CAUSATIVES AND THE EMPTY LEXICON: A MINIMALIST PERSPECTIVE

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PREFACE

This work grew from two observations. The first was that there was something deeply wrong with inferential role theories of lexical concepts. These problems had something to do with "compositionality": lexical concepts are required for the construction of more complex, phrasal concepts, and inferential role theories cannot fulfill that role: something else was needed. I thus originally planned to study the relation between lexical semantics, and inferential role semantics in particular, and the principle of compositionality, defending what has been called the "atomistic hypothesis" of lexical concepts. According to this hypothesis, the level of lexical expressions in language(s) falls quite close to the point at which semantic features, otherwise expressible with complex expressions (e.g., definitions), dry up, so that lexical concepts (meanings of single morphemes) become semantically unanalyzable.

This research problem was further suggested to me by the fact that my thesis adviser, Professor Gabriel Sandu, was also involved in the study of compositionality. The execution of this plan, in the form of a discussion of atomistic theory, is reported in the first two chapters of this thesis.

In going through the objections raised against the atomistic lexicon, I noted that linguists had made pretty strong claims that, unless we assume lexical semantic decomposition, we miss something important in terms of how our language and thought work, at least whith respect to what is known as 'explanatory adequacy.' This is, in my view, one of the most severe problems that the atomistic theory is faced with. It is also a problem that could, it seems, be tackled empirically.

I thus became involved in these linguistic matters, with practically no intuition about the outcome. What I understood when I began to penetrate this complex of issues was that *causatives* represented a particularly important type of linguistic evidence with respect to this controversy. I thus ended up studying them in an attempt to find a way to construct linguistic explanations (not descriptions or stipulations) from a theory that contains the atomistic lexicon as one of its components but does not use e.g. stipulative meaning postulates. This work is reported in the two subsequent chapters of the thesis (Chapters 3 and 4). These chapters comprise the bulk of the study, as from very early on I decided to concentrate on the problem of causatives, leaving many other problems of atomistic theory for further study. My hypothesis is that causativity is part of the 'logical syntax' of a

single sentence rather than being an aspect of its lexical semantics. Some additional questions concerning atomistic grammar are touched on in Chapter 6.

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ABSTRACT

What is the constitution of the meaning of morphemes (lexical concepts)? According to most theories, such meanings have a molecular or holistic internal structure: prototypes, exemplars, semantic networks, complex schemata, scripts, and even classical definitions. Recently, however, contrary opinions have arisen in cognitive science suggesting that lexical concepts are not semantically structured. Let us call this theory "lexical atomism."

It is argued in this thesis that, once certain conceptual issues have been clarified (Chapter 1), lexical atomism might indeed provide a more suitable alternative (Chapter 2). The theory is nevertheless problematic in that, among other things, most theories of grammar apparently require a decompositional account of the lexicon, and the atomistic version offers too much stipulation rather than explanation. This problem is solved in this thesis by providing a version of the minimalist grammar that encompasses the atomistic lexicon, does not use meaning postulates, and suggests a solution to certain problems in minimalist theory (Chapters 3, 6). It is then shown that this proposal suffices to explain the key properties of causatives without decompositions (Chapter 4). The hypothesis put forward in this study is that causativity is part of the 'logical syntax' of a single sentence rather than part of any of its lexical elements.

Contents

LIST OF ABBREVIATIONS

A-position	argument position
A′-position	non-argument position
ABS	absolutive Case
ACC	accusative Case
^{0}ACC	zero-accusative
AgrO	agreement projection (object)
AgrS	agreement projection (subject)
CAUS	causative
DAT	dative Case
DO	direct object
DP	determiner phrase
FUT	future
I	inflection (T + Agr)
IP	inflection projection
INE	inessive Case
IO	indirect object
LF	logical form
LLF	lexico-logical form
LGB	Chomsky (1981a)
LOC	locative Case
NOM	nominative Case
NP	noun phrase
PASS	passive (morpheme)
PAST	past tense
PP	prespositional phrase
PRO	anaphoric empty pronominial
pro	empty pronoun
SEM	semantic Case
T	tense (/aspect)
TP	tense projection
VP	verb phrase
X^0	lexical head
X′	intermediate projection
XP	maximal projection

Chapter 1

Introduction

1.1 Compositionality, grammar and causatives

A grammar determines the relation between sound and meaning. Given that truism, what are the principles that relate sound and meaning, say, in the case of human language(s)? One candidate is the principle of compositionality, requiring that the meaning of every complex expression is determined by the meanings of its constituents, their syntactic mode of combination, and by these only. For example, the meaning of the expression *brown cow* must be determined by the meaning of *brown*, the meaning of *cow*, and the syntactic rule $[_{\mathrm{NP}}\ [_{\mathrm{AP}}\ \ldots]\ [_{\mathrm{N}}\ \ldots]\ ]$ under which they have been combined.

This principle has provoked a lot of discussion in recent years, and several aspects of it are currently under scrutiny. I will begin by reviewing these, and will then focus on the more specific problem discussed in this thesis.

Firstly, the fact that the principle was important in guiding the development of certain influential semantic theories, such as those of Tarski and Montague, might have lead to specific assumptions concerning semantics that are dubious on various grounds. This is the position argued most forcefully by Hintikka and his associates (Hintikka & Kulas, 1983, Hintikka & Sandu, 1999, Hintikka, 2001) and some linguists (Chomsky, 1975), who have argued in favor of non-compositional semantics of various types. According to these theories, the meaning of a complex expression is not (always) determined by the meanings of its constituents and their syntactic mode of combination

(only).

The second question arose more or less as a reflection of the first problem, when it was proven that for each such non-compositional semantics it is possible to develop a compositional version that is in a certain important sense equivalent to the former, meaning that in addressing the pros and cons of different semantic systems it ought not to be the compositionality principle that matters (Hodges, 1998, Zadrozny, 1994, 1997; see Janssen, 1997 for a review). In fact, one could conclude that the principle is, in a sense, "empty," meaning that, from a purely formal point of view, it does not matter whether one assumes compositionality or not.

That debate still continues, reflecting the deeper and more general problem of determining precisely what role the principle of compositionality should and ought to have in the formulation of semantic theories, in the domain of both mathematics and the empirical sciences.

There is a third issue that has to do with the principle of compositionality and which, at least when considered only casually, seems to differ from the two mentioned above. Fodor and Lepore used the principle to argue that meanings of morphemes must be semantically unstructured,[1] contrary to the currently prevailing assumption in cognitive science(s) that they are semantically structured.[2]

Although Fodor and his colleagues use the term "compositionality" in their arguments, it is not clear what they intend this word to refer to if compared to the usage of logicians and linguists. It indeed turns out that quite a different principle - and quite a different host of issues and problems - is at

[1]See Fodor (1975, 1981a, 1998a, 2001), Fodor, Garrett, Walker, & Parkes (1980), Fodor & Lepore (2002), Kintsch (1974), Margolis (1998), among others. Yet proponents of the atomistic position are still few if compared to those developing any theories of inferential role semantics.

[2]Internal structures could be analysed in terms of prototypes (Hampton, 2000, Kamp & Partee, 1995, Searle, 1958, Smith & Medin, 1981, Rosch, 1973a), complex cognitive schemata (Barsalou, 1992, Kintsch, 1998), definitions (Aristotle, see Charles, 2000, Weitz, 1988; Carnap, 1937), images or image-prototypes (Rosch, 1973b, Russell, 1919), semantic networks (Findler, 1979, Quillian, 1968), family-resemblance (Wittgenstein, 1958), recognitional verificationist capacities (Peacocke, 1992), even in terms of a whole theories (Gopnik, 1988, Gopnik & Meltzoff, 1997, Keil, 1987, Murphy & Medin, 1985) or conceptual roles more generally (Block, 1986, Cruse, 1986, Field, 1977, Harman, 1982). Virtually every theory in this list belongs to the class of inferential/conceptual role semantics, since they construe concept identity in terms of its relations to other concepts.

Clearly the claim in this study, or elsewhere, could not be that lexical concepts lack an internal structure: it claims that they lack internal structure in terms of relations to other concepts.

stake in Fodor's case. For instance, the principle of compositionality, which is a restriction on the determination of the meanings of complex expressions, in itself says nothing about the meanings of primitive vocabulary (e.g., Horwich, 1998, p. 159-160). Fodor and Lepore have, of course, recognized this problem, writing that "the main problem of lexical semantics" is that "though we're sure that language is compositional, we don't know what the claim that it is amounts to" (Fodor & Lepore, 2002, p. 119).

Therefore, I will unpack this usage of the term "compositionality" as carefully as I can, showing where the difference lies, what consequences it has, and how this double usage of the same terminology causes confusion that could and should be avoided.

Very roughly, Fodor and Lepore use the term "compositionality" to refer to the construction of complex concepts in a way that is, in a certain sense, empirically sound (namely, it explains properties of the constructing of complex expressions such as systematicity, productivity and reverse systematicity). This principle is only weakly connected to the compositionality principle in the more traditional sense cited above. When the terminological aspects are clarified, however, it turns out that Fodor has been right: "compositionality," in one sense, places severe restrictions on the theory of (lexical) meaning.

I will recapitulate Fodor's arguments briefly, keeping an eye on the conceptual clarifications obtained earlier, and will end agreeing with his theory of a 'semantically unstructured lexicon.' The reason I wish to go through these arguments here once more is that lexical atomism, although entirely plausible in my opinion, clearly represents a minority view. For example, the entry on "concepts" in *The MIT Encyclopedia of the Cognitive Sciences* (1999) does not even mention it. I have the impression that it strikes most linguists and psychologists as plain wrong, but this negative attitude might emerge, I believe, not so much from the empirical content of atomistic theory, but from a misunderstanding of what is at stake.

Taking all that as an introduction to the main problem of my thesis, I will then turn to some of the problems inherent in Fodor's atomistic position. Some of these are discussed without any pretense of offering a solution, the idea being merely to note their existence and possible relevance to the issue at hand. Instead, I will concentrate here on a problem that could be called the *problem of explanatory adequacy*: lexical atomism appears to be a true non-starter from a linguistic point of view, as argued by several linguists.

Indeed, only few linguists, if indeed there are any, have accepted lexical atomism as their working hypothesis: most theories conjecture that "semantic decompositions" are part of the identity of lexical elements. This reflects the fact that, compositionality, systematicity and productivity aside, there is linguistic evidence that strongly suggests, or so it is argued, that lexical elements must have a semantic decomposition so that they are constituted, at least in part, by a set of analytic inferences.

The empirical evidence is then, in short, contradictory: one strand pointing lexical semantic decomposition, another to atomistic lexical entries. Both of these appear to me initially plausible, which thus creates what I believe to be a sufficiently interesting and important problem to merit a study of this magnitude.

After making sure that Fodor's evidence is reliable, its conclusions valid and the concepts used in the argument are sufficiently well-defined (Chapter 2), I will show how to incorporate atomistic theory into the recent Minimalist Program (Chomsky, 2000b, 2001) in such a way as not to pose any undefeatable threat to the demands of explanatory adequacy. Secondly, I will suggest how and why this theory can solve certain problems currently under study inside the Minimalist Program, exploring its consequences and some new problems it invokes (Chapter 3). I will argue that, contrary to what most linguists seem to believe, lexical atomism might be an interesting and useful empirical hypothesis. More specifically, I will first show how the proposal can solve the problem of explanatory adequacy in connection with *causatives*, as it is this class of linguistic expressions that has evoked the most forceful objection to the atomistic lexicon (Chapter 4). Beyond that, I will argue that the hypothesis gains independent support as well, since it can be used to solve a number of other, currently open problems in linguistic theory (Chapters 3, 6).

The very idea of minimalism, to turn to the linguistic part of this thesis, is easy to explain, although it is much more complicated to execute in practice. It is clear that some properties are necessary in a grammar of human language(s), such as that it must interact with meaning and speech production, and that it must involve productive recursive processes. Let us call the former "legibility conditions." The minimalist programme (Chomsky, 1995, 2000b, 2001) explores the possibility that this is also sufficient (Strong Minimalist Thesis), and that many of the previous assumptions covering the computational processes of human language(s) (C_{HL}) are in fact redundant,

C_{HL} being "perfect" in one sense or another. It is also assumed that C_{HL} is "optimal" in terms of some cost function defined over its computations or complete derivations.

Following these broad guidelines, I assume, like many others before me, that (1) X-bar theory is redundant and reduces to the "bare phrase structure" (§3.3); (2) there are no purely formal heads and projections (Agr-less theory); (3) the basic relation of agreement is represented by *Agree*, *Move* being instantiated by additional morphological requirements (§3.5); (4) language-specific variation is explained in terms of overt morphological properties of the lexical elements, and (5) optimality must be local in nature.

Other and more controversial assumptions are as follows: (6) the thematic argument structure itself is not part of any lexical element, but emerges from the principles of their mutual interaction (§3.3, 4.3); (7) the notion of causative vP projection is simplified (§3.3); (8) the Strict Lexicalist Hypothesis is revised by deriving, not stipulating, uninterpretable formal features in the lexicon, such as categorial features and structural Case (§3.6, 3.7); (9) the theory is presented in a representational (Bouchard, 1995, Brody, 1995) not in a derivational manner, although some derivational aspects remain; finally, (10) morphology is given an even more crucial role than it has had in many previous proposals (§4.5, 4.9, 6.4) and, (11) following Chomsky (2001), head movement and related evidence concerning word order is excluded from narrow syntax and assumed to be part of phonology and, more importantly, the process of linearization (§6.1). All these properties are minimalist in that they follow from the "legibility conditions."

Finally, I assume that the lexicon is semantically unstructured (§2) - a property that is itself minimalist in spirit, though not in terms of the Strong Minimalist Hypothesis.

Of these, the status of the Strict Lexicalist Hypothesis (8) is presumably the most controversial. I will follow the strategy suggested by Chomsky (2000b, 2001) that categorial features of XPs derive from a functional head F that heads them in configurations F-XP, with the improvement that F is taken to be a cluster of semantically interpretable relational properties of LF (following, in essence, the proposal given in Wunderlich, 1997). Structural Case is derived along similar lines. A 'structural theory' of formal features is thus developed in this study. It postulates, roughly, that we can explain the emergence of purely 'formal features' in terms of structural properties of 'thought,' or whatever linguistical relevant representation carries meaning

(such as Logical Form).

This particular minimalist framework is then applied to the properties of causatives (§4). My hypothesis is that causativity is part of the 'logical syntax' of a single sentence rather than an aspect of its lexical elements. I will show, in particular, that the theory does not need to stipulate meaning postulates in order to explain causative relations at the lexical level. Some additional linguistic phenomena are also discussed in Chapter 6, and it is shown that it is as good as any theory, if not even better if we hold on to the minimalist hypothesis that it explores progressively. Several languages are investigated, together with more controversial constructions such as the much studied Romance causatives.

1.2 Assumptions concerning compositionality

The principle of compositionality is used in various forms for various purposes, some of which are not comparable enough to warrant common terminology. In this section I will discuss such terminological issues, analyzing, first, the traditional sense of the compositionality principle, showing why it is only very weakly connected to issues pertinent to the analysis of language and thought. I then turn to several distinct but related principles such as those of abstractness and systematicity, and the thesis of the internal structure. The so-called "trivalization problem" of the compositionailty principle is also addressed.

According to the most traditional definition of the term, compositionality requires that meanings of complex expressions, called "hosts," are a function of the meanings of their constituents, or that they are "determined" by them:

(1) The meaning of a complex expression is a function of the meanings of its parts and their syntactic mode of combination, and of these only.

There is some discussion about whether the explicit principle of compositionality should be attributed first to Frege: I will discuss this matter briefly at the end of this section. Clearly, (1) is what Tarski (1935/1956) had in mind when he developed a compositional interpretation of first-order predicate calculus, a nontrivial undertaking due to the fact that certain expressions, namely those containing variables, have parts - open formulae -

which do not have meaning and hence whose meaning cannot, strictly speaking, be a function of the meanings of their constituents. To implement (1), Tarski assumed that open formulae *do* have semantic attributes that can be used to determine the semantic attributes of their hosts, ending up with what is today taken to be the "standard" interpretation of the predicate calculus.

Montague (1974) defined compositionality as the requirement of homomorphism between syntax algebra and semantic algebra, both to be understood as "structures" in the mathematical sense, a slightly less general version than (1). Closely related, if not identical, is the definition in terms of syntactic and semantic rules, requiring that for each syntactic rule there corresponds a semantic rule to make these two structures homomorphic (see Bach, 1974).

There is also a slightly weaker way to define (1) by requiring what could be called the "Substitution Condition," according to which substituting constituents of a meaningful expression for their synonyms yields synonymous expressions. Intensional contexts are typical counterexamples of the Substitution Condition and hence also of compositionality (Pelletier, 1994). A language with no synonyms is hence ipso facto compositional no matter what (Westerståhl, forthcoming).

The only difference between (1) and the Substitution Condition is that the latter does not require that the constituents have meaning, whereas (1) requires this since, to be defined at all, a function must have a domain. This is often called the Domain Rule and hence, if we assume the Domain Rule these definitions become equivalent. This equivalence shows how weak principle (1) is: it does not even require an interpretation, since synonymy is both sufficient and necessary. For relevant discussion on the various forms of (1), see Hodges (1998) and Hodges (forthcoming).

The principle of compositionality does not say anything about the type of required function between the meaning of the host and the meanings of its constituents, for it is only its existence that matters. Any additional restrictions, such as that the function must be effective and systematic, should not be confused with (1).

Compositionality must be distinguished from another closely related principle called the *principle of abstractness*. Consider the following statement on the compositionality principle from the literature on cognitive science:

> Nothing is constitutive of the content of a primitive linguistic expression except what it contributes to the content of the complex

> expressions that are its hosts; and nothing is constitutive of the content of the complex expressions except what it inherits from (either its syntax or) the lexical expression that are its parts. (Fodor, 1998b, p. 50).

The latter part is what is captured (arguably) by the functional principle of compositionality: meanings of the constituents determine the meaning of a complex. But what about the first requirement? This stipulates that the content of an expression is *at most* what it contributes to the (infinite number) of complex expressions it is a constituent of. This is not the functional principle of compositionality (1), but a "principle of abstractness," as it is referred to in the technical literature, requiring that if two constituents e and f differ in meaning, then there must exist at least two non-synonymous hosts that differ only in terms of e and f. This is a condition of synonymy: two constituents cannot differ in meaning if they do not contribute any meaning difference to any of their hosts.

Abstractness can be used to "extend" the semantics of a given system. For example, it is possible to begin with the ordinary meanings of sentences, the abstract meanings of their constituents, and, absurdly, even the meanings of the letters in the words. It has been proved that this can always be done, even so as to keep compositionality (Hodges, 1998, Zadrozny, 1994; see Janssen, 1997 for a review).

Principle (1) is not obviously connected to the ability to "understand" the meanings of complex expressions,[3] since it is easy to understand such even if they are not determined compositionally, and vice versa, there are compositional systems that cannot be understood by any finite being, for instance if the meaning function is not effective. For much the same reasons (1) is not connected to language learning: linguistic systems that do not obey the principle of compositionality are equally learnable, while a system that

[3]The following well-known citation serves as an example of an attempt to connect compositionality with understanding: "Since the set of sentences is infinite and each sentence is a different concatenation of morphemes, the fact that a speaker can understand any sentence must mean that the way he understands sentences he has never previously encountered is compositional: on the basis of his knowledge of the grammatical properties and the meanings of the morphemes of the language, the rules the speaker knows enable him to determine the meaning of a novel sentence in terms of the manner in which the parts of the sentence are composed to form a whole" (Katz & Fodor, 1963, p. 482; Katz remarked afterwards that they realised the influence of Frege only later, see Katz, 1972, p. xxiv).

is not recursive but is still compositional is not.

Often the principle of compositionality is connected with 'systematicity' between syntax and semantics.[4] A 'systematic' relation between syntax and semantics is nevertheless obtainable without (1). Compositionality alone does not even entail systematicity. A formal language with semantics may obey what Hintikka has called a "parallelism thesis" - viz., that syntax and semantics go "in tandem" maintaining a systematic relation to each other (see Hintikka & Kulas 1983, §10) - even if the system does not obey the principle of compositionality. Game-Theoretical Semantics is a good example of such a system: it is not compositional, but clearly the relation between syntax and semantics is 'systematic.' I will return to one definition of systematicity later, in section 1.4.

Sometimes the term "compositional" is used in yet another sense than (1), which derives from from Frege but is very common in cognitive science literature. What I have in mind is something like the following:

> Frege's notion of sense, as applied to complex expressions, involves a very narrow criterion of identity. Frege says that the sense of a complex expression, including a sentence, is composed out of the senses of its constituents. 'Composed out of' is a metaphor; but it is used deliberately by Frege to convey something stronger that the non-metaphorical 'determined by.' The value of a number-theoretic function is determined by the arguments of that function; but the number which is the value can be conceived otherwise than as the value of that function for those arguments. To say that the sense of a sentence is composed out of the senses of its constituent words is to say, not merely that, by knowing the sense of the words, we can determine the sense of the sentence, but that we can grasp that sense only as the sense of a complex which is composed out of parts in exactly that way; only a sentence which had exactly that structure, and whose primitive constituents corresponded in sense pointwise with those of the original sentence, could possibly express the very same sense. (Dummett, 1973, pp. 378-279).

[4]Hirst (1987) and Smith, Osherson, Rips, & Keane (1988) define compositionality to mean that meanings of complex symbols are 'systematically' determined by their composition.

The notion of compositionality, or "composition," mentioned above does not have much to do with the relations between language/thought and meanings, but instead is connected to the "narrow criteria of identity." According to this principle, parts of thoughts, or linguistic expressions, are constitutive components of those thoughts/expressions, and that this is (in part) what makes thoughts more fine-grained than their meanings, again a distinct property of compositionality and abstractness. Hosts can be made of constituents in a very literal sense, having an "internal structure," but although compositionality presupposes "constituents," constituency relations may be abstract and need not imply concatenation or the co-tokening of the expressions, for instance (see van Gelder 1990, Hintikka & Kulas 1983, §10 for a discussion; see also the discussion in Fodor, 1998a, pp. 40-42).[5]

If we go back to the main content of (1), one part of the elusive discussion about this principle has to do with the observation that it is, in a sense, a "trivial property." This is so because, given some semantic system - a language L, meanings M and an interpretation μ: E $\rightarrow$ M that is not compositional, it is nevertheless possible to develop another system (E$'$, M$'$, μ') that is compositional and related to the first system in a way that makes the two "equivalent" in a mathematical, if not empirical, sense. Whether the system is compositional or not is thus merely a matter of notation, and lacks substantial content.

