could be implied by ‘H’. The more concordant facts there are, the more a hypothesis’s plausibility is strengthened (Pólya 2008, 161–176). A plausible hypothesis ‘H’ is often presented by the speaker as the “best explanation” until discovery of other hypotheses deemed “better.” Reasoning through abduction presents a first hypothesis ‘H₁,’ which is then refined by formulating “better” hypotheses ‘H₂,’ ‘H₃,’ ..., or, in certain cases, rejected on the basis of conclusions drawn from observation.

Abductive reasoning can fail because of unacceptable conclusions as well as through a critical analysis linking the plausible hypothesis ‘H₁’ to

its consequences. A relation ‘ $H_2 \Rightarrow q$ ’ can be “more acceptable” than ‘ $H_1 \Rightarrow q$ ’, within a given theoretical framework either because it is considered more “economical” or because of “shared knowledge.” In current cognitive semantics research on verbal polysemy, it is often used in formulating an abstract invariant of meaning or, in the terms of Guillaume, a “signifié de puissance.” In this case, a formulation based on primitives already used in numerous semantic analyses and well grounded in cognitive perception and the domain of actions, is preferable to the introduction of new, sometimes ad hoc, primes to “explain” the same relations.⁸

5. Plausible hypotheses and probable conclusions

As mentioned above, induction leads to formulating universal laws through generalizations based on a significant sample of correlated facts. Thus, it has been noted that if the observed objects in a given field of study have property ‘Q,’ then they also have property ‘P,’

$$\forall i (i = 1, \dots, n) : [Q(a_i) \& P(a_i)]$$

hence, by generalization, the material implication:

$$[(\forall x) (Q(x) \Rightarrow P(x))].$$

This general law may take on a more flexible form if the field of the studied objects can be measured in terms of probability. In this case, the problem belongs to statistical theory. Observations show that objects having the property ‘Q’ very often also have the property ‘P’. In this case, one often formulates inductive laws in the form of material implications with a consequent which is merely probable:

$$[(\forall x) (Q(x) \Rightarrow \text{prob} (P(x)))]$$

i.e. if the property ‘Q’ is ascribed to an object from the study sample, the property ‘P’ is also ascribed to it, with a given level of probability. Of course, this works under the condition that one has succeeded in defining the probability space for the events in question.

We will illustrate this with the example of birds found in European cities. In some cities, one finds pigeons and crows. For one of these cities, a study of a large sample of birds has shown that: 1) pigeons are not black; 2) crows are black, with possible exceptions; 3) there are also black birds

that are not crows. Such statistical studies are tools for measuring probability based on bird observation, the high proportion of black crows as opposed to other black birds is noted; this result, grounded in inductive reasoning, can be used to bolster a statement such as:

- (3) *Un oiseau de couleur noire est très souvent un corbeau.*
 [‘A black bird is very often a crow.’]

This statement expresses knowledge “shared” by the inhabitants of the city in question. Let us now imagine a child who had picked up a black bird earlier and is now recounting the event: *I picked up a black bird*. One could respond: ‘*It’s probably a crow*. Is this answer the result of abductive inference? The answer is no. What we have here is not abductive reasoning but rather deductive reasoning providing a probable consequent (with a high probability qualitatively indicated by the linguistic marker *very often*):

- (a) *J’ai ramassé un oiseau de couleur noire.*
 [‘I picked up a black bird.’]
 (b) *Si c’est un oiseau de couleur noire, c’est très souvent un corbeau.*
 [‘If it is a black bird, it is very often a crow.’]

 (c) L’oiseau ramassé est probablement un corbeau.
 [‘The bird you picked up was probably a crow’.]

More broadly, this can be represented as in schema (1) showing deductive reasoning with a probable consequent.

Schema (1):

- | | |
|------------------------------------|----------------------------------|
| (a) Q(a) | [true proposition] |
| (b) [(∀x) (Q(x) => prob (P(x)))] | [law with a probable consequent] |
| ----- | |
| (c) prob (P(a)) | [probable consequent] |

The person speaking to the child about the black bird could also have stated a very strong prediction: *Ce sera (encore) un corbeau* [‘That will be a crow again’], thus indicating that verification would probably lead to establishing the fact that *C’est effectivement un corbeau* [‘Indeed it is a crow’], hence the use of the French Future in *Ce sera un corbeau* ‘*That will be a crow*.’ Further “shared knowledge,” based on inductive reasoning, could be expressed as:

- (4) *Les corbeaux qui sont en grand nombre, sont toujours de couleur noire.*
 [‘Crows, which are very numerous, are always black.’]

The dialog between the child who picked up a black bird and the adult (who has not seen the bird) could go as follows:

- (5) a. *J’ai ramassé un oiseau de couleur noire dans le jardin. Quel est cet oiseau?*
 [‘I picked up a black bird in the garden. What kind of bird?’]
 b. *Et bien, ça doit être un corbeau qui aura élu domicile dans le jardin.*
 [‘Well, it must be a crow who decided to live in the garden.’]
 c. *Pourquoi? Tous les oiseaux noirs ne sont pas des corbeaux.*
 [‘Why? Not all black birds are crows.’]
 d. *Oui, mais tu sais bien que les corbeaux sont toujours de couleur noire et qu’ils sont très nombreux dans notre région.*
 [‘Yes, but as you know, crows are always black and that there are lots of them around here.’]

The statement (5b) *Et bien, ça doit être un corbeau* [‘Well, it must be a crow’] is, in this case, the result of abductive reasoning; it expresses a plausible hypothesis, the justification of which is provided by the abductive inference below:

- (6) a’. *L’oiseau ramassé est de couleur noire;*
 [‘The bird picked up is black.’]
 b’. *Tous les corbeaux, nombreux dans cette ville, sont de couleur noire;*
 [‘All crows, of which there are many around here, are black.’]

 c’. *Il est plausible que l’oiseau ramassé soit un corbeau.*
 [‘The bird picked up is a crow is plausible.’]

This inference can be presented as in schema (2) showing abductive inference.

Schema (2):

- (a’) Q(a)
 (b’) [(∀x) (P(x) => Q(x))]

 (c’) P(a) is a plausible hypothesis

Response (5d) calls upon “shared knowledge” and is used to argue in favor of the plausibility of the reasoning expressed in (5b): the adult refuses to fully commit to the contents of his statement; he is only stating a hypothesis which he finds highly plausible, without excluding the possibility of other hypotheses. This would not be the case if he had used an assertive statement (*C’est un corbeau* ‘It’s a crow’), which would have excluded all other hypotheses.

The comparison of the two processes **(1)** (*deductive inference with a probable consequent*) and **(2)** (*abductive inference*) shows that they do not call upon the same “shared knowledge.” This can be shown by material implications. Although in both formulas the specific fact ‘Q(a)’ is the same, the material implication of schema **(1)** bears on the relation between the propositional forms ‘Q(x)’ and ‘P(x),’ with a merely probable *consequent*, hence the probable conclusion; whereas the material implication in **(2)** bears on the relation between ‘P(x)’ and ‘Q(x)’ and leads to a hypothesis which can only be plausible. There is an inversion of the material implication in (1) and (2). Let us further note that **(1)** shows logical inference: based on true premises, it leads to a true (probable) proposition.⁹ **(2)** does not entail logical inference; it leads to a hypothesis which is not necessarily true but only plausible.

To the inferential processes deduction **(D)**, induction **(I)** and abduction **(A)**, one must further add a fourth process for deduction with a probable *consequent* **(DP)**, which contrasts with **(A)** and **(D)**:

Process DP	Process A	Process D
q	q	p
q => prob (p)	p => q	p => q
-----	-----	-----
prob (p)	plausibility (p)	q

Process **(DP)** is not analogous to the inductive process **(I)**. One does however note that the proposition [q => prob(p)] of process **(DP)** is construed upon an induction which, rather than completely generalizing tends instead to introduce a probable *consequent*.¹⁰ The inference modes **(DP)** and **(A)** do not call upon the same knowledge shared by the speaker and co-speaker, but on different inferences; moreover, the first **(DP)** leads to a probable *consequent*, the second **(A)** states a plausible hypothesis which may be countered with another hypothesis.

The probability of the *consequent* in **(1)** is expressed linguistically by epistemic markers such as *probablement* ‘probably’ / *sans doute* ‘undoubtedly’.... The plausibility of the hypothesis in **(2)** is also linguistically marked by elements such as *ce serait donc* ‘that should be’ /

ça doit être alors ‘that must be’/ *ça devrait être* ‘that should be’... which rather indicate absence of assertion stemming from reasoning; such markers are often coupled with a marker tracing the reasoning (*donc* ‘therefore’ / *par conséquent* ‘in consequence’ / *alors* ‘then’...). In analyzing inferential linguistic markers, contextual and pragmatic conditions must be taken into account and the inferential processes used must be clearly stated. It is therefore necessary to indicate whether it is a case of inference through deduction as in (1) or inference through abduction as in (2); one must further specify the knowledge upon which the inference is based (common knowledge, shared knowledge, universal laws), and the theoretical framework applied.¹¹ Sentences which indirectly express reasoning with a probable *consequent* and those where abductive reasoning leads to a plausible hypothesis obviously do not have the same argumentative value in communication. We argue that the meanings of inferential linguistic units must be specified by the type of inference involved, without which it is impossible to establish relevant semantic conclusions for the analyzed linguistic constructions.

6. Enunciative variations on *Ce sera le facteur* [‘That will be the mailman’]

Although rarely used, a French sentence such as *Ce sera le facteur* [‘That will be the mailman’] is often analyzed and labeled the *conjectural* future (Wilmet 1979, 109), the future of *probability* (Brunot 1922, 531), of *probable hypotheses* (Imbs 1960, 54), of *explanation* (Wagner and Pinchon 1965, 349), the *epistemic* future (Dendale 1994, 33)... It is obvious that no one label, however apt, can account for the meaning of such an utterance. From the standpoint of a rigorous methodology applied to the semantics, any analysis must be able to answer the following question: “In what is the meaning identified under a given label different from the meanings ascribed to the enunciative variations around a single predicative relation?” The enunciative variations on a single predicative relation (in our example: <cela être le facteur> [that to be the mailman]) indeed make it possible to distinguish the various meanings and better characterize each one.

Let us take the family of enunciative variations in the pragmatic context of a dialog “the bell has just rung twice”:

- (7) *Ce sera le facteur.* [‘That will be the mailman’].
- (8) *C’est probablement le facteur.* [‘It’s probably the mailman’]
- (9) *C’est évidemment le facteur.* [‘It’s obviously the mailman’]

- (10) *C'est le facteur.* ['It's the mailman']
 (11) *Ce serait donc le facteur.* ['So it's the mailman']
 (12) *Ça doit être le facteur.* ['That must be the mailman']
 (13) *C'est peut-être le facteur.* ['Maybe it's the mailman']

In all of these utterances, the speaker calls upon pragmatic knowledge pertaining to what just happened. Sentence (7) is uttered following a deduction based both on the speaker's knowledge (material implication with a probable consequence: "When you hear the bell ring twice, it is *very often* the mailman") and on a fact ("We heard the bell ring twice"); the conclusion drawn is a highly probable proposition which can easily be verified or explained later (whence the use of the Future *sera* ['will be']).¹² Sentence (8), with the marker *probably*, is very similar to (7) because an analogous deductive mechanism is called upon; the conclusion is probable, as in (7), but (8) no longer indicates the idea of later verification or explanation. As for sentence (9), the speaker calls upon more constrained knowledge ('When the bell rings twice, it's *always* the mailman') with a conclusion that has become evident for him (marked by *obviously*). Sentence (10) is an assertive declaration uttered in additional pragmatic conditions such as "the speaker saw the mailman ring" or "the speaker knows that the mailman always comes at this hour"; in this case, the speaker assumes entire responsibility for the utterance.

Sentence (11), with the present conditional *serait*, is not the result of a deduction with a probable outcome—as in (7) and (8)—nor of a conviction leading to an obvious conclusion as in (9). In sentence (11), the speaker tables a hypothesis deemed plausible, while signaling that the plausibility is based on clues: the hypothesis is the result of abductive reasoning. If the hearer so requests, the abductive reasoning can be further specified: "the bell rang twice" is an established fact which becomes a clue in favor of the plausible hypothesis "it is the mailman." The plausibility rests on the speaker's knowledge ("When the mailman comes, he always rings twice"). When the knowledge is shared, the co-enunciator reconstitutes the speaker's abductive reasoning. The use of the conditional *serait* in (11) also signals that there are other possible plausible hypotheses. Sentence (12) is to be analyzed similarly, but the degree of plausibility is lower than in (11). In contrast to (11) and (12), sentence (13) shows no trace of reasoning; it is the statement of an epistemic judgment which displays a lack of certainty and denotes a situation which is merely possible. This analysis of the French examples (7) – (13) demonstrates that a theory must make it possible to distinguish between the various types of utterances expressing:

- (i) commitment (or engagement) of the speaker when producing an assertive declarative sentence—example (10);
- (ii) a certain speaker disengagement when producing a plausible hypothesis—example (11)—further signaling that contextual indications, based on abductive inference, underlie the hypothesis;
- (iii) utterance of a more or less probable conclusion, based on a deduction establishing a link between a hypothesis and its probable consequence—examples (7), (8) and (9);
- (iv) the epistemic formulation of a possible situation, both uncertain and not impossible—example (13).

The sentences denoting conceptual distinctions (ii), (iii) and (iv), are not always easy to tease apart without a context. They are all distinct from assertion (i) which presents an observed fact (based e.g. on perception) or an already well established fact. More precisely, they express the speaker's refusal to commit to the truth of the proposition. In other words, they show a certain disengagement in relation to the utterance of a plausible hypothesis in the case of (ii), measured commitment oriented towards a more or less probable conclusion in the case of (iii), and non commitment in the case of (iv). In sentence (iii) the inference of a probable conclusion highlights both its kinship to sentence (ii) (statement of a plausible, and therefore uncertain, hypothesis) and its fundamental difference, since the inference schemas are not identical and do not call upon the same shared knowledge.

To reinforce the necessity of specifying inferential pathways in analyses, let us take two examples which at first glance seem quite similar. Depending on whether or not one is aware that a shipwreck has taken place, the presence of a body washed up on the beach [observed fact], leads to the production of utterances such as (14) and (15), with quite different meanings, however:

- (14) *Ça doit être l'un des naufragés* [that must be one of the shipwreck victims].
- (15) *Ainsi, il y aurait eu un naufrage cette nuit* [so there must have been a shipwreck last night].

(14) is the conclusion of deductive inference with a probable outcome which calls upon shared knowledge: 'there was a shipwreck last night' and 'when there is a shipwreck, bodies very often wash up on the beach.' The sentence expresses a probable conclusion based upon the following reasoning: 1) the speaker notes the presence of a body on the beach:

STATE (q); 2) shared knowledge: ‘there has been a shipwreck’ and ‘(when there is a shipwreck), a body discovered on the beach is very often that of one of the victims of the shipwreck.’ This leads to the material implication: STATE(q) => probable (EVENT(p)) and then, on the basis of a *modus ponens* type deduction, to the conclusion: 3) ‘this body is probably that of one of the shipwrecks,’ i.e.: probable (EVENT(p)).

The difference between probability and plausibility explains the difference between (14) and (15). Indeed, (15) can only be understood as the expression of a plausible hypothesis based on inferential reasoning through abduction in a context where it is not known that a shipwreck has taken place. The abductive inference is analyzed as follows: 1) the speaker has noted the presence of a body on the beach: ‘STATE(q)’; 2) shared knowledge: everyone knows that when there is a shipwreck, there are always bodies that wash up on the beach, whence the following material implication: ‘EVENT(p) => STATE(q)’; in consequence, by abduction: 3) it becomes plausible that there has been a shipwreck: ‘plausible (EVENT(p)).’ This reasoning leads to the utterance of a plausible hypothesis.

7. How some languages grammaticalize this type of distinction

Many languages have grammatical devices to explicitly indicate the various processes specifying the inferential reasoning an utterance is based on. These grammatical devices are generally explicit grammatical morphemes known as evidentials and grouped together under the heading of evidentiality. Languages such as Tuyuca (Barnes 1984), Tariana (Aikhenvald 2003), Quechua (Faller 2002), Kashaya (Oswalt 1986), among many others, are examples of languages having an evidential system with more than one inferential morpheme depending on the type of inference. Existing work on the expression of inference strives to determine the parameters of variation across languages (Willett 1988; de Haan 1998; Aikhenvald 2004; Plungian 2010, etc.) but the semantic difference between inferential morphemes is not entirely clear and manifests itself in different ways cross-linguistically.¹³ To engage in debate on these questions requires that our descriptions be based on more precise concepts. We will therefore analyze some examples which are generally identified as inferential.

Let us take the example of Panare, a Cariban language of Venezuela (Mattéi-Müller 2007) which has developed particular morphological mechanisms whereby speakers must specify whether the fact they are

presenting has been personally verified, or whether it is a hypothesis based on observed clues and is therefore simply plausible.¹⁴

- (16) *a-tě-se* *měn* *kanawa* *Ehkara* *pana*
 Intr-go-PST:Imm Cop:Inan car Caicara DIR
 ‘The car just left for Caicara.’
- (17) *n-ti-yah* *kěn*
 3-go-PST:Rec 3Sg:An:NonVis
 ‘It has left.’ (The speaker saw it go)
- (18) *yu-tě-hpě* *měn* *kěn*
 3Intr-go-PERF:Infer Cop. 3Sg:An: NonVis
 ‘It has left.’ (therefore, it must have left)
 (Description by the author: The speaker notes that the person’s
 hammock is no longer there and infers that the person has left).

Thus, (16) and (17) are distinct from (18). Sentence (16), where the verb form bears the suffix *-se*, is a declarative sentence referring to an empirically observed fact. (17) denotes the state resulting from the same observed fact. In (18), the speaker neither verbalizes the resultant state as in (17), nor the occurrence of a recent past event as in (16). In (18), relying on clues (for example the person’s hammock which is no longer there) and shared knowledge (when you leave a place, you take your hammock with you), the speaker expresses the hypothesis, deemed highly plausible, that the person has left. Two other sentences, one in the affirmative, the other in the interrogative, also highlight the particular mechanism behind abductive reasoning based on shared knowledge passed down from generation to generation:

- (19) *Mareoka* *na-amaaně-hpě* *kě’* *e’ñepa*
 Mareoka 3Tr-do- PERF:Infer COP:An Panaré
 ‘Mareoka¹⁵ created (in all probability) the Panarés.’
- (20) *mo-na* *ka* *měn* *Mareoka* *ni-hpě*¹⁶ *e’ñepa-uya?*
 exist- Nmzr Inter Cop. Mareoka see- PERF:Infer Panaré-DAT
 ‘Mareoka, was he seen here by the Panarés?’ (*Lit.* For the Panarés,
 are there visual traces of Mareoka?)

In (19), the choice of *hpě* is based on reasoning corresponding to the speaker’s existing store of knowledge: if an event has taken place (*Mareoka created the Panarés*), then everyone knows it, and transmits their knowledge; then, if everyone knows it and says it, then the event in

question becomes a strongly plausible hypothesis. The interpretation in (19) makes it possible to understand why the question in (20) is coherent. (19) is not an assertion; rather, it leaves room for doubt.

In many languages (Albanian, Bulgarian, Farsi, Georgian...), the perfect has given rise to a series of perfect-like forms which can express abductive inference based on clues. Thus in Bulgarian, a southern Slavic language, speakers use these forms to present facts they have not themselves witnessed, but which left traces leading to a hypothesis, as in (21a, b) with the imperfective imperfect:

- (21) a. *Njakoj e otvarjal prozoreca*
 somebody be.PRES open.PAP.Impf window.Art
 ‘So someone opened the window’ [but it is closed at the time of utterance]
- b. *Koj e vližal v stajata i ne se e säbul*
 who be.PRES.3Sg enter.PAP.Impf in room.Art and NEG
 Refl be.PRES.3Sg remove shoes.PAP.Pf
 ‘Who entered the room and didn’t take off their shoes’ [but there is no one in the room]

It is these same forms which are used by detectives to elucidate a crime. They consider, through abductive reasoning based on a set of observable clues (broken window, traces of blood or other indications), the most plausible hypothesis built on abduction, a hypothesis which can be confirmed or infirmed by the discovery of new clues:

- (22) *Kradecät e vližal v kuxnjata prez prozoreca*
 thief.Art be.PRES enter.PAP.Pf in kitchen.Art through window.Art
 ‘The thief has entered the kitchen through the window.’

One finds the same type of examples in the Nakh-Daghestanian languages, such as Agul, where judging from chips and other visible clues (scratches...) the speaker verbalizes a hypothesis to explain the observed facts:

- (23) Agul (Maisak and Merdanova 2002, cited by Forker in press).
dak’ar daquna-a

window open.CVB-COP
'(Somebody) opened the window.'

or in

- (24) Bezhta (Forker in press)
[The police are investigating a burglary. Seeing an open window and footprints beneath it, the policeinspector says:]
c'ohor ž y- -ʔ-λ' : *biλo-ʔ* *Ø-eλ'e -na*
thief window-OBL-IN-TRANSL house-IN I-go-UWPST
'The thief has entered the house through the window.'

In general, abductive reasoning leads to hypotheses based on facts directly perceived in the situational context of the utterance by a speaker who chooses to retain the most plausible hypothesis expressed by the utterance. There are however many languages where inferential reasoning is expressed by two or more markers. This led Willett (1988) to distinguish inference “based on observation of physically perceivable evidence” from what he calls “inference by means of logic,” described as “some kind of mental construct.” The data from Ingush, a Nakh-Daghestanian language, show that an entire series of verbs in the Perfect, formed by the anterior converb + a tensed form of ‘be,’ are used to express inferred facts (Nichols 2001). Thus forms built on “the progressive of the future auxiliary combined with the anterior converb (with or without other auxiliaries) [...] imply that the speaker is making the inference at the moment of speech” (p. 260):

- (25) Ingush (Nichols 2001, 261; situation: We left some milk for the cat. We come into the room later and see an empty saucer. The cat is also absent, but we infer that it drank the milk.):¹⁷

Cyskuo shura dwa-manna xugjolazh jy
cat.ERG milk DX-drink.CVant J.FUT.CVsim J.PROG
'The cat must have drunk the milk.'

- (26) Ingush (Nichols 2001, 261) (it implies that the speaker is making the inference at the moment of speech)

Hwoa tolxa-bea xubbolazh baac caar
brain spoil-B.CS.CVant B.FUT.CVsim B.INFER.NEG 3PI.ERG

cyn
3Sg.GEN
'I guess they must not have hurt his brain.'

In contrast, the *Inferential perfect* (anterior converb + the future tense of delimited ‘be’) according to the author signals that “the speaker infers (not from evidence but logically) that the event must have happened” (p. 259):

(27) Ingush (Nichols 2001, 259)

Wa pielaa=chy wa=chy-jettaa shura cyskuodwa-
2s.ERG glass=in DX=in-J.pour.PPL milk cat.ERGDX

-drink-INFERpf

manna-xugjy.

‘The cat **must have drunk** the milk you poured into the glass.’

(28) Ingush

Dwa-oaghuora voallazhie=’a Daala twaisiitaa
DX-recline V.be_located.CVirr=& God.ERG sleep-CSind

xugvy yz, twaisaav yz
V.INFERpf 3Sg fall asleep.NW.V 3Sg

‘Right while he was lying there he fell asleep -- God **must have made** him **fall asleep**.’

(25) is a case of an inferential process which goes from what is ‘true’ to what is ‘plausible.’ In some examples, directly perceived clues favor a given hypothesis, which nonetheless remains more or less plausible. Other examples, such as (26), provide an explanatory hypothesis for a fact considered true. In both cases, the same type of reasoning is employed: given *q* (observed clue or true fact) and given that $p \Rightarrow q$, one comes to the hypothesis ‘*p*’ of which the plausibility is argued through ‘*q*’ (*q* then becomes a clue favoring *p*) or *p* becomes an explanation for *q*.

Let us now consider examples (27) and (28), with an *Inferential perfect*. The context suggests that this is, rather, a deduction with a probable consequent. This hypothesis would explain the translation proposed in the example (29) with the same marker:

(29) Ingush (Nichols 2011, 260)

A *Duqa xa joaccazh televizor=chy jer*
much time J.be.NEG:FOC.CVsim TV=in 3Sg

veira=q suona.
V.see.WP=CUM1Sg.DAT

‘I saw him on TV not long ago.’

B *Veina* *xugvy.*
 V.see.CVant be.INFER.V
 ‘Right, you probably did.’

Let us look now at two examples from Tariana, Awarak, a Northwest Amazonian language (Aikhenvald 2003, 2004, 2006). In her grammar of the language, the author defines two “inferred evidentials” (*-sika*, termed ‘generic’ and *-nihka/-nhina*, termed ‘specific’) which she illustrates with the following examples:

(30) Tariana

tfinu niwhã-sika di-na
 dog 3Sgnf+bite-REC.P.NonVIS 3Sgnf-OBJ
 ‘The dog bit him (he has a scar and I can make an inference).’

(31) *tfinu niwhã-nihka di-na*
 dog 3Sgnf+bite-SPEC.INFER.REC.P 3gnf-OBJ
 ‘The dog bit him (I can see obvious signs).’

The author specifies that the generic *-sika* in (30), is “used to describe an event or a state which the speaker did not observe, but about which they have enough general knowledge or common sense to draw conclusions” (Aikhenvald 2003, 293); the ‘specific’ *-nihka* (for the recent past) –and *nhina* (for the remote past), is “used to refer to something one has not seen, but which is based on obvious evidence which can be seen” (p. 287). The difference between the two lies “in access to direct evidence of something happening and to the degree of ‘reasoning’ involved.”¹⁸

Interpreting similar examples, Aikhenvald (2004, 2006) examines the various factors informing the choice of an “inferred evidential: *-nihka/-nhina*, called inferential evidential, refers to “inference based on visible or tangible evidence or result” and the ‘generic’ *-sika* (assumption evidential) to “inference based on logical reasoning, assumption or simply general knowledge.” These distinctions seem to be analogous to those in (25) and (27) in Ingush. (30) is a case of a material implication between an event and a state which, for its part, is concomitant with its resultant state (biting entails traces of biting). (31) relies on much more general knowledge which links an event to later consequences.

However, abductive reasoning can also call upon general knowledge to construe strong evidence in favor of a hypothesis’s plausibility. For example, in Tsakhur, a North Caucasian language, the auxiliary *wo-d* with

the periphrastic form of the perfect can be used to assess, based on rough estimates, the cost of a wedding ceremony:

- (32) Tsakhur (Maisak and Tatevosov 2007, 385)
 [After approximate calculations, the speaker assess the costs of the wedding ceremony]
q'o^ɛ-l-le miljon čura-ni-s ža-d wo-d
 two-IV-card million(IV)meat-OBL-DAT on-IV cop.PRES-IV
ikkan
 IV.need.Impf
 '[So I see that] two million are needed for meat alone.'

In Kashaya, a Pomo language, Oswalt (1961, 1986) isolates two suffixes *-qǎ* and *-bi-* as inference markers and calls them respectively *inferential I* and *inferential II*. According to Oswalt, *-qǎ* is used when the speaker has enough clues to state a fact, as in (33):

- (33) *mu cohtoc qǎ → mu cohtoc^hq^h*
 he leave-*qǎ*
 'He must have left, he has left' (said on discovering that the person is no longer present; the leaving itself not being seen [where one would have *cohtó·y*], nor heard [*cohtocin*].)

According to Oswalt (1986, 38), the suffix *-qǎ* “implies no lack of certainty” as can be expressed through other suffixes linked to direct observation. Compare (34) and (35). When a person enters a house and detects the smell of baked bread, he could say either (34) or (35):

- (34) *cuhni· mu[?]'ta-q^h*
 bread cook-INFER.I
 'Bread has been cooked'
- (35) *cuhni· mu[?]'ta mihšew*
 bread cook smell
 'It smells like cooked bread'

In (34), the smell is a clue to infer a highly plausible hypothesis: “bread has been cooked.” In contrast, in (35), there is no inference and the perception verb is used simply to specify an olfactory perception.¹⁹

The analysis of the suffix *-bi-* is more complex, as highlighted by Haan (2001, 201). Oswalt proposes two analyses for this suffix. In his grammar of Kashaya (Oswalt 1961, 243), the author distinguishes (36a) from (36b) and gives two interpretations, but a single English translation:

- (36) a. *sinamq^h*
 drown-INFER.I
 ‘He must have drowned’
- b. *sinamq[?]biw*
 drown: INFER II: ABS
 ‘He must have drowned’

The explanations clearly show that the context plays a very important role here. In (36a), the speaker has seen a body laid out on the beach or floating in the water, or has seen the boat in which the person went out to sea capsized. (36b) could be uttered in the same circumstances but then it also suggests that the person who went out to sea has still not returned. As mentioned in a previous publication (Desclés and Guentchéva 2011), we consider, contrary to Haan (2001, 202), that the reasoning involved is not the same. In (36a) the underlying reasoning is abductive, and is triggered by evidence (discovery of a body washed up on the beach) and by the general property, known to all, that “when there is a shipwreck, bodies wash up on the beach,” whence the utterance of a plausible hypothesis: there has been a shipwreck and the body on the beach is a victim of it. The reasoning in (36b) is construed differently: the speaker has seen a shipwreck on the beach or notes that a person gone to sea has not come back. Knowing, moreover, that a boat has sunk in the area or that the person has not come back by the usual time, and that there are often drownings, he deduces (but not by abduction) that the fisherman sailing the ship that wrecked has probably drowned.

Returning to the analysis of *-bi-*, Oswalt (1986, 41–42) considers that its use depends more on distributional features than semantic ones:

The chief difference between *-bi-* and *-qã* Inferential I is perhaps distributional; *-bi-* is never verb-final but must be followed by some other suffix. It occurs in four irregularly fused compound suffixes that form subordinate clauses: *-bina* inference plus coreference of the agents of the subordinate and superordinate clause; *-bem* inference with different agents in the two clauses; and *-binate* and *-beti*, which add the meaning ‘although’ to the precedent pair. In (S30 = (37)) there is a similar type of inference in each clause, but different morphemes are involved because one clause is subordinate to the other:

- (37) *du[?]k’u-bi-na* *cohtocch-q^h*
 finish-INFER.II-SS leave-INFER.I
 ‘He must have finished and left (the work is done and he is no longer here)’

In our view, (37) shows that in Kashaya, beyond the question of distribution, the notion of inference covers two different types of inference and makes it possible to string together two types of reasoning: a probabilized consequence (p) is deduced from a plausible hypothesis (q) posited following abduction triggered by an observed fact (r). The proposition (p) *du²k'u-bí-na* 'he must have finished (the work)' is a probable consequence derived from the following reasoning: 1) the plausible hypothesis (q) *cohtocch-q^h* 'he must have left,' construed through abduction based on the observation (r) *he is no longer here* and application of common sense ($q \Rightarrow r$) "when one has left *one's place of work*, one is no longer there"; 2) the general rule "one has usually finished one's work before leaving the work place." If our analysis is correct, there are two related propositions, the first based on abductive reasoning, the second on deductive reasoning, based on the plausible hypothesis established by the first. More precisely, let us posit:

r = 'he is not here;
 q = 'he has left his workplace';
 p = 'he has finished his work'

with two material implications:

$q \Rightarrow r$ ('when one has left one's work, one is no longer at the workplace')

and

$q \Rightarrow \text{prob}(p)$ (if one has left one's workplace, one has usually finished one's work).

The underlying reasoning in (37) entails two propositions: in accepting hypothesis q, one deduces the probability of p [verb form *-bí*]; given r, it is plausible that q; the hypothesis q is plausible [verb form in *q^h*] since one notes r (not expressed in the utterance but implicit in the context).

Let us compare (37) to (38) where "only the subordinate clause contains an inference, which is probably based on the visual evidence in the main clause" (Oswalt 1986, 41):

(38) *du²k'u-bí-na* *cohtó-y*
 finish-INFER.II-SS leave-VIS
 'He must have finished; (I just saw) he left.'

Leaving aside the complex problem of the status of subordination in a language such as Kashaya, it is clear that (38) presents a probable outcome (*du^hk'u-bi-na* 'he must have finished') construed from an observed fact *cohtó.y* 'he left': the material implication is as follows: if one leaves, one has probably finished one's work. The only use of *-bi-* in a non subordinate construction is when it combines with *-w* (interpreted as an absolutive) to signal, according to the explanation given by Oswalt (41), that a given event or state can be "partially perceived by any means but which become more interpretable by later evidence." Thus (39) expresses the reaction of a speaker who only recognized her husband once he came close:

- (39) *k^he* = ²*-bi-w*
 my man=ASS-II-ABS
 'It turned out to be my husband.'

We propose the following analysis for (39) which associates this use of *-bi-* with that highlighted in (37) and (38), namely, *-bi-* marks a probable conclusion in a material implication. In (39), it would be the expression of evidence which the speaker did not expect; the mechanism for expressing the observation is described as follows: 1) the speaker has a personal belief 'p => prob (non q)' [based on the perceived silhouette (p), it is highly probable that it is not her husband (prob (non q))]; 2) based on observation (i.e. 'p'), the speaker therefore deduces the strong probability of 'non q'; 3) however, she notes that in fact there is both 'p' and 'q,' whence her surprise: her expectations are not confirmed by further observation.

This use of *-bi-* is reminiscent of the admiring, observed in many languages, namely Balkan languages with perfect-like forms. For example, for (40) in Bulgarian, observations are contrary to the speaker's expectations, and are thus surprising for him:²⁰

- (40) Bulgarian
Tuj ne bilo zlato! Nikakvo zlato ne e
 this NEG be.PAP gold none gold NEG be.PRES.3Sg
 'Why, it's not gold! It's not gold at all!!! Good heavens! It's not gold! It's not gold at all!'
 (example cited by Andrejčin 1976, 346)

Contrary to languages such as Kashaya which has a full range of specific morphemes, the perfect-like forms in the Balkan languages combine surprise, various types of inference (by deduction or abduction,

depending on the context) and can co-occur with modal verbs. Thus the analysis of (41) is nearly identical to that given for (37):

- (41) *Svāršil* *si* *e* *rabotata* *i* *si* *e*
 finish.PPA.Msg Refl. be.3sg work.Art and Refl be.3sg

trāgnal

leave.PPA.MSg

‘He must have finished and left (the work is done and he is no longer here)’

8. Conclusions

In this article we have insisted on the necessity of taking into account two types of inference expressed in languages, namely: logical inference through deduction (modus ponens) and inference by abduction leading to the formulation of a more or less plausible hypothesis. The two types of inference are based on evidence or observed facts and common knowledge or knowledge supposedly shared by both speaker and co-speaker.

Common knowledge is often the result of a generalized induction based on a correlation of properties ascribed to the same objects within a set of observed elements, hence the statement ($\forall x$) ($P(x) \Rightarrow Q(x)$): if x has the property P , then it always has the property Q . Shared knowledge calls instead upon probable consequences expressed by ($\forall x$) ($P(x) \Rightarrow \text{prob}(Q(x))$): if x has the property P , then it very often has the property Q . Let us further note that, in our theoretical approach to language, probability is used qualitatively; this means that ‘ $\text{prob}(Q(x))$ ’ denotes that the chances of having ‘ $Q(x)$ ’ are much higher than those of not having ‘ $Q(x)$.’ As such, probability appears as a modality distinct from improbability, possibility, impossibility and certainty.

We also defined the notions of simple deduction and deduction with a probable consequent and, following Peirce and Pólya, we distinguished them from abduction. As seen above, some languages grammaticalize the results of these various types of inference and use different markers to indicate either a probable consequent or a plausible hypothesis. In both cases, the contexts and inference processes are different even though they call upon a material implication construed through induction.

Inference through abduction and deduction with a probable consequent are cognitive processes which humans use both in daily life and in scientific reasoning. In daily life, the processes are implicit; in scientific reasoning the goal is to make them explicit. It is interesting to note that

some languages have well-identified grammaticalized means to express these processes which other languages, such as English, lack.

Both abduction and deduction with a probable consequent lead to the expression of an uncertain proposition (a hypothesis in the case of abduction, a probable consequent in the case of deduction) which is distinct from propositions asserted with the speaker's commitment. Thus, depending on the context, a statement of uncertainty can refer either to modalities such as probability, improbability, possibility... bearing on a proposition, or to inference processes which state either a probable consequent or a plausible hypothesis. It may therefore be difficult to distinguish these various ways of expressing uncertainty, especially in languages which have no specific markers to distinguish them. Note, moreover, that even in proposition logic, it is often difficult to clarify the distinction between deductive inference (by modus ponens) and material implication between two propositions ($p \Rightarrow q$).

Here we have attempted to clarify various conceptual distinctions associated with forms of uncertainty. We believe it is indispensable to take these distinctions into account for semantic analysis in linguistics.

NOTES

Chapter One

1. I wish to thank my colleagues Bruno Leclercq and Gregory Bochner, as well as two anonymous reviewers, for their useful comments on a previous draft.
2. A note on the quoting conventions used in the body of this paper:
single quotes for the introduction of terminology, and for scare quoting,
italics for metalinguistic citations, and for emphasis,
double quotes for citations of other authors,
French angle quotation marks for meanings/contents,
These conventions are not necessarily followed in the examples (mainly because various writers stick to various rules).
3. Note that such a metalanguage can itself be formalised (see, e.g. Carnap 1934; Church 1956, 55).
4. This is the translation of a paper originally published in Polish in 1933.
5. In his discussion of Tarski's logic, Simmons, however, recognises just one alternative: "Tarski observes that the metalanguage contains both an individual name and a translation of every expression (and so every sentence) of the object language; this is crucial for the construction of the definition of truth" (Simmons 2009, 545).
6. If, as is the case in Carnap (1934) or Church (1956, chapter 8), the metalanguage is used to formulate the formation and transformation rules of the expressions of the object-language.
7. Of course, there is an infinity of sentences that fit in neither. Here, I intend *sentence* to mean "any string that fits either in L_m or in L_o (or, before the question is settled, in both)."
8. Rey-Debove would not accept this formulation, saying instead that the autonym *signifies* a homonymous expression. However, that difference in our views has no impact on the present discussion.
9. Whenever reference is involved, 'sentence' is to be understood as 'sentence produced in a context of utterance.'
10. As Bruno Leclercq (p.c.) remarks, there may be design features that *do* allow us to distinguish between what is truly linguistic and what is not, e.g. Martinet's 'double articulation.' However, my impression is that all these, based as they are on the observation of known human languages, remain exposed to Bennett's parochialism challenge.
11. I ignore the "group of young animals" sense of *litter*, which is a count noun but is not relevant here. Thanks to Raphael Salkie for pointing this out.

12. Granted, some hesitation surrounds pseudo-English items. We saw that the boundary between pseudo-English and English was fuzzy. This extra difficulty does not, however, affect the point I am about to make.
13. I say ‘obvious,’ but Droste, for instance, does not consider (8) and (9) metalinguistic sentences. See Section 2.4.2.1.
14. I do not regard the characterization of a language as a set of sentences as necessarily incompatible with a notion of the language faculty as part of humans’ genetic endowment. A Chomskyan i-language can also be understood as an infinite set of sentences, namely that set that can be generated by a speaker on the basis of her internalised grammar and her lexicon.
15. Lepore rightly suggests that ‘Demonstrative’ theories à la Davidson would also address the worries expressed here. However, for reasons I have articulated elsewhere (De Brabanter 2005, 26–29; 2017), Demonstrative accounts prove less satisfactory than Depiction theories.
16. Note in passing that Droste would not consider the first sentence to be metalinguistic.
17. Rey-Debove talked of ‘autonomous connotation.’ I borrow the term ‘hybrid’ from Clark and Gerrig (1990; with adjustments) and from Recanatì (2001). For more about hybrids, see issue 17 of the *Belgian Journal of Linguistics* (2005), and De Brabanter (2010b).

Chapter Two

1. I am indebted to Viviane Arigne and Christiane Rocq-Migette, my two former English linguist colleagues at Paris 13, for letting me express my views from the margins of the conference, as an outspoken linguist into epistemology behind the scenes of the two Paris 13 conferences, both as a member of the committees and of the audience. This chapter tries to answer some of the most challenging questions raised at the viva of my Habilitation thesis (Ballier 2004) by Prof. Claude Boisson, a phonologist by training who loomed large among the influential circles of French enunciative linguists, a dominant framework in France’s English departments over the last three decades. The second introductory paragraph is nothing but a direct objection he voiced at my viva.
2. Balzac first published this short story, *Le Chef-d’œuvre inconnu*, in 1831.
3. I regard this as a typically Marxist research question. In the kind of investigation led by Michel Pêcheux analysing the traces left by economic forces or ideology in discourse, which, in his work are called “matérialités discursives.” I reinterpret his concept to describe the manifestation of the workings of metalinguistic representations, just like in (Conein et al. 1981), material traces such as italics are the locus of ideology or theoretical representations.
4. It’s unfortunate that *stenography* is a mass noun in English, whereas *photography* can function as count or mass.

5. I tried to demonstrate in my PhD that it had some bearing on the conceptualisation of the linguistic system of English (Ballier 1997). In a nutshell, I contend that for enunciative linguists, the English language is analysed as a set of systems (microsystems) structured by markers. The aspectual system is analysed as a set of pragmatic and semantic conditions for the various combinatorial possibilities of *have + en* and *be + ing*. I made the point that assumptions among theories differ as to how they map their psycholinguistic/cognitive operation onto linguistic markers, ranging from one-to-one mapping to more complex configurations (Culioli 1995).
6. http://www.education.gouv.fr/pid285/bulletin_officiel.html?cid_bo=94717.
7. Notation is also evidence of our theoretical framework (and cultural heritage): *Grounding* and QUD within some linguistic circles are very plausible candidates for a revival of the *modus* and the *dictum*. Linguistics does run the risk of running the same circle again.
8. Culioli has the term ‘range of values,’ which I personally would consider to be Frege’s *Wertverlauf*, the range of possible values in his theory of arithmetics.
9. At the very heart of this topological representation, this actually takes place within the same dimension (Culioli 1968).
10. I would contend that here Culioli probably applies Frege’s concept of *Wertverlauf*.
11. Oddly enough, this concept is not at the core of the two papers he co-wrote with mathematician-trained Jean-Pierre Desclés. Culioli alludes to the *came* in (Culioli 2008) and the figure is very briefly described in (Ducard 2009 and Ducard 2016).
12. Culioli 1995, translated by Michael Liddle, out of the 1983 seminar transcribed by the late Jean-Claude Souesme.
13. Antoine Culioli, *Pour une linguistique de l’énonciation. Opérations et représentations*. Tome 1, Tome 2 and Tome 3. Paris: Ophrys.
14. But see Bouscaren et al. (1992) for a grammar of English in English based on Culiolian concepts.
15. I have suggested (Ballier 1997) that it was the research paradigm of enunciative linguistics: trying to offer a conceptualisation of the various possible interpretations of the ‘linear structure’ segmented into various ‘markers’ corresponding to mental operations with diverging conceptions of mapping between markers and mental operations.
16. Joly and O’Kelly 1993, 55–6, see also Joly and O’Kelly 1990, 92.
17. One of the reviewers surmised that this kind of representation became debatable if some theoretical tenets were disproved. It seems to me that these representations are not the thing itself, but emulation of categories. Culioli develops a similar argumentation, distinguishing between levels of representations (Culioli 1995). These tools are ways to emulate the semantic interpretations of markers. Therefore, it seems to me that the interest of the schemata still holds, whatever may be held again the overall theoretical framework. They are meant to “regulate the linguistic activity,” here in the guise of proposing core meanings or invariants. Formulating the invariant of

a marker (its core meaning) with a stenographeme is actually the ability to visualise, represent the whole possible range of interpretations. Similarly, to take another example from Guillaume, a sentence like “L’instant d’après, le train déraillait” is to be interpreted as meaning that the train derailed or that it almost did, depending on the context. This multiplicity of possible interpretations is what has to be explained by the linguist. In that sense, the aim of the game is a formulation of a core meaning that allows for this fluctuation of meaning. In these schemata, a graphic formulation of the invariant is proposed. The “truth” of the schema in its connection to the “truth” of the theory” is less important than the potentiality of explaining why there is such a possible range of interpretation, and potential contradictory interpretations for the same marker.