Empirical inquiry is another matter altogether, however, since it is targeted on an explanation that is, on the whole, the most elegant, general and simple - somewhat vague but unquestionable properties called for in the domain of naturalist inquiries - and not confined to formal equivalency. Thus, compositional and non compositional systems are not "mere notational variations," but involve substantial questions of this type as far as empirical sciences are concerned. Whether compositionality should be assumed is a different matter: here I claim only that it would not be empirically meaningless to assume it one way or another, given that there might be empirical reasons requiring settlement of the matter.

In short, I define the term "compositionality" as synonymous with principle (1).

[5] According to Fodor (Fodor, 1998b, §9, 10, Fodor & Pylyshyn 1988), a fundamental requirement of the constituency relation when combined with *classical cognitive architecture* is that it is a co-tokening relation.

The rest of this section is devoted to purely terminological and historical issues concerning the origins of the principle of compositionality: readers who do not wish to engage in these matters could well skip the whole passage.

Recently, there has been some discussion about whether or not it was Frege who invented the modern version of compositionality. In *Sense and Reference*, for example, he wrote that "subject and predicate [. . .] are indeed elements of thought; they stand on the same level of knowledge. By combining subject and predicate, one reaches only a thought" (Frege, 1948, this quotation are from Ludlow, 1997, pp. 569-570). These citations support the view that Frege would have accepted, at least during some periods of his career, the principle of semantic compositionality. Indeed, according to Gareth Evans, "Frege was the first to formulate a systematic theory of meaning for a fragment of natural language; systematic in that it sought to provide an explanation of how the significance of complex expressions, particularly sentences, depends upon the significance of their parts" (Evans, 1982, p. 7; Frege surely was not the first to discuss the composition of words into sentences and the relation of this composition to the formation of content, see Humboldt, 1836/1988, §17).

Yet not all authors think Frege adhered to the principle of compositionality. Some suspect he adhered to the principle of context dependency instead (Janssen, 1997, Sluga, 1980). According to Janssen, he might have accepted the principle of compositionality in his later writings, even though he had abandoned it earlier. The source of the debate lies in a cluster of Frege's conjectures, one of the most prominent appearing in *Grundlagen*, that "one should ask for the meaning of a word only in the context of a sentence, and not in isolation." Other similar remarks are scattered around Frege's work (see, e.g., Burge, 1986).

Frege's contextualism could also be interpreted as a methodological doctrine that placed emphasis upon the notion of truth, or even as a direct statement of compositionality. According to this interpretation, the meaning of a word cannot be anything other than what it contributes to the sentences it is a part of: if "you [...] identify the meaning of a word with its contribution to the meaning of the entire proposition, compositionality will hold virtually per definitionem" (Hintikka & Sandu, 1999, p. 226). According to this reading, again, Frege's remark on "contextualism" is actually a statement of the principle of compositionality, or at least it entails it. Similarly, Tyler Burge characterized Frege's methodology along the following lines: "Our interest in

the denotations of words is derivative from our interest in the denotations of sentences. That is, word denotation is important because and only because of the importance of some feature of sentences that is central to logical theory and functionally dependent on word denotation" (Burge, 1986, p. 101). These remarks do not point to the principle of contextuality, and it seems to me that there is no real problem in taking Frege to have at least anticipated (1). As the above citations show, it is not difficult to find textual evidence of this claim. My own interpretation, which I admit must be based on quite a superficial understanding of Frege's writings, is that Frege might have accepted the *principle of abstractness*, in addition to compositionality, by taking word meanings to supervene on sentence meanings.

1.3 Compositionality, natural languages, concepts and logical metalanguages

Compositionality presupposes a notion of "language." For the purpose of this study, especially the latter part, it is crucial to distinguish several senses of the term "language": natural languages, concepts or semantic representations, and logical metalanguages.

By "natural language" I mean *I-language* in the sense of the biologically encapsulated generative mechanism involved in understanding and production of linguistic utterances, to be contrasted with "E-language" which basically refers to an infinite set of expressions. From the perspective of cognitive science, E-language is a highly derivative notion, but on the other hand, it is this notion that is crucial to the compositionality principle in the sense defined in section 1.2. This is so partly because the "semantic interpretation" involved in the case of I-languages is, most likely, highly indirect (see Pelletier, 1994, among others): linguistic expressions are, after all, also *used* in complex social settings to do, and mean, various things.

Several cognitive scientists have argued, on the other hand, that there exists a separate level of semantic representations or concepts, with expressions that are individuated, in part, in virtue of their semantic interpretation. These putative semantic representations are linked with natural-language expressions by some means or another; indeed, one such proposal is presented in this study. These connections are crucial when one speaks of "lexical concepts," for instance, combining one notion ("lexical") from natural language

and one ("concept") from the level of concepts.

Finally, the meanings of natural-language expressions are often analyzed by translating them into a suitable "logical metalanguage." They are often again understood as E-languages, but it is not always clear, or it is left intentionally unspecified, if the logical metalanguage used is meant to be psychologically realistic or not. Expressions written in such a language might be read in the material mode, representing aspects of the *subject matter* itself rather than aspects of the psychologically realistic *representations* of that subject matter.

As an illustration, let us take a lexical element such as $open_{tr}$. It is, first of all, a grammatical lexical element containing a variety of features that are relevant to linguistic computation. Its usage is partly under our voluntary control, and such usage might be subject to intricate social-cultural practises. In poetry, for instance, it can be used in highly novel and ingenious ways, and basically, one could use it in any way one wants to. In a given situation, it might maintain a highly idiomatic, specific, or surprising meaning. That is all about its status as a "lexical element," a formal symbol which is put into use in a social setting. However, despite its usage, it also seems to have a certain "core meaning" which remains identical in an infinite number of linguistic expressions at which it is a constituent part. The intuition is that *open*, first and foremost, expresses some meaning that is different from, say, *close* and which we could, furthermore, categorize by perception or demonstrate by using our body. At that level one speaks of concepts (see Fodor, 1998 for an example).

Finally, opening is an event that happens in the world and bears certain relations to other events. Such facts and their mutual relations can be described in logical metalanguage (2a-b):

(2) a. If Erebus opened the door, then the door opens
b. $\text{Open}_{tr.}(\text{Erebus, door}) \longrightarrow \text{Open}_{intr.}(\text{door})$.

Such expressions are often regarded as "analyses of the meanings" of the corresponding natural-language expressions, a practise that is perfectly reasonable if one keeps it in mind that the thesis that such expressions are not mere descriptions of facts of the world, or descriptions of the "logical structure of the world," requires much argumentation. I will return to this topic over and over again, for the issue of "lexical decomposition" has to do with precisely this distinction.

My own view, which emerges as I proceed, is quite close to Fodor's in that I assume, the finite amount of idiosyncracy aside, that the grammar of natural language(s) reflects grammar of concepts quite transparently, but that the grammar (or "logical syntax") of logical metalanguages, which is often considered to capture the properties of either one, does not in fact adequately reflect many properties of natural languages or concepts.

1.4 Assumptions concerning productivity and systematicity

In this section I will discuss more thoroughly two important principles of languages and thought: productivity and systematicity. I will argue that they are both important and characterize human languages and thought, but that they are not identical in content with the principle of compositionality. A definition of systematicity, as a property that is also relevant to the empirical data discussed here, is given.

A universal property of human language (and thought) is its ability to grasp a 'discrete infinity,' the 'creative aspect of language.' One must nevertheless assume that the mind/brain is finitely constituted and, within a selected range of problems, solves them in a finite time. Then we face the tension of explaining how a finite being is capable of grasping such a discrete infinity. When speaking of any mental resource allowing us to 'grasp infinity,' I will speak of "generative capacity," or on some occasions of "representational generative capacity" if there is semantic interpretation involved.

According to Fodor and Lepore (Fodor & Lepore, 1996, p. 254-255), the study of such generative capacity must involve two components as follows:

(3) (Effective syntax.) A representational generative capacity includes "an effective compositional function, which maps a finite basis of simple mental representations onto an infinity of complex mental representations together with their structural descriptions"

(4) (Effective interpretation.) A representational generative capacity includes "an effective interpretation function, which maps arbitrary mental representations, simple or complex, onto their semantic interpretations."

It follows from these properties that the syntactic and semantic properties of an infinite number of hosts must be determined by some finite set of 'basic axioms.' Since semantic interpretation must be finitely specifiable, it must be sensitive to the structure of mental representations (Fodor & Lepore, 1996, p. 257). Some authors seem to identity "compositionality" with as weak a property as the conjunction of (3) and (4).[6]

Assumptions (3) and (4) are not very restrictive, in a certain sense. I assumed that a human being is a finite being, and that the semantic interpretation must constitute an effective procedure, constituting a finite number of facts, assuming the structural description of mental representations as inputs, yielding an infinite array of meanings as output. According to these assumptions, any piece of knowledge, or any collection of beliefs as long as it is of finite cardinality and the process eventually 'halts,' might enter into the determination of meaning for a representational system. Pelletier (1994), in fact, argues that we should loosen the functional principle of compositionality (1) into the conjunction of (3) and (4) exactly in this sense.

Earlier, the productivity of language was explained by relying upon the Markov models, an old associationist idea now resurrected by many connectionists. Markov models handle infinity by allowing loops, and they are constituted of a finite amount of information. This class of models is already powerful. Hence productivity alone does not lead one to assume *constituents*: additional properties, such as unbounded dependencies, must be taken into account to prove that loops are not sufficient to capture the properties of the generative capacity under study (Chomsky, 1957). This is often, if not always, neglected in literature discussing the prospects of connectionism. However, in order to attain what Kant characterized as the property that "every concept must indeed be considered as a representation which is contained in an infinite multitude of different possible representations" (Kant, 1787/1993, B39) we need to assume that the "effective compositional function" cited by Fodor and Lepore is based on non-causal constituency relations.

It is not uncommon to assume that the principle of compositionality is related to "systematicity." Systematicity, I will argue, is a strong condition imposed on the nature of the meaning function: in other words, it indicates how the meaning of complexes must depend on the meanings their its constituents (namely, systematically), not just that it depends. This is the principle that matters most in the present context.

[6]See Schiffer, 1987, p. 179, Baker, 1988, p. 3, Cresswell, 1985, p. 138.

Prima facie, there are three senses of "systematicity" found in the literature that must be distinguished:

(i) systematicity as a property of behavior to be explained;
(ii) as a property of a representational generative capacity; and
(iii) as a property of generative capacity (syntax).

The general idea in the literature on cognitive science has been to explain the systematicity of behavior in terms of systematic representational generative capacity, although it often happens that one takes systematicity to involve "compositionality."

Systematicity of behavior (i) refers to the fact that cognitive capacities emerge in huge 'clumps': only in a finite number of cases do (certain) cognitive capacities emerge in finite sets.[7] This property alone would seem to correlate with the explosion of grammar, and thus with creativity of language and thought. However, these 'clumps' are not just any clumps, but they are systematic: they are related to each other in their form and content. For instance, learning *Erebus* is not, in the typical case, related to learning *Charon is in the park*, it is related to learning expressions such as *Erebus is in the park*. This characterisation, of course, presupposes an understanding of systematicity.

Productivity already entails one aspect of systematicity, since it requires that the form and content of the hosts and their constituents are related to each other in a way that is effective, thus it cannot be 'random.' This is how the term "systematicity" is sometimes used in linguistics: there is a systematic rule for correlating the meanings of complex expressions with the meanings of their constituents. According to this definition, idioms are not systematic. Many morphological processes, although systematic from a formal point of view, are not systematic semantically.

Sometimes a stronger property seems to be at stake. The idea is, for instance, that sentences containing some constituent, such as *Erebus*, all involve Erebus, the meaning of *Erebus*, in their truth conditions, in some way or another. In Fodor's words,

[7]For example, if the learner learns to use the expression *Erebus*, he or she has often learnt, by the same token, to use an infinite number of other previously unlearnt expressions *Erebus loves Charon*, *Erebus loves Erebus*, *Erebus and Charon love James*, depending upon the catalogue of other expressions the learner has already mastered. Generally, these clumps are astronomical or infinite in extension.

> [...] the things that the expression (mutatis mutandis the concept) 'brown cow' applies to are exactly the things to which the expressions 'brown' and 'cow' apply. Likewise, the things that 'brown cow in New Jersey' applies to are exactly the ones that 'brown' 'cow' and 'in New Jersey' apply to [...] It says (to repeat) that the semantic value of thought (/sentence) is inherited from the semantic values of its constituents, together with their arrangement." (Fodor, 2001, p. 6).[8]

What Fodor is describing here is not only that the semantic value of thought is inherited from the semantic values of its constituents, but that the semantic value of the host is inherited from the semantic values of its constituents in a particularly strong way. One could say that the inheritance is completely "transparent." We could derive such transparent systematicity, I argue, by assuming that the meaning of *black cow* (that is, black cows) is constituted by the meanings of *black* and *cow*, thus by the properties of being black and being a cow, and by nothing else (see Fodor, 1998a, p. 99; Fodor & Lepore, 2002, p. 15). If this were the case, then it follows that the expressions "apply" in exactly the same way that Fodor intended. A non-systematic form of a relation between the constituent and the host, on the other hand, would be such that, for instance, it is Erebus' favorite pet that figures in the truth conditions of a sentence that has *Erebus* as its constituent, assuming that *Erebus* means Erebus; more complex cases are easily imaginable. What has happened here is that the semantic rule for computing the meaning of the complex host from the meanings of its constituents is "complex."[9]

One way of understanding this requirement is to be found in Wittgenstein. According to him, "The reality that corresponds to the sense of the

[8] In Fodor's vocabulary, "[c]ompositionality is the name of whatever exactly it is that requires this kind of thing to be true in the general case."

[9] Mathematicians and logicians have approached the property of "systematicity" from a similar perspective. Their endeavour is relevant not only to the capturing of the content of the property of "systematicity," but also to the fact that systematicity, if characterised differently, could save the compositionality principle (1) from being formally vacuous (e.g., Zadrozny, 1994). The general a priori strategy has been to try to restrict the class of appropriate functions in (1) so that only "systematic" functions are allowed, "systematicity" connoting "simple" or "natural." The nature of such a function and the restrictions that might enter into it depend upon the nature of the totality of meanings. For instance, if it is possible to deal with the relevant function in algebraic terms, one could propose that the function ought to be a polynomial of some degree, or to be as "simple" as possible (Zadrozny, 1997).

proposition can surely be nothing but its components parts, since we are surely ignorant of everything else." (Wittgenstein, 1961, 20. Nov. 1914). In other words, if I interpret Wittgenstein correctly, transparent systematicity can be defined thus:

(5) (Strong transparent systematicity.) (i) The content of the constituent is a constitutive component in whatever is the content of its host; and (ii) the content of the host is constituted by nothing else than the contents of its constituents and their rule of combination.

Constituency on the level of syntax is, according to the principle of systematicity, here interpreted as constitution on the level of meanings, and this is all there is in the constitution of the meaning of the host. By this I mean something like the following. First, if *brown* means being brown, and *cow means* being a cow, then whatever *brown cow* means must be something from which the properties of being brown and being a cow cannot be detached. Second, brown cow is nothing else than being brown, being a cow, plus their logical type of combination. According to this principle, a brown cow cannot fail to be brown, but it cannot fail to be a cow either.

It seems that this is how language basically works, at least to some extent. It is easy to see that this property satisfies the core property of systematicity. Let K be the set of expressions containing *brown* as its constituent. Then each of the entities represented by the expressions in K are constituted by the property of being brown, and hence all truth conditions of sentences involving the constituent *brown* involve the property of brownness. It follows that the word *brown* more or less now also "applies" to whatever it is that these expressions mean. If we take this constituent away, substituting it with another nonsynonymous constituent such as *green*, then the hosts denote different entities, one for something that is brown, the other for something that is green.

It seems that this notion is sometimes associated with "compositionality." However, compositionality states that the meaning of the host containing the constituent *Erebus* depends upon the meaning of *Erebus*, and it does not have to depend on the meaning of *Erebus* in any systematic way. Suppose that *old* means old, *bachelor* means bachelor, and *old bachelor* means caterpillar. This is "unsystematic" behavior, yet I could still imagine a 'contribution,' trivially a sort of compositional dependency.

However, systematicity is a strong principle in the sense that it entails

compositionality: part (i) explicates a certain kind of dependency, part (ii) states that this is the only kind of dependency. It also entails abstractness, since there cannot, then, exist superfluous meaning properties: meanings of constituents all end up constituting the meanings of the hosts, and nothing else constitutes them. On the other hand, compositionality and abstractness do not entail systematicity. Abstractness requires only that for any two nonsynonymous constituents there exists a corresponding nonsynonymous host.

Given these entailments, it is well worth trying to reverse the explanation by assuming compositionality, abstractness, computability and some other, still unknown independent property or several such properties, and to derive systematicity instead. One could claim that this is what Fodor is after in his desire to make 'compositionality' (with its somewhat unknown properties) explain productivity and systematicity. I have not managed to establish such a reductive definition, and I certainly think, though cannot of course prove, that it is systematicity in the sense of (5) that is fundamental.

Systematicity and productivity are important because they are principles that entail a number of peculiar properties of the mind/brain, quite unique in the biological world. Our language and thought are both productive and systematic, even to an extent that has led some authors to claim that thoughts are essentially systematic and productive (Cocchiarella, 1986, Clark, 1991, Evans, 1982 and Peacocke, 1992). However, systematicity is surely an empirical problem as well since, if thought is systematic by virtue of its constitution, then it remains to be explained as how human beings, and presumably other species as well, can entertain thoughts (see Hadley, 1994, Phillips, 1999; see also Braine, 1963, Bever, Fodor, & Weksel, 1965). Evans (1982) argues that, although there may be thoughts (say Martian thoughts) that are not systematic, our cognitive generative capacity - language and thought - does seem to obey it (p. 104, note 22), so that we should take it as an "ideal, to which our actual system of thoughts only approximately conforms" (p. 105). This is the position of Fodor, too, who writes: "Systematicity seems to be one of the (very few) organizational properties of minds that our cognitive science actually makes some sense of" (Fodor, 1998a, p. 98).[10]

[10]The crucial aspect of this principle is, according to Peacocke (1986), not that it bars imperfections in systematicity as such, but only that if, and when, such imperfections appear, they are not themselves conceptual and not part of the theory of concepts (see pp. 50-51).

Systematicity is, to summarize the discussion so far, both a surprising and an undeniable feature found in the biological world: language and thought seem to obey it. However, I do not argue that systematicity alone is sufficient to explain the semantics of conceptual or linguistic representational capacities, since indeed it is not. Logical connectives, insofar as we take them to be 'constituents' of expressions, are presumably not systematic in the sense of (5). Or is that which is not green constituted by the property of being green? Idioms and linguistic constructions such as N-N combinations (*drug campaign*) are clearly not systematic, and there are many others as well. Cnsider a theory that states that all linguistic expressions mean Bill Paxton. This is not ruled out by systematicity (nor by compositionality), since the property of being Bill Paxton (the meanings of the constituents) surely constitutes the property of being Bill Paxton (the meaning of the host). Some pleonasms seem to come to close to this property, such as *yellowish yellow*, so a constituent could mean the same as its host. Hence, I conclude that systematicity is necessary, but not sufficient. I will not discuss these problems or the often complex linguistic phenomena they invoke here since, for the present purposes, it suffices that some fragments of language and thought are productively systematic.

In other words, I do not claim, but nor do I deny, that systematicity as defined in (5) is an all-encompassing organization principle of the language or thought. My sole claim is that a proper semantic theory of language and/or thought must invoke sufficient means to explain the phenomenon of productive systematicity, and it is this that requires one to postulate more abstract atomistic lexical elements. Systematicity is a principle that, although it does not explain everything, explains something important.

Yet another property is what Fodor calls "reverse compositionality." This refers to the generalization that if somebody masters a host expression, he or she ipso facto masters its constituents. That is, if one knows the linguistic properties of *pet fish*, one also knows the linguistic properties of *pet* and *fish*, including their meaning. If this were the case, as it appears to be, then lexical elements are not allowed to contain semantic properties that they do not contribute to any of their hosts. In other words, hosts contain all the information there is to determine the constituents, much the same as in the case of, say, chemical molecules.

Systematicity entails both compositionality and abstractness, but about reverse compositionality? If the content of a host is constituted by the content

of its constituents and their mode of syntactic combination, then whenever one tokens a host constituted in part by its meaning, one must ipso facto token its constituents. Thus, reverse compositionality also follows.

In *Concepts* (1998), Fodor considers the well-known problem, mentioned earlier, that the compositionality principle (that is, principle in the sense of (1)), "can be trivialized" but that "there is a hunt on for ways to make the notion rigorous" (p. 94). Recall that the "trivialization problem" is that any interpreted language that is noncompositional can be provided with compositional semantics such that the two systems are otherwise equivalent. For example, it is possible to design a meaning function that gives some meaning to each letter in the alphabet and determines, compositionally, the meanings of each word and sentence. That interpretation can satisfy abstractness as well. However, this trivialization problem is not serious in Fodor's case since, as we have seen, it is not just principle (1) but also systematicity (5) (i-ii) that count, and the trivialization problem does not ipso fact trivialize systematicity even if it trivializes compositionality.

Consider the meaning of *cat*. Suppose we have an interpretation that assigns meanings to the letters and correctly predicts that the meaning of *cat* is the property of being a cat, along with the rest of the lexicon. Then, according to systematicity, the meaning of *cat* must be constituted by the meaning of *c*, the meaning of *a* and the meaning of *t*, not just that it must be a function of them. However, there is hardly any plausibility in such a claim. For instance, it implies that all entities the name of which contains the letter *c* are constituted, in part, by the same property, viz., the meaning of *c*.

More generally, there are stringent constraints on the axioms that determine the syntactic and semantic properties of the entities of the representational capacity, and thought compositionality alone is a trivial property (you can obtain it always by letting every other parameter vary freely): it, in connection with the rest of the critical properties such as systematicity, is not *ipso facto* trivial (Janssen, 1997).

To summarize, systematicity and productivity are important properties of the representational capacities of man, and possibly to some extent of some of the beasts, that must be explained. They are unique and surprising features of the biological world, produced by unknown evolution and implemented by some kind of neurobiological mechanisms. Thus, that explanation might ultimately invoke some biological or neural properties, but for the present

purposes it suffices to assume (1)-(5), either as descriptive or explanatory principles, meaning that, as there is so much stringent data to back up these principles, they are not likely to be "reduced" away or to lead us completely astray.

It is not claimed here that the notion of systematicity and productivity is all there is in composing complex expressions and their meanings out of their constituents, but its role cannot be denied either: it explains something about the general picture, but allows departures from it.

1.5 A note on the thesis of "internal structure"

Many cognitive scientists claim that thoughts have an internal structure.[11] Sometimes the fact that mental representations, logical forms, or items in a generative capacity have internal structure are confused with claims of compositionality, synonymy, systematicity of the above kind, and effectiveness of interpretation.[12] Rather, compositionality *presupposes* a notion of constituency.