18. This is tantamount to arguing that linguistics is not sheer mathematisation and logification of speech. If language cannot undergo a whole *Begriffsschrift*, we have to admit the defeat of the logical representation of speech. One possible strategy lies in the analysis of the multiple facets of stenography. This trajectory is not unlike the two Wittgensteins, moving from the quest of logical univocity trailblazed in the *Tractatus* to the plurality of language games advocated in his *Philosophical Investigations*.
19. In his pronouncing English Dictionary, the *Grand Repository of the English Language* (1775), Spence actually had two systems: the dictionary made use of a set of symbols that included new symbols, mainly ligatures of existing letters, whereas the prose works were published in a simplified spelling using only regular alphabetic characters (Pouillon 2016). In Spence’s view, the symbols have the same status as graphemes.
20. This section resumes some of the theoretical discussions I have had with Véronique Pouillon, discussing her re-conceptualisation of the syllable among 18th century orthoepists and trying to account for her notion of “signposting” (Pouillon, forthcoming). She does not necessarily endorse the whole semiotic and conceptual apparatus to be outlined afterwards, but she has been instrumental in spelling out some of the metalinguistic dimensions of the 18th century innovators (see our joint paper, Ballier, Beal and Pouillon).
21. The continuing use of the Latin alphabet as the basis for transcription (in the IPA for instance) partakes of an even more insidious graphocentric bias.
22. Note that <zh>, though it is not a standard orthographic sequence, does make use of graphophonemic conventions, in that it parallels <sh>, creating the voiced counterpart of the palatal fricative.
23. <http://www.scs.leeds.ac.uk/amalgam/amalgam/amalhome.htm>.
24. See, for example, Gaillat’s PhD (2016) on the re-annotation of *that* according to its functional realisation as proform or as deictic.
25. In a nutshell, exchanging data and annotation formats to compare linguistic data across corpora.
26. It should be noted that even if great care is being taken to maintain a distinction between a phonological and a phonetic level, some transcriptions may remain, willingly or not, ambiguous. So-called ‘phonetic’ dictionaries

are actually quite ambiguous between a phonological transcription interspersed with transcriptions of connected speech processes, vowel reductions, alternative pronunciations.

27. I am indebted to Geneviève Nore for pointing out this sort of distinction to me.
28. I suppose ‘padawans’ would be the proper term to refer to Jedi linguists. The obscure side of the force is ambiguity or jargon, to refer to the usual infamous criticism levered against linguists.
29. The irony is that my supervisor’s *Magnus Opus, Interpretation as Pragmatics*, is slightly less considered than other works he would qualify as less important.
30. One should investigate the complex conventionalization of meanings, following David Lewis’s *Conventions*, that has presided over the main notational conventions largely used by linguists (such as the asterisk before ungrammatical sequences).
31. See, for example, Harris, *The Linguistic Wars*, about the generative semanticists.
32. I have defined my language game looking at linguistic theories as an attempt to understand why and where we disagree, rather than awarding medals to best models. It makes more sense to me to outline watersheds/dividing lines and account for dissenting views (Ballier 2010).

Chapter Three

1. Under this type (i) terminological move, anything ‘meta-linguistic’ is *ipso facto* also ‘metalinguistic’ as pertaining to ‘metalinguistic discourse,’ but the converse is not always true. For more detail and exemplification, cf. Pamiès (2015, 231, note 1).
2. For a fairly substantial bibliography of relevant work pertaining to each of I–VII, cf. Pamiès 2001 (37–39 for contributions at least co-signed by Chomsky himself; for [classified in a parallel, I-to-VII, way] major dissertations or monographs by other authors, and textbook-like detailed presentations, 41–43 and 37 [respectively]).
3. For a concise summary of the modifications characterising each of the transitions (from I to ST, from II to EST, from III to REST, from IV to TM, from V to GB/P&P, and from VI to MP) cf. Pamiès (2001, 39–41). For snapshot views of each state from II to VII (*per se* leaving the reader to figure out what each transition to the next might have consisted in, cf. the last paragraph of note 15 *infra*) and references therein.
4. Of course, given the somewhat radical nature of some state-to-state modifications, one could conceivably hold that the common thread proposed in the main text is too tenuous to string I–VII together, and one could conceivably construct some argumentation or other concluding that the very idea that Chomskyan theory could be one in seven hypostases is a myth that does not bear scrutiny.

In the terms of Pamiès 2001 (41 [end of note 92]), which left the possibility open : “pour peu que prenne de l’ampleur l’intervalle chronologique séparant deux présumés ‘états’ d’une même théorie [...], à force de retouches et de remodelages, on constate [parfois] un tel bouleversement des appareillages formels et conceptuels mobilisés pour élaborer les solutions [...] qu[’on] ne peut plus guère manquer de se poser,” “sans préjuger de la réponse,” “la question de savoir si les problèmes à résoudre eux-mêmes (et donc, cruciallement, le mode de constitution de l’objet) n’ont pas subi une métamorphose trop radicale [...] pour ne pas être le signe que (*exit* alors ‘la’ théorie chomskyenne), fût-ce sous des allures de révisions dans la continuité, le passage d’état à état a fini par induire un hiatus qualitatif, un changement de cadre théorique.”

To carry out the dismemberment, it would seem, one would just have to pick and choose among, of course non-exhaustively listed, the following dozen abrupt type (b) changes and/or about-turns in stage-to-stage transition that could be invoked:

- (i) as of II, expansion of the object (semantics moves in),
- (ii) as of II, *via* a new competence/performance distinction, counterbalancing (i), shrinking of the object (performance moves out),
- (iii) as of VI, *via* a new I-/E distinction, reshaping of the object (sudden pronouncement that E-languages never really existed),
- (iv) as of VI, *via* the instalment of LF, shrinking of the object (counterbalancing (i), a large part of former semantics moves out),
- (v) as of VI, *via* a new core/periphery distinction, shrinking of the object (anything messy moves out),
- (vi) as of VI, *via* a new I-/E- distinction, concurring with (iii), reshaping of the object (sudden pronouncement that the idealisation to the “ideal speaker-hearer” of a “homogeneous community” never really made sense),
- (vi) as of VI, *via* a new component/module distinction, reshaping of the object (massive complexification of the postulated inner architecture),
- (vii) as of VII, by joining the ‘naturalisation of science’ band-wagon, reshaping of the aims and strictures of the investigation,
- (viii) as of VII, *via* introduction of ‘interfaces’ and postulation of ‘SEM,’ massive shrinking of the object (‘real’ semantics, i.e. ‘real’ truth and reference and ‘real’ understanding move out, or are deemed to lie beyond the reach of scientific investigation),
- (ix) as of VII, *via* focus on biophysics and ‘evo-devo’ considerations on the origin of language and evolution, reshaping of the object by expansion ‘beyond explanatory adequacy’ of chosen aims and relaxing of strictures (untestable speculation allowed in).
- (x) as of VII, *via* new focus on ‘third factor’ considerations, concurring with (viii), shrinking of the object (part of the constraints on computations moves out),

- (xi) as of VII, *via* exclusive consecration of ‘merge,’ reshaping of the object (huge simplification of the postulated inner architecture of VI, sudden pronouncement that ‘constructions’ and associated specialised rules never really existed),
- (xii) as of VII, *via* new quest of ‘perfection,’ concurring with (x) and (xi), reshaping of the object (re-formulation of chosen aims).

However (though of course we will address some of those points as we go along - though, for lack of space, barely touching on the last three), we won’t make any attempt in that direction and will stay put, keeping to a conservative stance on the issue. To a marginal extent, because (even supposing the distinction between fully-fledged theory and mere research program may be ignored) precisely delimiting the paradigm-to-paradigm frontier(s) might well prove tricky. Essentially, because too little is at stake here to justify the effort, since, given our purpose, if such argumentation could be convincingly carried out, the only consequence would be that we would have to switch from ‘Theory’ to ‘Theories’ in the title.

Incidentally, though Chomsky himself would most likely subscribe to the view that there has been no change of paradigm in the I-VII trajectory, he would definitely reject such appellations as ‘Chomskyan theory,’ since for him “generative grammar is [...] not a theory any more than chemistry is a theory,” “[i.e. not] a theory, advocated by this or that person,” “[but] a topic, which one may or may not choose to study” (Chomsky 1986, 4).

5. For instance, by way of random, but telling selection, cf. the titles of Chomsky 1961 (“Some Methodological Remarks on Generative Grammar”) and (1964) 1966 (*Topics in the Theory of Generative Grammar*), Jackendoff 1972 (*Semantic Interpretation in Generative Grammar*), Chomsky 1972 (*Studies on Semantics in Generative Grammar*), Belletti et al., eds 1981 (*Theory of Markedness in Generative Grammar*), Chomsky 1988a (“Generative Grammar. Past, Present and Future”), or, throughout the text, Chomsky 2000 *New Horizons in the Study of Language and Mind* (e.g., 6, 7, 122). Cf. also, by extension, in Chomsky 1982 (significantly titled *The Generative Enterprise*), the equation of “‘linguistic theory’ and ‘generative grammar’ (54, 5 [respectively]), or the title of the collection in which Weibelhuth ed. 1995 was published (Blackwells, *Generative Syntax*).
6. On ‘explication’ (as distinct from ‘explanation’), cf. Pamiès (2001, 26–27, note 64).
7. Before this watershed, for instance in *Syntactic Structures* (Chomsky 1957, 17, note 2 and associated main text:) the state-of-the-art picture was a far cry from that of an integrated theory of linguistic description. It was held that “the relations between semantics and syntax” “can only be studied after the syntactic structure has been determined on independent ground.” So that, with “grammar” thought to be “autonomous and independent of meaning,” semantics was contained and left in the cold at the outer fringe of “use of language.”

Looking back at the genealogy of this move from marginalisation to admittance of semantics within linguistics proper, Chomsky has this

comment (Parret ed. 1974, 37): “In *Syntactic Structures* I assumed a ‘use theory’ of meaning under the influence largely of Wittgenstein and Oxford philosophy. Subsequent work of Katz, Fodor, Postal and others suggested the possibility of a semantic theory as a more integral part of grammar, and I adopted this view in *Aspects*.”

8. Endorsed as a move in the right direction in Chomsky (1965, 221, note 33): “for steps towards a substantive theory of semantic interpretation [...], see Katz and Fodor (1963) [...]”
9. In the spirit of Katz and Postal 1964, as acknowledged in Chomsky, 1965 in his notes 10 (198): “Aside from terminology, I follow here the exposition in Katz and Postal (1964). In particular, I shall assume throughout that the semantic component is essentially as they describe it” and in note 33 [idem, 221]: “for steps towards a substantive theory of semantic interpretation [...], see [...] Katz and Postal (1964),” [from which I] “borrow extensively” (Parret ed. 1974, 37).
10. That is to say not in actuality, because of the quasi total unavailability of half the required theoretical tools, as was openly recognized by Chomsky: This view of “grammar as” “ultimately, [...] a device for pairing phonetically represented signals with semantic interpretations, this pairing being mediated through a system of abstract structures generated by the syntactic component” (Chomsky 1964, 52) “[pre]suppose[s] that] we accept the notion of ‘semantic representation’ [...] a representation of the meaning of a sentence in some universal system of representation analogous to universal phonetics” (Chomsky, in Parret, ed. 1974, 37). In other words, it “assume[s] given two universal language-independent systems of representation, a phonetic system for the specification of sound and a semantic system for the specification of meaning.”
And indeed, for “the former, there are many concrete proposals: for example the system described in chapter 7 of Chomsky and Halle (1968),” or summarised in Chomsky ([1955] 1975, 157 ff). But for the latter, Chomsky had to admit that “in the domain of semantics there are [...] problems of facts and principles that have barely been approached, and there is no reasonably [...] well-defined ‘theory of semantic representation’ to which one can refer.” For want of any firmer ground, it was therefore on an article of faith (or optimistic bet on the future) that Chomsky chose to base his theoretical construction: “I will, however, assume that such a system [of semantic representation] can be developed and that it makes sense to speak of the ways in which the inherent meaning of a sentence, characterized in some still-to-be-discovered system of representation, is related to various aspects of its form” (throughout for orphan quotations, Chomsky [1969] 1971, 183).
11. On the standard definition of ‘recursive set’ in terms of abstract automata theory [i.e. in terms of ‘enumerated/recognized by a Turing machine’ and ‘recursively enumerable set’], cf. Pamiès (2001, 232–238); see also notes 13 and 14 *infra*.
12. In spite of the somewhat sophisticated, technical-like style of formulation, it must be kept in mind that, resting as it does on the crystal-gazing assumption

that some “still-to-be discovered” “universal system” of “semantic representation” “analogous to universal phonetics” “can be developed” in the future (excerpted from the quotations of note 10 *supra*, cf. Chomsky [1969] 1971, 183; 1964, 52 twice; [1969] 1971, 183 twice), [6](#) has never been a summary of fully-fledged proposals, but at best a blue-print for what might, hopefully, be achieved one day in future investigations.

13. More precisely, though still somewhat simplifying (but see note 39 *infra*), “we must require of [...] linguistic theory that it provide for [...] specification of a function f such that $SD_f(i,j)$ is the structural description assigned to sentence s_i by grammar G_j , for arbitrary i, j ,” “and we assume that mappings are effective—that there is an algorithm for enumerating sentences [and] structural descriptions [...]” (Chomsky 1965, 31, 202, note 18).

14. ‘Recursive set’ may also be defined, not in terms of ‘procedure,’ but in terms of such concepts of abstract automata theory as ‘enumerated/accepted by a Turing machine’ or ‘recursively enumerable set.’ For instance, “ A is a recursive set” may be defined as “both A and the complement of A are recursively enumerable,” where “ A is recursively enumerable” is defined as “ A is accepted (enumerated) by some Turing machine” (Wall 1975, 280).

But the apparent discrepancy between definitions in terms of procedure and definitions in terms of abstract mathematical automata may be elucidated *via* the conceptual tools presented in the first section of Pamiès 2015 (“Formalisation: naïve theory, formalised theory, abstraction an empirical content,” 8–10 and 232–235, notes 6–41), since ‘procedure’ and ‘algorithm’ may then be viewed as naïve theory concepts which can be formalised by resorting to the formal apparatus of Turing machines.

For more on these and related issues, apprehended in a broader perspective—with, on the purely formal side, this fundamental “result of Recursive Theory” that “the classes of partial functions (and hence of total functions) obtained [not only] by Turing, [but also by] Kleene, Church, Post, Markov and others are identical, i.e. are just one class” (Partee et al. 1993, 518), and, on the formalising side, Church’s received ‘thesis’ (or rather ‘conjecture,’ cf. the “*proviso*” in Pamiès 2015, 9) that these extensionally equivalent formal apparatuses constitute adequate formalisations exhausting the intuitive/naïve notion of ‘computability’—cf. Pamiès (2001, 230–243, particularly notes 646, 658–660, 669 and, on 243, 700–702 and associated main text).

15. That is, leaving aside the detail of the formal way the meaning and sound “correspondence” is supposed to be syntactically “mediated,” thus deliberately ignoring as inessential exactly what rules are claimed to relate what (sub)components or modules and how—thereby treating as merely incidental the sea change impact of (cumulated) state-to-state rearranging, weeding out, uprooting or implanting of postulated components or modules and inter-relating rules (on the component/module distinction, cf. Pamiès 2001, 39, transition from V to GB/P&P).

By thus factoring out some element of stability, one may hope to curb what might otherwise easily foster an impression of chronic impermanence, if not of sheer falling apart and dis-integration of the whole theoretical patchwork.

Of course, taken separately, the modifications from one stage of Chomskyan theory to the next may be of the un-disturbing reshuffling variety. For instance, at a time when deep and surface structures remained unchallenged as two distinct syntactic components, the transition from Standard to Extended Standard Theory could be thus characterised: “The theoretical outline developed in *Aspects* [...] postulates that deep structure [but not surface structure] is mapped onto semantic representation by interpretive rules. This is often called the ‘Standard Theory.’ [...]. My own subsequent work, influenced [...] in particular by Jackendoff [1972] led to what is now sometimes called the ‘extended standard theory,’ which takes semantic representation to be determined by a mapping of [both] deep structure and phonetically interpreted surface structure” to “semantic representation” (Chomsky, in Parret, ed. 1974, 37–38).

But to get a raw-feel sense of the potential school-for-scepticism kaleidoscopic impact of the whole range of cumulated state-to-state transitions, one may try chain-perusing the figures 3-1–3-7 laid out in slide fashion at the end of our main text: [from respectively] van Riemsdijk and Williams (1986, 172, Figures 10.1 and 10.2; 173, Figures 10.3 and 10.4; 310, Figure 18.1), Abraham et al eds (1996, 5, (3) b), Webelhuth ed. (1995, 357, (2)), Adger ([2003] 2004 [146]), each of which schematically encapsulates a synoptic view of one particular state [or ‘model (of grammar)’] commonly distinguished in the development of Chomskyan theory [(using a received terminology conflating ‘theory’ and ‘state of a theory), “the Standard Theory model of grammar,” “the Extended Standard Theory model of grammar,” “the Revised Extended Standard Theory model of grammar,” “the T-model of grammar,” “the organization of the [components and] modules of Government-Binding Theory,” the state-of-the-art “architecture” of the Minimalist model in 1995 (twice) and 2003, respectively].

16. Hence, “suppos[ing] that a satisfactory theory of universal phonetics and of universal semantics were at hand” (Chomsky [1967] 1972, 124), and granted the availability of the postulated algorithm of note 13 *supra*, via a minor *ad hoc* redefinition of ‘sentence,’ the definition given in [1] may be enriched accordingly, and

[1] a language may be redefined as a set of sound-meaning pairs:

“Still taking a language to be a set of sentences, let us consider each abstract “sentence” to be [124:] “a particular kind of sound-meaning pair,” [125:] “a specific pairing of a phonetic representation with an abstract structure of some sort [...] that incorporates information relevant to semantic interpretation” (Chomsky [1967] 1972, 125, 124).

In the same vein, but going all the way from mere sentences to fuller structural descriptions and capitalising on the claim that the correspondence between phonetic and semantic representations [*aka* ‘interpretations,’ *aka* ‘readings’] is mediated by the syntactic component, Hek van Riemsdijk and

Edward Williams extend the above redefinition of a language as “a certain (infinite) class of abstract” ordered pairs (Chomsky [1967] 1972, 125) to a redefinition of a language as an (infinite) set of triples (Riemsdijk and Williams [1986] 1987, 4): “each sentence has a structure that mediates the connection between its sound and its meaning. [...] A grammar, then, is the rules for the formation of syntactic structures and associated sounds and meanings, and”

I” “a language is the set of all such triples defined by the grammar:”

“ $L = \{ \dots(\text{sound, syntactic structure, meaning}) \dots \}$.”

But, regardless of whether it is defined as in **I** or redefined as in **I** or in **L**, granted the availability of the algorithm postulated in note 13 *supra*, and given the definition of ‘generate’ [main text, insertion *locus* of note 11], it inescapably follows that a ‘language,’ as defined in generative linguistics at the time of *Aspects*, is supposed to be a recursive set.

17. As an abstract object, a canonical tree may be defined either **(i)** in terms of necessary and sufficient conditions for membership in the defined class (Pamiès 2015, 32, *Step 1*), or **(ii)** by constraints on representations narrowing a wider class of ‘arboriferences’ (idem, 33, *Step 2*, and 256, note 153) or **(iii)** by constraints on derivational rules (idem, 43 *Step 3*). For a demonstration that the definitions of types (i)-(iii) given for ‘canonical tree’ (idem, subsection 6.1, 32–34) are equivalent definitions of one and the same class of formal objects, cf. Pamiès (2001, 173–184, referred to in Pamiès 2015, 255, note 146).

A formal object may be defined as **(iv)** “in the mathematical sense,” “a structure composed of one or more sets together with one or more relations on those sets” (Pamiès 2015, 14, 15 (XII)).

Sameness (*aka* identity) of formal objects may be rather loosely defined as **(v)** a relation between “mathematical structure[s] comprising the same number of sets comprising the same number of relations with the same properties and comprising the same number of [...] members” (idem, 35), a definition which can be sharpened in terms of one-to-one mappings insensitive to trivial intra-isomorphic variation or scriptural musical-chair permutations (idem, 239–40, note 71), including reference to Pamiès 2001 for possible links with mathematical Representation Theory).

Finally notational equivalence may be defined as follows: **(vi)** two formal representations are notational variants of each other if and only if they represent the same formal object (cf., in substance, Pamiès 2015, 15 (XIII)).

18. Namely (carrying on with the same conventions as in note 17 *supra*):
- For fig 1-11, deciphering instructions targeting a type (i) specification of the formal object represented (Pamiès 2015, 32–33),
 - For fig 1-12, deciphering instructions targeting a type (ii) specification of the formal object represented (idem, 33),
 - For fig 1-1, 1-2 and 1-20, deciphering instructions targeting a type (iii) specification of the formal object represented (idem, 34, (XXXII)),
 - For fig 1-3, deciphering instructions also targeting a type (iii) specification of the formal object represented (idem, 35, first emendation of (XXXII)),

For fig 1-8, deciphering instructions again targeting a type (iii) specification of the formal object represented (idem, 35 (second emendation of (XXXII))).