It is useful to divide the assumption of an "internal structure" into two different assumptions. The first is that there are constituents, together with some characterisation of what they are and how we ought to study them (i.e., what constitute their diagnostic properties). This is presupposed practically everywhere in cognitive science literature, connectionists notwithstanding, although here it must be remarked that there is no agreed-upon test to detect constituents of thought as compared to the much more firmly established notion of constituency in the study of natural language(s). The other assumption is that mental representations, say representations of I-language,

[11] For instance, Fodor writes: "Propositional attitudes are complex objects; propositional attitudes have parts [. . .] The constituents of the belief that John is bald include: the part that expresses the property of being bald and the part that specifies John" (Fodor, 2000, p. 15); for classical theories of thought based on this assumption, see Newell & Simon, 1963, 1972, Newell, 1990, Schank & Abelson, 1977; such a position was explored by Bolzano, Frege (senses), Carnap (intensional isomorphism) and some linguists from more modern eras, such as Cresswell (structured meanings) and Lewis (1972), although for different reasons than the ones that primary occupy me.

[12] Here is one statement which, I believe, confuses these aspects: "Any item is appropriately said to have a compositional structure when it is build up, in a systematic way, out of regular parts drawn from a certain determinate set; those parts are then the components or constituents of the item." (van Gelder 1990, p. 356).

are individuated based on these constituents (and their properties).

In sum, the idea of having an "internal structure" is to coordinate assumptions concerning constituency and those concerning identity at the level of syntax. Systematicity and compositionality, in contrast, coordinate constituency and content.

Chapter 2

Lexical atomism

2.1 Introduction

A semantic theory is required to effectively map the expressions in a language, or more generally in a representational capacity, onto their meanings. Needless to say, such mapping must be "correct" with respect to empirical data. Productivity, systematicity and reverse systematicity are obvious properties one likes to explain, but there are others as well. In this chapter I will recapitulate Fodor's arguments showing that systematicity and productivity, together with some other empirical evidence, suggests that the meanings of lexical elements could not be analyzed in terms of other words or phrases, or, in the case of a stronger theory, in terms of other concepts, verbal or non-verbal.

The core idea behind the argument is easy to explain. Think of a word, or any expression, in a language. Since the usage of an expression is, to a great extent, under our voluntary control, the usage of some such individual expressions might be regulated by intricate socio-cultural practises, which are infinitely complex. Such practises are, in turn, easily conceived of as comprising of "semantic features." Hence the *meaning* of that expression is complex and, therefore, is constituted of lexical features and relations; from this perspective lexical atomism looks trivially false. The general idea behind what follows is that such features (of word usage for example) are idiomatic: they are part of a single expression considered in isolation. Another way of looking at language is to consider expressions as both (i) being composed of constituents (compositionality, systematicity) and (ii) being constituents of an infinite number of other expressions (abstractness, productivity). Many

'idiomatic meaning properties' of expressions do not satisfy these conditions, meaning that, if we look at language as a whole, such features are to be explained by invoking another kind of theory, say a non-productive and non-systematic theory of language use. The "compositional theory" provides a theory of basic recursive capacity, whereas the theory of language use, if there were such a theory in the naturalist sense of the term, provides something in addition. Since we know that language is productive, and that the syntax and semantics of an infinite number of expressions cannot be constituted of idiomatic properties, the need for a compositional theory is obvious. Similarly, since language can be used in most ingenious and novel ways, a theory of language use is required as well.

In what followsm the names of concepts are written by using capital letters, and words are referred to by writing them in italics. For example, the expression *pet fish* expresses the concept PET FISH, which means pet fish. PET FISH is a *name*, thus an alternative would be to say that *pet fish* means the concept XYZ, which means pet fish.

2.2 The compositionality argument

Many cognitive scientists and philosophers have claimed that concepts are constituted of "statistical" conceptual roles. For example, concept A might be constituted of concept B such that the conditional probability of being A, given it has feature B, is less than 1. This is the basic idea of the prototype theory. Indeed, it is near certain that organisms have (representations of) prototypes and exemplars, exhibiting correlational (appearance) features in the process of categorization, for example.[1]

Suppose X is a concept and [A X] is an intersective concept, such as PET FISH, formed from A (PET) and X (FISH). Intersective concepts constitute a representative example of transparently systematic and productive concepts.

[1]The 'prototype theory of concepts' is nevertheless more a collection of related theories and ideas rather than a single, homogeneous theory. According to some proponents of the theory, concepts are represented as typical exemplars of the category, while others think that prototypes are statistical averages of individuals that fall within a concept; still others think that a prototype is an image, or that images can constitute one aspect of a prototype. For a classic presentation of different versions of this theory, and of some crucial evidence, see Smith & Medin (1981) and Tversky (1977). My interest here is the more specific claim suggesting that prototypes, bundles of statistical conceptual roles, constitute meanings (concepts)

The problem is that the statistical features of A and X are not related in any lawful way to the statistical features of [A X]s. The fact that a typical pet fish lives in a bowl is in no way predictable from the properties of typical pets and typical fish. Why would the combination of a dog-looking creature (a prototype of pet) and a salmon-looking creature (a prototype of fish) yield a small goldfish swimming in a bowl (a prototype of pet fish)? The derivation or 'composition' of the semantic features of complex concepts cannot be based on the semantic features of their constituents if those semantic features are statistical conceptual roles, because there is no way of *predicting correctly* such semantic features of the complex concepts from the semantic properties of their constituents. Such prediction is impossible, in turn, because the putative typical features constituting the meaning of complex concepts are, in the general case, random with respect to the typical features of their constituents. Thus, in Fodor's words,

> What makes something a typical member of the set of Xs needn't be, and generally isn't, what makes something a typical member of some arbitrary sub- (or super-) set of the Xs. And even when it is, it's generally a contingent fact that it is; a fortiori, it isn't a necessary truth that it is; a fortiori, it isn't a linguistic truth that it is, since, I suppose, linguistic truths are necessary whatever else they are. (Fodor, 1998b, p. 60).

Presumably, the easiest way of seeing why typical features behave randomly is that typicality is based on extensions of the concepts and their intersections. A pet fish typically lives in a bowl because of the fact that a large slice of the extension of 'pet fish' is intersected with the extension of 'lives in a bowl.' However, the fact that the concept PET FISH has the extension it has, and the fact that the concept LIVES IN A BOWL has the extension it has, and hence the fact that their intersection is what it is, are all randomly distributed and not covered by any kind of laws.

On some occasions the prototypes of intersective concepts do correctly predict the prototype(s) of their host(s). This might have led some authors to believe that there is no problem in composing prototypes. For instance, a typical red house could be taken to be a composition of typical red plus typical house. In this and similar cases the world behaves in a way that the prototype theory would predict. This is how we put typical red houses together: namely, by spraying typical red paint over a typical house. However, it is accidental

that this is so. Imagine a world, or a religious country, in which the law allows only skyscrapers to be painted red. Then a typical red house would not resemble a typical house: a skyscraper is an untypical house. Yet despite these facts a typical red house would still be a house painted red.

Thus, the human mind is not restricted to the semantic power available from prototypes and their 'composition,' or to the power of any kind of statistical conceptual roles, but has access to more systematic and more abstract semantics. I will turn to some empirical examples that illustrate the argument further below. Meanwhile, we are left with a theory in which the features associated with a concept cannot be statistical, but they must be jointly *necessary and sufficient*. This theory works well for an infinite number of concepts / linguistic expressions (*red cat*), non-trivially for another infinite set of expressions (*good cat*), and does not work at all for a finite set of expressions, namely, lexical concepts (*good*, *cat*, *red*), since lexical concepts, according to current consensus, lack complete definitions. Nevertheless, definitions are both systematic, productive and compositional.

There is a long tradition according to which the meanings of morphemes are analyzed in terms of other, somehow more primitive concepts, and these features "define" the word in question. Suppose a community of speakers would have it that the complex word *pet fish* was abbreviated to *pish*. Then the meaning of *pish* would constitute a definition, and thus *pish* and its definition *pet fish* would be synonymous. Moreover, both *pish* and *pet fish* would be mapped onto the same underlying complex representation, say [pet fish]. The classical theory that relies on definitions states that this is, in fact, what is true of most lexical concepts: their meaning is given by a definition, and the lexical word itself is some sort of "syntactic abbreviation."

The problem with this proposal is that it has been proven to be very difficult to find complete definitions for most lexical words. Although *pet fish* has a definition, words such as *love*, *knowledge* and *kill* seem to resist complete analysis. It is possible to approximate their meaning by using phrases, such as *cause to die*, but that never seems sufficient. Moreover, it was because definitions were so hard to find that cognitive scientists turned to the prototype theory in the first place. According to the above argument, however, the meanings of morphemes cannot be analyzed in terms of statistical features or by prototypes. What kind of features are they constituted of, then, if not statistical or necessary ones?

A feature is either necessary or impossible, or something in-between, but

there is nothing else to work with. Therefore, one could argue, lexical concepts lack all kinds of semantic features and must thereby be semantically unstructured (Fodor, 1975, 1981a, 1994a, 1998a, 2001, 2003, Fodor, Fodor, & Garrett, 1975b, Fodor et al., 1980). In other words, there is no "conceptual analysis" of lexical concepts, contrary to what almost everybody apparently assumes nowadays.

I will now turn to some empirical issues in order to illustrate the argument with concrete linguistic material. Consider the following examples (from Jackendoff, 1990, p. 102):

(6) a. * a telephone pole was all along the road
b. a telephone pole was along the road
c. the telephone poles were all along the road

According to Jackendoff, the PP *all along the road* at the end of sentence (6)(c) derives from the conceptual constituent *on the road* with the feature [+distributed]. This results in a corresponding change in the respective surface forms, converting *along the road* into *all along the road*. This feature encodes the fact that something is "distributed" over an area, as in the above examples, while [-distributed] means it is placed in an "ordinary location," both with respect to the argument of the function, in this case *on*. Thus, *all along* presupposes that "the subject in some sense extends over the whole space subtended by the object of the preposition" (Jackendoff, 1990, p. 103).

It is certain that there is a difference between the interpretation of the above sentences, and that it might have something to do with facts such as whether something is distributed or not. On this, I agree. However, for this machinery to *explain* the contrast in examples (6)(a-c), the appearance of the feature must be combined with some other constituent in the conceptual structure, in this case *the telephone pole*, the "conceptual" subject. We must therefore include a feature that corresponds to the subject and codes the fact whether the entity represented by the logical subject is distributed or not. In that case, the explanation of the phenomenon results when the two features interact. If the features "match," the conceptual structure results in a semantically well-formed expression (given the correspondence rules which map conceptual structures onto natural language expressions). If they do not, the result is deviant.

We thus need a mechanism to determine the feature [+distributed] for any "logical subject" on the level of conceptual structures (again, it does

not matter how we represent these subjects on this level). If we look at language from the perspective of individual expressions, there is no problem: we may assume that telephone poles are not typically distributed. On the other hand, we cannot always look merely at individual expressions. In order to obtain a compositional explanation, the feature must be derived from its *constituents*. In this case, the meaning of *pole* and the meaning of *telephone* must suffice to determine the fact that, when they are combined under the relevant syntactic rule, what they denote is indeed not distributed. In general, given two expressions X and Y under the relevant rule of combination, we must be able to determine whether [X Y] is typically distributed or not. The problem is that there are no laws which could determine the distribution of features such as [+distributed] insofar as they are typical. For instance, a telephone pole can be distributed if destroyed appropriately:

(7) the thunder and lightning smashed the telephone pole into a thousands pieces; consequently, the telephone pole was all along the road.

This sentence is semantically well-formed, since the telephone pole, which is typically not distributed, could, in appropriate circumstances, be so. Considered from a slightly different perspective, the problem is that one cannot predict that a concept X has a feature [-distributed] just because it contains the constituents TELEPHONE and POLE, since the concept expressed by phrase (8) contains both constituents but the entity is still distributed:

(8) the telephone pole that was smashed into thousands of pieces [+distributed]

Thus, since there is no way to *predict* the typicality of such features from the constituents (viz., whether telephone poles really are typically distriubted or not), the data cited above cannot be explained in the way Jackendoff proposes *if we are interested in the productive and systematic aspects of language.* True, the sentence is understood as anomalous, but that seems to be a matter of non-productive, idiomatic and accidential fact concerning that individual expression, or whatever it refers to - in this case modern telephone poles.

Pustejovsky, who presented strong arguments in favor of a decompositional lexicon involving both definitions and prototypes and uses similar data to support his arguments (e.g., 1998), indeed accepts the fact that the effects of semantic features can be "overruled" in a suitable context. He thus introduces the notion of "semanticality" (corresponding to syntactic grammaticality), which comes in degrees, and claims that the attribution of typical

semantic features to the lexical component explains judgements on such "semanticality." What thus looks like an *untypical* situation from the perspective of truth-conditional semantics (e.g., distributed telephone pole), fails on "semanticality" according to Pustejovsky's theory: from this perspective, Pustejovsky is attempting to explain truth-conditional *un*typicality (or in some cases truth-conditional tautology) by using lexical decomposition and compositionality. According to him, when untypicality is first detected by putative "generative semantic representations," context could then be brought in to help by providing a suitable interpretation. For instance, the fact that a book is typically used for reading can be used to predict a feature mismatch in sentence (9).

(9) ??Erebus began the book by kicking it

Because the putative "semantic system" informs the speaker/hearer that the sentence is unsemantical, the pragmatic module begins to seek an intelligible interpretation. Pustejovsky's point is that knowledge of the world is "transferred" here into the lexical component of the semantic representations depending on its salience (typicality).

Similarly, when semantic representations do succeed in generating well-formed expressions, they are given meanings that represent only typical cases. For instance, if a book is used for reading, and this information is part of the lexical entry for *book*, then a sentence such as *Charon began the book*, according to Pustejovsky's theory, reads 'Charon began to read a book.' This sentence is *not*, however, synonymous with the original sentence. Clearly, the sentence can may mean that Charon began to write a book, to bake a book, and so on. The putative 'semantic system' gives a sentence one interpretation, viz., the most typical or 'default' one, but *there is a more abstract truth-conditional interpretation available as well*. The compositionality argument explained above shows, *if interpreted according to Pustejovsky's assumptions*, that this latter more abstract truth-conditional interpretation, which is *necessary* in any case, is not possible if prototypes are used, hence prototypes are not sufficient.[2]

[2]Whether Pustejovsky's more specific assumptions are correct is another matter. For instance, it is possible that the more abstract truth-conditional semantics is part of the 'generative interpretation' of a sentence and its semantic representations, and that Pustejovsky's mechanism, which produces the 'default readings,' is secondary and relies upon background information - the data Pustejovsky presents does not distinguish these two proposals.

What I will assume in what follows, contrary to Pustejovsky's proposal, is that a putative generative mechanism that provides interpretations of sentences must always get them right - if there is a 'subcomponent' that does not satisfy these goals I will not attempt to characterize its properties. In other words, the meaning of *Charon began the book* should not be 'Charon began to read a book' - if the latter 'defective interpretation' is in some sense 'part of the the former' I do not care: it is neither sufficient nor necessary for the true meaning.

Consider the following suggestion (found in Jackendoff, 1991, after Pelletier & Schubert, 1989, see also Copestake & Briscoe, 1996, Nunberg, 1979). The problem is, to repeat, that features such as [+distributed] cannot be determined from the constituents. Perhaps they are determined by some universal contextual mechanism; call it, in this case, the "universal grinder" that, when given a package of semantic features, crunches them to yield a 'distributed version' of that entity. Thus, observe the operation of the universal grinder in the following examples:

(10) a. I raise lambs (lamb: [-substance])
b. I eat lambs (lamb: [+substance])

The verb *eat* "activates" the universal grinder, at least for some of its complements. The feature [+substance] is thus controlled by the context of the expression, and not determined compositionally. The benefit is that, while *telephone pole* is now in fact two distinct expressions, with or without the feature [+distributed], there is still a 'generative' mechanism controlling the distribution of the features.

It is clear, however, that this does not solve the compositionality problem. The fact that the problem of determining features such as [+distributed] from the properties of constituents is transformed here to the problem of applying the universal grinder. Hence,

(11) a. A one-lime long earth-worm was all along the road
b. the TV cable was all along the road,

neither of which show any trace of the application of the universal grinder. Given the compositional explanation, the triggering of the universal grinder should be predictable from the constituents, but it is not and cannot thus be predicted, for the reasons discussed. The assumption of a universal grinder

could be just a notational variant of the proposal that certain semantic features are distributed among the expressions in a certain way.

Before I offer a clear diagnosis of these problems, consider (12) and its treatment along these lines in Jackendoff, 1997, §3.2 (see also Jackendoff, 1991, Pustejovsky, 1991, Talmy, 1978, Verkyl, 1993):

(12) the light flashed until dawn

The sentence carries a sense of repeated flashing, while there seems to be no such feature present in the lexical items. The appearance of this feature is due to the content of the prepositional complement *until dawn*. Similarly, the following sentence is unambiguous with respect to whether it carries the feature [+repeated] or not:

(13) Bill kept crossing the street

The appearance of this semantic feature is thus the result of "interactions of the lexical meanings with each other" (Jackendoff, 1997, p. 51), exactly as one would have put it with [+distributed]. The process of combining the content of the verb and its complement is called "aspectual coercion." For already familiar reasons, Jackendoff ends up assuming that aspectual coercion is a result of a function that executes the required semantic modifications (i.e. turns an event of flashing into a repeated sequence). The problem then arises as to *when* this function is to be applied. For the purpose of compositional explanation, the application of the function must be executed by virtue of the features of the constituents of the VP (or their conceptual counterparts), in this case by virtue of the features of the verb and its complement. Similar problems now emerge as above. Given some (any) expression as the complement of the preposition *until*, one cannot determine whether to apply some semantic function without basic knowledge of the quite accidental and idiomatic facts of how things happen to be in this world. Thus (14a) comes out as 'non-repeated,' but (14b) carries a different sense, perhaps the sense of repeated playing:

(14) a. the boy played tennis until dusk
b. the boy played tennis until his career was over

It seems that the VP-external subject has some influence too, as can be seen in the following pairs:

(15) a. the plane flew until dawn
b. the flying squirrel flew until dawn

(16) a. the man dove until he died
b. the fish dove until it died

The activation of the function 'repetition,' or the inhibition thereof, is probably at least partly due to the considerable amount of background information amalgamated in the interpretation of the expression(s). This shows again that there are no laws when it comes to typical properties: accidental features of the world that could well be otherwise enter into the determination of these phenomena.

I think the following is a better way of capturing what is going on. First, in using compositional semantics, one computes the meaning of expressionas such as *the man*, *the fish*, or whatever is the "logical subject" of the sentence. Then, given meaning, a considerable amount of background information, say information about fish and men, is combined in the process. The end result, a combination of two kinds of information, meaning and beliefs, nevertheless *presupposes* meaning. One cannot have information about fish without being able to think about them first. My suggestion is, then, that these two components, a theory of meaning and a theory of belief fixation, must be kept distinct. Nevertheless, they are both involved in the "interpretation" of expressions. For instance, assume that a generative capacity that generates 'default interpretations,' as assumed by Pustejovsky, exists. Such a system is neither sufficient nor necessary for the composition of the true, rather than the default, meaning.[3]

It is tempting to argue here that these failures of systematicity and productivity merely represent a hostile attitude towards pragmatics, as if one would not allow any kind of pragmatic knowledge - knowledge of the causal texture of the world - to enter into linguistic analysis. This objection is correct in one sense, but false in another. Pragmatic aspects of expressions, if we admit that the aspects discussed above are pragmatic, are surely not to be excluded from any investigation executed under the title "linguistic analysis." It might even be the case that some of these features result from computational processes in which it is taken for granted that lexical elements are associated with semantic features. The mistake is in assuming that these

[3]It is also possible, but not argued here, that synthetic beliefs rather than meanings are also involved in the computation of linguistic expressions inside the language faculty.

semantic features, or the pragmatic phenomena of the types discussed above, have any bearing on the constitution of the semantic features of the relevant lexical elements. Systematicity and productivity are effectively cutting pragmatics out of such a constitutive role inside the lexicon, never questioning its importance to other phenomena, linguistic or otherwise.

From a slightly different perspective, the point is that, whereas systematicity and compositionality are concerned with generating an infinite number of syntactically and semantically distinct expressions, surely one important property of human language and thought, pragmatics, and the phenomena cited earlier reflect more the idiomatic interpretations of such expressions. There is no question of priority, but simply a difference in the level and type of explanation.

Recently, there has been some discussion on 'compositional idioms' which is relevant to the issue at hand (Jackendoff, 1995, Nunberg, Sag, & Wasow, 1994, Pitt & Katz, 2000). Consider the following noun phrases:

(17) a. a plastic flower
b. a rubber chicken

According to Pitt & Katz (2000), noun combinations like these contain a semantic feature [+imitation], which is not derivable from the constituents (for the reasons given above). Expression (17a) means something like 'an imitation flower made of plastic.' Although such a semantic feature is not determined by the content of the constituents, the paradigm is productive and differs in its semantic properties from idioms such as *kick the bucket*, as well as from "fully compositional" cases such as *swimming mammal*. As feature [+imitation] is not necessary for the interpretation of expressions such as (17b), and cannot be predicted from the semantic properties of the constituents alone, the authors claim that the expressions are ambiguous, one reading corresponding to the "fully compositional" content, and the other corresponding to the "compositional idiomatic" content. The senses of the constituents are subject to a function delivering the extra meaning, in this case via the following 'idiom schemata':

> (Idiom schemata.) $<[_{\text{NP}}\ y\ x]> =$ imitation $<x>$ made of $<y>$ if $<x>$ is a physical object and $<y>$ is a material substance (Ibid., p. 324, p. 423).

The above idiom schema is general enough, so that no separate dictionary entry is needed for all of the complex expressions. This points to the fact that violations of systematicity come *in degrees*.[4] However, the interpretation being optional (both "semantically and logically [sic]"), the question arises, again, as to when exactly such a schema is applied, if not always. For example, *plastic bag* is not interpreted as 'an imitation bag made of plastic.' This is so because, in our society, bags are usually made of plastic.

I am not drawing an *arbitrary* distinction between extra-linguistic facts and semantic facts, moving a lot of the former out of the latter. The fact is that these faculties, the semantics 'inside of our head' and the constitution of the world 'out there,' are really two different entities: for one thing, the semantics is systematic and productive whereas the world is not. If Erebus loves Charon, then it does not always follow that Charon loves Erebus, although if one can *think of* Erebus' loving of Charon, then one can, ipso facto, think of Charon's loving of Erebus as well. The same goes with productivity. If it is the case that snakes are not dangerous, it does not ipso facto follow that rattling snakes are not dangerous. The limitation is not in our thinking, for we could certainly think of any kind of snake as dangerous (productivity); the limitation is in the world.