19. First (still carrying on with the same conventions as in note 17 *supra*), it is established that, for each of the place-holders under consideration, the assigned DI_{FR} (LT_{FR}) is a fully specified member of the class of canonical trees, in that the specified formal object represented satisfies the strictures chosen for defining that class

- of type (i) in the case of Figure 1-11 (Pamiès 2015, 33 (intermediate result 1)); of type (ii) in the case of Figures 1-12 and 1-1 (idem, 33 (intermediate result 2) and 256 (note 154)); and finally, of type (iii) in the case of Figures 1-2, 1-3 and 1-8 (idem, 34 (intermediate result 3) and 35 (intermediate results 4 and 5, respectively).

From then on, it is established (idem, 35 (XXXIII)) that,

- under (v), the distinct *locus tenens* of figures 1-1, 1-2, 1-3, 1-8, 1-11 and 1-12 have in common to represent one and the same canonical tree,
- from which it finally follows that they are (idem, 35 (XXXIV)) notational variants of each other.

In Pamiès 2015 (drawing on much more detailed work in Pamiès 2001), these considerations are integrated into a broader demonstration, which is based, not only on discrepancies from one ('algebraic' and/or 'geometricised') formal representation to the next within one equivalence class of notational variants, but also on a triple inscription/grapheme/symbol distinction, compounded with an 'inscriptional diagram'/'abstract graph' distinction (for the triple distinction, cf. Pamiès 2015, section 1 on "Inscriptions, graphemes and pure distinctiveness," 15–17 and 240–1, notes 72–83), together with, for references to Pamiès 2001, 236, note 48 and 240, note 78; for the twofold distinction, cf. Pamiès 2015, 236, note 52).

The contentions claimed to be conclusively established on that richer basis are that **a)** strong or weakened, the formalist attempts at reducing mathematics to the manipulation of inscriptions or graphemes is doomed, in that **b)** (*contra* [\[4\]](#) in the main text *infra*), the objects represented by formal representations are "self-existing mathematical entities" (241 (note 85)), so that **c)** "our mathematical theories are descriptions of an abstract mathematical realm, that is, a non-physical, non mental, non spatiotemporal" and "acausal" aspect of reality" (idem, 242, note 86, quoting Balaguer 1998, 8, 3).

20. As a formal object (again, carrying on with the same conventions as in note 17 *supra*), this canonical tree is a mathematical structure in the sense of (iv), ideally to be specified "in terms insensitive to trivial intra-isomorphic variation or scriptural musical-chair permutation." For a characterisation of its constitutive sets and (each with its properties) relations—and by way of approximation to the diaphanous, quintessential ideal of (v), cf. Pamiès 2015, 257, note 165).
21. Chomsky occasionally evokes another case of type [\[1\]](#) vacuity [henceforth, of 'vacuity₁'], due, not to inaccessible explicitness through meta-meta... infinite regress, but rather presupposition failure. In such cases, a theory is

vacuous because the specific questions it asks are so empirically ill-conceived [in that the posited self-contained or autonomous (sub)domain about which the questions are asked turns out to be deprived of (insular) existence] that they are doomed never to receive the kind of specific answers that they are meant to obtain. For instance (counterfactually): “The study of biologically necessary properties of language is a part of natural science: its concern is to determine one aspect of human genetics, namely the nature of the language faculty. Perhaps the effort is misguided. We might discover that there is no language faculty, but only some general mode of learning applied to language or anything else. If so, then, universal grammar in my sense [would be] **vacuous**, in that its questions w[ould] find no answers apart from general cognitive principles” (Chomsky 1975, 29).

But the following few lines of the above excerpt may be read as suggesting another type of vacuity [henceforth, *vacuity*₂], with the term ‘vacuous’ used in a different sense, to mean something like ‘purely conceptual, hence devoid of empirical content’: “But still, universal grammar conceived as a study of the biologically necessary properties of human language (if such exist) is strictly a part of science. The criteria of success or failure are those of science. In contrast, the study of logically necessary properties of language is an inquiry into the concept “language.” I should add at once that I am sceptical about the enterprise. It seems to me unlikely to prove more interesting than an inquiry into the concepts of “vision” or “locomotion.” But in any event, it is **not** an **empirical** investigation, except insofar as lexicography is an empirical investigation, and must be judged by quite different standards” (ibid.).

Finally, for further use and reference, by a type (i) decision explicitly spelling out what is tacitly at work in the above paragraph, we choose to call ‘*vacuous*₃’ a theory for which the required empirical refutability turns out to be *de facto* impossible.

To summarise, then, a theory is *vacuous*₁ if its self-prescribed goals are unattainable, it is *vacuous*₂ if, by not going beyond purely conceptual consideration, it has no grip on the empirical, and it is *vacuous*₃ if it turns out to be *de facto* empirically insolvent (for further subdivisions, cf. note 152 *infra*).

22. It is not obvious whether or not being ‘vacuous’ in the sense of [11] [henceforth, ‘*vacuous*₁’] entails being *vacuous*’ in the third sense suggested in note 21 *supra* [‘*vacuous*₃’].

On the one hand, it could be claimed that the entailment does not hold. The argument could then run as follows: let us assume that, in a given theory *T*, typically, a sentence *s_i* assigned formal semantic representation *SRs_i* can only be understood to claim that *s_i* has semantic content *SCs_i* if we read *SCs_i* into *SRs_i* by resorting in an unaccounted way to bits and pieces of the very semantic competence that *T* as a whole is supposed to explicitly account for.

By definition, *T* is thus *vacuous*₁. But, so the first argument goes, for all its type one vacuity, it is still perfectly conceivable that there should be a conflict between the content that we surreptitiously read into *SRs_i* and our

intuitive understanding of s_i . So that, provided introspective judgements (whichever way elicited) count as theoretically predicted content, then, so the first argument would go, vacuous₁ T may conceivably turn out not to be vacuous₃. So that (for a theory) ‘to be vacuous₁’ does not entail ‘to be vacuous₃.’

But on the other hand, under the same assumptions and notational conventions, it could also be argued that the entailment does hold. The claim would then be that what we have in such typical cases is not a significant process of confrontation between theoretical claims and empirically gathered intuitions. The only thing we have is an insignificant clash between [as expected] empirically gathered raw intuitions (as to what to understand by [utterances of] s_i) and [a far cry from the required explicitly constructed predicted content] empirically gathered raw intuitions (as to how to understand ultimately natural language instructions for deciphering [formal representations theoretically assigned to utterances of] s_i). Hence, so the second argument goes, it is because T is vacuous₁ that it is vacuous₃. In other words, (for a theory) ‘to be vacuous₁’ does entail ‘to be vacuous₃.’

Pending more cogent considerations (though rather favouring the second argument), we shall leave the issue undecided at this point, and, at this transitory juncture [i.e. until we get to 15c], provisionally retain the sole contention that, under $\square\text{-}\square$, Chomskyan theory is vacuous₁.

23. As of Chomsky 1962, but fully spelled out in Chomsky 1965.
 24. But for Johnson-Laird (1987, 154), this vindication of (partial) empirical immunity does not quite bear scrutiny. Given (as we have seen) that in Chomsky’s own words “a generative grammar” is “a characterization of the intrinsic tacit knowledge that underlies actual performance” (Chomsky 1965, 140) and that “as Chomsky would allow, the phenomena of performance depend on two major components: mental processes and a tacit [mental] representation of grammar,” so that if “a grammar posited as an account of what the mind computes is assumed to be [mentally] represented in the mind,” then, one would expect, “its rules and strictures are also assumed to be directly used by the processes underlying perception and production.” But “if the resulting [all-inclusive] psychology of language [purporting to show both what competence is used and by what underlying processes] is not corroborated by experimental results, then it should follow that one or the other of the two components is in error.” So that, “*pace* Chomsky,” Johnson-Laird objects (1987, 154 for all orphan quotes in this note), performance experiment data may conceivably be a source of empirical confirmation for a would-be adequate theory of competence. To such an extent that “if no evidence is forthcoming to suggest that the structures [...] and rules” “of the grammatical theory” are “directly employed in comprehension of speech, then it is reasonable to assume that the grammar, [even if] it is an accurate account of the [sound/meaning pairing] function that is computed, is not [mentally] represented in the brain.”
- In support of his contention that “this hypothetical case history” is not just idle speculation, Johnson-Laird adduces by way of a tacit cruel reminder

what he takes to be the outcome of the ‘psychological reality debate’ over the status of transformations at the time of ST: in his view of things, at that time, the conflict between theoretically assigned degree of complexity and experimentally measured ease and difficulty of actual processing by flesh and blood subject did not end up in my-home-theory-is-my-castle stalemate, but in the fair and square demise of “the standard theory of transformational grammar.”

25. As of [1977] 1978 [Chomsky, “A theory of Core Grammar”], developed in Chomsky ([1979] 1981).
26. Palmer (1979, 2) was wont to say that “natural languages are notoriously untidy.” The claim that an explanatory theory of competence (a theory of UG) need only account for the acquisition of core grammars forces tidiness into natural language. Without doubt, apart from irregularities, everything is regular. For a vigorous criticism of this idealisation to core versus periphery as, in substance, empirically ill-founded, butchering through instead of carving at a joint, and unjustifiably leaving out not-so-simple but fundamental idiom constructions to obtain an artificially contrived, imaginary Minimalist simplicity at any cost, cf. Jackendoff and Pinker (2005, 10–15).
27. For an instance of a marked construction involving the lifting of a general overriding constraint normally inhibiting SSC [i.e. the Specified Subject Condition, cf. Chomsky ([1979] 1981, [143, 144], analysis of examples (21) and (25)-(27)). We shall briefly encounter an example of normal application of SSC in the course of [15].
28. For instance (Chomsky 1975, 104, 100) “Pronouns not yet assigned antecedents may be taken to refer to entities designated elsewhere in the sentence, though this is never necessary and is not permitted under certain conditions, for example in (27)” [“(27) *he* said Mary kissed John”].
29. Cf., before the publication of Fodor’s *The Modularity of Mind* (1983): “If, indeed, the mind is modular in character, a system of distinct though interacting systems, then language-like systems might be acquired through the exercise of other faculties of mind” (Chomsky 1980, 28), and “the grammar is put to use, interacting with other mechanisms of mind, in speaking and understanding language” (Chomsky 1975, 28).
30. In dissymmetric fashion (nobody is perfect), the total eclipse of the level of ‘semantic representation’ is recorded in the synoptic schematic representations mentioned in note 15 *supra*.
 <present throughout in Riemsdijk and Williams ([1986] 1987) from Figure 10.1 (stage II, ST) to Figure 10.4 (stage V, TM) [172–173], it disappears from the radar screen in Figure 18.1 (stage VI, LGB/P&P), never to return in Abraham et al. eds 1996 [5, (3)b], Webelhuth ed. 1995 [357, 2], or Adger (2003) 2004 [145]>;
 but, regrettably, the early introduction of LF (at stage IV, REST) is not indicated in Figure 10.3 (stage IV, REST) (Riemsdijk and Williams, 173) (in which it is somehow diluted in the ‘semantic representation’ box), the first explicit mention of LF being somewhat deceptively deferred until Figure 18.1 (stage VI, LGB), that is (cf. note 4 (iv) *supra*) until the concomitant

instalment of LF and demise of the level of ‘semantic representation.’ In similar fashion, the ephemeral syntactic sub-level of ‘shallow structure’ is not indicated in Figure 10-4 (stage V, TM) (ibid.), even though, at that stage, shallow structures serve as “ultimat[e]” input to LF, in that “interpretive rules relate shallow structures to LF” (Chomsky 1977, 5, 6).

31. “In what might look like an answer to *à la* Lewis debunking of Markerese, Riemsdijk and Williams ([1986] 1987, 184 and 183) draw an analogy between phonetics/phonology and broader semantics/LF:

“We believe that many philosophers and linguists insist that Logical form be interpreted” (ie. that it is necessary “to give a set-theoretical interpretation of Logical Form”) “because otherwise it is merely another language; they feel that in translating English into this other language (the logical forms of English sentences) we have gotten no closer to meaning. We believe this reasoning is based on a misunderstanding of the empirical nature of Logical Form. To draw the analogy with phonology: phonetic representation is a “language” into which phonological rules translate phonological representations. We do not conclude from this that phonetic representation is “no closer to sound” than phonological representation. Furthermore, though it remains unclear exactly what the correct “interpretation” of phonetic is to be—whether an acoustic wave, a pattern of auricular sensations, or a pattern of motor neural commands—phonologists have nevertheless succeeded in discovering a great deal about the nature of phonological and phonetic representations and the mapping between them. This shows that it is not necessary to say in detail how a representation is to be “interpreted”—that is, how it relates to some larger theory—in order to learn about it and its relation to other representations.”

But, in spite of a passing evocation of (the meta-language of) LF as a kind of Markerese, their argumentation does not begin to tackle the overreach vacuity problem. By merely denying the need for the linguist to venture one step further up, in the terms of [9], it does not question (indeed, it takes for granted) the validity of the vain endless escalation from “abstruse to recondite specification” which is at the core of the conundrum. We return to the vacuity of model-theoretic interpretation in D.

32. Though it “may very well” be difficult to “br[ing] to consciousness” “the tacit knowledge of the speaker” (Chomsky 1965, 21), and even though it may be argued that it is impossible to “directly ta[p] competence, because even the exercise of linguistic intuition is an aspect of performance” (Johnson-Laird 1987, 150), still, “it must be recognized that there is no escape from the control exercised by [...] intuitive and introspective judgments” (Chomsky, in Parret ed. 1974, 40) and that “there is no way to avoid the [...] assumption that the speaker-hearer’s intuition is the ultimate standard that determines the accuracy of any proposed grammar” (Chomsky 1965, 21).
33. A more common type (i) designation for what Chomsky calls the ‘Galilean style’ of scientific investigation is ‘the hypothetico-deductive method,’ and, rather than Galileo, Newton is often referred to as the forerunner who broke free of Cartesian a priorism:

In “the hypothetico-deductive method, [...] as theorized by Descartes, the proper form of a theory is seen as a mathematical system in which particular empirical phenomena are explained by relating them back deductively to a small number of general principles and definitions. The method, however, abandons the Cartesian claim that those principles and definitions can themselves be established, finally and conclusively, before inquiring what light their consequences throw on actual scientific problems and phenomena” (Toulmin 1976, 379).

As for Newton, “though [he] was powerfully influenced by Descartes’s mathematical example, he followed his methodological maxims only up to a point. Granted that the theory of motion and gravitation of Newton’s *Principia* did indeed conform to Descartes’s recipe—adding further dynamical axioms, definitions, and postulates to those of Euclid’s geometry—Newton, nonetheless made no pretence of proving, in advance of empirical evidence, that these additional assumptions were uniquely self-evident and valid. Instead, he treated them as working assumptions to be accepted hypothetically for just so long as their consequences threw light, in exact detail, on hitherto unexplained phenomena. Inevitably, the epistemic claims to be made on behalf of such explanations fell short of Descartes’s full “deductivist” ambitions. Newton knew of no phenomena, for instance, that evinced the mechanisms of gravitational attraction and saw no point in “feigning hypotheses” about them [did not try to contrive some way of deductively deriving them from something].” “In this way, Newton devised in practice—almost inadvertently—what philosophers of science have since labelled the hypothetico-deductive method” (idem, 378–379).

34. On idealisation [“principe d’exclusion”] as partially constitutive of the object of a theory, cf. Pamiès (2001, 74 (B-2.2)).
35. Historical considerations aside, “the hypothetico-deductive method” may be “characterize[d]” “as follows:” “From a general hypothesis and particular statements of initial conditions [hence ‘hypothetico’], a particular predictive statement is deduced [hence ‘deductive’]. The statements of initial conditions, at least for the time, are accepted as true; the hypothesis is the statement whose truth is at issue. By observation we determine whether the predictive statement turned out to be true. If the predictive consequence is false, the hypothesis is disconfirmed. If the observation reveals that the predictive statement is true, we say that the hypothesis is confirmed to some extent. A hypothesis is not, of course, conclusively proved by any one or more positively confirming instances, but it may become highly confirmed. A hypothesis that is sufficiently confirmed is accepted, at least tentatively” (Botha 1981, 33, quoting Salmon 1967, 18). On Popperian refutability strictures, and issues of universal and existential quantification tacitly at work in the above characterisation, cf. Pamiès (2001, 82–84).
36. For a defence of this idealisation as not only harmless, but helpful, cf. Chomsky ([1973] 1975, 52, note 66): “there can be no objection, as far as I can see, to the idealization. On the contrary, it would be only natural to expect that the investigation of the more complex real situation will be

- successful only insofar as” it incorporates the proposals of the “highly idealized theory.”
37. Cf. also, though couched in much vaguer terms, the remark by Riemsdijk and Williams ([1986] 1987, 184) that “Like LF, the existence of sentence grammar is an **empirical** issue [...]. In principle, it could turn out that it is impossible to characterize sentences in and of themselves without reference to their roles in various conversations, situations, etc.”
 38. On the following generalisation, borrowed from Lees (1960, 8) : <“middle Verbs” (i.e. Verbs like *resemble* “which do not take Manner Adverbials freely”) “are, characteristically, the Verbs with following NP’s that do not undergo passive interpretation”> as “suggest[ing] that the Manner Adverbial should have as one of its realizations a ‘dummy element’ signifying that the passive transformation must obligatory apply,” cf. Chomsky (1965, 103 *ff*).
 39. [Completing the algorithm of note 13 *supra*,] “linguistic theory” must also provide for specification (Chomsky 1965, 202, note 18) “of a function m such that $m(i)$ is an integer associated with the grammar G_i as its value (with, let us say, lower value indicated by high number” (idem, 31). At the time of *Aspects*, this [blue print for a] specification was meant to formalise the evaluation measure (in terms of empirically significant degree of complexity) of competing grammars (accounting for the same data? the simpler, the better). For later amendments, cf. note 105 *infra*.
 40. Which, incidentally, leads to a (largely theory-internal) defence of [14]: “it is for this reason that the discovery of peculiarities and exceptions (which are rarely lacking, in a system of the complexity of a natural language) is generally so unrewarding and, in itself, has so little importance for the study of the grammatical structure of the language in question, unless, of course, it leads to the discovery of deeper generalizations” (Chomsky 1965, 218, note 28).
 41. In Chomsky’s own account of Cartesian philosophy, *res cogitans* and creativity are so indissolubly linked that the justification for postulating the one is the otherwise impossibility of accounting for the other: “whereas” “human reason [...] “is a universal instrument which can serve for all contingencies,” “the organs of an animal or a machine have need of some special adaptation for any particular action.” Hence the necessity, “Descartes concludes, to “postulat[e]” “in addition to body,” “a substance whose essence is thought” [to] “play the role of a “creative principle” alongside the “mechanical principles” that account for bodily functions.” By thus “arguing from the presumed impossibility of a mechanical explanation for the creative aspect of the normal use of language” to the postulation of a *res cogitans* whose essence is creative thought, “Descartes expands on his [early] conception of the “cognitive power” as a faculty [...] which “is properly called mind when it either forms new ideas in the fancy or attends to those already formed,” acting in a way that is not completely under the control of sense or imagination or memory” (Chomsky 1966, 5, quoting Descartes [1637] 1955 part V, 116–117; then 78, note 9, quoting Descartes [1628] 1955, 39).

42. As Jacques Bouveresse puts it (Parret, ed. 1974, 39) when interviewed by Herman Parret, “Chomsky’s philosophical eclecticism is really strange. He has the habit of setting apart in very different theories more or less isolated elements which interest him for one reason or another, without preoccupying himself excessively with the general philosophical context in which it appears.” For instance, given that “for Descartes, there is really a spiritual substance which contains thoughts, representations, volitions, etc.,” “I have wondered how Cartesianism can remain Cartesian after one has eliminated the essential element: dualism—the fact that man is constituted of two substances of a different nature, body and mind, one associated with the other and capable of acting on the other.” <to be pursued in note 44>.
43. For instance, in characteristic elusive, mundane style Bergson rejects as incongruous the very idea that such ‘spiritual’ mental entities as memories have anything to do with such gross material consideration as a location in the brain: “Mais si le souvenir n’a pas été emmagasiné par le cerveau, où donc se conserve-t-il? À vrai dire, je ne suis pas sûr que la question “où” ait encore un sens quand on ne parle plus d’un corps. Des clichés photographiques se conservent dans une boîte, des disques phonographiques dans des casiers ; mais pourquoi des souvenirs, qui ne sont pas des choses visibles et tangibles, auraient-ils besoin d’un contenant, et comment pourraient-ils en avoir ? J’accepterai cependant, si vous y tenez, mais en le prenant dans un sens purement métaphorique, l’idée d’un contenant où les souvenirs seraient logés, et je dirai alors qu’ils sont tout bonnement dans l’esprit” (Bergson: *L’énergie spirituelle*, quoted by André Cresson in *Bergson*, Paris, PUF, collection “Philosophes.” On Bergson as clearly a dualist, cf. also Barthémy-Madaule 1989, 7 and 8: “*Matière et mémoire*” “est une condamnation du parallélisme des matérialistes mécaniques,” “c’est-à-dire [du] terme à terme des manifestations corporelles et physiques.” “Elle établit la possibilité d’une indépendance de l’âme à l’égard du corps, donc d’une survie de l’âme.”
44. Therefore, to complete Bouveresse’s criticism of Chomsky’s “remarkable ability to reinterpret, more or less pertinently and exactly, traditional philosophical doctrines in the sense of his own theories,” “he is on the Cartesian side when the creative aspect of language use is stressed, but he does not exclude in principle the possibility that La Mettrie could finally be right, in other words that [in some reactualized sense] an integrally mechanistic explanation of linguistic behaviour might some day be provided—exactly what Descartes considers completely impossible.” With such liberties taken with the original, Bouveresse concludes, “with regards to [Chomsky’s] Cartesianism,” “Kant” or Leibniz” (“because he thinks that what appears in our perception and knowledge at the conscious level is only the visible tip of an iceberg, so to speak, most of which remains submerged”) are “in many respects” “a more adequate reference than Descartes.”
45. On the non empirical nature of questions (1)-(3), cf. Chomsky ([1955] 1975, 39: “at this level of discussion, there is no question of ‘right or wrong.’ It is

merely a question of where one's interests lie." For instance, "if someone prefers not to adopt a 'realist interpretation' of linguistic theory, and thus to make no claim for the empirical validity of the theoretical principles he adopts, I see no argument that could demonstrate to him that this conception must be abandoned. Or conversely."