Suppose expressions that have a variety of semantic features that are not predictable from the constituents are, in fact, semantically ambiguous, so that the actual application of the universal grinder need not be predicted at all (see e.g., Pustejovsky, 1998, p. 301, note 3 for such a proposal and Jackendoff, 1997, §3.3 for a conflicting view). We may assume that lexical semantics is responsible for the generation of the 'total semantic space,' or more philosophically, the "logical space," containing "possible readings" of the expressions, together with mechanisms to single out those that are

[4]What does it mean, exactly, that systematicity comes in degrees, as the above example points out? I think such generalizations arise from some semi-regular aspects of the world. Consider again the above idiom schemata. It is a fact about the world we live in that, in most cases, if one takes some physical object and a material substance and applies the above rule, what results is something very unnatural, sometimes something altogether impossible. This, I believe, is the "law" behind idiom schemata. However, this already presupposes the meanings of the constituents and the semantic function associated with the syntactic combination, hence it presupposes a compositional theory of semantics. The interpretation then involves the notion of "imitation" because, in our current society, we construct a lot of such imitations: it is often the most plausible interpretation, again a matter of pragmatic knowledge concerning accidential properties of the world we currently live in.

semantically odd.

In following this idea in order to see where it leads, let us impose the feature [±distributive] on a primitive item in a "conceptual structure." For instance, returning to the example given earlier, there may be one item, *pole*, which is [±distributive]. The explanation of the semantic oddity of certain expressions also comes out naturally: we might set up rules that tell us which of the possible combinations of features are odd or anomalous, and which are good or semantically well-formed. Now consider (6a) again, repeated here as (18):

(18) *the telephone pole was all along the road.

The fact that this is semantically odd (if it is, barring the contextual overdriving of this phenomenon) cannot be explained by assuming that the telephone pole or *telephone pole* has the feature [±distributive]. The explanation relies, rather, on the disambiguation process. There is no compositional theory to explain why this is so: *the destroyed telephone pole was all along the road* does not seem to come out as odd. Thus, whatever the status of this explanation might be, the phenomenon of semantical oddity, as in the above, if any, is not of compositional origin. It derives from some other source. Again, it must be emphasized that, since the disambiguation process relies upon knowledge of the world due to the fact that the feature under inspection is only typical of the elements referred to in the lexical concept, there are special difficulties in finding any laws to describe the process.

I do not claim that one could not take a telephone pole to be not distributed "as a default." However, that is not a lawful fact, it is a brute fact about typical telephone poles. I do not even claim that this argument violates Jackendoff's theory, since Jackendoff might well be concerned about how brute facts of the world, or prototypes, enter into the determination of semantic deviance!

Consider again the following example (see Pustejovsky, 1995, §9.3; also Briscoe, Copestake, & Boguraev, 1990, Jackendoff, 1992, Levin, 1993):

(19) a. Charon began the book
b. Charon began to read the book

One can begin 'a thing,' the book, as well as an activity, to read. Does the lexicon therefore contain different entries for *begin* + NP and *begin* + VP? If not, how do we explain, compositionally, the semantics of examples

such as (19). How does *begin* + [NP *a book*] yield the semantics of beginning to read a book, viz. an event?[5]

According to Pustejovsky, the lexical entry for *book* contains information about its telic role, that it is used for reading, and in some other cases for writing. It also specifies its constitution (what it is made of), formal features (what it is) and agentive roles (how it came into being). Yet again, such a lexical entry involves semantic features that are only typical. Thus, while Pustejovsky claims that **enjoy a rock* (and related cases) is not well-formed since rock does not have a telic role, *the climber enjoyed that rock* is perfectly understandable. Pustejovsky assumes that in this and similar cases, "rock itself takes on a new meaning, by virtue of having telicity associated with it" (Pustejovsky, 1998, p. 294). How, then, is this 'new meaning' determined compositionally from the features of the constituents? This is the question that matters in terms of compositionality, productivity and systematicity.

Alternatively, as I have pointed out earlier, one could interpret the above lexical entry as an instruction for a "typical interpretation" of certain syntactic constructions, indicating a likely mapping between them, say in LF, and their semantics, explicated at some higher level, say that of "conceptual structures." Thus, at the conceptual level, we construct all form-content possibilities corresponding to *begin to read a book*, *begin to write a book*, . . . , but possibly not *begin a book*, while the LF contains the syntactic construct *begin* NP; the lexical entry thus specifies a default linking between these two forms, each taken from a *distinct* representational generative capacity. This is the position taken by Jackendoff, for instance; insofar as I understand Pustejovsky, this is his position too. However, this involves us in a different theory, one of connecting two representational generative capacities each with their *own*, presupposed semantics. Moreover, some significant part of this theory appears to involve belief fixation rather than meaning. Admittedly, this is a coherent approach as such, yet it is not what I am after in this study: the constitution of a *single* productive and systematic representational capacity that gets the truth-conditional semantics just *right* for all sentences.

Nouns are said to undergo a contextual alteration between a range of senses. The following examples illustrate these effects (adapted from Pustejovsky, 1995, §3.3). *The lamb is running in the field*, *Erebus ate lamb for breakfast* (Count/Mass), *Erebus broke the bottle*, *the baby finished the bottle*

[5]In other words, NP arguments like *book* are "coerced to the appropriate type required by its governing verb, in this case an event" (Pustejovsky & Bouillon, 1996, p. 135).

(Container/Containee), *the window is rotting*, *Charon crawled through the window* (Figure/Ground), *the newspaper fired its editor*, *Erebus spilled coffee on the newspaper* (Product/Producer), *Erebus travelled to New York*, *New York kicked the mayor out of office* (Place/People). I think it is illuminating to consider these alterations as alterations of "perspectives" in terms of how we topicalize the matter at hand in each of the examples, to borrow Chomsky's slogan. In each of the sentences, the context somehow focuses on one aspect of the content of the noun, such as Figure/Ground. However, the "disambiguation process" is contextual and pragmatic, involving what we know about the regularities of the world. In suitable contexts they fluctuate, as we expect these features to be merely typical.

For example, if Erebus is a brute, he may eat lamb live; or if he had an exceptionally big mouth, he could eat the whole lamb with one bite. We do not fail to understand these expressions, nor their truth conditions, having only the sense of a marked *untypical case*. The reader may easily find relevant contexts for any of them that shift the perspective. Thus, it is not possible (or reasonable) to capture such alterations compositionally in a "representational generative capacity." The alterations are not explainable from the properties of the constituents and their form: rather, they are perspectives provided by various factors, such as discourse context, upon the more literal content in which there is as yet no contextual alteration.

Thus, semantic features, when they are only typical, lead instantly to the proliferation of clear counterexamples when they are considered part of more complex linguistic representations due to the failure of compositionality. But if we are to know what is both sufficient and necessary in terms of bestowing to bestow the true semantic interpretation to linguistic expressions, such counterexamples could not be allowed. Therefore, statistical features, or conceptual roles based on statistical features, do not constitute lexical elements.

One way of reacting to this result, as many cognitive psychologists have done, is to assume that, since there is obviously no strict compositional mechanism available, knowledge of the world is involved in the determination of the prototype of complex expressions (concepts) (e.g., Hampton, 2000, Murphy, 1988, Smith & Medin, 1981, Smith & Osherson, 1984). In order to know what typical pet fish look like, one needs knowledge of the world. For instance, in order to find out what are typical properties of the religious rituals of Eskimos, one would need to go out and look, or to consult an authoritative

book. The point here is that these properties could be anything, and clearly are not subject to any laws. They are just random: there cannot be any science, any determinate laws, that state what kind of religious rituals Eskimos must have. This holds for an infinite number of concepts (the meanings of expressions mentally understood). In effect, in order to know about religious rituals one needs a mechanism for belief fixation, not only for meaning, but then these properties do not suffice for semantic laws either. Thus, given the meaning of its constituents, one can surely understand what *religious rituals of Eskimos* means without knowing what kind of rituals Eskomos typically have. The same goes for the religious rituals of ancient Finns, of Native Americans, and so on. This is why psychologists have doubted the prototype theory: most complex concepts lack prototypes, but they do not lack meaning, and the two are not coextensive.

According to one proposal, the lexicon first and foremost consists of some kind of "core meaning" that takes care of the requirements of compositionality, but then in addition there are also prototypes, or other conceptual roles. Let us call this the "dual theory" (see Armstrong, Gleitman, & Gleitman, 1983, Block, 1986, Cowie, 1999, Horwich, 1997). Since most complex expressions (concepts) lack prototypes, we could claim that primitive concepts are constituted of (i) core meaning plus (ii) prototypes.

It is clear, however, that this theory is not compositional in the sense required by empirical evidence. Suppose you have the concept PET FISH, or know the meaning of *pet fish*. Recall that, according to a law governing our language and thought, we therefore also have the expressions (concepts) *pet* and *fish*. Suppose that the meaning of *pet fish* did not require knowledge of prototypes of any kind, but that the meaning of *pet* and *fish* needed such knowledge. Then it also follows that, in general, you could know the meaning of *pet fish* without knowing the meaning of *pet* and *fish*: the latter requires something that is "extra" with respect to the former. This is clearly against empirical facts, because one normally understands the constituents if one understands the complex expression. Thus, prototypes do not constitute lexical meanings, not even in addition to the "core meaning" (see Fodor, 1998b, §3, 4).

Some psychologists assume that processing complexity characterizes the distinction between complex and simple concepts, whether they be definitions or prototypes. Indeed, most psychologists find the evidence more balanced against the decompositional view (see e.g., Aitchinson, 1987, Fodor, Fodor,

& Garrett, 1975, Fodor et al., 1980, Kintsch, 1974, Paivio, 1971, Thorndyke, 1975), although some do not (see McNamara & Miller, 1989 for a review).[6] There are also authors who think that features of processing time or memory recall are not proper diagnostic tests for matters having anything to do with lexical decomposition. This is understandable, since it is possible that complex concepts are processed in some sort of "chunks."[7]

I also found a number of papers citing subjects' verbal descriptions (interpretations) of certain complex concepts as evidence of the internal nature of concepts. Typically such evidence is marshaled as evidence of some specific decompositional theory. David Pitt is explicit on the matter and proposes that "direct questioning about the semantic properties and relations of expressions would seem the obvious strategy" (Pitt, 1999, p. 152) for studying the internal structure of concepts. It is obvious, however, that this is an unreliable diagnostic test: such interpretations could reflect subjects' (synthetic) beliefs (associated features) as well as the internal structure of their concepts (constitutive features). This empirical test does not resolve the question at hand.

2.3 Fodor's lexical atomism

The semantic theory of natural language and thought must be such as to determine, or 'compose,' the meanings of complex representations based on the properties of their constituents. Empirical data indicates that this happens systematically and productively, and that prototype theories violate both conditions. It also appears that the semantic features of lexical concepts - a finite subset of all concepts - could not be exhausted by giving their definitions in terms of other morphemes in a language.

This suggests that there is something wrong in the strategy of decomposing lexical concepts into semantic features in the first place, hence one is invited to entertain the possibility that lexical concepts do not have semantic decompositions. In other words, it is suggested that lexical meaning

[6] One 'psychological test' of the internal structure of concepts is suggested in various studies on (on-line) classification. Such tests are notoriously unreliable for the internal structure of concepts, however. People categorize objects in the world by relying on contingent "appearance properties." The semantic properties of concepts go far beyond categorization processes (Eco, 1999, pp. 224-336, Geach, 1957, Kripke, 1972, Laurence & Margolis, 1999, Putnam, 1975, Rey, 1993).

[7] See McNamara & Miller, 1989, Gonsalves, 1988, Katz, 1977.

is, at least in part, in some sense 'inscrutable' and cannot be analysed by using other words (/phrases). The meaning of a lexical element cannot be exhausted by resources available in language, whether in terms of definitions, prototypes or conceptual roles.

Let us say that lexical meanings involve 'tacit knowledge': we 'know' about their meaning (not necessarily all of it), but that knowledge is not 'accessible' in language, as succintly put by Moore in a famous passage in his *Principia Ethica*: "If I am asked 'What is good?' my answer is that good is good, and that is the end of matter" (p. 6). This tacit knowledge might involve other cognitive faculties, such as kinaesthetic abilities, or, as a behaviorist would put it, even relations between the mind and the world. To quote Russell: "It is no more necessary to be able to say what a word means than it is for a cricketer to know the mathematical theory of impact and of projectiles. Indeed, in the case of many object-words, it must be strictly impossible to say what they mean, except by a tautology, for it is with them that language begins" (Russell, 1940, p. 26). If so, this could begin to explain why lexical meaning is inscrutable, and why "language begins" from its words, and why philosophical 'conceptual analysis' is so hard, if not impossible.

In the context of this theory as a background, 'semantic oddity' or 'semanticality' discussed in the prevous sections represents not a failure of meaning, but rather a failure of managing to express something that is typical. If there are regularities in the distribution of typical features among constituents and their hosts, then, assuming the atomistic hypothesis, it follows that these are not regularities of *meaning*, but regularities of the *world*. In other words, it is a regularity of the world we live in that if one begins a book, then one often begins to read it; or that a plastic thing is often something *made* of plastic. From this perspective much of lexical semantics, such as Pustejovsky's generative lexicon, appears to represent an instance of some sort of generative ontology rather than generative semantics - a viable research strategy in itself and of much practical use, no doubt.

I will assume the existence of two versions of atomistic theory from now on, a weaker and a stronger one, that must be distinguished for the purposes of this study. In the weaker form (A), the meanings of morphemes cannot be analyzed or defined by using other words in 'ordinary' natural language(s). For instance, it is not possible to define *kill* completely in terms of any kind of linguistic phrase, such as *cause to die*, a phrase understandable by the layperson. I will take conclusion (A) as given here. It is apparently

empirically well supported, and indeed accepted by many linguists as well: there is a difficulty in defining the meanings of words by using other words so that, to follow Russell's phrase, "language begins in its words."

In its stronger form (B), lexical words are indefinable because they express primitive concepts (see Fodor, 1998a). Thesis (B) is stronger and more controversial, since "concepts" is a wider category than concepts expressible in "ordinary language," but it is meant to be an explanation of (A). Whether there are other explanations of fact (A) in addition to (B) is already a difficult enough problem to warrant more extensive discussion.

I will assume the weaker form (A) in this study. This does not mean that the stronger theory is rejected, however. I will leave the matter unsettled. I will manage with (A) here because I will restrict myself to the linguistic faculty and its interface representations, thus (A) is all I need for the purposes of this study, and the status of (B) can be left open.

I will now discuss Fodor's atomistic theory, since to the best of my knowledge it is the only one currently available. The key features of this theory are discussed, and some of the problems are addressed, especially those pertinent to the present study. However, I will not attempt to solve all of these problems here, nor will I offer a complete review of them. My purpose is more modest: to look at the relevant geography of the problem in order to prepare a more thorough discussion of one of the most interesting issues. Moreover, I will not end up by accepting this particular theory as such, although I find it a very valuable starting-point.

Fodor's particular version of the atomistic lexicon can be summarized as follows (1975, 154ff. for i-iii, 1998a for iv, explanations follow):

> (i) "the vocabulary of [thoughts] is very rich" so that "much of the lexical elaboration of surface sentences are also available at the level of representation where messages are made explicit" and that "we need a correspondingly complicated metalanguage to represent their logical form," "resources of the inner code are rather directly represented in the resources of the codes we use for communication";
>
> (ii) conceptual relations between non-logical terms are couched in terms of meaning postulates, not definitions (nor prototypes);
>
> (iii) there is no level of representation at which lexical concepts

(morphemes) are represented by their supposed definitions (or prototypes);

(iv) meaning is denotation and information.

Let us scrutinize each assumption in turn in order to find a starting-point for a more intricate analysis, to locate potential problems and avoid misunderstanding.

Item (i) means that there must be a close relationship between the property of being a morpheme at the "surface level" and being a primitive element at the level of semantic representation, "messages." Many details of (i) remain to be spelled out exactly to make it empirically meaningful. This, indeed, is covered in Chapters 3, 4 and 6. The most important aspect of this hypothesis, assumed in this study as well, is that the metalanguage used to analyze the meanings of ordinary language expressions, or their logical forms, must come quite close in complexity to the form and content of the corresponding surface expressions. For instance, it might be the case that the best and most truthful way to express what the sentence *Erebus killed Charon* means is to say that it means that Erebus killed Charon. One could take this as a "guiding principle" of some sort, and to make it more precise as one proceeds, allowing for exceptions. Yet I think it is an important guiding principle and conforms to the minimalist theory of grammar: it states that, basically, to have a grammar you need thought (LF) and some sensory-motor interface (PF), and not much else. There are no "intervening complexities."

Lexical atomism raises the question of explaining the massive productivity and systematicity in linguistic phenomena visible on the lexical level, pretheoretically understood. For instance, it is well-known that morphological causatives involve a certain systematic and fairly productive number of semantic, syntactic and morphological properties. We seek to explain why these properties appear together, and why in certain variations and not in others. Those who assume semantic decompositional analysis of the lexicon try to achieve this goal, and succeed rather well, by assuming that all causatives share a common semantic element at some deeper below the surface level, say CAUSE. According to this analysis, a causative situation expressed by a lexical or morphological causative involves two events, the causer event and the causee event, and at some deeper linguistic level this duality in metaphysics is mirrored in the often biclausal nature of the corresponding linguistic representation.

Fodor (1975), among others who deny the decompositional hypothesis in favour of the atomistic position, assumed that semantic properties are couched in terms of (one-way) meaning postulates, hence the item (ii) in the above list. Thus, the fact that killing entails dying is captured by an implication that connects killing necessarily to dying. For instance, the meaning postulate that captures the inference from *kill* to *cause to die*, states that, necessarily, if x kills y, then y dies:

(20) $\Box(x)(y)((\text{kill}(x,\ y) \longrightarrow \text{die}(y))$.

What is important is the claim that, since there is no complete definition of *kill*, these meaning postulates can never form a bi-conditional of the following type:

(21) $(x)(y)((\text{kill}(x,\ y) \longleftrightarrow \ldots \text{die}(y) \ldots)$

This, in turn, means that the meaning of *kill* is presupposed, not analysed: for the former clause to have truth conditions, the predicate *kill* must have independent meaning. To put it succintly, when such meanings are given, the meaning postulates thus interpreted and their validity somehow guaranteed, we have a theory that explains such inferences in terms of properties of the subject matter itself rather than of the *representations* of that subject matter. These are thus "material implications." Exactly what does this mean?

One idea is that the one-way implications flow, somehow, from the properties of the *subject matter* itself, rather than from the representations (or concepts) of that subject matter. It is a matter of the constitution (or "metaphysics") of number three rather than the meaning of the numeral *three* that number three is a prime number. In the case of many mathematical concepts it seems correct, as Fodor among others has pointed out, to distinguish concepts from the respective subject matter in the sense that a child who masters the concept of three does not need to master the concept of prime, much less their necessary connection, so as to allow us to break the concept-constitutive connection between the concept THREE and the concept PRIME, while leaving their necessary, "metaphysical" connection. Thus, the psychologically realistic possession conditions for concepts are a useful device for separating concepts from the subject matter they are concepts of.

The claim is not, however, that these properties are *ipso facto* "mind-independent" if they do not flow from meanings or concepts. Rather, they are just part of the subject matter, *whatever* the metaphysics of that subject

matter is. Thus, they may be part of the external world, or part of our minds, or both, but not part of the apparatus we use to *think* or *speak* about them.

A very rough picture of what is going on could be drawn as follows. Certain categories are presented to us in our "everyday experience," most of which are also "lexicalized" and hence part of the input to the language faculty. The concept of number three is among them. However, these categories are defective in the following way: they are not accurate mirror images of the subject matter. How *this* is possible is the tricky problem of semantics and its relation to epistemology: I take it to be a psychological fact (e.g., three → prime). Obtaining knowledge of whatever it is that our concepts are concepts of is thus another line of inquiry altogether, and it is the result of belief-fixation processes that are described by the meaning postulates. The interesting problem then remains as to how it is possible to have a priori knowledge of such subject matter, if not by means of definitions and logic (analyticity)?[8]

However, many linguists, including Jackendoff and later Fodor himself (see also Pustejovsky, 1998), have complained that relying upon meaning postulates is not explanatory: one can stipulate them in any way, as each lexical item "is a world unto itself." Is it not the case that what inferences hit us as 'analytic' is arbitrary? On the contrary, it is very typical today to find "grammatical reflexes of [analytic?] semantic properties" of lexical elements. Several types of words are formed systematically in a way that suggests that the inferential patterns they enter into have resulted from the semantic features used in their construction, and therefore from the semantic features that constitute their meaning.

There are, again, several questions that must be carefully separated. Firstly, how can we have synthetic a priori knowledge *at all*? This is "Kant's problem": since we know a priori that certain connections between concepts are universal and necessary, we need to have internal, cognitive resources to which we have "immediate epistemological access," rather than a mysterious ability to 'see' necessities outside of our minds. The decompositional theory

[8]This discussion could be placed in a much wider context: it brings us to what philosophers, at least since Kant, have wondered, namely, how is a priori knowledge possible. What makes three a prime number and killing entail dying, and why are these conceptual connections so strong? More problematically, if they are not based on conceptual connections (analyticity), as Kant would have argued at least in the case of mathematical knowledge, what are they based on and, perhaps even more interestingly, how do we know a priori that such connections are necessary?

explains the mechanisms of such immediate access rather well: definitions plus principles of logic suffice. If there are no definitions, however, then this explanation is not sufficient. I do not have any solution to this problem, but note that it would not be an attractive line of inquiry to explain *all* synthetic a priori knowledge in terms of *linguistic* decompositions. There is no reason to suppose that the concept THREE would contain all properties and relations that are entailed by it, less so the lexical element *three*. Rather, the linguistic problem is that, in some cases, lexical inference tends to go in tandem with many linguistic properties to suggest that at least some such knowledge is of linguistic, decompositional origin. This is precisely where causatives become important: they differ from numbers, for example, in that these causative decompositions seem to have grammatical reflexes, and they tend to be part of the very word-formation process. I will return to this point later.

This brings us to conclusion (iii), that there is *no* level of representation that contains decompositional material for the meaning of words, although we could ask in what terms we refer to such decompositions? What if we switch to scientific language from an ordinary language. What if the cognitive faculty providing semantic decompositions for lexical concepts is not analyzable in ordinary language, only in a technical language? Given the evidence, we could assume that a decompositional level exists, and yet be 'inaccessible' in an ordinary language.