46. For an affirmative answer, cf. Kant's admittance of the existence of things-in-themselves (to which we return in a following note). For a negative answer, cf. Berkeley, who, convinced that "the admission of an extramental world would lead to materialism and atheism," "denied the existence of an independent world of bodies by teaching that their existence consists in perceptibility, *esse es percipi*," thus contending that "things cannot exist unless perceived by some mind" (Rolbiecki 1942, 38).
47. On typical cases of rejection (if not phobia) of any inobservable entity within the context of empiricist metaphysics and epistemology, cf. Pamiès (2001, 77–80 (I.2), and particularly 79, notes 10–12).
48. In (1)-(3), we are freely playing on the wording of a famous Reply to Chomsky by Quine (1969) to which we shall have occasion to return (cf. notes 81 and 80 *infra*).
49. For a positive answer, cf. Leibnitz's view, momentarily vindicated by Kant in his "early 'precritical period" of "Nature, organic as well as inorganic, as a whole of interconnected natural laws," a "reality [that] can be apprehended in and through concepts" (Kraushaar 1942b, 158). On Leibnitz and rationalist metaphysics and epistemology, cf. Pamiès (2001, 81–85 (I.3)), and particularly 83 (note 21).

For a negative answer, cf. Kant's 'critical' contention that, though things-in-themselves exist, they are unknowable. More precisely, Kant argued that the forms or "intuitions" of "*sensibility* [space and time] and *understanding* [the twelve categories (or "synthetic forms of the understanding," or "pure principles of the understanding") "arranged in groups of three under the heads: quantity, quality, relation and modality"]," are "formal demands of reason." And that since "they are [thus] necessary conditions of any experience of Nature," they "cannot be properties of things-in-themselves" (Kraushaar 1942b, 159). As a result, (*ibid.*) "theoretical knowledge is limited to the realm of experience, and within this realm we cannot know '*things-in-themselves*,' but only the way in which they appear under a priori forms of reason; we know things, in other words, as '*phenomena*.'"

In other terms, with 'transcendental' defined as "appl[y]ing to the conditions [allowing for] experience and anything related thereto" (Ewing 1942a, 319), and 'transcendent' as "that which is beyond, in any of several senses" (Long 1942c, 319), since "the forms [of sensibility and of the understanding] are valid in experience only because they are necessary conditions of experience, there is no way of judging their applicability to objects transcending experience" (*ibid.*) (Kraushaar 1942b, 159): in slogan form, "transcendental knowledge is possible[,] [...] transcendent knowledge is not" (Ewing 1942a, 319).

As a consequence, “to accept the *a priori* forms of reason, valid only in experience, as constituting the nature of ultimate reality” would be but an “illusion” (Kraushaar 1942c, 319).

50. In Psillos’s formulation [with underlying assumptions as to what ‘semantic(s)’ is, to which we shall have occasion to return]:
 “Scientific realism is a philosophical view about science that consists of three theses:

The metaphysical thesis: The world has a definite and mind-independent structure.

The semantic thesis: scientific theories should be taken at face value. They are truth-conditioned descriptions of their intended domain, both observable and inobservable. Hence, they are capable of being true or false. The theoretical terms featured in theories have putative factual reference.

The epistemic thesis: Mature and predictively successful scientific theories are well confirmed and (approximately) true of the world. So the entities posited by them, or entities very similar to those posited, inhabit the world” (Psillos 2006a, 688).

51. The theoretical claims of *P*’ (which we shall treat as roughly equivalent) are the following:

p1 “individuals employ highly restrictive principles that guide the construction of grammar” (Chomsky 1975, 11),

p3 “powerful constraints [...] are operative restricting the variety of languages” (Chomsky 1975, 11),

p4 in the case of the acquisition of language “a schematism exists delimiting the class of cognitive structures [i.e. the class of grammars] that can be attained” by human organisms (Chomsky 1975, 21),

p5 “U[niversal]G[rammar] provides an elaborate and highly restrictive schematism to which grammars must conform” (Chomsky 1977, 18),

p7 “the discovery or ‘creation’ of grammar by the language-learner” is “a rule-governed act” in that “[not] all hypotheses compatible with the data [are] available as ‘permissible grammars’” (Chomsky 1974, in Herman Parret (ed.) 29–30).

52. The theoretical claims of *P*” (which we shall treat as roughly equivalent) are the following:

p2 “the system of principles [guiding the construction of grammar for individuals in a speech community] [is] a species property” (Chomsky 1975, 11),

p6 “humans are innately endowed with a system of intellectual organization” which is “the basis for the acquisition of such structures [as grammar]” “in the case of the acquisition of language” (Chomsky 1975, 137).

53. The allegedly factual statements of *Q*’ (which, when non-identical, we shall treat as roughly equivalent) are the following:

q1 “individuals in a speech community have developed essentially the same language” (in spite of “the limitations of” “the fragmentary evidence available,” and without access to any explicit “explanatory theory” that might

conceivably have guided them in the acquisition process (Chomsky 1975, 11, 10, 11),

q4 in the case of the acquisition of a particular language L “a rich, complex, highly articulated cognitive structure,” the grammar of L, is “attained with considerable uniformity among individuals [...] on the basis of scattered and restricted evidence” (Chomsky 1975, 21),

q5 “Somehow, from the disordered flux of ordinary linguistic experience, a rich and highly articulated system of grammatical competence develops in the mind in a specific way, fairly uniformly in a given speech community despite considerable variety in care and exposure” (Chomsky 1977, 18),

q6= tacitly *q4* (Chomsky 1975, 137),

q7 “[uniform] learning [of by and large the same particular grammar] is possible [and widespread] communication is [unproblematic] among humans with comparable linguistic experience” (Chomsky 1974, in Herman Parret (ed.) 29–30).

54. The identical allegedly factual statements of *Q*” are the following
q2 = *q3* “humans are [...] not designed to learn one human language rather than another” (Chomsky 1975, 11).
55. Pending further detail, in the case of *p1*, ..., *p6*, the form of the inferential argument is essentially: $P \rightarrow Q$ and *Q*, hence *P*. In the case of *p7*, the form of the inferential argument is essentially: obviously, $\sim P \rightarrow \sim Q$, hence (by contraposition) $Q \rightarrow P$ [*aka P* is a necessary condition for *Q*].
56. Definition of ‘transcendental argument’: in essence, “a transcendental argument is simply a form of deduction, with the typical pattern: only if *p*, then *q*; *q* is true; therefore *p* is true”; “a transcendental argument is supposed to proceed from a fact to its sole possible condition” (Wilshire 1992, 20), as if one could have $[(p \rightarrow q) \wedge q] \Rightarrow p$ [on ‘ \rightarrow ’ and ‘ \Rightarrow ’ as belonging, respectively, to the object-language and meta-language (of Predicate calculus), cf. Pamiès 2015, 231 (note 1)].
57. Given the truth-table defining the standard interpretation of material implication, if $P \rightarrow Q$ holds, there is one and only one case in which the conditional $P \rightarrow Q$ is false: when *P* is true and *Q* false. So that if $P \rightarrow Q$ is true, the truth of *P* excludes the falsity of *Q*. In other words (the truth of) *P* is a sufficient condition for (the truth of) *Q*.
By widely received type (i) metalinguistic decision, whenever an explanation *E* is offered for a fact *F* (*E* being said to explain *F* because, if *E* is granted, *F* is argued to logically follow from *E*), then *F* (the fact in need of explanation) is the *explicandum*, and *E* (the explanation offered) is the *explicans*. On necessary conditions, sufficient conditions, *explanans*, *explicandum* and related issues of explanation *versus* explication, cf. Pamiès (2001, 25–27).
58. Assuming that
p1 [antecedent, *aka* protasis] \rightarrow *q1* [consequent, *aka* apodosis] and *q1* [alleged fact], hence it must be the case that *p1* is the sole *explanans* [*aka* sufficient condition] for *q1* <conclusion of the transcendental argument>
is the underlying argument form of the following excerpt (analysed accordingly):

“individuals in a speech community have developed essentially the same language,” even though “it is clear that the language each person acquires is a rich and complex construction hopelessly underdetermined by the fragmentary evidence available,” and even though “the conscious mind [...] is frustrated by the limitations of available evidence and faced by far too many possible explanatory theories, mutually inconsistent but adequate to the data[,] or [...] can devise no reasonable theories” [underlying complex consequent, alleged fact $q1$]. “**This fact can be explained only on the assumption that** [transcendental inference] these individuals employ highly restrictive principles that guide the construction of grammar [underlying protasis $p1$, conclusion of the transcendental argument]” (Chomsky 1975, 11),

then the *explanans* $p1$ is clearly seen to be inferred from $q1$ by transcendental argumentation.

59. Given the truth-table defining the standard interpretation of material implication, the conditional $Q \rightarrow P$ is false when Q is true while P is false, so that when $Q \rightarrow P$ holds, Q cannot be true unless P is also true: in other words, (the truth of) P is a necessary condition for (the truth of) Q .

60. Assuming that $\sim p7 \rightarrow \sim q7$ is the underlying form of the following excerpt (analysed accordingly):

“were it not” [the case that] “the discovery or ‘creation’ of grammar by the language-learner”[is] “a rule-governed act” [in that] “all hypotheses compatible with the data would be available as ‘permissible grammars’” $\langle \sim p7$, counterfactual antecedent of the underlying conditional \rangle [, then] “no [uniform] learning [of a grammar] would be possible, there would be no communication, except, purely by accident, among humans with comparable linguistic experience” $\langle \sim q7$, counterfactual consequent of the underlying conditional \rangle (Chomsky, in Herman Parret (ed.) 1974, 29–30),

then, since $\sim p7 \rightarrow \sim q7$ is logically equivalent to $q7 \rightarrow p7$ (Wall 1972, 30 (Conditional Law b)), $p7$ is clearly held to be a necessary condition for $q7$.

Harping on the assumed rough equivalence of the members of P' , the counterfactual antecedent could be reformulated as (in the terms of $p1$, $p3$, $p4$, $p5$ and $p6$, respectively): ‘were it not the case’ ‘that the discovery or creation by the language-learner is guided by highly restrictive principles,’ ‘that powerful constraints are operative restricting the variety of languages,’ ‘that, for the acquisition of grammar, a schematism exists delimiting the class of cognitive structures (i.e. the class of grammars) that can be attained by human organisms,’ ‘that UG provides an elaborate and highly restrictive schematism to which grammars must conform,’ or ‘that humans are innately endowed with a system of intellectual organization which is the basis for the acquisition of such structures as grammar in the case of the acquisition of language.’

Harping on the assumed rough equivalence of the members of Q' (and tacit identity of $q4$ and $q6$), the counterfactual consequent could be reformulated as (in the terms of $q1$, $q4$ / $q6$ and $q5$, respectively): ‘there would be’ ‘no such things as speech communities because individuals would have no way of

developing essentially the same language on the basis of the fragmentary evidence available to them,' 'no way for such a thing as the grammar of a particular grammar to be attained with a sufficient uniformity among individuals on the basis of scattered and restricted evidence,' 'from the disordered flux of ordinary linguistic experience, no way for a rich and highly articulated system of grammatical competence to develop in the mind in a specific way, fairly uniformly in anything like a linguistic community.'

61. Definition of 'transcendental proof': "In Kant's philosophy, proof by showing that what is proved is a necessary condition without which human experience would be impossible and therefore valid of all phenomena" (Ewing 1942b, 321). But, since "Kant [would typically] tr[y] to commend his major premises (for example in his arguments about causality and substance" [or about "the possibility of pure mathematical knowledge, the possibility of making objectively true statements," or "the fact that there is a unitary system of time"])" "by showing what would result if the protasis (i.e. p) did not hold" (Wilshire 1992, 20), more generally: argument purporting to prove that a given protasis p is the only conceivable *explanans* for a given q of any kind by showing that postulating that p does not hold yields unsustainable consequences.
62. This 'proof' would run as follows: granting that $\sim p7 \rightarrow \sim q7$, it follows by contraposition that $q7 \rightarrow p7$, hence (by definition), that $p7$ is a necessary condition for $q7$; (prohibitively) granting as established that $p1$ is indeed the only sufficient condition for $q1$; and granting that $p1, p7$ and $q1, q7$ may be held to be equivalent (re)formulations of P' and Q' (respectively), it follows under such premises that P' is the sole necessary and sufficient condition for Q' .
63. Assuming that $p2$ <antecedent, aka protasis> \rightarrow $q2$ <consequent, aka apodosis> and $q2$ <alleged fact>, hence it must be the case that $p2$ is the sole *explanans* (aka sufficient condition) for $q2$ <conclusion of the transcendental argument> is the underlying argument form of the following excerpt (analysed accordingly):
 "humans are, obviously, not designed to learn one human language rather than another <underlying consequent, alleged fact $q2$ >; the system of principles **must** <transcendental inference> be a species property <underlying protasis $p2$, conclusion of the transcendental argument> (Chomsky 1975, 11),
 then the *explanans* $p2$ is clearly seen to be inferred from $q2$ by transcendental argumentation.
64. By widely received (cf. for instance, Lipton [1996, 2005] 2006) type (i) metalinguistic decision, such inference to a plausible *explanans* P is called 'inference to the best explanation,' attained *via* a 'best explanation argument.'
65. Assuming that $p4$ <antecedent, aka protasis> \rightarrow $q4$ <consequent, aka apodosis> and $q4$ <alleged fact>, hence $p4$ is a plausible *explanans* (aka sufficient condition) for $q4$ <conclusion of the best explanation argument>

is the underlying argument form of the following excerpt (analysed accordingly):

“**It is reasonable to suppose that** <inference to the best explanation> [in the particular case of the acquisition of language,] a schematism exists delimiting the class of cognitive structures [i.e. the class of grammars] that can be attained [by human organisms] <underlying protasis p_4 , conclusion of the best explanation argument>. Hence <explanatory virtue of p_4 > [in the case of the acquisition of a particular language L], a rich complex, highly articulated cognitive structure [, the grammar of L, is] attained with considerable uniformity among individuals [...] on the basis of scattered and restricted evidence” <underlying consequent, alleged fact q_4 > (Chomsky 1975, 21), then p_4 is clearly seen to be inferred from q_4 by best explanation argumentation.

66. Assuming that

p_6 <antecedent, aka protasis> \rightarrow $q_6=q_4$ <consequent, aka apodosis> and q_6 <alleged fact>, hence p_6 is a plausible *explanans* (aka sufficient condition) for q_6 <conclusion of the best explanation argument>

is the underlying argument form of the following excerpt (analysed accordingly):

“What kind of cognitive structures are developed by humans on the basis of their experience, specifically in the case of acquisition of [a particular] language [L]?” [Tacit answer: “a rich, complex, highly articulated cognitive structure,” the grammar of L, “attained with considerable uniformity among individuals [...] on the basis of scattered and restricted evidence”] <underlying consequent, alleged fact $q_6 = q_4$, as recoverable (from p. 21 to p. 137) in the overall context of Chomsky 1975> “**Without prejudicing the outcome of th[e] investigation, we may say that** <inference to the best explanation> “humans are innately endowed with a system of intellectual organization, call it the “initial state” of the mind” which is “the basis for the acquisition of such structures[as grammar]” “in the case of the acquisition of language” <underlying protasis p_6 , conclusion of the best explanation argument> (Chomsky 1975, 137), then p_6 is seen to be clearly inferred from q_6 by best explanation argumentation.

67. Assuming that

p_5 <antecedent, aka protasis> \rightarrow q_5 <consequent, aka apodosis> and q_5 <alleged fact>, hence it may be the case that it must be the case that p_1 is the sole *explanans* (aka sufficient condition) for q_5 <conclusion of the muffled transcendental argument>

is the underlying argument form of the following excerpt (analysed accordingly):

“Somehow, from the disordered flux of ordinary linguistic experience, a rich and highly articulated system of grammatical competence develops in the mind in a specific way, fairly uniformly in a given speech community despite considerable variety in care and exposure” <underlying consequent, alleged fact q_5 >. “**To account for this normal human accomplishment, it seems that**

we must assume that <muffled transcendental inference>
 “U[niversal]G[rammar] provides an elaborate and highly restrictive
 schematism to which grammars must conform” <underlying protasis $p5$,
 conclusion of the muffled transcendental argument> (Chomsky 1977, 18),
 then $p5$ is seen to be clearly inferred from $q5$ by muffled transcendental
 argumentation.

68. Assuming that
 $p3$ <antecedent, aka protasis> \rightarrow $q5$ <consequent, aka apodosis> and $q5$
 <alleged fact>, hence it may be the case that it must be the case that $p3$ is the
 sole *explanans* (aka sufficient condition) for $q5$ <conclusion of the muffled
 transcendental argument>
 is the underlying argument form of the following excerpt (analysed
 accordingly):
 “Given the richness and complexity of the system of grammar for a human
 language and the uniformity of its acquisition on the basis of limited and
 often degenerate evidence <underlying consequent, alleged fact, slight
 reformulation of $q5$ > **there can be little doubt that** highly restrictive
 universal principles **must** <muffled transcendental inference> exist
 determining the general framework of each human language and perhaps
 much of its specific structure as well” <underlying protasis, slight
 reformulation of and elaboration $p3$, conclusion of the muffled transcendental
 argument> (Chomsky 1980, 232),
 then $p3$ is seen to be clearly inferred from $q5$ by muffled transcendental
 argumentation.
69. Since the element of doubt (may) has here the upper hand (wider scope), one
 could contemplate choosing ‘down-graded’ or ‘toned down’ (transcendental
 argumentation) as softer type (i) metalinguistic designations. But it would
 neutralise the distinction between plain ‘it may be the case that P ’ and
 somewhat bizarre ‘it may be the case that it must be the case that P .’
 Furthermore, it would sweep under the carpet any suggestion that this have-
 your-epistemic-cake-and-eat-it insistence on maintaining at all costs a
 lingering presence of must in the shadow of may could well be symptomatic
 of a difficulty in refraining a ‘transcendental’ urge. In this context, ‘muffled’
 is in fact a bowdlerised alternative to ‘muzzled,’ which, though more aptly
 conveying the idea that what is more or less grudgingly held back is
 aggressive, potentially over confident self-assertedness, is much too
 terminologically incorrect to be retained here.
70. Two propositions are said to be ‘logically unrelated’ when they are neither
 equivalent, nor contradictory, nor such that anyone of them follows from the
 other.
71. We have seen that Chomsky holds that $p2 \rightarrow q2$, and $p3 \rightarrow q3$. But while
 $q2 = q3$, $p2$ and $p3$ are logically unrelated (hence the partition of P between P'
 and P''), in that, given (restated here for ease of reference)
 $p2$ “the system of principles [guiding the construction of grammar for
 individuals in a speech community] [is] a species property,” and
 $p3$ “powerful constraints are operative restricting the variety of languages,”

- granted the existence of powerful constraints restricting the variety of languages, it does not logically follow that this system of principles is a species property (it could conceivably be a consequence of some non-genetic cause), and thus p_2 does not logically follow from p_3 [hence p_2 and p_3 are not logically equivalent];
- granted the existence, as a species property, of a system of principles guiding the construction of grammars, it does not logically follow that the constraints of this system are powerful enough to restrict the variety of languages (they could conceivably be not restrictive enough to offer more than a fairly open-ended guide for the construction of grammar) and thus p_3 does not logically follow from p_2 either;
- finally, p_2 and p_3 are not contradictory, since there is nothing logically inconsistent in the claim that powerful constraints (*aka* ‘principles’) restricting the variety of languages are a species property (of *Homo sapiens*), and as a matter of fact, the conjunction of p_2 and p_3 is a central theoretical claim in Chomsky 1975.

Therefore, the distribution of truth-values among them turning out to be purely a matter of contingent fact, unrestricted by analytic connections, p_2 and p_3 provide an example of two logically unrelated *explanans* for one *explicandum* [one alleged fact explained, two distinct explanations offered].

72. We have seen that Chomsky holds that $p_1 \rightarrow q_1$, and $p_3 \rightarrow q_3$. But while p_1 and p_3 (as is obvious from context in Chomsky 1975, 10–11) are meant to be two loosely equivalent formulations for one and the same *explanans*,

p_1 “individuals employ highly restrictive principles that guide the construction of grammar”

p_3 “powerful constraints [...] [are] operative restricting the variety of languages”

q_1 and q_3 are logically unrelated (hence the partition of Q between Q' and Q''), in that, given (restated as above for ease of reference)

q_1 “individuals in a speech community have developed essentially the same language” (in spite of “the limitations of” “the fragmentary evidence available,” and without access to any explicit “explanatory theory” that might conceivably have guided them in the acquisition process,”

q_3 “humans are [...] not designed to learn one human language rather than another,”

- granted uniformity of the grammar attained in each linguistic community (in spite of all sorts of difficulties), it does not logically follow that all languages are learnable by humans (it could conceivably be the case that the language uniformly spoken by at least one community is not learnable by any member of at least one other community), and thus q_3 does not logically follow from q_1 [hence q_1 and q_3 are not logically equivalent];
- granted that all languages are learnable by humans, it does not logically follow that all humans are equally good at learning them (it could conceivably be the case that there should prove to be significant discrepancies in the grammatical competence attained from one member

to the next in a linguistic community), and thus $q1$ does not logically follow from $q3$ either;

- finally, $q1$ and $q3$ are not contradictory, since there is nothing logically inconsistent in the joint claim that all languages should be uniformly learnable by the members of a linguistic community, and as a matter of fact, the conjunction of $q1$ and $q3$ is a central empirical claim in Chomsky 1975.