This could be precisely the case with concepts such as the number three, the decomposition of which is not available in ordinary language, and requires something highly technical. Dedekind explained the origin of his work on the nature of numbers, including the number three in a letter: "It is a synthesis constructed after protracted labor, based upon a prior analysis of the sequence of natural numbers just as it presents itself, in experience, so to speak, for our consideration." This labor led to a plethora of technical concepts devoid of "everyday experience." We could interpret mathematical activities such as those pursued by Dedekind and others as what philosophers have earlier called clarification or the analysing of concepts, namely, if we assume that whatever it is that turns out to constitute the number three is determined by the faculties of our mind/brain and not by the objective mind-independent 'reality out there.' It then follows that one could represent those entities without understanding all of their necessary consequences, and thus have the concept three without having the concept prime. This would

not mean that there could not be a decomposition of the number three using intentional/mentalist vocabulary provided that it is some mentalist faculty that lies behind its properties. Why cannot this be so in the case of other lexical concepts besides numbers? Is there a hidden essence in doorknobs and cars? We could argue that the concept of dogs might be represented as a complex and abstract non-linguistic 'definition.' These decompositions, although they might contain intentional constituents, would not be expressible in ordinary natural language, a serious possibility due to the fact that many cognitive functions might well be modularised, their interaction limited to the "interface levels." In other words, if most concepts cannot be defined in ordinary language, this does not yet show that such definitions could not exist in more technical but still intentional vocabulary.

Thus, consider a grammatical concept such as FINITE SENTENCE. It might not be completely, let alone correctly, decomposable in ordinary language due to the fact that the concepts and principles used by the language faculty responsible for this concept cannot be accessed in a similar way as the concepts of everyday experience. A technical, natural inquiry is thus called for. However, it then turns out that FINITE SENTENCE has a decomposition: it consists of a TP projection as selected by a certain kind of C, both individuated by certain kinds of features and mutual relations, as they are embedded in a complex linguistic theory. Why cannot the same be true of DOORKNOB? Presumably, not all concepts (intentional states) can be input to the language faculty, hence they cannot be the material our language faculty begins to "lexicalize," artificial scientific endeavors aside.[9]

Jackendoff, for example, claims that most words lack definitions since they are constituted of non-linguistic cognitive material, such as visual stereotypes, whose semantic content cannot be captured by linguistic phrases. He argues that what the inscrutability of the lexicon (i) entails is that "if there are principles of lexical conceptual composition, they are not entirely identical with the principles of phrasal conceptual composition" (Jackendoff, 1990, pp. 37-38). He goes on to suggest that there are "nondiscrete" elements that can fill the semantic residuum left unfilled from phrasal definitions, and that we could construct lexical concepts "compositionally" from such elements,

[9]Fodor (e.g., 1998a) remains sceptical: though there exists a highly nontrivial theory of numbers, it might be questioned whether a nontrivial theory of, say, doorknobs would be ever forthcoming. Whether such a theory is or is not forthcoming depends, of course, on what kind of property the property of being a doorknob is. To this question, I do not have an answer.

among them one like CAUSE.

Pinker, too, assumes that there is a constrained set of semantic features that attach to verbs and constrains the set of possible verb meanings, and although they do not provide a complete decomposition of verbs (assuming (A)), it is these abstract features that enter into the computation of linguistic expressions, the residuum being grammatically irrelevant. He does not try to "come up with a small set of primitives and relations out of which one can compose definitions capturing the totality of a verb's meaning. Rather, the verb definitions sought will be hybrid structures, consisting of a scaffolding of universal, recurring, grammatically relevant meaning elements plus slots for bits of conceptual information about things like shrimp, butter, fame, and so on" (Pinker, 1989, p. 166). The semantic residuum is filled with "grammatically irrelevant conceptual slots." He wrote that "linguistic processes, including the productive lexical rules that extend verbs to new argument structures, would be sensitive only to parts of semantic representations whose elements are members of this set" (Ibid.).

Exactly as in the case of Jackendoff, there is thus a linguistically salient level of semantic representations (a "special subsystem [. . .] with well-defined syntax and vocabulary") that has a decompositional structure, but "semantic structure translated into a paraphrase need not be exactly synonymous with the single word it is designed to represent" (Ibid., p. 168).

In short, some lexical features seem to have grammatical effects, while there need not be a complete definition leading to strict synonymy in terms of those features, as Konrflit & Correra (1993) put it: "Decompositions [...] intend to capture the core aspects of the verb meanings, without implying that all aspects of the meanings are represented" (p. 83). McNamara insists, too, that "the apparent nondefinability of concepts places very few constraints, if any, on theories of semantic representations" because "many critical semantic components may be perceptual and consequently may not be expressible in a spoken language," and because "a word meaning may be a collection of necessary and nonnecessary attributes that constrain the word's semantic and syntactic assignment" (McNamara & Miller, 1989, p. 358).

In sum, these responses, if I am correct, aim to combine the observation that most lexical elements are not definable (A) with the claim that they still have semantic decomposition given that they have semantic ingredients that cannot be expressed in natural language. Lexical concepts have decompositions that are not expressible in their entirity in ordinary language.

The problem here is that, as Fodor pointed out (Fodor, 1998a, p. 45), this does not yet *explain* why there are 'cognitive elements' that cannot be expressed in phrases. What Jackendoff and Pinker are, in effect, saying is just that 'there are aspects of lexical meaning which cannot be expressed by linguistic phrases.' Suppose killing is decomposed into two parts, one representing 'cause to die,' and the other begin something X, which is a "non-discrete element":

(22) $(x)(y)$ (x kills $y \longleftrightarrow x$ causes y to die & X).

The claim is that what the language faculty sees here is the right-hand side of the biconditional, which is converted into a single morpheme *kill*. 'Cause to Y' is the common meaning aspect behind several words (*killing, murdering, opening, sinking*), whereas X is the component that distinguishes killing from murdering (i.e., when Y = die), for instance. The point is that when X is interpreted according to the conceptual-intentional system, or whatever it is that produces the interpretation for *kill*, it corresponds to something outside of the language faculty, that cannot be explained in linguistic phrases. But it is evident then that *kill*, as opposed to other causatives, means just X, which is something that cannot be decomposed! Exactly *why* is it impossible to use phrases to express the "nondiscrete cognitive elements" X that are supposed to make the meanings of words indefinable? It is because of this that, in order to explain why phrases are insufficient to capture the complete semantic properties of words, Fodor claims that words express primitive concepts, and he thus ends up with the stronger form of lexical atomism (B). More specifically, he claims that X expresses a *primitive concept* and is lexicalized as *kill* (see Fodor, 2003, pp. 59-80).

In this study, I will not argue in favor of or against the stronger assumption (B), because all of the empirical issues discussed here can be settled by assuming only (A). To put it in more technical terms, the deepest semantic representation I will ever consider is the "Logical Form," which is the *interface* between the language faculty and meaning. "Lexical atomism" is formulated at that level. The point is, then, that this weaker theory is still *compatible* with (B), and this shows that atomism, whether strong or weak, is a *linguistically plausible/relevant/interesting* hypothesis.

Finally, meaning, according to Fodor, is based on information (Fodor, 1994b, 1998a). A symbol token means x just in case it is "nomologically locked" into a property of x-ness. Being nomologically locked means, roughly,

that there is a lawful connection between the tokening of something being x and the symbol. This constitutes Fodor's fourth thesis that meaning is denotation and information (iv). I regard this assumption as the most controversial in Fodor's theory, and thus I will not discuss it here.

In sum, and still on a fairly general level, Fodor claims that morphemes on the surface level correspond to primitive semantic elements (primitive concepts), that meaning postulates (strongly held beliefs) 'explain' much of the inferential relations inside the lexical stock, and that lexical atomism is interpreted to mean intentional atomism so that there is no decomposition of lexical morphemes that uses intentional vocabulary - not even in a technical semantic vocabulary that is more developed than the ordinary everyday language whose origins lie in our "intuitive" everyday experience. Finally, Fodor suggests that meaning is constituted of denotation and information: it is a nomological link between the mind and the world.

The difference between these two explanations, Fodor's on one side, and linguists' on the other, are summarized in the following figure:

theory	morphemes	linguistic levels	concepts	world
I	kill	kill	KILL	cause to die
II	kill	cause to die	CAUSE TO DIE	cause to die

By "linguistic levels" I mean representations that are tokened inside the language faculty. In Fodor's case (I), there is no linguistic level or level of concepts on which the morpheme *kill* would be represented in terms of a phrase. The only decomposition takes place 'in the world.' Here the picture is complicated by the fact that what appears 'in the world' might be constituted, in part or in whole, of what hides inside the human mind: in Fodor's theory, if and when (1998a) there are such mind-dependent components, they are not 'linguistic' or part of the 'intentional level.'

Linguists, those cited above and others, adopt hypothesis II, which states that such decompositions are part of some level of linguistic representation, that of concepts or semantic representations, at the interface between semantics and syntax, or even inside the language faculty. I discuss such proposals in Chapters 3 and 4.

I will not try here to find an alternative explanation for the fact that lexical elements cannot be defined: I will assume that this is so and go on to study its linguistic consequences.

2.4 The problem of explanatory adequacy

Lexical atomism has not gained unreserved acceptance among cognitive scientists, quite the contrary. The reasons are many: some are arguably based on sheer misunderstanding, yet there are others that are more serious.

Prima facie there is nothing wrong with the atomist position: it is in agreement with productivity, systematicity, and a lot of other crucial empirical data. I believe that the atomist hypothesis should even be taken as the null-hypothesis: if there is no evidence to the contrary, we should accept it. It is more surprising, I believe, if it turned out that the meanings of words could be expressed in terms of other words than if they could not. In addition, a lot of data, e.g., many phenomena concerning 'semanticality,' can be explained equally well by invoking pragmatics, or some kind of 'generative ontology,' rather than semantics. It is just that there *is* evidence against the null hypothesis.

One pertinent and often discussed type of evidence against atomism concerns causatives. I make no attempt to characterize this class of expressions here, rather leaving it to section 4, but the problem is that one can explain the semantic and syntactic properties of causatives elegantly by assuming that the lexical elements that appear in such constructions have a semantic decomposition, in which the one semantic feature represents 'causation.' The linguistic computation, word formation and the like, is then sensitive to this feature, whereas precisely because something representing 'causation' is a semantic feature it can be used to derive some of the semantic properties as well. By way of illustration, consider the following sentences:

(23) a. Erebus killed Charon
b. Charon died
c. Erebus caused Charon to die

These sentences are related to each other: (a) first implies (b), a fact that apparently has something to do with the lexical elements *kill* and *die*. Therefore, it is common today, as it was back in the days of Generative Semantics, to assume that the lexical element *kill* contains the semantic feature(s) 'cause to die.'

How this idea is implemented technically varies, and some of the methods are reviewed later, but one may assume that (a) is represented at some more abstract linguistic level much like sentence (c), so that we could take the

surface expression (c) to express, or to very closely correspond to, the phrasal decomposition of (a). In other words, sentences (a) and (c) both express the same underlying proposition or deep structure/logical form, hence they are synonymous qua linguistic. Then due to some computational operations taking place in the language faculty, (c) would be converted into (a), thus explaining why it is related to (b). Even better, it often becomes possible to explain some grammatical properties of the relevant constructions by using these assumptions (Hale & Keyser, 1993).

Note that if (a) is represented as (c) at some deeper linguistic level of representation, then this already violates the weaker formulation (A) of the atomistic theory, since (c), the putative decomposition, is expressed in 'ordinary language' by the simple sentence (c). This is important, for advocates of decompositional theories have sometimes claimed that their decompositional structure (c) is somehow 'technical' and not, therefore, exactly comparable to the corresponding clause (c) in the ordinary language but rather to some formula in the 'logical metalanguage,' say: what the qualification "not . . . exactly" does here of course makes the whole difference, if there is any. I will return to this matter later. Thesis (B) bans all kinds of decompositions insofar as they are interpreted as being a description of some level of mental representation. I return to this matter later, too.

Fodor has replied to this decompositional claim, quite correctly, that it is possible to explain the grammatical effects by assuming that the semantic features that appear to take part in the computation inside the language faculty do not constitute the lexical elements, but are just merged into them, for one reason or another. One possibility is that they are so merged when we come into possession of more knowledge of their meanings (§2.3). According to Fodor, "Saying that lexical items have features is one thing; saying that lexical items are feature bundles is quite another" (Fodor, 1998a, p. 63, footnote 14). According to this explanation, it is belief fixation rather than meaning or analytic inference that accounts for grammatical evidence.

Although this is only a possible strategy, it brings us to the real problem of explanatory adequacy: why are these semantic features not - if they do not constitute the lexical elements - arbitrary? Looked at from a slightly different perspective, the fact that killing entails dying seems to be known to us in an a priori manner. If it is not based on analyticity (conceptual containment), what is it based on? More interestingly, from what cognitive resource do we derive the very justification for this entailment, or the intuitive feeling that

this connection holds, no matter what? In terms of Fodor's theory, one has to say that we may somehow possess a priori knowledge of their metaphysics (constitution).

Pinker (1989, §5) argued along these lines. First, he claimed that certain morphological and syntactic properties of verbs seem to be sensitive to their abstract semantic attributes. Whether this is actually so need not concern us here. Then he considers the possibility that these semantic attributes exist because of 'general mechanisms of belief fixation,' as would be implied in the case of Fodor's atomistic theory. The problem, according to Pinker, is that this strategy is not explanatory: it does not constrain verb meanings in any way since "any culturally salient distinction could be used as a dimension or feature helping to define similarity, and the syntax could reflect those similarity clusterings" (Pinker, 1989, p. 166). However, one could not and does not use any culturally salient distinction in that way. It seems to me, furthermore, that the fact that killing entails dying is not merely a culturally salient entailment, and there must be a deeper, cognitive explanation.

Chomsky, defending the existence of analytic truths and the decompositional lexicon (see Chomsky, 1988, pp. 32-34, Chomsky, 2000c, pp. 61-67), argues, "To the extent that anything is understood about lexical items and their nature, it seems that they are based on conceptual structures of a specific and closely integrated type," so that there is an "a priori framework of human thought, within which language is acquired," providing "necessary connections among concepts, reflected in connections of meaning among words" (Chomsky, 2000c, p. 62-63). He then argues that, for example, assuming that the causative system is not a result of the internal structure of lexical items "establishes nothing unless it is shown that an alternative approach in terms of some [. . .] theories of belief fixation or semantic importance" (Ibid., p. 64) can be developed.

If I may return to the case of mathematical knowledge, similar patterns of inferential relations are found in mathematics. For instance, natural numbers have properties, like 'being a prime,' attached to some of them. These patterns, even if they cover what one could claim to constitute a 'lexical level,' are not random, arbitrary or merely conventional (one could argue), but they must be based on something. This is even more evident in the case of geometry and calculus. For instance, in Kant's theory - representing an impetus for much later discussion on this topic - the validity of such judgments was based on what he called "pure intuitions." Whatever they are based on, this

source must be found inside our minds, since otherwise it would be totally mysterious how we could convince ourselves of their necessity in an a priori manner, as Kant put it in his *Critique of Pure Reason*:

> If the objects with which our knowledge is concerned had been things in themselves, we would not have been able to possess any a priori concepts of them. For where would we get them from? (A127)

Fodor discusses at length the argument from theories of intuitions about conceptual connectedness to lexical semantic features or lexical concept-constitutive inferences (1998, §4). He noted, correctly in my view, that the main reason for assuming concept-constitutive inferences is because "an inference that constitutes the concepts which enter into it can be known a priori to be sound" (p. 69). If such a priori knowledge does not result from the concepts themselves, how could we otherwise comprehend such inferences a priori as necessarily true? To the best of my knowledge, Fodor does not solve the problem and, what is worse, information semantics (Fodor's assumption iv) does not seem to be the right kind of theory with which to address this question.

The example from mathematics is also instructive in the sense that it is clearly the case that the necessary relations between arithmetical objects do not have grammatical reflexes other than being 'evident' in their meaning, meaning that they are presumably not to be explained by relying upon the presence of lexical semantic features detected in the *language faculty*. The case of causatives is an interesting class of examples precisely in the sense that causativity, as a semantic phenomenon, at least seems to involve some grammatical reflexes (§4). One could thus claim that such grammatical reflexes provide an argument in favor of lexical decomposition. It is *this* argument that I will attempt to refute here.

2.5 Conclusions

A theory of language and thought requires a theory of meaning such that it explains how complex expressions/concepts inherent their semantic properties from the semantic properties of their constituents. This fact alone, when accompanied by empirical facts concerning such 'composition,' appears to

severly constraint theories of lexical meaning in ruling out those based on statistical semantic features (prototype theory). Since there is independent evidence that lexical words cannot be defined by using other words in (ordinary) language(s), we are tempted to reason that there might exist some general difficulty in constituting lexical semantics in terms of conceptual role theories of any kind.

One part of this problem is to explain that difficulty. Fodor, for example, suggests that lexical concepts are primitive concepts, and that this explains why it has been so hard to find semantic decompositions for them. Linguists, who in virtue of having independent linguistic evidence have assumed that linguistic decompositions exist, have argued that the difficulties result from the fact that the meanings of lexical expressions express semantic ingredients that cannot be expressed by using phrasal expressions (e.g., visual prototypes and the like). This is not so much as explanation than a restatement of the basic fact, however; it leaves unexplained why these ingredients are not expressible by using phrases.

It still remains that there might not exist a *linguistically salient level of representation* at which lexical elements would be replaced by their definitions, prototypes, and the like. Given that this hypothesis is rather well-supported, it is particularly interesting to find linguists who entertain the contrary hypothesis on the basis of quite intricate data and sound reasoning. This had led them to find alternative explanations for the difficulties in finding appropriate conceptual roles that constitute lexical concepts (i.e., in terms of definitions and prototypes).

The other strategy, not seriously entertained so far but attempted here in what follows, would be to keep the atomistic hypothesis and seek alternative explanations for the linguistic evidence.

Chapter 3

The minimalist framework and the atomistic lexicon I

3.1 The minimalist framework

In this chapter I will formulate a version of the minimalist theory such that it (i) removes the need for semantic decomposition from the lexical component of the grammar, (ii) avoids stipulation in the case of lexical inference and (iii) explains the syntactic and semantic properties of causatives, among other linguistic phenomena. Hence, the purpose is to propose a framework that could 'unify' two types of apparently contradictory evidence: one from considerations having to do with systematicity and productivity (§2), one from linguistic considerations having to do with grammar (§4, 6).

Whether this problem is meaningful depends on whether one takes both types of evidence seriously. It is apparently possible to ignore either one, or both, but I believe both are equally compelling and should not be dismissed through ignorance.

I will begin by reviewing some recent assumptions concerning the universal grammar, developing some of them further in order to satisfy the goals mentioned above and especially what has been called the Strong Minimalist Hypothesis. Some core grammatical evidence is discussed here, but that is dealt with more fully in Chapters 4 and 6. There is also a computer simulation of the theory, which is not reported here.

A grammar of natural language comprises the pairing of form and meaning. If S is a set of forms s_1, . . ., and M is a set of meanings m_1, ..., we may take the grammar of some language L to consist of the finite

specification of pairs <s_i, m_i> for that L. Sometimes our attention turns to a universal grammar, which is a finite specification of pairs <s_i, m_i> insofar as these objects are common to all natural languages, abstracted from variations between possible human languages. I adopt the latter goal here, in line with current proposals that seek to find an underlying "psychological reality" behind grammars (and language learning from the impoverished linguistic stimulus available to learners) (Chomsky, 1965). According to this perspective, the study of natural language(s) is comparable to any naturalist inquiry: "The theory of core grammar, at least, is based on fundamental principles that are natural and simple, and that our task is to discover them, clearing away the debris that faces us when we explore the varied phenomena of language and reducing the apparent complexity to a system that goes well beyond empirical generalisation and that satisfies intellectual or even esthetic standards" (Chomsky, 1981a, p. 14).

The notion of the "psychological reality" of grammar(s) under this research objective is to be understood as the search for 'explanatory adequacy.' This means that we seek to explain the properties of linguistic expressions among the world's languages by using the primitives and principles that the child inherently possesses in order to succeed in learning any of those languages by using the limited evidence and cognitive capacities available. The rest of the grammatical rules must be regarded as socio-cultural noise that is adopted from the environment, although they can, of course, also be studied, not to mention carefully distinguished from the lawful aspects of the grammar.

The object of the study is the biologically determined initial state of the language faculty in which these primitives and principles dwell: hence some authors speak of "biolinguistics" (Jenkins, 2000). Since the child can learn any of these languages from scratch, without reliable stimuli or 'sophisticated intelligence capacity,' it follows that the world's languages must, contrary to outside appearance, be very similar to each other. Any putative complicated rule system, as still often postulated in the name of 'descriptive adequacy' for individual languages, already presents insuperable barriers to the learner, as such systems have done for many generations of adult linguists. How could the mentally retarded child learn such rules (viz., a language) on practically one occasion (i.e., a few years) if a single rule already poses difficulties to adult linguists of many generations, in the past and in the future?

In other words, the development of a generative grammar has been shaped

by the conflict between descriptive adequacy and explanatory adequacy, descriptive adequacy pointing to deep and perplexing differences between languages, and the requirements of explanatory adequacy being just the opposite. This sense of "psychological reality" nevertheless departs from, but does not necessarily contradict, other usages (i.e., more functionalist theories or cognitive grammars). For example, from very early on, the study of the Universal Grammar, in the context of the generative framework assumed here, took the ultimate goal of linguistics to be the construction of a "language as an instrument or tool" (Chomsky, 1957, p. 103), whose one, but not only, use is communication. Theoretical frameworks that emphasize the actual use of language in its communicative function hence do not depart from the assumptions made in this study: I likewise emphasize that role. One potentially meaningful point of departure is the assumption that we nevertheless attempt to "describe [language's] structure with no explicit reference to the way in which this instrument is put to use" (ibid.), hence we clearly depart at least from the functionalist perspective that makes reference to extra-linguistic principles and primitives in explaining linguistic data.

According to some recent assumptions in the study of the UG, there is a module in the brain, the 'language faculty,' in which the mind/brain performs linguistic computation.[1] There also exists what has been called 'conceptual-intentional systems' (C-I), integrating linguistic input/output into other mental faculties such as vision, thinking, moving and other 'belief structures.' Some aspects of meaning are determined at the C-I level only, but some take an active part in the linguistic computation. Where to draw the line is an interesting empirical issue, which is currently open. Suppose we could not talk: all that would be left - many aspects of semantics and pragmatics, no doubt - would belong to a realm outside of the language faculty (C-I), and what we would get in addition when we began to speak in the manner we

[1]The hypothesis that linguistic processing is modular is supported by moderately strong empirical evidence. Linguistic computation seems to be localized in the brain, and as such is separated from the association with meaning: its functioning is independent of general intelligence, the ability to learn, and so on (Bellugi, Birhle, Neville, Jernigan, & Doherty, 1992, Bellugi et al., 2001, Curtiss, 1981, Linebarger, Schwartz, & Saffran, 1983, Yamada, 1990, Smith & Tsimpli, 1991); its maturation seems to be regulated by a biologically determined critical period (Newport, 1990, Skuse, 1984); finally, linguistic properties are, as expected, generally different in nature from the semantic properties. For instance, linguistic expressions have a linear order due to the limitations of our articularitory systems, while meanings do not (there is no such limitation). This creates an empirically meaningful tension between form and meaning (Chomsky, 1957, §9).

do, is grammar. Clearly, the same conceptual structures could be expressed in an infinite number of ways, so we need to keep these two mental faculties distinct, apart from their interactions.

Much of the difficulty in dealing with lexical atomism and interpreting that position coherently (§2.3) has to do, at least in my case, with the problem of locating the putative *level of concepts* in the matrix of other assumptions concerning the language faculty. What we know is that the lexical and complex elements that appear at the interface between the language faculty and the conceptual-intentional system are much like concepts as Fodor uses the word. They may have some additional syntactic properties as they are handled by the C_{HL}, and obviously quite literally *used* by the conceptual-intentional systems as the language faculty is, at least partially, under our voluntary control. My assumptions concerning the relations between these systems of representations become evident as I proceed.

A *universal grammar* is a specification of the initial state of the language faculty: a *particular language* is specified by combining properties of the UG, with some variation included. By "language" I mean *I-language*, the generative capacity underlying the pairing of sound and meaning for that language, to be contrasted with "E-language" representing a set of linguistic expressions or utterances - a highly derivative notion in the study of the mind/brain.

In assuming the technical concepts of GB theory and minimalism, I will likewise assume the general framework underlying these theories, referred to as the "Principles and Parameters theory." This theory represents a radical break from the mentalist grammars of the more traditional sort, including the *Aspects* model.

The core idea of the P&P theory is that, in the search for explanatory adequacy, we assume no language-specific rules or constructions: all linguistic expressions are considered as a result between interaction of various components of the UG, language-specific parameters that are set in one way or another in the course of the maturation of the language faculty, and of course some random variation as well. I will keep to this radical proposal here, postulating no language-specific (narrow-syntactic) rules.

Given the recent suggestions concerning the minimalist theory, I assume that the variation among languages is restricted largely to the morphological and phonological components, a narrow syntax (and semantics) being a component of the UG and its core grammar. This is an important hypothe-

sis and guides the research through-and-through. For instance, consider the restrictions in the extent to which predicates are involved in the causative alternation in English. Given that virtually no such restrictions exist in Finnish, it follows that such restrictions cannot be diagnostic of properties of (narrow) syntax: the difference should, rather, be attributed to Morphology.

Linguistic computation is assumed to be purely syntactic. It assumes formal entities, *representations* in terms of linguistic levels or *derivations* in terms of constructing syntactic objects, as its input, and produces a set of other formal entities, expressions, as output. In particular, meanings say, mind-world -relations, do not enter into the determination of linguistic expressions inside the language faculty. When they do determine aspects of linguistic expression, they are conveyed to linguistic computation by formal semantic features at the "interface," or anything that is "computable" in the human brain (Chomsky, 2000a, pp. 73-74, 2000c, Fodor, 1981b, Jackendoff, 1997). "If semantics is," Chomsky writes, "the relation between sound and thing, it may not exist. If semantics is the study of relations like agency, thematization, tense, event-structures and the place of arguments in them and so on and so forth, that is a rich subject but that is syntax; that is, it is all part of mental representations" (Chomsky, 2000a, p. 73). I will work with semantics in the latter sense here.

Most of the present investigation is conducted in the framework of the Minimalist Program (Chomsky, 1995, 2000, 2001). These theories, in turn, are direct descendants of the Government and Binding theory (Chomsky, 1981a). Both are variants of the Principles and Parameters theory. When the details and differences do not matter, GB-theoretical terminology such as the X-bar notation is used for simplicity, although all of the essentials are borrowed from minimalism. In the latter part of this thesis, I occasionally use terms such as "d-structure" and "s-structure," by which I mean, if interpreted in terms of current minimalist theory, a logical form viewed from the two respective perspectives: d-structure correlates with a more core thematic structure without movement, s-structure with Spell-Out. This heuristic usage was chosen for the sake of readability when the arguments I discuss were themselves framed in the framework of the older GB theory, and when the syntactic details did not matter for the issue at hand.

Moreover, the Minimalist Program itself does not yet represent a coherent theory, but is rather a collection of new proposals and ideas: as Norbert Hornstein put it: "Minimalism is not a theory but a set of guidelines for

constructing grammatical theories" (Horstein, 1999, p. 61). I discuss these broad guidelines in this chapter, returning to more substantial and technical matters in sections 3.3 and beyond. Most of the material presented here comes from Chomsky (1995, 2000b, 2001). The work is organized so that Chapter 3 introduces the theory and included discussion of only some core data, whereas Chapters 4 and 6 work more with the empirical material, namely, causatives in the world's languages insofar as they bear on the present issue.

Central to minimalist program is what Chomsky has called the Strong(est) Minimalist Thesis (Chomsky, 2001, p. 96, SMT):

(24) (Strong Minimalist Thesis, SMT.) Language is an optimal solution to legibility conditions.

Whether this thesis is true or not is what the minimalist program seeks to find out. How "perfect" is the human language (Chomsky, 1995, 1-11)? "Legibility conditions" involve the conceptual-intentional system (C-I) and the phonological-articulatory-system (P-A), thought and sensorimotor systems, respectively. P-A is often taken to consist of the level of phonological form (PF), as it is taken here as well. The properties of these two systems, when used in an "optimal manner" to implement a natural language, are assumed to explain all peculiar properties of the UG and, therefore, the properties of the core grammar of any natural language. We are lead to imagine a super-engineer who faces the task of building a language faculty under certain external requirements, trying to find the most minimal and optimal design to satisfy these conditions, while creating no unnecessary complications (Chomsky, 2000c).

Some evolutionary hypotheses are clearly implied here as well (see Martin & Uriagereka, 2000). It could be that a minimal amount of 'crystallization' of neural tissue is sufficient to give the organism this ability, supposing it already has the external systems.

The minimalist hypothesis represents an attempt to reach 'beyond' explanatory adequacy by testing the hypothesis that language is, in a more or less clearly defined sense, perfect and optimal system, involving no unnecessary primitives and principles given the legibility conditions. This hypothesis differs from the weaker goal of trying to find the most elegant and perfect theory by proposing that, contrary to appearances, the language faculty may itself be surprisingly optimal.

Here is another way of looking at the minimalist hypothesis. In the era of the GB theory, the grammar of human language(s) was thought to be composed out of various independent modules and their respective principles, usually formulated in terms of filters, plus certain selected levels of representations, some of which were internal to the language faculty (e.g., d-structure, s-structure, surface structure, logical form). But why do any such language-specific and highly surprising principles hold? At some point the answer can be given only in terms of truly obscuring biological facts and their complex mutual relations: thus, in the present state of understanding one could only say that "the brain just happens to be (develop in) that way" - a non-answer, that is. The minimalist program, in effect, is an attempt to seek a *cognitive explanation* in terms of interface representations and notions of optimality. Given the current level of understanding concerning biology and cognitive architecture, this latter strategy is to be preferred.

Some optimality properties of human languages are rather trivial. For example, linguistic expressions are not usually duplicated or repeated beyond necessity. Some such properties are surprising, and certainly real, such as economy-based restrictions covering the 'displacement property.' Clearly, however, there are imperfections that militate against SMT, and this is what makes the minimalist hypothesis interesting and surprising, if true. Suppose that P is such an imperfection, such as formal features (Case, Agreement), functional projections (Agr), or some weird property of "displacement" itself. In this case, three options persist (Chomsky, 2000b, §3.5):

(25) (Imperfections.)(a) P is real, and an imperfection; (b) P is not real, contrary to what had been supposed; or, (c) P is real, but not an imperfection; it is part of a "best way" to meet design specifications.

Option (a) stands against SMT (24, p. 64). Option (b) is interesting, for this means that P can be reduced, or explained away, in a way that is consistent with minimalist guidelines, hopefully in a more elegant manner. For example, the putative reduction of the X-bar theory to a "bare phrase structure" represents this choice (to be discussed presently). In the case of most imperfections, (b) will be assumed in this study. Consider option (c). The more or less standard explanation for "displacement" in the minimalist framework has been to try to show that it involves the deletion of formal features for the sake of interpretability at the level LF. The question then arises as to why there are such formal features, given SMT. In this study, I

adopt option (b) concerning formal features in the lexicon and displacement, and thus I attempt to show that they can be eliminated.

3.2 Minimalist architecture

Let us now turn to the minimalist architecture itself. According to the GB theory, separate d- and s-structures are assumed to be part of the language faculty, in addition to PF and LF. However, if language is an optimal solution in terms of the legibility conditions, then we would expect no such levels of representations to exist, hence they would be unreal (adopting option b). This assumption is one key aspect of the minimalist program.

With the s-structure and the d-structure discounted, evidence that has been explained by relying upon the former and its own representational conditions is now explained under the assumption that some properties of the language faculty are derivational rather than representational. What this means is that a linguistic expression is built by C_{HL} in a step-by-step fashion and sent at a certain point to the phonological system (PF), while the other part continues towards LF and C-I. This point of departure is called *Spell-Out*. No conditions that are internal to the language faculty, with the possible exception of PF and LF, need to be satisfied "all at once," but rather the derivation is allowed to proceed on its own course, observing the given conditions of optimality and *Full Interpretation* at the interface. *Full Interpretation* (FI) requires that the interface level contains only features that are interpretable by the computational systems accessing these interfaces: for example, LF must contain features that are understandable by the conceptual-intentional systems. Computations that converge from Spell-Out into LF now represent covert syntax, and the rest is overt syntax. In order to keep language "usable," no new selections of phonological or semantic features can be made from the numeration after Spell-Out, and only PF and LF remain as real linguistic levels.[2]

Let us now turn to the basic properties of the computational component C_{HL} in order to gain a better understanding of the optimality properties. Assume that the language faculty involves a computational operation C_{HL} that constructs linguistic expressions <PF, LF> from an array N(umeration)

[2]The Y-shaped architecture was sometimes replaced in the later versions of the theory with more dynamic notion(s) of Spell-Out.

of lexical choices, each lexical element associated with an index to represent how many times a particular item can be drawn from the numeration.

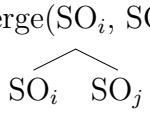
Lexical choices are bundles of features, semantic, formal and phonological. It is often assumed that these features are already present in N ("Strict Lexicalist Hypothesis"). A *linguistic expression* is a pair of interface representations PF and LF. The aim is to derive an explanation that takes C_{HL} to map the Numeration into Exp(ressions) so that the functionality condition is observed (Thus, C_{HL}: N $\longrightarrow$ Exp). The idea is then that C_{HL} must minimize some "cost function" when it produces the syntactic object, the derivation crashing if a wrong choice is made.

In the early days, the cost function was thought to screen out the derivation that had the fewest number of steps and made only local operations. The Numeration is said to determine the *reference set* for the computation, meaning that all possible derivations from this set are taken into consideration when comparing derivations relative to the given cost function. Implementing this idea computationally is almost senseless however, since it invokes processes that are global in terms of comparing complete derivations, and therefore extremely complex computationally. Reactions to this problem have been various, but the general picture seems to be that it would be fruitful at least to attempt to seek economic principles that are local and computationally more simple (Chomsky, 2001, Collins, 1997, Frampton & Gutman, 1999). To a great extent, this explains many recent proposals concerning the developing minimalist program, and I will likewise restrict myself to extremely local operations. For example, in Collins' proposal when it is a question of deciding which of the possible moves in the derivation of linguistic expressions are taken to be optimal the computational procedure uses only local information available at the syntactic object at hand without comparing any of the potential outcomes. This has what I believe the welcome consequence that some of the explanatory burden of Numeration will be gone.

In pursuing SMT, then, what assumptions concerning C_{HL} must be made, given conceptual necessity alone? Clearly, language must involve some process capable of recursion. Suppose that C_{HL} contains a computational operation Merge, which takes two syntactic objects, SO_i and SO_j, and constructs a new syntactic element, SO_i, SO_j by merging SO_i and SO_j. Following SMT, C_{HL} and Merge will not insert any auxiliary features, such as syntactic labels, into these representations besides those that are included in N ("Inclusive-

ness," Chomsky, 1995, p. 228). Thus, the following figure represents the Merge operation:

(26)



However, it seems necessary to determine the head of the newborn SO, so let us take Merge to merge SO_i and SO_j into K = [SO_x [SO_i, SO_j]] with $x = i$ or j. If $x = i$, then SO_i is the *head* of K. Which syntactic object is actually the head? One could speculate that this choice derives from the thematic properties of these elements. Endocentricity is derived, not stipulated.

Are there conditions placed on the application of Merge? For one thing, one could assume that (pure) Merge can apply only at the root (Chomsky, 1995, p. 248) and that it can form only binary sets, presumably the simplest and most economical possibility available (Chomsky, 1995, §4, Kayne, 1984, Larson, 1988, Radford, 1997, §9). For the present purposes, let us assume that there are no other conditions; this is in agreement with most minimalist theorizing and presumably consistent with SMT. Thus, the application of Merge is essentially free.[3] The derivation is said to converge if it produces one syntactic object SO, such that it satisfies the legibility conditions (PF, LF), does this in an optimal way and exhausts the numeration. Otherwise the derivation crashes, producing a linguistic expression that is deviant. Of course, N multiple representations exist for each array of lexical choices, many of which even satisfy the legibility conditions but not conditions of optimality, or vice versa.

The operation of pure Merge produces linguistic objects, beginning what has traditionally been explained as the 'thematic core' of linguistic representations (d-structures), thus merging the VP internal arguments around the verbal head. Focusing on these thematically motivated structures of linguistic representations, I will refer to "argument structures" of head verbs.

[3]In Chomsky's terms, a "guiding intuition of the Minimalist Program is that operations apply anywhere, without special stipulation, the derivation crashing if a 'wrong choice' is made" (Chomsky, 1995, p. 231). What about the selectional properties of the elements drawn from the numeration? One possibility would be to assume that they are observed as part of Merge, thus Merge cannot merge two objects if their selectional properties do not match. The other would be to rule out such constructions at the level LF. Chomsky (2000, pp. 133-134) proposes that selectional properties of elements from numeration can be used to predict the head of the resulting construction, thus deriving the relevant asymmetry.

Movement is discussed in section 3.5. The subsequent sections deal with such argument structures (§3.3) and their thematic properties (§3.4), respectively. It is at this point that I have to depart somewhat from the standard minimalist framework due to the fact that, since I have taken lexical atomism as my working hypothesis, I cannot invoke lexical semantic features such as s-selection in explaining the properties of argument structures and their thematic properties. Argument structures cannot be projections from the lexicon. Instead, I will argue that their properties and their thematic properties can be predicated along the lines of the general non-lexical principles of the UG.

3.3 Argument structures: pure First Merge

The role of the lexcion has become increasingly significant in the development of the theory of syntax, in particular the framework of the Universal Grammar (Stowell, 1992). Ever since the introduction of the *Aspect* model, which still took lexical items to be 'Bloomfieldian exceptions,' a large proportion of the rules of syntactic rewriting were implemented in the lexicon in terms of syntactical subcategorization frames in order to avoid the duplication of such information in the base component.

At first, these features were taken to consist of syntactic information, but later, essentially with the help of certain more abstract rules of the UG, it become possible to suggest that the syntactic frames could be predicted from the semantic properties of the lexical items, together with the principles of the UG (Chomsky, 1986). In fact, it is assumed today that what have previously been referred to as rules of grammar are now seen as projections of the semantic properties of the lexicon. Indeed, we have progressed far from Bloomfield's conception of the lexicon as "an appendix of the grammar, a list of basic irregularities" (Bloomfield, 1933, p. 274). This now seems just what the inscrutability of lexical semantics pretends characterises lexicon: it is semantically unstructured.

However, what is essential in terms of the present theory is the fact that *argument structures are basically relational: they relate lexical items to each other*. Thus, the fact that *kill* takes an Agent at [Spec, VP] is not a fact of killing per se, but a fact about whatever represents the Agent and killing. It is a fact about the Agent's being the killer and therefore involves at least some sort of predication. If this is so, then predication is not intrinsic in

killing, but it is a more general and indeed fundamental aspect of language 'across the board.' A general principle ought to be in operation. Thus, it does not suffice to grasp the lexical elements in order to master the proper notion of 'argument structure'; one must also grasp the way these lexical elements interact, especially under the influence of the relation of predication. This basic thought-forming relation is, furthermore, a fundamental part of our cognitive apparatus and everyday understanding of how objects in the world interact.

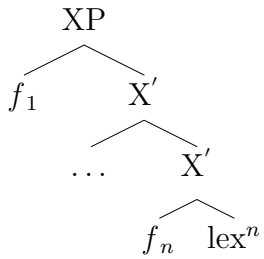
Thus, according to the present proposal, the lexicon has a more abstract role: lexical items do not assign theta roles to any of their arguments. Rather, what assigns theta roles is a "pre-syntactic" subcomponent of the Universal Grammar, which is discussed below. The principles that assign thematic roles to the lexical elements, again discussed below, is a component that works not with a single lexical element, but with several in combination.

Let us proceed with the details. What follows is a proposal concerning how to build the argument structures of heads through the operation of First Merge, while omitting the issue of movement. The major difference between this proposal and standard minimalist theory is that I do not assume neo-Larsonian vP shells and that, in an idealized sense, there are no idiomatic semantic features that are part of the lexical elements themselves, although of course these structures are based on semantic, interpretable properties. I will first present the more formal details, and then return to the semantics.

I will follow Fukui (1995) and Chomsky (2000b, p. 126) in assuming that intermediate X-bar positions (X') in lexical projections are unlimited in LF, and Napoli (1989) in assuming that the head of any XP, X being a non-functional head, is semantically a predicate, and that specifiers and complements are its arguments. The X-bar theory is, to a great extent, a grammatical reflection of the cognitive predication relation. Nevertheless, I will follow minimalist theory in that the X-bar theory is used only heuristically, and linguistic representations are reduced to a binary "bare phrase structure" (Chomsky, 1995, pp. 242-249). Lexical elements are not attributed categorial features at this point, but are assumed to represent abstract properties and relations, depending on their valency or adicity that is freely attached to any lexical element: a property is represented by one-place predicates, a relation by n-place predicates ($n > 1$). Such lexical elements are never pronounced, or rather they are pronounced only when they have been assigned some categorial feature or another. In other words, I will remain neutral concerning

the Strict Lexicalist Hypothesis, returning to it in section 3.6. Thus, I take words such as *envies* and *enviousness* to represent the intensional property or relation of 'enviousness' (see Fodor, 1998a, 1998b, §3, 4, Fodor, 2001), corresponding to one lexical element at the LLF level and, furthermore, to one primitive concept at the putative level of "concepts." A lexical element lex with adicity n, lex^n, is said to *project* with $f_1, \ldots, f_n$ empty positions, giving a linguistic representation (27) of what in the standard logic is expressed in an atomic formula (28):

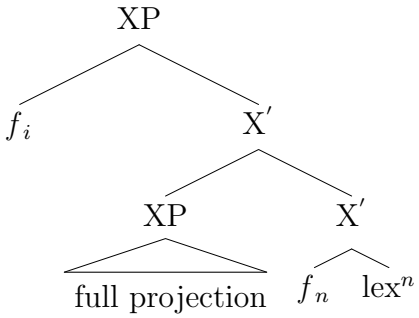
(27)



(28) $\text{lex}^n(f_1, \ldots, f_n)$

Lex^n is an *unsaturated predicate* with n empty slots for its arguments: when all these slots are filled with arguments, the resulting construction has adicity 0 and it represents a *saturated proposition* (Higginbotham, 1985) or full projection (FP). These propositions are, again, intensional entities, and they differ from declarative judgements (more below). The arguments may comprise, and in some case must comprise, other XPs of arbitrary complexity, thus recursion enters into the picture here. LLF structure (29) thus generates the meanings of expressions such as (30)(a-c):

(29)



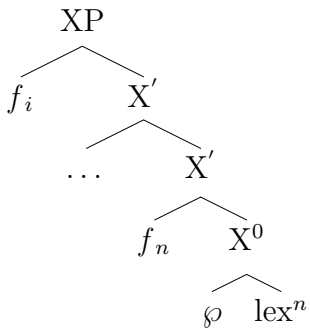
(30) a. It seems that [$_{XP}$ Charon ferries them across the river]

b. I think [$_{XP}$ Charon ferries them across the river]

c. It struck me that [$_{XP}$ Charon ferries them across the river]

There are no syntactic labels except for XP, as assumed in the "bare phrase structure," although the emergence of XP could be predicted from the thematic properties of the head (namely, its valency). Thus, apart from XP, there are no labels (Collins, 2002), and the matter is further sharpened as we go along. In keeping predicates distinct from arguments, suppose that in order for lex^n to project, it has been merged with an operator "$\wp$" that indicates that it is an unsaturated predicate. This operator corresponds to the inverse of a nominalization function (Chierchia & Turner, 1987, Cocchiarella, 1985), which converts 'nominals' such as killing into unsaturated predicates such as '- kill -'. Otherwise, lexical elements represent saturated properties or relations. Example (31) thus represents a full saturated projection/proposition:

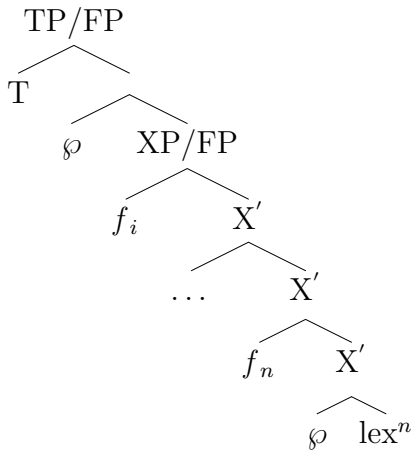
(31)



Let us assume that propositions, represented by LLF representations of the above kind, are abstract entities that can be instantiated in concrete spatio-temporal particulars, or events, hence XP can be merged with another predication operator $\wp$ and predicated of the events located in T (tense). If this suggestion is correct, then tensed sentences contain an "overt event variable" located in the constituent T (for similar proposals, see Chomsky, 2000b, Davidson, 1967, Lepore & McLaughlin, 1985, Higginbotham, 1983, 1985, 1986, Platzack & Rosengren, 1998, Pollock, 1989, Vlach, 1983).[4] All in all, a declarative sentence is represented at LLF as shown in (32).