Therefore, the distribution of truth-values among them turning out to be purely a matter of contingent fact, unrestricted by analytic connections, $q1$ and $q3$ provide an example of two logically unrelated *explicanda* for one *explanans*.

[one explanation offered, two distinct alleged facts explained].

73. As summarized by Botha (1981, 35), “the inference from evidence to an explanatory hypothesis” (“variously called “abduction,” “retroduction,” or “regressive reduction”) “is nondemonstrative since it is always possible to conceive of more than one, alternative, hypothesis explaining a given problematic phenomenon.” So that in more technical terms (idem, 30, n 31), with minor notational adjustments (since in his italics-free notations, Botha uses ‘ \supset ’ instead of ‘ \rightarrow ,’ and ‘ \therefore ’ (together with standard tabular disposition for argument forms) instead of ‘ \Rightarrow ’): “th[e] argument form [...] represented by $[(p \rightarrow q) \wedge q] \Rightarrow p$ is [a] fallacy” known as “the fallacy of affirming the consequent,” a “non-valid form of argument,” or again (idem, 35) “the inference from the correctness of an observational prediction to the truth of the hypothesis is non demonstrative.” On “the fallacious inference from B and $A \supset B$ to A,” cf. Alonzo Church 1942a, and 1942c.

On some further background for related issues of induction, ampliative generalisation, abduction, retroduction, prediction, explanation, and demonstrative versus non demonstrative inference, cf. Pamiès 2001, 20–27. On musical-chair graphemic uniform substitution and its relevance for a criticism of formalist attempts at reducing mathematics to scriptural manipulation, cf. Pamiès 2015, 15–17 (2–3) and 241 (note 81).

74. In Kant’s philosophy, the problem with Kant’s attempts at commending his major premises by transcendental proof is that even if he could show, *via* a consideration of its unsustainable consequences, that such or such of his favourite *explanans* is not only “a sufficient[, but] also a necessary condition,” “he did not make clear why it should be taken as the sole such condition” (Wilshire 1992, 20). More generally, the problem with any attempt at transcendental proof is that, given a proposition q , there may always be more than one necessary and sufficient condition for q .

Which does not mean, of course, that, in particular, any *explanans* goes. One might for a second be puzzled by a valid indirect proof like, (taking alleged fact q as sole premise):

1. q
2. $\sim(p \rightarrow q)$ (Indirect Proof)
3. $\sim(\sim p \vee q)$ (2, Conditional Law a)
4. $\sim\sim p \wedge \sim q$ (3, De Morgan’s Law a)

5. $\sim q \wedge \sim \sim p$ (4, Commutative Law b)
6. $\sim q$ (5, Simplification)
7. $q \wedge \sim q$ (1, 6, Addition)
8. Contradiction
9. $p \rightarrow q$ (7, Complement Law c)

[On the definition of indirect proof and the proof-style notational conventions used here, cf. Wall (1972, 44–45), and Pamiès (2001, vol 2, 396–404, Annexe 4). For the rules of inference used to obtain lines 6 and 5, cf. Wall (1972, 39); for all the other (logical equivalence) rules used, idem, 30.]

But such proof does not break amazingly new ground by discovering that, given a proposition q , any proposition p whatsoever is a sufficient condition for q . All it does is, trivially, rediscover that $q \Rightarrow (p \rightarrow q)$, hardly a surprise, since (given the truth function conventionally associated with ‘ \rightarrow ’) regardless of the choice for p , $q \rightarrow (p \rightarrow q)$ is a tautology.

[For the relation between object-language ‘ \rightarrow ’ and meta-language ‘ \Rightarrow ,’ cf. Pamiès 2015, 231 (note 1).]

75. As an indication that Chomsky is just paying lip-service to the epistemologically correct, one may note that, just after having conceded to Stich that “the argument from the poverty of the stimulus is certainly inconclusive” and that, in substance, there may be other *explanans* for Q than P , he immediately derides the idea by suggesting that “it might turn out, for example, that it takes place by black magic” (Chomsky 1980, 267, note 29).
76. From this point of view, couched in confident ‘transcendental proof’-like style, the following excerpt is of crucial importance:

“Consider [...] the question whether cognitive functions are both diverse, and determined in considerable detail by a rich innate endowment. If the answer is positive, for some organism, that organism is fortunate indeed. It can then live in a rich and complex world of understanding shared with other similarly endowed, extending far beyond limited and varying experience. Were it not for this endowment, individuals would grow into mental ameboids, unlike one another, each merely reflecting the limited and impoverished environment in which he or she develops, lacking entirely the finely articulated, diverse and refined cognitive organs that make possible the rich and creative mental life that is characteristic of all individuals not seriously impaired by individual or social pathology” (Chomsky 1980, 45–46).

Generalising as it does from just UG and the faculty of language to the postulation of an open class of innate restrictive schematisms underlying every cognitive faculty apparently transcending experience, its ambition (even though an abridged, smoke-screen version of the argument exists, with a watered-down “we are, I believe, led to the conclusion that” modal qualification for the conclusion” (idem, 41) is to designate in advance what must a priori exist, thus providing, at all stages in the development of the theory, the driving force, rationale and goals for ongoing and future investigation. On this deeply-entrenched mode of apprehending research as the source of an ever-broadening speculative maelstrom, a sort of unstoppable, infernal transcendental machine churning out postulated

universal grammar after postulated universal grammar (“of faces” [*aka* of face recognition], of “scientific theories” [*aka* of the “science-forming [abductive] capacity,” of each of the talents of “the gifted few,” of the appreciation of “literary genres [...] with aesthetic value for humans,” “of accessible systems of music” (Chomsky 1980, 250–52), of the place and role of people in a social world, of aesthetic or moral judgment, of “artistic creativity,” etc.) (Chomsky, in Parrett ed. 1974, 29), cf. Pamiès (1984, in particular, 207, note 68).

77. In derogatory terms, ‘while theories are untrue, the scientific show must go on.’ In laudatory terms, as Norbert Hornstein once put it in slogan form in a talk at GLOW [Salzburg 1984]: “always trust the theory.”
78. We here extend to epistemology the common-place acceptance of ‘suspension of disbelief’ in studies of fiction or stage conventions. So, just as when we set aside our knowledge that animals cannot talk upon reading *Animal Farm* or *Little Red Riding Hood*, or when parents attending a primary-school performance of a remake of *The Three Musketeers* obligingly pretend that this plump kid of theirs, for all his makeshift attire and charcoal moustache and beard, is indeed none other than the Cardinal de Richelieu in the flesh; so, in similar fashion, and even though we all know better, we are supposed to remain poker-faced when claiming, at each relevant time *t*, that each of the schematic figures referred to in note 15 *supra* is a kind of faithful X-ray snapshot of what the intrinsic mental architecture or configuration of the faculty of language really is like.

In a nutshell then, the most interesting feature of Chomsky’s way of qualifying his affirmative answer to (3) is that though he occasionally emphatically re-asserts that such hypothetical claims as his own are highly uncertain, if not certain soon to be proved inaccurate or false (20g), nevertheless he insists that for the time being they should be held to be as good as true, just the way it is done in physics.

79. On a maximally generalised version of the indeterminacy [*aka* underdetermination] thesis, cf. Psillos (2006b, 576): “It is commonly argued that there can be totally empirically equivalent theories—that is, theories that entail exactly the same observational consequences under any circumstances. In its strong form, this claim (let’s call it the Empirical Equivalence Thesis, *ETT*) asserts that *any* theory has empirically equivalent rivals (some of which might hitherto be unconceived. *ETT* is an entry point for the epistemic thesis of total underdetermination: that there can be no evidential reason to believe in the truth of any theory.”
80. Stemming from “a discussion of the difficulties that would arise if we were to attempt to translate the language of a hitherto isolated tribe” (“radical translation”), “the principle of the indeterminacy of translation [...] says that it is possible to compile incompatible manuals for translating one manual into another, all of which fit all observable speech dispositions, and that there is no sense in asking which is the right manual. [As a consequence, since] it is only in exceptional cases that we can talk of the meaning of a single sentence, and when our statements about the world conflict with experience, they do

not do so individually, but as a system, we have [by extension of the principle of the indeterminacy of translation] [...] what might be called the Quine-Duhem conventionalist thesis that **any statement can be held to be true no matter what is observed**, provided that adjustments are made elsewhere in the system.” So that, as further consequence, “Quine took a conventionalist view even of the theses of ontologists” [“as to what there is”], since it is in keeping with the Quine-Duhem thesis to hold that “the integration of established theories, which is one of the aims of scientific work, may lead to any one of many equally satisfactory account of the world, each with its ontic theory, and there is **no sense in asking which is the true one**” (Presley [1967] 2006, 216, 217, 218).

81. As for this celebrated “there is no fact of the matter,” cf., for the substance, the quotes in bold type in note 80 *supra*, and for the *verbatim* original of the excerpt quoted here (in Chomsky 1980, 15), Quine 1969, referenced in Chomsky (1975, 182).
82. On that account, as of 1990, Quine would seem to escape charges of unfair treatment, since by then his views on ‘indeterminacy of reference’ had come to corrode not only Chomskyan theory, but even physics:

“Naturalism [being] the view that [...] it is up to science to determine both what there is (ontology) and how we know what there is (epistemology) [...], Quine maintains that the best current science tentatively and fallibly plumps for a physicalist ontology and an empiricist epistemology. Since he maintains that what a (formalized) theory says there is [its ontology] is determined by the range of values of bound variables of that theory, and since the bound variables of the best current scientific theory of the world (viz. physics) range over both physical objects and numbers, then, given his naturalism, Quine’s physicalism embraces both concrete objects, and abstract objects. He is a scientific realist regarding (observable and inobservable) physical objects and a Platonist realist regarding numbers (or sets). However, in *Pursuit of Truth* (19[9]0) Quine downgrades the philosophical importance of ontology, including physicalism. He does so because of ontological relativity (i.e. indeterminacy of reference). The thesis is that a theory’s ontology can be supplanted *salva veritate* by any one-to-one mapping of it. Ontological relativity [indeterminacy of reference] thus engenders an attitude of indifference toward various equally apt ontologies for a given theory, including physical theory so called” (Gibson [1996] 2006, 220).
83. In ‘Rc,’ by expedience, ‘c’ is not just a mnemonic reminder of the prototype ‘cerebral’ variant, but more generally of the ‘corporeal’ general case. Thus, ‘Rc’ should be understood as an embodiment [*aka* incarnation] in whatever anatomico-physiological substrate (not necessarily cerebral) might prove to be involved. For instance, in the case of the posited mental structure theorised as UG, Rc is far from being confined to the brain. Being *ex hypotesi* innate, under standard current assumptions, its anatomico-physiological location is supposed to be ubiquitously distributed over the whole organism, in the DNA molecules duplicated in the cell nuclei.

84. In a less compact, spelled out formulation: ‘ $R_{(R_m, R_c)}$ ’ <the way a mental representation R_m is/comes to be biologically assigned/allotted an anatomico-physiologico-organic incarnation/embodiment R_c >.
85. *Aka* ‘falsifiable,’ cf. e.g. Chomsky, in Parrett ed. 1974, 47—though Popper himself explicitly preferred ‘refutable’ (‘réfutable’) (cf. Pamiès 2001, 82, note 25).
86. On the historical filiation of this approach, cf. Wilbur Long 1942c, and [angle-bracketed] 1942b, “Mind”: “In contrast to” “the conception of mind in terms of substance” <which viewed the mind, “generically considered, [a]s a metaphysical substance which pervades all individual minds and which is contrasted with matter or material substance”>, “a mind, according to the process theory, is a relatively permanent pattern preserved through a continuously changing process. Leibniz’s doctrine of the self-developing monad signalises the transition from the substance to the process theory of mind.”
87. For a general definition of ‘identity element for an operation,’ cf. Wall (1972, 155–156). On the particular case of the “null string [...], denoted by e ” as “the two-sided identity [element] for concatenation,” cf. Wall (1972, 165).
88. On the definition of “linguistic level[s]” as minimally containing “an ‘alphabet’ of symbols (called ‘primes’) which can be combined by an operation called ‘concatenation’ to form strings”—and otherwise possibly “differ[ing]” in the “complexity” of their “internal algebraic properties.” cf. Chomsky ([1955] 1975, 66, § 4). Even though a ‘linguistic level’ is characterised in *LSLT* as “essentially a system of spelling” [ibid.], for an argumentation concluding that formalist attempts at reducing mathematics to purely scriptural manipulation (either of inscriptions or of graphemes) are untenable, cf. Pamiès (2015, 10–15 (section 2), and particularly 14 (XI).
89. The difficulty of interpreting this excerpt is compounded by the quite unusual acceptance of the original term which we replace here by ‘instantiation’: normally, ‘realization’ and ‘realize’ are free variants for type R_c ‘physical(ly) represent(ation)’—as is the case in the following excerpt from a parallel study of the “visual system” (Chomsky 1980, 227–228): “we might [...] as[k] how the structural principles and postulated elements are actually realized in the physical study of the brain.” But in the passage quoted in the main text, “realizations” is clearly used in a different way and means something like ‘permissible fully fledged tokens of the same abstract universal type,’ hence our emendation.
90. For example, in “we cognize the grammar that constitutes the current state of our language faculty and the rules of this system as well as the principles that govern their operation. And finally, we cognize” “universal grammar,” “the innate schematism,” “the set of properties, conditions or whatever that constitutes the ‘initial state’ of the language learner, hence the basis on which [tacit] knowledge of language develops,” ‘grammar’ ‘universal grammar’ and ‘schematism’ may only be understood as R_m ’s, because “cognizing” [a term introduced, by type (i) decision, as a superordinate of “knowledge” which, contrary to its hyponym, is not confined to accessibility of consciousness] is

defined in such a way that [hence the disambiguation] anything cognized is mentally represented (whether open to introspection or “in the interesting cases, inaccessible to consciousness” (Chomsky 1980, 69 and 70, with some reshuffling).

91. On ‘*explication*,’ ‘*explicandum*’ versus ‘*explicatum*’ and ‘*analysandum*’ versus ‘*analysans*,’ cf. Pamiès (2001, 27, note 64); on ‘*definiendum*’ versus ‘*definiens*,’ cf. Alonzo Church (1942b, 74).
92. In their defence of the innocuity of the systematic Rm/Rc ambiguity in the use of ‘Universal Grammar,’ Riemsdijk and Williams (1987, 5) give away the name of the game when they take for justifying premise the validity of the realist stance in need of justification: “there is no conflict between these two views as long as one takes a ‘realist’ view of linguistic theory [i.e. of Universal Grammar].”
93. [Presupposing note 83.] In the case of UG, Tmc would offer an empirically falsifiable explanatory account of how the universal grammar posited by Tm is Rc-represented in the ‘genotype’ that current biological theory Tc specifies in terms of genes and double helix DNA molecules. In the case of particular competence grammars, Tmc would offer a similar account of how, given the kind of brain that Tc claims that we have, we actually come to have access to the (sound, meaning) pairs enumerated by the formal machinery of Tm generative grammars.
To take the analogous example, an isolated theory Tartl of articulatory phonetics may specify vocalic sounds in terms of positions and movements of our so-called speech organs; and an isolated theory Tacst of acoustic phonetics may specify the same speech sounds in terms of frequency, intensity, simple and complex period sounds, and timbre. In this much better understood domain, by contrast with Tmc, to bridge the gap between articulatory Tartl(Rartl) description and Tacstl(Racstl) description, an overarching theory Tartlacst is easily formulable in terms of currently available concepts (resonator, acoustic filtering and selective enhancement of harmonics) and formal tools (Fourier analysis of complex periodic function). Together with its constructs, this overarching theory, however idealised, incomplete, underdetermined by evidence and destined for obsolescence as it may be, is sufficiently predictive, falsifiable and empirically corroborated to be deemed unquestionably worthy of suspension of disbelief ontological credit. Cf. also, going much deeper into the texture of the corporeal, [47](#) *infra*.
94. On ‘*emergence*’ [presupposing the existence of a hierarchy (or hierarchies) of coexisting ‘levels of reality’] as the spontaneous leap from one mode of organisation of reality to a radically different mode of organisation of the same reality <say, from the sub-atomic to the atomic, from the corporeal to the mental, from the individual to the social, or from individual choices to economic determinants>, cf. Humphreys ([2005] 2006). For an example of work in progress, cf. Petitot 1990.
95. On ‘*supervenience*,’ elaborating on the basic notion that “there is supervenience when and only when there cannot be a difference of some sort A (for example, mental) without a difference of some sort B (for example,

physical),” cf. McLaughlin ([1996, 2005] 2006). On “non-reductive materialism” as claiming that, “while everything [hence in particular the mental] depends on the physical, it does not reduce to the physico-chemical, but rather supervenes upon it,” cf. Campbell ([1967] 2006, 17–18).

For a less elliptic, hinging on the concept of supervenience, but somewhat ‘metaphysical’ view of mind/brain relations—which, in the context of our main text, would seriously need to be boosted into some form of Galilean style scientific account—cf. Papineau (2006, 92–93): “mental properties are functional properties that [may be] realized [not] by [...] physical properties, or disjunctions of physical properties, [but by] [...] some other kind of property that is metaphysically fixed by (supervenes on) physical properties, but not strictly identifiable with them.” Of course, “this denial of type-identity for mental and physical properties means that the mental and physical causes of behavioral effects cannot be strictly identical. However, [...] as long as mental causes supervene metaphysically on physical causes, they are not fully distinct from them, and there is already a built-in explanation for why there should always also be a physical cause (as required by the causal closure of the physical) whenever a mental cause produces a behavioural effect. [In other words,] the denial of type identity creates some space between mental and physical causes, but not so much as to render it mysterious that they are always found hand in hand.”

96. On ‘connectionism,’ elaborating on the basic notion that “connectionism is an approach within cognitive science that employs neural networks [...] as the basis for modelling mentality,” cf. Horgan ([1996, 2005] 2006). On a particular interpretation of connectionism which is irrelevant here, cf. note 100 *infra*. For an example of work in progress, cf. McClelland et al 1986.
97. From inception (Chomsky & McGilvray 2012, 241–242) “canalization” is meant to explain a “surprising” convergence (the way “humans and other organisms seem to develop into a relatively uniform ‘type’ despite different environments, ‘inputs’ and genetic coding”) in terms of “non-genetic physiochemical, ‘processing’ and other constraints” “limit[ing the] set of options” “available” for the development of organisms.” But since “these [constraints], by limit[ing] possible mutations too” may also “limi[t] possible organic structures and operations,” and since, furthermore, we are told that (idem, 45) certain “locality conditions or other efficient computation conditions” may have “contributed to the outcome of language,” even though “probably [they] d[o]n’t have anything to do with language, or even humans, [or] perhaps even biological organisms,” the “channel[ling]” effect of canalisation might not inconceivably be called on as a concept playing some role in a theoretical account of the emergence leaps from the inorganic to the organic and, in the case of *Homo sapiens*, from the corporeal to the mental in general and the faculty of language in particular.
98. For a spelled-out blue print of what would be required for the mind/brain—hardware/software analogy to be fully worked out, cf. [35] and [36] *infra*.
99. It would seem to be Chomsky’s guess that if such a theory Tmc could one day “explain how a structure constructed of cells can have such properties” as

those theorised by T_m, then the theory T_c it would mobilise would “ha[ve] had to undergo [such] radical revision” that T_{mc} would not amount to a “reduction” of T_m to T_c as we know it now, but rather to a “unification” and metamorphosis of T_m and T_c (cf. Chomsky 2000, 106, 107). On “‘body’ [a]s an evolving concept” of theories of the corporeal, cf. Chomsky (1980, 89). On “large-scale reduction [a]s rare in the history of the sciences,” with “even the reduction of biology to biochemistry [a]s a bit of an illusion, since it came only a few years after the unification of chemistry and a radically new physics” having undergone “the quantum revolution,” cf. Chomsky (2000, 107 and 145).

100. Should such a superseding rival theory be constructed which accounts for the same data (identity of coverage) but without having to resort to an autonomous sub-theory T_m with specific constructs and modes of inner organisation (greater paucity of design), then T_{mc}, T_m and strategy [18] would be refuted as being totally on the wrong track, there being no reason at all to postulate <off with [19]> that there exist any R_m distinct from R_c. If such a falsifiable and empirically corroborated theory were available, then it could provide strong support for such reductivist claims as Changeux’s ‘identification of mental objects with physical events’ or ‘physical states’ [“l’identification des objets mentaux à des événements physiques” (Changeux 1983, 334); “des états physiques” (Changeux and Connes 1989, 175)] or Patricia Churchland’s contentions that “mental properties are to be reduced to “neural-network properties.”” (Chomsky 2000, 107, quoting Patricia Churchland). On reductive materialism [*aka* “central-state physicalism”] as holding that “mental states” are “states of the nervous systems,” cf. Campbell ([1967] 2006, 12–13). On a particular “interpretation of connectionism claim [ing] that connectionist models do not really employ internal representations at all in their hidden units (and a fortiori, do not employ internal representations with language-like structures,” cf. Horgan ([1996, 2005] 2006, 444).

On Chomsky’s claim that neither connectionism nor eliminative materialism has been capable of offering any such superseding theory, cf. Chomsky (2000, 104).