[4]Thus, according to Platzak and Rosengren (1998), "Finiteness anchors the event in time and space, by identifying a point on the time line with the speaker's here and now" (p. 190)

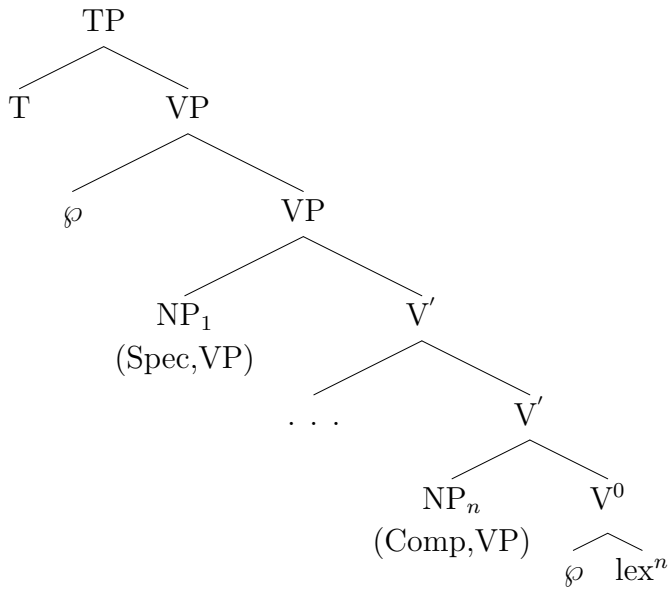
(32)



Here "FP" refers to full projection, but it can be associated with "XP" with the reservation that it is not yet to be interpreted as a "maximal projection," since the number of arguments (X′-level projections) and adicity are not yet restricted in any way. In the vocabulary of the more familiar GB theory, the highest FP is a TP, the lower is a VP without an internal vP projection (but with an unlimited number of slots for arguments), but note that, in avoiding the Strict Lexicalist Hypothesis, the lexical elements at the abstract LLF level *do not so far contain any formal categorial features*: there is as yet no V to project a VP.

In line with a recent version of the minimalist theory, I will not assume Agr projections (Chomsky, 1995, §4), but T may well contain a strong EPP feature that must be checked. C may contain mood and force, its positions being the target of A′ movement, but it is not shown above and will not be discussed here. Since the lower FP represents a proposition and could be compared to VP, it is a "phase" in the Chomskyan sense (2000b, 2001). The status of the upper FP is more complicated, but if we take it to contain an empty C (which is nevertheless ignored here), then it, too, corresponds to a phase. Because of these similarities, the above LLF representation could be labelled, for heuristic purposes, as shown in (33):

(33)



In what follows, I will use these labels when they simplify the exposition, but the underlying form is to be understood as containing no labels. The theory makes no reference to these labels, FP notwithstanding. As I allow any number of intermediate LLF projections, I follow Bouchard (1995, §2.1.2.1), for example, in preventing all vacuous projection, rejecting the Uniform Two-Level Hypothesis of Chomsky (1970).[5]

Assume the positions of arguments are predicted by their thematic properties, in the manner indicated in several recent works such as that by Hale & Keyser (1993). Note, however, that I do not take these thematic properties to be *part of the lexical elements*, assuming the atomistic lexicon: they are rather part of the UG. Thematic properties are discussed in section 3.4. Once they have done their work, they disappear from C_{HL}, leaving only the formal LLF properties. This is a fairly common assumption: the thematic roles of the arguments are represented in the 'deep syntax' (cf. Baker, 1988, Burzio, 1986, Chomsky, 1995, Grimshaw, 1990, Hale & Keyser, 1993, Jackendoff, 1972, Pinker, 1989, Rappaport & Levin, 1988).

In sum, lexical elements at the LLF level represent intensional entities such as properties and relations, being first unsaturated and then predicated from each other: the resulting abstract propositions are further predicated of

[5]It is also assumed that the dative structure is not flat, due to the assumption of binary branching at the LLF level. This assumption is supported by a variety of data concerning binding and quantifier scope, for example (see Fujita, 1996, Kayne, 1984, Larson, 1988, Levin, 1993, §2).

events to represent a (tensed) judgement. Syntactically, the resulting structure (LLF) is close to the Agr-less and double-Spec "bare" LF structures proposed in recent minimalist theories,[6] and could be taken as a certain abstraction from such minimalist LF with some syntactic properties and relations missing, essentially because of clarity and readability.

LLF representations are comparable to a "formal language" with an interpretation, and indeed the theory has been implemented on a computer platform. The representations can be easily translated into a first-order language and given a fairly standard model-theoretical interpretation, with the exception that the model is to contain intensional entities, such as redness, as primitives (Bealer, 1982). Thus, LLF is interpreted, in the core cases, solely in terms of truth-conditional semantics, abstracting away from language use and other pragmatic aspects. This is important, for language is full of phenomena that are not to be explained in terms of representational levels individuated, in essence, by their truth-conditional semantics: I will, of course, return to this topic later. In any case, LLF is only a small part of what we mean by the "interpretation" of linguistic expressions, but, in addition to being, or so it is argued, a linguistically active level of representation, it nevertheless attempts to satisfy the essential properties of concepts in Fodor's theory as well (Fodor, 1998a, §2).

Considered from a slightly different perspective, it is a level of representation that meets requirements from both linguistic data and systematicity/productivity, but it is not a full explanation of either: it is an interface representation, connecting aspects of both. I will return to these questions in a more detailed manner later (§6.1), discussing various options concerning the status of LLF.

A word of caution is in order concerning my usage of the word "lexical element" in what follows. According to the theory of LLF, a lexical element is a primitive element that appears either in a predicate position or in an argument position at the LLF level, and it is neutral with respect to its Case

[6] Chomsky (1995, §4) proposed a way of eliminating some of the explanatory burden by applying what has been called a "double Spec hypothesis." According to this hypothesis, some heads in certain languages have formal features that can be eliminated twice by inserting two elements into the two spec-positions inside this head's maximal projection. If this analysis is pursued successfully, and applied to a variety of Agr-related data (e.g., Pollock, 1989), then it could be shown that elements such as Agr are not real, but (successful) fictive abstractions (option b). If there are no Agr projections, then according to one proposal, the features of the subject are checked at [Spec, TP], and those of the object at [Spec, vP]. This is the structure that will emerge here.

or syntactic category. In other words, my "lexical elements" are more abstract entities than those that linguists might have become used to: they are abstract since they have been tailored to constitute, in part, the interface between concepts and the language faculty in a way that is consistent with what we know about lexical concepts, productivity and systematicity. This means, for instance, that their morphological properties, say restrictions in causativization, are not encoded at this level. Rather, such restrictions are part of an independent morphological component (§6.4). Taking such facts into account produces a more specific and more concrete notion of "lexical element." Although these destrinctions are merely terminological conventions, it is important to keep them in mind in order to avoid misunderstanding and to remain clear about the (so far relatively narrow) explanatory scope of the putative LLF-representations.

From a more personal perspective, LLF representation was meant from the beginning to be a level of 'propositional thought' containing everything that is relevant to the operation of the language faculty. For all I know, propositional thought does not contain nouns, verbs, or their concatenations, but comprise more abstract elements of 'logical subjects' and 'logical predicates'; LLF is a Fregean implementation of elements of the latter type.

3.4 The Θ-theory

In the previous section I spelled out some minimalist assumptions concerning the argument structures, making two novel suggestions. First, I assumed, but have not yet argued, that there are no vP projections. I will return to the motivation for this simplification later. Secondly, I argued that the thematic properties determining the construction of the core argument structures around the head predicates (argument structures being syntactic reflexes of thematic properties) ought not to be part of the lexical elements, taken in isolation, but something that appears when several lexical elements are combined by the operation of pure First Merge.

The purpose of this latter assumption is connected to the atomistic hypothesis: since according to that hypothesis one cannot assume that lexical elements contain semantic lexical decompositions, general principles of the UG must be in operation. Prima facie, this seems to be so, for the argument structures do follow systematic and productive principles.

What follows in this section is a discussion of the more semantic aspects

of the theory of argument structures - what the crucial thematic properties are and how they influence grammar. I will propose, in line with Russell (1940), Grimshaw (1990) and Tenny (1988), that the aspectual dimension of the event structures of predicates is an important ingredient in projecting thematic properties onto argument structures. What emerges is a theory that takes the thematic theory out of the lexicon and raises it in part to the status of a general theory of UG, and in part to the level of other non-semantical grammatical constraints.

In projections with two or more participators (/arguments), these participants must be distinguished from each other, as *Brutus killed Caesar* differs in its truth-value from *Caesar killed Brutus*. So far, the difference is encoded in terms of dominance hierarchy in its lexico-logical form. In model-theoretical semantics this is usually done with respect to the ordering $<f_1$, . . ., $f_n>$ of the arguments. However, the problem is that "ordering" is not cognitively meaningful, although dominance hierarchy (constituency) most certainly is. Why is Brutus "ordered first" with respect to Caesar, or vice versa? Clearly, to avoid circularity we must answer this question before we observe their "order" in the corresponding sentence.

This problem has traditionally been accounted for by postulating thematic roles and their syntactic linking. A considerable part of what would previously have been called part of syntax, subcategorization features in particular, is today often explained on the basis of such semantic properties of predicates (Stowell, 1992). If items in the lexicon lack semantic structure, then one could anticipate that such a task becomes impossible. This is not so, however. We must only jettison the assumption that the relevant semantic properties *constitute the lexicon*. Rather, they must follow from *general principles of UG such that they are not of lexical origin*. This, in turn, requires a theory of thematic roles and their syntactic linking that is general enough to support such a detachment of thematic information from the lexical component.

Traditionally, thematic roles have been characterized by notions such as Agent, Patient and Theme. However, it is well-known that "as soon as we try to be precise about exactly what Agent, Patient, etc., 'mean,' it is all too subject to difficulties and apparent counterexamples," as David Dowty put it in a review on thematic roles (Dowty, 1991, p. 549). I will not use notions such as Agent, Patient or Theme here.

Instead, consider the 'aspectual theory' presented in Russell (1940). Ac-

cording to this theory, *Brutus killed Caesar* differs from *Caesar killed Brutus* in the sense that, in the former, Brutus engaged in the activity of killing first, whereas in the latter it was Caesar. This appears to be a remarkable insight since it turns out that structural argument positions mirror temporal involvement in the aspectual structure of the event. The rest of the semantic interpretation, i.e. the notion of Agent and Patient, Causer and Causee, etc., are taken in this account to be a matter of "pragmatic interpretation" at the C-I level, and irrelevant to linguistic computation. Indeed, they appear to be *special instances* of the aspectual dimension: the Agent temporally precedes the Patient in the event, if it is ever to be its "source" or "originator," and likewise, the Causer temporally precedes the Causee.

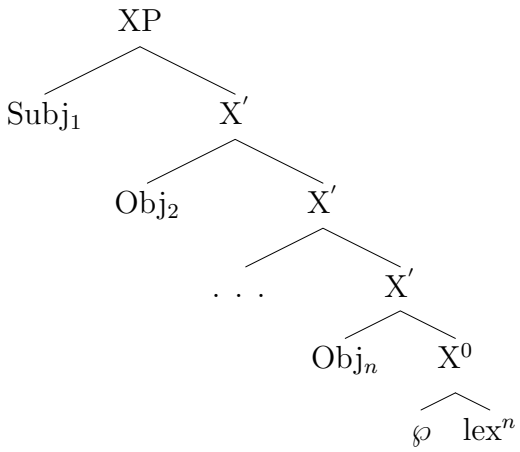
Finding the correct level of abstraction in semantics that is relevant to linguistic concerns is essential (Bouchard, 1995). As I pointed out earlier, it is also essential in atomist theory in that it allows one to detach the problem of syntactic linking from the lexicon.

Assuming that only aspectual properties are linguistically relevant does not imply that there is no such notion as Agent or Patient. The claim is that it is the aspectual dimension that enters the computation of representations at the (L)LF level, the rest being part of the C-I system. In other words, the language faculty does not 'see' whether some constituent it processes represents an intentional Agent or not.

Moreover, there are languages in which Agents - in the pragmatic sense of the term - are grammaticalized in various linguistic constructions by various means, hence the above claim is valid only for the purpose of the individuation of thematic roles at the LLF level. I do not wish to imply by this that all computational processes are insensitive to these pragmatic notions.

If (34) is a lexico-logical representation representing the core argument structure of the predicate lex^n, as assumed in the previous section, the temporal order of the constituents 1, . . ., n in the event of lex^n-ing is as indicated by these indices, Subj_1 being the first, Obj_2 being the second, and so forth.

(34)



Since I am adopting an aspectual individuation of arguments, let Subj$_1$, . . ., Obj$_n$ in any full projection be called *a-arguments*, "a" from aspectual.

One reason for accepting Russell's insight concerning the individuation of arguments (participants) is that many highly obscure thematic roles can be subsumed under this generalisation. By way of illustration, consider the following list of "thematic cores" of predicates (Pinker, 1989, p. 73):

(35) a. X causes Y to have Z
b. X acts on Y
c. X acts
d. X is in a location or state or goes to a location or state
e. X causes Y to go to Z
f. X causes Y to go into a state by causing Z to go to Y
g. X goes to Y

All these examples, perhaps (f) notwithstanding, are instances of aspectual individuation, thus they may be subsumed under the same generalisation. In every case, X's participation in the event precedes that of Y, and the same is true of Y and Z.

Keenan (1976) argued, on the basis of a cross-linguistic survey, that agents and causal forces are universally encoded as subjects. Similarly, following a wide cross-linguistic survey, Dixon (1994) concluded, "What has always seemed to me remarkable is that different languages, from all over the world, show a fair constituency [in that] it is almost always the Agent of AFFECT verbs, the Donor for GIVING, the Speaker for SPEAKING and the Perceiver for ATTENTION that are identified as [subject]" (p. 8), so that agents of

transitive predicates like *cut*, *give*, *tell* and *see* are "consistently treated in exactly the same way, in all aspects of morphology and syntax, across every type of human language" (p. 115). A welcome consequence of Russell's theory, however, is that we do not need to invoke an obscure notion like "Agency" to explain these facts, or any of the putative underlying essences, such as the notions of "causation" (cf. Croft, 1991), "intentionality," "source of self-energy" (Chafe, 1970), "potency of the agent" (Hopper & Thompson, 1980), "relevant to the success of the activity" (Dixon, 1994), or "object affectedness" (Pinker, 1989). These are instances of the more general theory, the agent being the "originator" of the event, hence *preceding* all other participants, whatever other characterisations and semantic elucidations are provided by the C-I system. There is also psychological evidence that novel transitive verbs are not interpreted causally, and that something more abstract must be involved (Naigles & Kako, 1993).

To provide further evidence, Hopper & Thompson (1980) showed that direct objects correlate with a participant that is acted upon or caused to undergo a change (see also Dryer, 1986), both instances of the generalisation suggested by Russell. It has also been established empirically that when interpreting novel 'transitive events,' both children and adults assume the scheme Agent/Patient, again a subcase of the Russellian generalisation (Behrend, 1990, Clark, 1993, Gleitman, 1990). The insight provided by Russell promises to subsume all these as special cases.

I will show later that much the same is true of causatives, representing a bona fide case of the aspectual individuation of arguments (see Comrie, 1985 and Lakoff, 1987, pp. 54-55). In fact, my putative atomistic explanation of "causativity" is already visible here: since causativity is a bona fide example of aspectual relation - viz., a relation between the causer and causee - its overwhelming productivity and systematicity in the world's languages may be explained as *arising from the properties of thematic roles and their syntactic linking*. This amounts to explaining causativity without lexical decompositions or meaning postulates: rather, we begin from general properties of the interface between the language faculty and C-I systems.

If we consider the properties of expressions from the perspective of modular linguistic computation, assuming the above generalisation, we find that there is no causation, volition or permission, and no thematic roles that are visible, only the temporal order of the arguments, encoded in lexico-logical representations. This means that, in terms of linguistic computation, only

formal features of the lexico-logical form, on which the aspectual individuation of arguments are "projected," matters.[7]

Let us explore the empirical meaning of these assumptions further. Consider a typical inchoative (36)(a-c).

(36) a. the vase break
b. the vase was broken
c. Erebus breaks the vase

Example (a) involves only one argument. There is no internal temporal structure to sort out the participators. In (c) there are two arguments. We predict that the external argument represents a participator who participates in the event of breaking before the vase. We grasp the transitive *break* in a sense involving a volatile agent, in this case Erebus. In terms of modularity, however, this interpretation and its pragmatic niceties are supplied by the C-I system (in an indefinitely detailed manner). This agentive interpretation supplied by the mind is not necessary vis-à-vis the syntactic properties of the expressions, as shown by (37)(a-b) below.

(37) a. the wind broke the window
b. the disagreement broke the relationship

None of these involve Agency or volition. Rather, they involve causation and aspectual order. The crucial parameter seems to be Animacy: if wind were considered as an animate being, then the agentive interpretation would again be possible, thus again involving a pragmatic judgement, presumably part of the C-I. But from a syntactic point of view, (36)(a-c) are identical to (37)(a-b) in the relevant sense. Finally, (36)(b) involves an implicit or vacuous cause, to which I will return later.

Consider the fact that not all causatives involve direct or indirect causation: sometimes the relation between the causer and causee (or the caused event) is permissive ('let') or assistive ('help'). There are languages in which the causative morpheme carries all of these meanings instead of the causative proper, and languages in which some other features of the sentence may alter the force of the causal bond (Comrie, 1985, §2.2, Talmy, 1985). This

[7]As Pinker put it: "Thematic information goes into determining a verb's argument structure, but that is the extent of its influence; the rest of the syntax cannot 'see' it directly" (Pinker, 1989, p. 71; for similar proposals, see Burzio, 1986, Hale & Keyser, 1993, Pinker, 1989, Rappaport & Levin, 1988).

phenomenon now has a natural explanation. All different cases and slightly differing causative forces are subsumed under the same linguistic generalisation provided by Russell. The rest is a matter of C-I.

It has been noted that the thematic role of the external argument is assigned not by the head verb alone, but rather "compositionally" by the VP (Chomsky, 1981a, Marantz, 1984). This problem, too, is eliminated under the present proposal. Consider (38)(a-e) (from Marantz, 1984, p. 25):

(38) a. Erebus threw a basketball
b. Erebus threw support behind the candidate
c. Erebus threw a boxing match
d. Erebus threw a party
e. Erebus threw a fit

The problem is that *Erebus* receives a number of thematic roles, depending on the direct object of the verb. However, the problem itself depends on the individuation of these thematic roles. If this is done aspectually, then the problem disappears. In each case Erebus participates in the event before the direct object.

A lexical element may be attached with the feature [+not aspectual], meaning that it lacks an internal event dynamics, and if an event structure is important in distinguishing arguments from each other, it follows that such lexical items may be "combined" in one argument. However, there is evidence that a structural position corresponding to the second argument may appear in such a case, resulting in expletive structures - a structure with an uninterpretable "quasi-argument" (§6.3).

Although the aspectual theory of thematic roles provides potential generalisation, there exists a group of noncanonical verbs that behave differently. These include verbs of mental states, perceiving and receiving, among others. In some cases, predicates have both forms, as possibly in the case of predicates such as *receives/reaches*. These are problematic in terms of for the aspectual theory, and also in terms of the more traditional theory insofar as such more robust thematic roles are drawn into the linguistic structures by general rules. On the other hand, given that thematic roles emerge from the lexicon, these exceptions do pose a somewhat milder problem, for there is plenty of room for lexical idiosyncrasy and stipulation.

Let us assume that the aspectual individuation of arguments explained above is *canonical*, and that the rest are *noncanonical*, to be explained by

postulating specific mechanisms of some sort. This is not a meaningless assumption empirically. Pinker (1984, §8) cited evidence to show that children had difficulties precisely in learning noncanonical verbs such as *receive* or *strike as* if compared to the canonical ones, all conforming to the aspectual patterns suggested here: this suggests that children approach the individuation of such arguments by assuming that they are ordered aspectually. According to Pinker, the differences are, in fact, "dramatic," so that "verbs that intrinsically violate the canonical mapping [. . .] are virtually nonexistent in children's speech" (Pinker, 1984, p. 307, see also Slobin, 1984).

Marantz (1982) reports a study confirming a similar pattern: children had great difficulties in learning noncanonical verbs.[8] Much the same is true of adults. When presented with a novel unknown verb plus a situation in which agents and patients are present, adults pair, by default, agents with the external subject, and the patient with the position of a complement, exactly as is predicted. Dowty (1987) further notes that these noncanonical verbs are few in number, low in frequency, acquired late, and more common in elevated than in casual speech.

If the canonical pattern is aspectual, we would expect children to make another type of mistake: they could easily overgeneralize by creating new aspectual arguments where adult grammar does not tolerate them. This phenomenon has been confirmed in several empirical studies (Bowerman, 1974, Braine, 1971 among others). Children overgeneralize in a predictable way (for similar data from other languages, see Berman, 1982, Figueira, 1984, MacWhinney, 1985, Slobin, 1985; see also Pinker, 1989). Such errors are quite persistent, lasting for several years (Pinker, 1989, §7.1.3).

For this reason, let us assume that some predicates belong to the marked case and are marked with the feature [+noncanonical] to suggest that their arguments are linked to syntactic positions differently, but also exceptionally.

Given the thematic individuation of arguments based on their aspectual properties, plus the fact that the lexico-logical form has the relevant structure, the notion of "thematic hierarchy" can be derived. If there is a causee, it is always dominated by the causer in the lexico-logical form. If there is an Agent and a Theme, the Theme is always dominated by the Agent; in general, if there is an Agent, it dominates Themes, and Goals. The Benefactive always appears after the Theme. Thus, it follows that the hierarchy is not uniform, but depends upon what thematic roles are present. There is

[8]The study is unavailable, but is cited in Marantz, 1984 and Pinker, 1984.

some independent evidence that this assumption is correct (as suggested in Bouchard, 1995, Jackendoff, 1972, Grimshaw, 1990). The proposal is similar to the UTAH principle suggested in Baker (1988), and adopted in several works since then (e.g., Burzio, 1986, Chomsky, 1995, Hale & Keyser, 1993, Rappaport & Levin, 1988), in correlating thematic positions almost one-to-one with a representational structure. Some differences become evident later.

When we assess the aspectual prominence of some participant in an event, care must be taken to ensure that the description involves only the event subsumed under the given predicate, and not some larger context, and that the properties described thereby are really necessary (constitutive) for the event under inspection.