101. Than those of Salmon, already quoted in note 35 *supra*.
 102. Cf. the “come to have access to” in note 93 *supra*.
 103. In Johnson-Laird’s terms (1987, 153, echoing Bouveresse) “Even if a grammar describes the intuitions of the idealized native speaker quite perfectly, [...] it is [just] an account of a function that is computed, but not necessarily an account of the speaker’s unconscious knowledge of the language, which may be represented in a quite different form.”
 104. For instance, in the terms of Johnson-Laird’s “ecumenical” views on (and vested interest in) the “proper” division of labour “between linguistics and psychology,” “linguistics aims to specify the function to be computed (from speech to the representation of meaning and vice versa) and psychology aims to specify the procedures by which these functions are computed (the interpretation and production of sentences)” (Johnson-Laird 1987, 152).

105. Such an effective procedure simulating acquisition without taking the corporeal into account would be meant to carry out the algorithm blue print of note(s) 39 (and 13) *supra* at the time of *Aspects*. After the abandonment (cf. Riemsdijk and Williams 1987, 13), of the idea of an evaluation metric (cf. Chomsky 1975, 148) and the introduction at stage VI of the concept of parameter to theorise linguistic variation, it would presumably be meant to fix all the parameters specifying the targeted tacit competence grammar.
106. Cf. Alonzo Church 1942d, “*Petitio principii*” and “Fallacy.”
107. Neither can we find (trying to go one step up in the hierarchy from less to more theorised and/or idealised after having tried in vain to go past the ultimate step down), in the case of physics, any homologue of the (mind, brain) pair. More precisely, if ‘Rphy’ is the physical texture which physics is the ‘Tphy’ theory of <where Rphy tentatively plays the part of the homologue of the corporeal texture Rc which Tc is the theory of>, there is no theory ‘T?’ <playing the part of the (non existing) homologue of Tm> studying Rphy in the abstract that be deemed as worthy of ontological recognition as Tphy, though it is in no way related to the constructs of Tphy <just as recognition-eager Tm is in no way related to any of the constructs of Tc>.

Of course, should there be such a homologue (T?, Tphy) of the (Tm,Tc) pair, then it would be demonstrated that denying parity of ontological status of Tm with T? (and hence, by assumption, Tphy) is a flagrant case of discrimination. It is precisely to establish this conclusion that Chomsky (1980, 189–92) attempts in essence a demonstration of the same form [with, in the phraseology of the controversy alluded to in note 24 *supra* ‘lesser or greater recognized degree of (either physical or) psychological reality’ instead of ‘ontological disdain or recognition’]:

[189:] “Consider the problem of determining the nature of the thermonuclear reactions that take place deep in the interior of the sun. Suppose that available technique permits astronomers to study only the light emitted at the outermost layers of the sun. [...] [190:] Suppose now that an ingenious experimenter hits upon a method for studying events taking place in the interior of the sun, namely, study of the neutrinos that are released by the assumed thermonuclear reactions in the solar interior and that escape into space.”

“Are we now entitled to attribute ‘a higher order of physical reality’ to the constructions that were only postulated before? Not really.” “We can only say that with our more direct and more conclusive evidence, we may now be more confident than before that the entities and events postulated are physically real—that the theoretical statements in which reference is made to these entities, processes and so on are, in fact, true.” And “[since] no empirical evidence can be conclusive. [...] at best, we can settle on one of indefinitely many possible theories that account for crucial evidence, attributing physical reality to whatever is postulated in that theory.”

[191:] “Our investigation of the apparatus of the language faculty, whether in its initial or final steady state, bears some similarity to the investigation of

thermonuclear reactions in the solar interior that is limited to evidence provided by light at the periphery. [...] We [...] try, as best as we can, to devise a theory of some depth and significance [...], testing our theory by its success in providing explanations for selected phenomena. Challenged to show that the constructions postulated in that theory have “psychological reality,” we can do no more than repeat the evidence and the proposed explanations that involve these constructions. [...] We cannot prove [them] to be true.”

[191–192:] “Needless to say, the evidence that supports the linguist’s constructions is incomparably less satisfactory than that available to the physicist. But in essence the problems are the same and the question of psychological reality is no more and no less sensible in principle than the question of the physical reality of the physicist’s theoretical constructions[:]” in either case, [190:] “it is senseless to ask for some other kind of justification for attributing [...] reality to the constructions of the theory, apart from consideration of their adequacy in explaining the evidence and their conformity to the body of natural science as currently understood. There can be no other grounds for attributing [...] reality to the scientist’s constructions.”

For ease of reference, let ‘Rs-ext’ be what is going on at the outermost layers of the sun, ‘Rs-int’ what is going on deep inside the sun, ‘Ts-int’ a theory accounting for Rs-int, ‘Ts-ext’ a theory accounting for Rs-ext, and ‘Ts’ an overarching theory accounting for the relations between Rs-ext and Rs-int. With the help of these short-hand type (ii) notations, Chomsky’s argumentation may be critically analysed as follows:

Since Ts-ext (for lack of access to it) does not take into account, *aka* abstracts away from Rs-int, Ts-ext may be viewed as the abstract study of Rs-int. In other words, on the face of it, Ts-ext could be viewed as being to Ts-int, equivalently, what T? would be to T_{phy}, hence what T_m is to T_c. However, for all its abstracting away from Rs-int, and regardless of the availability or unavailability of any overarching theory Ts <not as much as tacitly alluded to in the above excerpt>, Ts-ext, we are told, should be held by solar scientists to be “not really” less worthy of ontological acceptance than Ts-int. Which, in substance, would demonstrate that, since T_m is a homologue of Ts-ext, it is totally unfair that T_m should be ontologically disdained on grounds of its (strictly analogous) abstractness and structural mc-defectivity.

In spite of this superficial air of analogy, however, short of providing the ‘T?’ example needed, this ingeniously contrived demonstration does not hold.

To show that (Ts-ext, Ts-int) is an instance of the sought-for (T?, T_{phy}) homologue of (T_m, T_c), it ought to be the case that, just as, in its account of R_m, T_m does not resort to any of the constructs of T_c, so, in strict parallel fashion, Ts-ext does not resort to any of the theoretical constructs of Ts-int. But, crucially, this is not the case:

To account for Rs-int, Ts-int resorts to the theoretical constructs of thermonuclear physics (among them, ‘neutrino’). But since a fundamental concept of nuclear physics is engraved in the formulation of its defining type

(b) goals, there is no way Ts-ext could conceivably infer anything having to do with “thermonuclear reactions” from such Rs-ext phenomena as “emitted light” without resorting to constructs of the very same theory mobilised by Ts-int.

So that, upon close examination, (Ts-ext, Ts-int) fails to be an instance of (T?, Tphy), could not be used to rebut charges of structural mc-defectivity, and as a result (*pace* Chomsky), the intended demonstration of unfair treatment of Tm collapses.

The difficulties with Chomsky’s solar argumentation are made worse by the confusing interference of issues of asymptotic progress, obsolescence of supplanted theories, and provisionality of ontological acceptance. In all likelihood, given the powerfulness of the theoretical tools of thermonuclear physics, a theory capable of accounting for Rs-int would also be capable of accounting for Rs-ext. So that, instead of having two coexisting unrelated theories in the absence of any overarching theory <as is the case with Tm, Tc, Tmc>, what we have here is just one theory at time t <Ts-ext> being swallowed at t+i by an overarching theory Ts <of which Ts-int is just a digested part>. So that, in a sense, Ts-int <in fact, Ts>, as viewed in retrospect at time t+i is a closer approximation to the ‘inner workings’ of the world than Ts-ext. But in another sense (hence, presumably, the embarrassed “not really” in the excerpt quoted above), under 22h, at time t, Ts-ext and its constructs are conventionally to be held as just as beyond ontological suspicion as Ts-int (in fact Ts) at time t+i.

108. In the no-nonsense terms of Johnson-Laird (1987, 147)’s matter-of-fact pronouncement, if we “compare its course with, for instance, that of Crick and Watson’s theory of the structure of DNA, “judged by the strictest criteria of scientific achievement, the Chomskyan program has yet to succeed.”
109. For some intimation of the multifarious ways, after the heroic age of “vacuum tubes,” various minerals have been used as material hardware supports by reason of their various properties to construct all sorts of devices “hav[ing] two states,” on transistors as exploiting the properties of semiconductors, first germanium, then silicon; on optoelectronic devices as using the semiconducting properties of “compounds formed by the elements from column III of the periodic table, such as aluminium, gallium and iridium, with those from column V, such as phosphorus, arsenic and antimony;” and on the theoretical and technical possibility, if not cost effectiveness of “Josephson-junction devices” exploiting (*via* “the Johnson effect”) the superconducting properties of “numerous metals [which] completely lose their resistance to the flow of electric current at temperatures approaching absolute zero,” cf (*verbatim* and/or in substance, Scace 1992, 213, 214, 215–216, respectively).

Finally, on the fantastic(al) theoretical perspective [though extreme difficulty (verging on impossibility) of technical realisation] of applying the theory of quantum mechanics to devise “a quantum computer—or, more accurately, the abstract quantum computer that one hopes someday to be able to embody in actual hardware,” cf. Mermin (2007 [for the preceding quote, xii]) and, in a less restrained mode, Corge (2011).

110. But not necessarily by one and the same person. As a result of the division of labour, “one can be a masterful practitioner of computer science without having the foggiest notion of what a transistor is, not to mention how it works” (Marmin 1992, xiii), while the tip-top hardware specialist who, being well-trained in hard-core physics, has a crystal-clear view of all the answers may just as well be totally ignorant of, or uninterested in, the gentle art of compiler programming. [On the electronic function of transistors, cf. Scace 216–219. On the fundamental “properties, states, varieties and behaviour” of “matter” that made the conception and realisation of transistors possible, cf. Charles A. Wert and Howard Kent Birbaum 1992. On compilers specifically, cf. Aho et al ([1986] 2006).]
111. “[À] un niveau encore inférieur au niveau 0,” Tanenbaum ([1990] 1991, 21).
112. On the multi-purpose, open-ended metaphorical use of ‘interface’ in French, cf. Compiègne (2010, 163–164).
113. On Chomsky’s personal, non-committal view on the subject, cf. Chomsky ([1994] 2000, 105): “computer modelling might make some contribution” “to the naturalistic inquiry into the language faculty,” though it has never been a particular interest of mine.”
114. Where the term ‘intentional’ is used by reference to the problem of the elusive ‘aboutness’ of language’ “reintroduced [...] into philosophy by [...] Brentano.” Not to be confused with the “intending” acceptance of the term [having to do with such “specific mental states, events, or processes” related to “action” as “beliefs, judgments, expectations, perceptions, fears desire, and hopes], and not to be confused either with “intension” [roughly equivalent to ‘meaning’: ““creature with a heart” and “creature with a kidney” apply to the same things” (“have the same extension”) but “they have different intensions” (Jacob 2006, 709, quoting Quine), “intentionality is the power of minds to represent, stand for or be about things properties, and states of affairs. The English word *intentionality* stems from the Latin verb *intendere*, which can be used to denote the act of stretching a bow string with the aim of propelling an arrow into its target. In Brentano’s sense intentionality is the mental tension whereby the human mind aims at objects” (idem, 708).
115. Where ‘intentional’ (as opposed to ‘extensional’) may also (cf. note 114 *supra*) be used by analogy with the way it is employed in mathematics, where a function f may either be defined (intensionally) by providing a formal way of calculating all and only the ordered pairs (x, y) such that y is the image $f(x)$ of x by f ; or (extensionally), by giving the list of all and only those ordered pairs [which is possible only when the set of all (x, y) s is finite—so that, even though it may loosely be said to be ‘extensionally given’ when it is apprehended without referring to any underlying generating grammar, strictly speaking, an extensional definition of an (unbounded) epiphenomenal E-language is impossible.
116. However, when, just two years after the publication of *Rules and Representations*, Chomsky’s interviewers (Huybregts and Riemsdijk) asked, after having stressed that “this must have shocked the rest of the world,” what made him think that “the notion of grammar [...] is fundamental, rather than

language, which is epiphenomenal,” he reiterated that “though I would like to say that this seems to me an obvious point, [...] I had never realized it before,” “I never realized that clearly before” (Chomsky 1982, 107 and 108). Of course, Chomsky is not the only one to be prone to selective or reconstructive memory, and he would even seem to be in excellent company, since, according to Prestley ([1967] 2006), “from declaring, in 1947, that he did not believe in abstract entities, [by 1960, Quine] had come not only to accept such entities but also to claim that he had always done so.”

117. This contention has next to nothing to do with the “epiphenomenalist” views of those “contemporary philosophers of mind [like] Frank Jackson and David Chalmers” who, though “shar[ing] Leibniz’s conviction that mental states cannot possibly be physical,” “allow that brain processes cause conscious mental effects but deny that these conscious states then have any converse influence on the physical realm.” So that “by viewing conscious states as “causal danglers” that exert no independent influence on the physical realm, they avoid any conflict with the thought that the causal closure [of the physical domain] leaves no room for anything non-physical to make a difference to physical effects” (Papineau 2006, 92)—the only common denominator being that what is held to be an ‘epiphenomenon’ (language in one case, conscious mental states in the other) is equally demoted to mere (Long 1942, “Epiphenomenon”) “by-product” status.
118. In the same place, Chomsky invokes considerations of “level[s] of acceptability” against the idealisation to a homogeneous external language, thus eroding the competence/performance distinction (cf. 13a and 13c *supra*).
119. One may note that what is thus now rejected was arguably implicit in 16a—with its tacit claim that a speaker who knows “the language [of his “speech community”] perfectly” can only be an “ideal[isation],” one may also note that the reference to a “speech-community” (never mind homogeneous) becoming totally superfluous, the argumentative interest of Q’ (cf. 22b and note 53 *supra*) becomes null; that the idealisation to competence of 15c is radically modified; and finally, any theoretical interest in the subject being lost, that only lip service may be paid to the (non-impossibly non-epiphenomenal) fact that language should be “socially shared” (e.g. Chomsky 1980, 278, n 37).
120. In this version of naturalistic ontology (contrary to Quine’s), (*pace* Katz 1981, 1984 rebutted in Chomsky 1986, 33–36), there is no place for such out of this world entities as sets. A contention which would seem to be as hard to swallow as [40], since, six years after *Rules and Representations*, Riemsdijk and Williams ([1986] 1987, 4, 3) still held [cf. note 16 *supra*] that, just like artificial languages, natural languages exist as sets, the only difference being that the latter are more structured than the former—in that while artificial, formal grammars only generate sets of strings, natural language grammars generate sets of triples (sound, syntactic structure, meaning). Which, given that “in either sense,” “a grammar [...] [whether an Rm or the construct of Tm theorising it] is a ‘criterion for membership’ or a definition of [a] set,” would seem to amount to tacit admittance that part of the knowledge we say a

person has when they know a language is somehow knowing a ‘criterion for membership’ for a set. In other words, it may seem in retrospect that as late as 1986, the view of things of distinguished collaborators prefaced by Chomsky himself arguably still implies that there is an abstract entity (a set) external to the speaker that the speaker has come to gain internalised knowledge of.”

On the non-existence, not of languages as non-epiphenomenal sets, but of sets themselves, Chomsky confesses his annoyance or frustration at having to make do with them as tools for theory construction: “The work that I have done since *The Logical Structure of Linguistic Theory* just assumes set theory.” We know [they] don’t make any sense” because “we don’t have sets in our heads,” “and [I] hope that someday somebody will make some sense of them.” But “if we want a productive theory-constructive [effort], we’re going to have to relax our stringent criteria and accept [them]” (Chomsky 2012, 91). And as result of this partial, reluctant relaxing of criteria, flagrant ontological contradictions surface now and then in the metalinguistic discourse of Chomskyan theory. For instance [cf. 28b], when it is viewed both as an Rm and as the identity element of an algebra, by [19] and [22], as a mental representation, ‘e’ must be granted existence, but being also a set-theoretic entity, by [4] it must be denied existence. For a similar case of theory-internal ontological inconsistency, cf. [57] *infra*.

121. Meinong’s theory covers not only impossible, but also possible objects. It extends to both extents and subsistents [i.e., cf. Hauseur 1942, “abstract and eternal entities, values, universals in a non-mental [...] world”] and allows for some degree of recursivity: “Of possible objects—objects not having a contradictory *Sosein*—some exist and others (for example golden mountains) do not exist. If existence is thought of as implying a spatiotemporal locus, then there are certain subsistent objects that do not exist; among these are the *being* of certain objects and the *nonbeing* of various other objects. Since there are horses, there is also the being of horses, the being of the being of horses, the nonbeing of the nonbeing of horses, and the being of the nonbeing of the nonbeing of horses. And since there is no Pegasus, there is the nonbeing of Pegasus, as well as the being of the nonbeing of Pegasus and the nonbeing of the being of Pegasus” (Chisholm [1967] 2006, 115).
122. However, it can be argued (cf. Chisholm [1967] 2006, 115, 116) that [*pace* Russell], Meinong’s theory does not “violate the law of contradiction” and that “Russell’s theory of descriptions” does not “constitute a refutation,” since it “merely presupposes that Meinong’s doctrine is false,” and “does not provide an adequate paraphrase.”
123. Frege conceived of mathematical notions as out-of-this-material-world abstract entities. And, in order “to construct a semantics for mathematics,” between the mathematical “signs” [*aka* “marks,” *aka* “letters”] in need of “semantic” interpretation and the “entities” in terms of which to interpret them, he introduced, as a “mediat[or],” the concept of “sense” [*Sinn*]. But as he also “viewed a sense as an abstract object,” Frege’s original approach to semantics, being doubly Platonistic, is doubly abhorrent to ‘mentalists.’

hence either to be discarded or (for instance *à la* Fodor) ‘psychologised’ (McGilvray in Chomsky 2012, 251 and 252).

124. “Even if one believed with [Fodor] that a distal cause [or impingement] of the acquisition of a MOP somehow [originally] constituted the external content [sic] of [a] term associated with [a] MOP, ‘an external content,’ introduced in this way would be irrelevant to how a person [subsequently] used the MOP,” the fact would still remain that “it is the acquired MOP that sets the agenda for what counts as the needed patterns and other characteristics of the impingements, [whereas] the ‘real’ distal causes matter very little.” Therefore, instead of considering that “a distal cause [or impingement] of the acquisition [or growth] of a MOP somehow constitute[s] the external content of the term associated with the MOP, “the resultant [acquired] MOP, not some external causing thing or property is the best [and “only relatively fixed”] place to look at for the ‘content’ of the word or concept” (McGilvray in Chomsky 2012, 210).
- For instance, it may of course be argued that a “child/organism will develop a DOGmop from some impingements with doggish characteristics D.” But that is just begging the crucial question of “what counts as a doggish characteristic D”—to which the [nativist, internalist] Chomskyan answer is that “being doggish depends not on [‘out there’] dogs, but on the nature of the internal MOP-production system and what it demands [from] specific ‘triggering’ inputs [to acknowledge them as instances of doggishness]” (idem, 218)—an answer which, presupposing that MOPs “develops [...] according to innate principles,” implies that most of our commonsense concepts, at least, seem to be innate too” (idem, 178–179).
125. For a formal statement of the inference rule of *Modus Tollens* (informally, $p \rightarrow q$ and $\sim q$ entails $\sim p$), cf. Wall (1972, 39).
126. Chomsky’s internalist and naturalistic contentions preclude any idea of technologically-induced cognitive ‘mutations’ resulting from the impact of such artifactual innovations as the graphic notational resources of handwriting and typography that could (and arguably did, cf. Pamiès 2015, 241 n. 83) transcend from without those intrinsic “limits of possible organic development.”
127. “The ‘creative aspect’ of ordinary language use” is “its property [of] being both unbounded in scope and stimulus-free. Thus Descartes maintains that language is available for the free expression of thought or for appropriate response in any new context and is underdetermined by any fixed association of utterance to external stimuli or physiological states (identifiable in any noncircular fashion)” (Chomsky 1966, 4–5).
128. In typical fluctuation of epistemic stance [cf. 22c–22f *supra*], the transcendental self-assertedness of the excerpt cited in the main text is ‘muffled’ [in the sense of 22e *supra*] by the use of “seem” and “perhaps” a few lines before: “There is no doubt that one can progressively limit ‘philosophy’ so that it approaches the null set of interesting questions, or perhaps limit it to questions that while extraordinarily interesting **seem** to lie beyond the domain of inquiry as we can currently conceive them (or **perhaps**

ever conceive them, given the nature of our intelligence), for example ...” (Chomsky 1980, 1979). And essentially the same point is down-graded to more cautious “maybe” in yet another excerpt: “Concerning [...] what’s involved in free choice of action,” “I don’t think there are even glimmerings of” “understanding of what might [...] underlie the phenomena.” “These are aspects of human thought and behaviour which just elude our intellectual grasp at the moment and **maybe** in principle forever” (James Peck, ed. *The Chomsky Reader*, 22).

129. Judging by Kraushaar’s analysis (Kraushaar 1942a, “Freedom”) of Kantian views on “the autonomy or self-determination of rational beings”: “Kant considers the reality of freedom an indubitable, albeit an **inexplicable**, fact, and places it at the fulcrum of his entire system, theoretical as well as practical,” it would seem that Bouveresse was not too wide off the mark when he underlined (cf. note 44 *supra*) the importance of Kantian reminiscences in Chomskyan theory.
130. And before that, Marr and Poggio (1976), cited at the time by Chomsky as “a report on very promising recent research” on “the analytic systems involved in identification of three-dimensional objects under various conditions.”
131. Gehring’s work (2005) “on the eye and the PAX-6 gene” “basically shows” that, for an organism, “the basis for reacting to light” is that “a particular class of molecules,” “the rhodopsin molecules” “happen to have the property that they transmit light energy in the form of chemical energy;” and that, from an evolutionary and genetic point of view, “all visual systems (maybe even phototropic plants) seem to begin with some stochastic event that got [this] particular class of molecules into a cell,” “and after that comes a series of developments which apparently are very restrictive. There’s a regulatory gene that seems to show up all over the place, and the further developments, according to his account, are highly restricted by the possibilities of inserting genes into a collection of genes, which probably has only certain physical possibilities (...) the third factor [cf. Chomsky 2005 and notes 4 and 97 *supra*], which gives you the variety of eyes” (Chomsky 2012, 46).
132. On “Descartes” as having “offered what amounts to a computational theory of vision, one that indicates that the visual system ‘solves problems’ such as determining visual depth by performing a geometric calculation of sorts entirely in the mind, given ‘input’ concerning the degree of convergence of the eyeballs” and why, given his contact-mechanics preconceptions, he nevertheless “insist[ed] that a science of mind that offers sensations of depth and other mental phenomena is out of reach,” cf. Chomsky (2012, 74, note 1).
133. On the way Kant conceived of the mind as configuring the flux of sensations into an experienced coherently structured whole, cf. Toulmin (1976, 379): “Euclidean axioms are required, Kant claimed, not merely for science alone; they specify explicitly cognitive structures (of the mind) that are implicitly involved also—as so called forms of intuition (specifically of space and time)—in the prescientific rational organization of sensory experience into a coherent, intelligible world of substantial objects seen as interacting by

- causal processes.” Given Chomsky’s frequent reference to Marr, cf. also note 44 *supra*.
134. On ‘sensa’ and ‘sense-data,’ cfr. Hirst and Fiedor (1967) 2006.
 135. For the same reason, it is as yet no use wondering how <thereby dissociating ‘processing’ from ‘use’ (cf. 13c *supra*) and accepting Johnson-Laird criticism [note 24 *supra*]> a formalised theory of the ‘adverbial’ use of concepts, if it were available, could conceivably succeed in dissolving the overreach problem into the machine-internal multifold arrays of some (non mc-defective) MSE counterpart of SEMs. For some elaboration on what is tacitly suggested here, cf. 64 *infra*.
 136. Cf. note 148 *infra*.
 137. For a comprehensive presentation of the major theories of the domain, cf.—ranging from Frege to Kamp *via* Montague and Kripke—King 2006.
 138. On issues of (“£u”) veridiction and formal (“S₀”) truth-assignment, cf., *mutatis mutandis*, Pamiès (2001, 261–64 and notes 780–87).
 139. Similarly (among countless examples) in “John took a good look at him, but it was too brief to permit a positive identification,” though we may say that “*it* can refer to the look that John took,” “no one believes that there are looks that a person can take, to one of which the pronoun *it* in the first sentence refers” (Chomsky 1986, 44–45).
 140. In terms of this r-redefinition of the denotation of an antecedent, the principles at work in 55 may be reformulated as (cutting many corners) “an r-expression must be free” (Chomsky 1986, 79).
 141. Without having to resort to Quinean views on use of variables and ontological commitment (cf. note 82 *supra*), thus referring only to Chomskyan type (b) contentions.
 142. Chomsky claims that the problem posed by such [using Ryle’s terminology] “systematically misleading expressions” “is quite a different matter from the issue of fictional or abstract entities such as Pegasus, justice or the set of prime numbers [since] the flaw in the argument or John’s lack of talent are neither fictional nor abstract entities” (Chomsky 1981, 344, n 3). But this is irrelevant for us here, if only because—given that the “principles of pronominal reference” (and associated constraints on co- and disjoint reference) apply in strictly identical fashion to such examples of contrasting pairs as ‘the living-deads would think they are smart to be still around’ versus ‘they would think that the living-dead are smart to be still around’—it seems clear, anyway, that domain D must at least host (in the terms of 42) such Meinongian ‘impossible objects’ as, on a par with that of ‘round square,’ the denotation of ‘living-dead.’
 143. Even though, in this context, the use of ‘individual’ in the excerpt cited in 57 could be argued to amount to tacit partial admittance of the broad redefinition of ‘individual’ proposed (as a corollary of the redefinition of ‘universe of discourse’) in Pamiès 2015, 31 (XXXI).
 144. At a stage when SEM had not yet supplanted LF and the proliferation of levels, components and modules was not so much of a preoccupation, it was suggested that “this step [towards domain D] in the process of interpretation”

- “should be considered to be in effect an extension of syntax, the construction of another level of representation beyond LF, a level at which arguments at LF are paired with entities of mental representation, this further level then entering into “real semantic interpretation” (Chomsky 1981, 324).
145. For a tentative suggestion that there might be “systems of I-beliefs” “on the ‘other side’ of SEM,” cf. Chomsky (2012, 188, note 1).
146. Cf. note 148 *infra*.
147. Bouveresse (cf. note 44 *supra*) would probably have a few remarks to make about this presentation of Cartesian dualism as a ‘scientific hypothesis.’
148. This type (i) designation is coined on a par with that of ‘mc-defectivity:’ 1. a theory T_m of mental representations is held to be [‘mc-defective’] when its constructs [e.g., levels, or rules interrelating them] fail to be theoretically related to the constructs of a theory of the corporeal (and in particular the cerebral); 2. it is held to be [‘mC-defective’] when its constructs [e.g. MOPs, or packages of SEM features] fail to be theoretically related to the constructs of a theory of the conceptual system(s) on the other side of the SEM/CI interface; 3. it is held to be [‘mI-defective’] when its constructs [e.g. domain D or r-quasireferents] fail to be theoretically related to the constructs of a theory of the intentional system(s) on the other side of the SEM/CI interface; and 4. A theory T_m of mental representations is held to be [‘mCI-defective’] when its constructs are theoretically related to neither of the constructs of the theories of the conceptual or intentional systems on the other side of the SEM/CI interface.
149. With Y [described in unanalysed object language] a cloud, a trail in the grass, a noise, a silence, a smell, a hair, a patch of non melted snow, a hallucination, a remembrance, a dingo, a disciple, a shadow, an innuendo, someone there, a look on someone’s face, risen hackles on the back of a cat, etc.
a speaker may conceive of all, none or some of those instances of Y as of a dog, a threat, a wolf, a nuisance, etc.—depending on whether, on some occasion for a given instance his mind/brain is set on the m-fold array of values characteristic of T_{adv} (MOPdog), T_{adv} (MOPthreat), T_{adv} (MOPwolf) or T_{adv} (MOPnuisance) and resulting (so the story goes) in a DOG-ly, THREAT-ly, WOLF-ly or NUISANCE-ly apprehension (*aka* interpretation, understanding or configuration) of that instance on that occasion—with the proviso that it is incumbent on T_{adv} to at least begin to elucidate [under 46, it cannot be done] the apparently totally erratic dialectical interplay of internal and distal determinants [under 46, no such ‘determinants’ can exist] of those decisive experience-shaping m-fold assignments of values.
150. In this respect, crucially, though the predictions of the overall T_{adv} machinery (if such a theory were available) would have to, and could be empirically tested by confrontation to facts of ‘real’ (mis)understanding, none of its inner constructs [and arrays of m-fold numerical value assignment in terms of which X would be dissected] could be directly the object of elicitable intuitions of speakers.

Unfortunately, this central point is obscured in the presentation of McGilvray. In order, presumably, to cover up the emptiness of the suggestions that can be made today, his expository technique is to give an air of familiarity to the cruelly unavailable Tadv (F_{x1}, ..., F_{xn}) specifications by notationally representing this construct as the capitalised conventional spelling of x—so that Tadv (MOPdog), *aka* Tadv (SEMdog) will be allotted the reassuring notation ‘DOG.’ But the price to pay for this deceptive transparency is that it unwittingly fosters the impression, not only that (contrary to the adverbial account) there is a concept DOG hanging around in the head for a ghost in the machine homunculus to apprehend, but also that part of the apparatus of the theory is a pseudo-expliciting formal construct DOG itself in need of explication—whereas this may only be a pre-Tadv, not a Tadv problem.

151. [Leaving out references to stage II work], in an argumentation subsuming the essence of 49-62 *supra*, Chomsky (2000, 132) concludes that “it is possible that natural language only has syntax and pragmatics” (understood as “the study of how this instrument, whose formal structure and potentialities of expression are the subject of syntactic investigation, is actually put to use in a speech community.”) But the point is just the same: as long as the syntactic representations [SEM, r-expressions or whatever] reaching the SEM/CI interface are left dangling with no possibility of showing how well they dovetail with and contribute to other systems relying on to empirical prediction, falsification and corroboration, those representations will remain weird bits of formal ‘storytelling’ about ‘potentialities of expression,’ and not scientific investigation.
152. Which suggests we add two subtypes of type 1 vacuity in the typology of note 21 *supra*:
 a theory is vacuous_{1A} if its self-prescribed goals are unattainable because of the unintelligibility *per se* of (the *locus tenens* of) formal representations; a theory is vacuous_{1B} if its self-prescribed goals are unattainable because of the limits of our species-bound intellectual capacities.
153. Which can in no way boast of the success of Marr’s theory, which followed its own strictly parallel course and in no way drew on concepts or contentions of Chomskyan theory—the “program” of which, nearly another thirty years after Johnson-Laird’s (reajusted) comments (cf. note 108 *supra*) “judged by [basic] criteria of scientific [investigation] has yet to succeed.”
154. Cf., in the least un diplomatic language that they could muster, Pinker and Jackendoff’s comment on “Chomsky’s recent approach to syntax, the Minimalist Program” (Pinker and Jackendoff 2004, abstract), that “it is sufficiently problematic that it cannot be used to support claims about evolution,” and their references to “numerous critical analyses of Minimalism [...] [which] differ considerably in politeness but are remarkably similar in substance” (idem, 23 <page reference to the free upenn.edu prepublication version>).
155. Which may have to do with “the seemingly perpetual ‘treason of the clerks’ as successive generations of generative linguists part company with the

founding father” already noted by Johnson-Laird (1987, 147–48), but is no doubt related to Pinker and Jackendoff’s debunking of the alignment with the last oracle effect: “[m]uch work cannot be taken as empirically vindicating Minimalist hypotheses about the empirical nature of language, but rather as carrying out a mandate to implement th[e] vision of Chomsky” (Pinker and Jackendoff (2004, 23 <page reference to the free upenn.edu prepublication version>).

Chapter Four

1. E.g. Houdé, O. et al. 1998. *Vocabulaire de sciences cognitives*. Paris: PUF; Evans, V. 2007. *A Glossary of Cognitive Linguistics*. Univ. of Utah Press.
2. Consulted works: Kant, E. 1957. *Krytyka czystego rozumu*. Warszawa: PWN; Husserl, E. 1992. *Filozofia jako ścisła nauka*. Warszawa: PWN; Mill, S. 1962. *System logiki dedukcyjnej i indukcyjnej*. Warszawa: PWN; Frege, G. 1977. *Pisma semantyczne*. Warszawa : PWN; Russell, B. 1967. Denotowanie. In: *Logika i język*, J. Pelc. Warszawa: PWN; Prechtl, P. 2007. *Wprowadzenie do filozofii języka*. Kraków: WAM.
3. Consulted works: Ajdukiewicz, K. 1960. *Język i poznanie*. Warszawa: PWN; Carnap, R. 1956. *Meaning and necessity: A Study of Semantics and Model Logic*. Chicago: University of Chicago Press; Montague, R. 1970. *Universal Grammar*. Los Angeles: University of California; Bocheński, J. 1993. *Współczesne metody myślenia*. Poznań: W drodze; Popelard, M.-D., Vernant, D. 1998. *Éléments de logique*. Paris: Seuil.
4. To remain in the sphere of linguistics research, Benveniste writes: “L’essor de la pensée est lié bien étroitement aux capacités des hommes, aux conditions générales de la culture, à l’organisation de la société qu’à la nature particulière de la langue. Mais la possibilité de la pensée est liée à la faculté de langage, car la langue est une structure informée de signification, et penser, c’est manier les signes de la langue” (1966, 74). In other words, thought is not based solely upon the actual structures of a given language, but significantly transcends them.
5. For example the imperfect tense described by Desclés would have two prototype uses just like description (*il faisait beau ce jour-là*) and ground for a past event (*quand elle faisait la vaisselle, quelqu’un a sonné à la porte*). His semantic invariant would be a temporal boundary opened on the right of an interval of instants.
6. Up to a point, Harris defended the similar idea in the 1950s.
7. Médina J., Morali C., Sénik A., 1988, *Philosophie*, Paris: Magnard.
8. Prechtl P., 2007, *Wprowadzenie do filozofii języka*, Poznań: WAM.
9. “Contextual factors in metaphor creation in discourse”: plenary lecture presented by Kövecses at the conference Cognitive Linguistics in the Year 2014. Częstochowa, 15–16 September 2014 (paper in press).

10. Heinz A., 1978, *Dzieje językoznawstwa w zarysie*, Warszawa: PWN; Freeman H., 2008, *Language, Culture and Hegemony in Modern France*, NJ: UB Communications, Parsippany.

Chapter Five

1. Serious personal problems have prevented me from making the corrections which may have improved my text in the eyes of my two reviewers, whom I thank here for their comments. As a compensation, I can only pledge to do justice to their remarks in a future text. One of my reviewers, in his very detailed remarks, deplors that so little is said about semantic studies in France. I must emphasize that the subject of this paper is the *historical* constitution of the Roschian notion of prototype and its transmission to “globalized” linguistics. There have been, of course, many fruitful discussions of the notion of prototype coming from various circles, not only in France (see e.g. Dubois ed. 1991), but in other countries as well, and in this respect, the present paper has left gaping holes and ignored a vast number of studies. For example, nothing is said about typicality in developmental psychology (Cordier 1993). The same reviewer regrets that issues related to structuralist semantics, polysemy, logic and concepts (incl. the medieval universalia) be not sufficiently tackled. Obviously, such issues would take me far beyond the purview of this paper, and their discussion would belong in a thorough conceptual analysis of the notion of prototype, of its nature and role in psychology and philosophy, of its use in linguistics, and of alternative accounts in all of these fields. I have refrained from undertaking such a formidable task not only because it would not have been manageable in a paper of this size and with the desired amount of detail, but also because I am sadly aware of my limitations.

Chapter Six

1. Vendler (1967, 1968) calls them “container nouns.” Francis (1986) uses the term “anaphoric nouns,” Ivanič (1991) “carrier nouns,” and Conte (1996) “anaphoric encapsulators.” Within applied studies, Hinkel (2001, 2004) calls shell nouns “enumerative or catch-all nouns,” and Flowerdew (2003, 2006), Flowerdew and Forest (2014) “signalling nouns.” However, even within this more applied-oriented literature, the term “shell noun,” coined by Schmid (2000), is the one that is accepted and used (see Aktas and Cortes 2008; Caldwell 2009).
2. Cf. Wilson (2000) for metarepresentation, and Noh (2000) for metarepresentation as representation by resemblance.
3. Not all illocutionary nouns are deverbal nouns derived from illocutionary verbs. Some nouns enter the English language before the corresponding verb.

However, most of them are nominalizations of, or morphologically related to speech act verbs (cf. also Schmid 2000, 148).

4. Chapter VIII of Schmid (2000) is the only exception. However, the aim of Schmid's study is to give a broad brush picture of the category of shell noun *per se*. Thus, it is the choice of that author not to delve into a finer-grained analysis of each individual category included in his research.
5. Cf. Vanparys (1996) and Proost (2007) for similar approaches.
6. WordNet is a lexical database for the English language that groups words into sets of synonyms (*synsets*), providing short definitions and usage examples. It is available at <https://wordnet.princeton.edu>.
7. Cf. Schmid and Küchenhoff (2013) for the advantages and disadvantages of reliance and attraction measures.
8. The following notation is used: <0,001 = highly significant difference ***; <0,01 = significant difference **; <0,05 = significant difference **; <0,1 = little significant difference (*).
9. In very simple words, the Euclidean distance between two nodes is merely the distance one would measure with a ruler.
10. Quantitatively, N-BE-zero that has more occurrences than N-zero that. However, one has to consider that 27% of the occurrences of N-BE-zero that are with the noun *guess*.

Chapter Seven

- * We would like to thank Margaret Dunham for her accurate translation and to express our gratitude to the two anonymous referees for their helpful and kind remarks, observations and suggestions. We would like to thank Viviane Arigne and Christiane Migette, the organizers of the International Workshop *Les discours métalinguistiques 2*, who provided much help with the final versions of this manuscript.
1. See, for instance, vol. I: 28–31 and vol. V: 113–121.
 2. The semantic value of the utterance (1) is called ‘mediative’ rather than ‘evidential’; see the discussion on these notions in Guentchéva (1994) and Guentchéva and Landaburu, eds. (2007); other authors (e.g. Dendale and Tasmowski, eds, 1994, 2001) introduced the term “évidentialité” in French with entirely different connotations from the English ‘evidentiality,’ causing much theoretical and descriptive confusion.
 3. The details of the analysis are to be found in Apresjan (1973, 9–00) which we interpret as illustrating an abductive process (Desclés 1996, 2000).
 4. For many years Silvestre de Sacy and then Champollion insisted on the nearly exclusively ideographic nature of hieroglyphs: “I persisted on the wrong track until the evidence of the facts [forced] me to recognize phonetic value for a host of hieroglyphic groups included in the inscriptions decorating Egyptian monuments of all ages” (Champollion: *Précis...*). See the history of Champollion’s discovery in Jean Lacouture, *Champollion, une vie de lumière*, Edition Grasset et Fasquelle, 1988.

5. Indeed, the single linguistic unit ‘is’ (and its equivalents in many Indo-European languages: English, French, Ancient Greek...) denotes several semantic values: identification (*Paris est la capitale de la France* ‘Paris is the capital of France’); an object’s inclusion in a class (*Socrate est un homme* ‘Socrates is a man’); inclusion across classes (*Les hommes sont mortels* ‘Men are mortal’); localization of an object in reference to a place (*Le livre est sur la table* ‘The book is on the table’)...
6. Also see, among others, Benveniste 1960/1966; Lyons 1967, 1968/1970; Rygaloff 1973; Desclés and Culioli 1981...
7. See also Desclés (1990, 21–23).
8. One may find an example of a quest for a semantic invariant in the analysis of the polysemy of the French verb *donner* (“give”) in Desclés (2011).
9. The logical reading of ‘prob (P(a))’ is the following proposition: “there is a real number x between 0 and 1 such that the measurement of the probability of ‘P(a)’ is x ”. The outcome of formula (1) is therefore a proposition which is true if one accepts the premises.
10. One then has the new *formula of a probabilized induction*:
(PI) based on highly frequent correlations (observed facts within a representative sample):
for $i = 1, \dots, n$: prob (P(a_i)) given that one has ‘Q(a_i)’ with a high measure of probability (generally higher than ½), one generalizes by positing: [(∀x) : Q(x) => prob (P(x))]
11. I.e.: ‘q & (q => prob(p))’ or ‘q & (q => p)’ or ‘p & (p => q)’ ...
12. Let us recall here the now classical example where, as in other examples, this use of the future is followed by additional informations:
Françoise, pourquoi donc a-t-on sonné la cloche des morts? Ah ! mon Dieu, ce sera pour Madame Rousseau. Voilà-t-il pas que je l’avais oublié qu’elle a passé l’autre nuit. (Proust)
‘Françoise, for whom did they toll the passing-bell just now? Oh dear, of course, **it would be** for Mme Rousseau. And to think that I had forgotten that she passed away the other night.’
13. For instance, as pointed by de Haan (2008, 73) following Goddard’s description (1912) cited by DeLancey (1990), in Chipewyan (Athabaskan language), “two sets of morphemes appear to distinguish between physical inference and mental inference, although the difference is not always easy to make in practice.”
14. The following abbreviations are used in the examples below: 3: third person; ABS: absolutive; An: animate; Art: article; ASS: assumed evidential; B: gender agreement marker; COP—copula; CS: causative; CUM: cumulative focus; CVant: anterior converb; CVirr: irrealis converb; CVsim: simultaneous converb; DAT: dative case; DIR: directional; DX: deictic prefix to verb; ERG: ergative case; F: feminine; FOC: contrastive focus clitic; FUT future tense; GEN genitive (case); IMM: immediate; Impf: imperfective; Inan: inanimate; INFER: inferential; Inter: interrogative; J gender agreement marker; M: masculine; NEG: negative; NF /nf: non-feminine; Nmzr: nominalizer; NONVis: non-visible/ non-visual; NONWITN / NW:

nonwitnessed; OBJ objective case; OBL- oblique; PAP: past active participle; PERF: perfect; Pf: perfective; PL: plural; PRES: present; PROG: progressive; PPL: participle; PST: past; Rec: recent; REC.P recent past; Refl: reflexive; SG: singular; SPEC: specific; SPEC.INFER specific inferred; Tr: transitive; V: gender agreement marker; VIS: visual; UWPST: unwitnessed past; WP: witnessed past tense.

Interlinear conventions:

- : boundary for major derivational morphemes (verb prefixes, valence affecting suffixes, and nominalizing suffixes); = clitic boundary; &: coordinating or chaining clitic particle interlinearized =&.

15. In the Panaré mythology, Mareoka is the ancestor who created nature, animals and objects.
16. It is interesting to note that this same suffix *-hpë* can combine with noun bases, some of which have no independent existence (Mattéi-Muller 2007), to signify ‘trace,’ ‘remains’ or ‘resultant state’: *pata* ‘foot’—*patahpë* ‘footprint’; *pana* ‘ear’—*panarīhpë* ‘with a cut ear, deprived of an ear’...
17. The original glosses and translations have been kept. Note, however, that some translations are not necessarily in line with the approach advocated here.
18. Aikhenvald (2003, 294) further specifies that the ‘generic’ is used for “information obtained by reasoning or common sense through observing evidence of an event or a state without directly experiencing it.” However, it is “preferred in some traditional stories,” whereas the ‘specific’ is used with “information obtained through observing direct evidence of an event or a state.” She further notes that neither marker is used in the present.
19. Contrary to Hengeveld and Hattner (2015), we do not consider a statement such as ‘It smells like cooked bread’ to express a deduction.
20. On this subject, see our analysis of the admirative in Guentchéva (1990).

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