For instance, one experimental paradigm, which aims at describing the direction of causality and hence also the direction of aspectuality in an event, involves asking subjects to fill in explanations of the event. For instance, given a verb such as *notice* within a complete sentence, *Charon noticed Bill, because . . .*, subjects were required to complete the sentence after 'because . . .' (Brown & Fish, 1983, Au, 1986). The result was that, with some verbs (e.g., *recognized*), the explanation involved some property / event concerning the patient (*Erebus recognized Charon, because she . . .*), whereas some other verbs involved the subject (e.g., *amazed*)(*Erebus amazed Charon, because he . . .*). It was reasoned that the latter verbs involved "object causality," as if the causality, and hence the aspectuality, would flow from the object to the subject, contrary to the theory proposed here. Only the other type of verbs with "subject causality" would conform to the pattern presented here.

Clearly, as pointed out by Malle (2001), these *because* clauses provide *explanations for the occurrences of the events, and not of the internal aspectual properties of the event itself.* The *because* clause thus describes another event and, which is worse, an event that only typically precedes the other event and is thus not even a constitutive component of the event in question. In other words, even if Erebus might recognize Charon because Charon is tall, the very event of recognizing, not the event of being tall, is surely caused by Erebus by being, in the first place, in a certain characteristic mental state. Furthermore, Erebus might recognize Charon for countless other reasons as well; for example, Charon might be exceptionally beautiful, or Erebus might have paid some attention to her, for no apparent reason.

In order to illustrate another problem involved in the individuation of the relevant event, let us consider the distinction between *launch* and *en-*

trainment causatives. In both cases the causer causes the patient object to undergo some action, but in the former, not in the latter, the agent provides only the initial impetus. The agent in the entrainment causative is involved throughout the whole event. Example (39)(a) illustrates a lauch causative (*kick*), and (b) illustrates an entrainment causative (*drag*):

(39) a. Erebus kicked the bear
b. Erebus dragged the bear

In both cases, intuition suggests that Erebus somehow "originates" the event. However, it might be questioned whether this initial impetus is really *part* of the event described by the predicate itself. Suppose it is true that Erebus dragged the bear; then substract the fact that the bear was actually dragged (moved). What is left is Erebus' intention, and attempt to drag that does not, alone, constitute the event of dragging the bear. What constitutes the actual dragging, then, seems to be the event of Erebus' and the bear's *simultaneous* movement. If their activities are simultaneous, why, then, does Erebus participate in the event first, before the bear, as we presuppose in order to explain the positioning of the arguments inside the argument shell at the LLF level? In other words, what we observe intuitively as Erebus' participation in the event before the bear seems to occur outside of the event boundaries of the actual dragging.

However, although Erebus surely cannot drag the bear if the bear does not move, the bear could not be dragged unless Erebus, or someone else, initiated the dragging. In other words, although there is no implication in the inititation of dragging of actual dragging, actual dragging entails that someone has initiated it:

(40) The bear was dragged → someone initiated the dragging

In other words, if we presuppose that the event described by the relevant predicate, in this case *drag* (as a transitive verb), is true, the event seems to extend over the boundaries by simulatanous activity of the agent and the patient.

Similar problems, infinitely complex, emerge when we consider the duration or ending of the event of dragging. Thus, suppose Erebus became tired and had a short pause, then continued dragging the bear. Does this constitute one or two draggings? What if the pause was longer?

It is not a major problem that such matters are, in some sense, inherently complex and presumably part of the conceptual-intentional system, hence part of linguistic "interpretation" in its widest possible meaning: it becomes a problem only insofar as they threaten to make the aspectual theory empirically tautologuous, or null, by allowing one to manufacture or taint aspectual properties of events without any constraint, forcing the theory to be true. I do not believe that Russell was wrong, or that the proposal is empirically empty: rather, the problem is to set up a battery of diagnostic tests. For instance, given two participators X and Y plus any verb V, it is possible to ask whether the informant finds the statement 'X V Y $\longrightarrow$ X precedes Y in the event of V-ing' true or not. Even better, it should be possible to design psychological tests to detect aspectual interpretation more reliably. This task was not attempted here, however, but was left as an unresolved open problem. Suffice it to say that such tests could be designed, and that the proposal is thus at least empirically testable more reliably than here. Finally, if this is a problem in aspectual theory, it is likewise a problem in a theory invoking more traditional thematic roles.

Although it seems that obscure thematic roles can be reduced to one predicate, 'temporal precedence,' I will occasionally use the terms "Agent" and "Patient" to refer to 'what precedes aspectually' and 'what is preceded by aspectually,' respectively, when there is no danger of ambiguity. Thus, when Erebus loves Charon, I say that Erebus is the Agent, Charon the Patient, although with the intention that these terms refer to aspects of both lexicological structure and C-I.

It could be claimed that the present theory cannot attain strict atomism: some predicates have an internal, aspectual event structure. This is needed to construct the appropriate argument structures and XP projections. Hence one could argue that the problem posed by atomism is not solved, but only re-stated in new terms.

This argument is based on a misunderstanding. It is not claimed here, as a decompositional theory would insist, that these *event structures are part of the lexical elements*. Take the lexical element representing 'enviousness,' for example, and consider (41)(a-c):

(41) a. Erebus envies Charon
b. Erebus is envious of Charon
c. enviousness is a bad personality trait

Hence the internal event structure arises in some context (a), but is not present in others (c) - thus it is not a constitutive part of the lexical element itself, which maintains its identity across (a-c), as assumed. In other words, aspectual individuation arises according to UG principles in certain complex constructions, and not as being an intrinsic part of the lexical elements. It is a principle of interaction between arbitrary lexical items (e.g. Russell, 1940, Verkuyl, 1989). I will return to the computation of the surface properties, and the categorial features in particular, of these elements later.

Similarly, one could claim that adicity is already a "semantic feature," associated with lexical elements at the LLF level. Yet, again, adicity does not *constitute* the root lexical elements, since it may be associated with any root element *freely* by the principles of UG.

Finally, some 'arguments,' such as adverbs, times, locations, places, instruments and so on (generally adjuncts), cannot be individuated by their aspectual position in the event since they appear to "cover the whole event." Suppose an argument can be merged with a "discourse element" corresponding to more robust thematic roles. These discourse elements are symbolized by "d", hence [d, e] is a valid representation of an LLF argument. As I proceed further in the theory of syntactic linking, I will associate these arguments with a number of syntactic properties, such as prepositions, postpositions, adjuncts, datives and other indirect arguments. Obviously, these matters could not be discussed here without more detailed coverage of the theory of syntactic linking, so I will return to this in more depth later.

If I may summarize the discussion so far, I have presented a theory of core argument structures with a few novel proposals. First, there are no vP shells, but the VP (FP/XP) involves an unlimited number of arguments. Hence, there is no notion of maximal projection at the LLF level. Secondly, if there are selectional features in the lexical elements for the First Merge to operate with, they are associated with these elements according to general principles of UG that rely upon a theory of predication involving, crucially, an aspectual dimension. The lexical stock is not constituted of such semantic features, nor are there thematic features in the operation of C_{HL} inside the language faculty.

In many versions of the standard theory of pure First Merge, the thematic properties of lexical elements guide the construction of the VP shells, the lexical elements containing mutually matching thematic features. For instance, the predicate *love* requires an NP with a thematic feature of the

Patient, thus it is merged with a constituent bearing that feature. Mutual features "lock" the constituents in their proper positions. This proposal is not counter to the one proposed here, as long as one assumes that the assignment of these thematic features to the lexical elements is based on a general theory of UG. For instance, it could be suggested that the theory of predication is responsible for the emergence of such features.

I do not argue for this proposal here, and only point out that it is, in principle, compatible with the one I entertain.

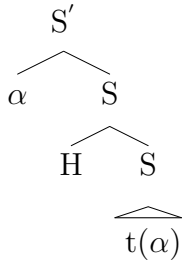
3.5 Agree/Move

Although I am not deeply concerned with the phenomenon of movement in this study, as it appears to be somewhat irrelevant to the question of the abstract lexicon, I will nevertheless present the basics of the minimalist analysis of movement. These aspects of the theory of grammar are briefly discussed on a few occasions later. Furthermore, it is movement, if anything, that has played a prominent role in the formulation of the minimalist hypothesis. Finally, it is movement that is important in discussing the status of the formal features in the lexicon and the Strict Lexicalist Hypothesis.

There are empirical reasons for assuming that C_{HL} contains the operation(s) Move and/or Agree in addition to Merge. The latter establishes an agreement relation between two elements, the former appears to move elements overtly or covertly. The following list of examples illustrates the combined results of overt Move and Agree:

(42) a. Charon$_i$ was-ϕ found t$_i$
b. Charon$_i$ seems-ϕ t$_i$ to be found
c. what$_i$ seems-ϕ t$_i$ to be found
d. the girl$_i$ who seems-ϕ t$_i$ to be here

Thus, sometimes expressions appear in surprising locations that are not expected based on their semantic roles alone. In order to describe this phenomenon, an operator called Move is postulated as part of C_{HL}. Suppose Move can target an element α inside some syntactic object S and Merge it to its "edge," forming a new syntactic object S$'$, as follows (Chomsky, 1995, p. 250):



What are the conditions for applying Move, given SMT? According to SMT, language is an optimal mechanism for satisfying legibility conditions, so there ought to be a reason for the existence of operation Move. It is assumed in minimalist theory that Move can and must be used to eliminate uninterpretable features from the lexical elements before the construction is moved to LF, those features being banned due to the principle of Full Interpretation. Thus, two apparent imperfections, uninterpretable features and displacement, are in fact only two sides of the same coin.

What remains to be explained, and what I will return to presently, is why there are uninterpretable features in the lexicon to be deleted in the first place, and why it is movement, rather than some simple operation Delete, that can and must be used to delete them.

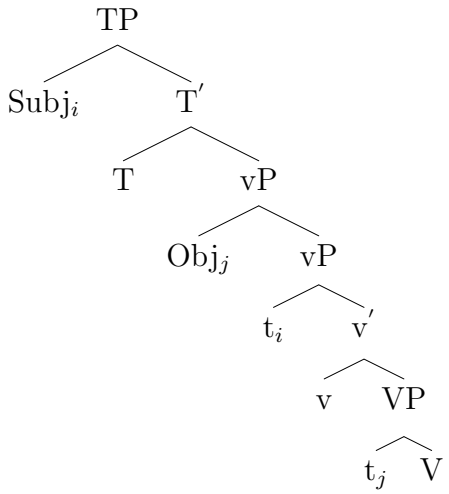
I assume that some element in S, presumably the head H of S with some unchecked feature, must "probe" α. I say that such an element will "attract" α, which is said to be the goal or target. Thus, probing/attract means that some formal feature is eliminated (Erased) in the process, and it is thus a "last resort" operation to satisfy Full Interpretation.

What this feature is depends on the specifics of the theory. According to one proposal, a nonsubstantial head such as v, T or C, can eliminate its formal (uninterpretable) N feature(s) (EPP feature) by probing an element with a categorial feature, and perhaps only when they extend the resulting structure (Chomsky, 1995, pp. 189-190). In other words, only nonsubstantial elements can probe, and they must probe elements with categorial features (Chomsky, 1995, p. 232). The goal may eliminate its Case feature in the process, but it is plainly not obligatory because of multiple agreement structures of various kinds (see Chomsky, 2000b, Carstens, 2001). In this manner, we hope to reduce EPP to strong N feature of I, wh-raising as the strong N feature of C, and so on. The features of the goal are not eliminated if they are interpretable, hence a further head may attract the same element at some later stage in the derivation, resulting in the effects of successive cyclic

movement.

In the following example, the subject NP has been probed to delete the uninterpretable EPP feature of the T, and the object NP has been probed to delete the uninterpretable EPP feature of v, as shown in (43).

(43)



Between the goal and probe, there must be no elements that could probe the goal; thus attract/probe must in this sense be local (Chomsky, 1995, p. 311). In the following examples, the locality condition is broken. In (a), it is the auxiliary that is the closer target for the probe, and in (b) it is the expletive. The wh-constituent has moved from too far in (c).

(44) a. * fix$_i$ Erebus can t$_i$ the car

b. * Erebus$_i$ seems it is certain t$_i$ to be here

c. * how$_i$ did Erebus wonder what Charon fixed t$_i$

Furthermore, an element can attract a goal α only if it c-commands that element. Finally, an element H cannot probe another one if that lies "too far" inside the structure, to preserve the descriptive correctness of strict cyclicity. It is generally thought, and empirically much supported, that the probe H is not able to attract elements in the complement of its own complement, and can only attract the "edge" (its specifier and head). For instance, the embedded wh-element cannot rise to check the interrogative EPP feature of C because a potential landing site has been occupied by *whether*, and C cannot target it in its original location.

(45) * which car$_i$ did Erebus wonder whether Charon bought t$_i$?

In his later work, Chomsky (2000b, 2001) assumes that derivation proceeds in phases, so that when a phase is completed, only the "edge" is visible for further computation (Chomsky, 2000b, p. 108, "The Phase-Impenetrability Condition"). The locality conditions are taken to be part of the definition of Move, as they then reduce the computational complexity of the derivation considerably (Chomsky, 1995, pp. 266-268).

What does the operation Move actually move? Chomsky (1995, p. 252) proposes, on both empirical as well as conceptual grounds, that it moves as much material as is necessary for convergence, and nothing else. This is reasonable, given SMT. Then, if convergence has to do with the elimination of formal features in the case of Move, then this operation should move only those formal features and nothing else, assuming that the formal features cannot disintegrate in the process (Chomsky, 1995, p. 265). Move is thus not called "Move α," but "Move F," "move (a) feature(s)" (Chomsky, 1995, §4.4.4). In overt syntax, PF convergence often requires phonological material to be moved (a form of "generalised pied-piping"), but in covert syntax only the required features are moved.

This relation - the elimination of some feature or another - was later replaced by operation Agree, which establishes the local agreement relation between the features in question. This means that the core syntactic dependency is explained by relying upon a relation between features: one is deleted or erased by the other, a precondition being that the features are identical. Thus, in some cases the elimination of formal features may proceed by displacement (Move/Agree), and sometimes this is not needed (Agree)(Chomsky, 2001).

In any case, Move is allowed only for the deletion of some formal feature or another ("Greed" if the deletion serves only α, "Enlightened Self-Interest" if it can serve other purposes as well), and since Move involves more operations than Merge, Merge is applied first if possible. One instance of a case in which Merge is not possible, and thus Move is instantiated as a "last resort," is case when a constituent *is* required (by virtue of an EPP feature) to occupy a non-thematic position, barring pure Merge from such a position (Chomsky, 2000b, p. 106).

There is also the question of the timing of these operations, which is crucial in explaining certain language-specific variations, especially in word order. In some versions of the theory, Move must be delayed to the covert

syntax, if possible ("Procrastinate"), and in some versions it is vice versa ("Earliness"). It seems too early to make a definite choice among these options: what can be explained in terms of one option can be explained in terms of the other as well by adjusting some of the many auxiliary parameters of the theory. Let us assume Procrastinate. Language-specific variation in word order is explained by using the feature checking / elimination and distinguishing between weak and strong features, the former forcing "overt checking" because of FI at the PF level. If overt checking is not forced, then Procrastinate makes it covert. Since word order is subject to language-specific variation, and language-specific properties are assumed to be reducible to morphosyntactic properties of lexical elements, this seems a promising start - provided that (1) the distribution of weak / strong -features is learnable and (2) the strategy leads to explanatory proposals, not just to re-statements of the basic facts.[9] As is typical of current minimalist theorizing in general, many alternatives to this account exist.

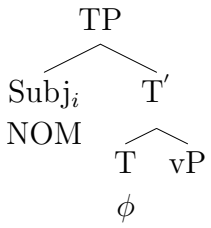
Given these technical preliminaries, some deeper questions emerge. Move, together with Agree, deletes formal features from the lexicon that are not interpretable (EPP, Case), hence the existence of these operations could be said to follow from the principle of Full Interpretation. Why, then do such formal features exist in the lexicon in the first place, given that they appear to violate SMT? The Strict Lexicalist Hypothesis requires that the lexical elements appear in the Numeration with all of their morphosyntactic features in place, but there is no explanation for this principle - more so when it obviously violates the Strong Minimalist Hypothesis.

Chomsky has recently moved towards a 'structural explanation' of uninterpretable features. Consider the operation Agree, which obtains between a probe and a target. The idea is that when an element is probed, say to the [Spec,TP] position, and when an uninterpretable EPP feature of T is deleted in the process, the target NP is valuated a nominative Case, and the T is valuated the ϕ-features of the NP. Valuation means that the lexical element, when it appears in the Numeration, contains only an *undifferentiated Case-feature*. This feature is then valuated either as nominative or accusative,

[9]There is no guarantee in the current minimalist theory that conditions (1) and (2) hold. If there are other aspects (head initial/final parameter etc.) that determine word order, then learning the weak-strong distinction would easily become impossible. Further, current minimalist theory does not appear to be constrained enough to count as explanatory: there is too much freedom in stipulating properties of lexical items whenever some word ordering is encountered.

depending on its structural position. Thus, in the following LF representation, the Agree holds between Subj_i and T. The EPP feature of T is deleted, the Subj_i is valuated nominative Case and T is valuated ϕ-features of the subject, as shown in (46).

(46)



Chomsky, following Marantz, also speculates that categorical features might emerge similarly. It is this idea that I will continue to develop in the next section: *formal features are reflections of the structural properties of propositional thought* (LLF representations).

3.6 The derivation of formal features

If properties of linguistic expressions are derived from the legibility conditions, it is hard to see a forthcoming explanation of purely formal features, such as the categorial or the structural Case, prima facie unmotivated semantically. If there is no such explanation, then the minimalist hypothesis must be wrong in this respect.

This problem is even more pressing in terms of minimalist theory since the core explanatory strategy of the minimalist program depends on the uninterpretable properties associated with the lexical elements, mainly since they are used to explain the displacement property of natural languages. Martin (1999) noted that, since formal features "are not directly motivated by C-I or P-A, their existence is surprising if C_{HL} is perfect in the above sense" (Martin, 1999, p. 1). Pesetsky and Torrego similarly argued that "one of the most long-lasting controversies in linguistics concerns the existence of purely formal grammatical features - features utterly without semantic value" (Pesetsky & Torrego, 2001, p. 364). In the best case, either they are unreal (b), or their imperfection is only apparent (c).

Chomsky (2000b, pp. 119-126) reasons as follows. First, he points out that uninterpretable features, as well as the "displacement property," are clearly "imperfections" with respect to SMT, as explained above: they cannot

be explained trivially by relying upon the legibility conditions, the properties of sound and meaning. He proposes that the displacement property is actually motivated by its ability to produce more "discourse-oriented" semantic properties sensitive to surface order, such as topic-comment, presupposition, focus, and so on, and that the formal features are the mechanisms for implementing such semantic properties. The implementation follows from Full Interpretation, requiring, as already mentioned, that only semantically interpretable features may be present at the LF level. Thus, if formal features are present, they must be deleted before the derived expression is well formed. In Minimalist Program it is assumed that certain configurations delete formal features, so the C_{HL} is forced to establish such configurations, "displacement" begin a welcome but possibly also a non-optimal consequence. In other words, C_{HL} uses formal features to express certain semantic or pragmatic properties of sentences, and what results are side effects such as EPP.

However, this proposal, even if true and fitting the explanation of structural Case and agreement, could hardly begin to explain the emergence of categorial features. This is because the operations Agree and Move that implement feature deletion do not apply to categorial features: the "probes," elements that initiate movement, often (or always) lack them (Chomsky, 2000b, p. 123). Even pretheoretically, it is difficult to imagine how the essence of categorial features could come even close to being "mechanisms to implement displacement."

I will argue that an interesting simplification of the minimalist theory is achieved if formal features such as the categorial and Case features are determined not as intrinsic properties of the lexical elements appearing in the Numeration, but as properties encoding structural information about the respective lexical elements appearing inside whole propositional frames at the LLF level (the idea goes back at least to Chomsky, 1970). The proposal is thus a type of 'structural theory' of formal features, in the spirit of the valuation theory of Case (§3.5), for instance. Since the structural properties of lexico-logical forms are semantically interpretable, Full Interpretation and SMT are not violated thereby. This simplification supports the atomistic hypothesis that is assumed and argued for in this study, since when the lexical elements become more abstract syntactically, much of the appeal of the semantic features used in explaining the various idiosyncratic properties vanishes. For example, if *give* (V) and *giving* (N) arise from the same lexical element GIVE, containing no categorial features, that *lexical element* should

not encode the fact that *give* requires three arguments: *giving* does not require any.

My explanation is close to that of Bouchard, who proposed what he calls the principle of Full Identification (Bouchard, 1995, pp. 93-4):

(47) (The Principle of Full Identification, PFI.) Every syntactic formative of a sentence must have a corresponding element in the semantic representation. Every formative of a semantic representation must be identified by a morphosyntactic element in the sentence with which that representation is associated.

In the present proposal, categorial features are among the "morphosyntactic elements" that are then identified with formal properties of semantically interpretable representations, or semantically interpretable linguistic representations (Logical Form, LF). Categorial features are thus formal in the sense that they represent formal properties of semantic representations, but semantic in that those representations are interpretable: they take part in the compositional interpretation of LLF representations.

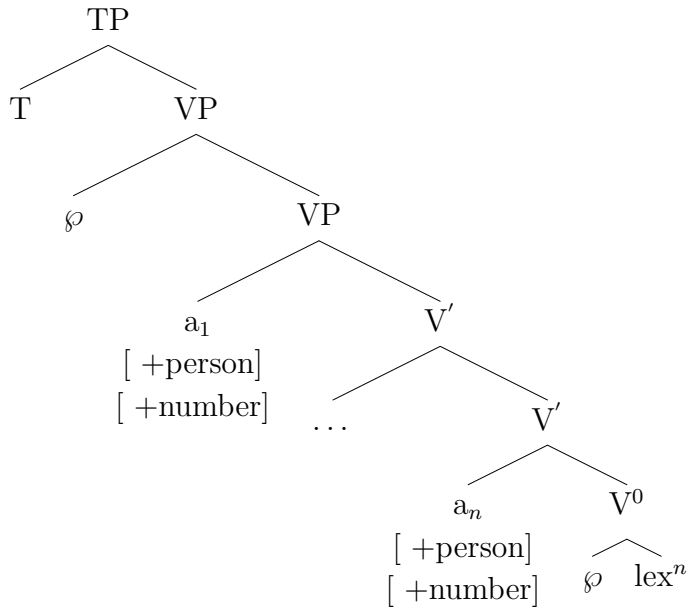
Since PFI is a rather strong principle, and it is executed here in a very literal form in that much of what follows depends on it, it requires some comment. According to Bouchard, PFI follows from the basic minimalist tenet of explaining the mapping between form and meanings without going beyond virtual necessity. We expect syntactic trees and semantic structures to be, on the relevant abstract level, the very same thing. Uriagereka (1998) suggests that something like PFI can actually be derived from FI: "C_{HL} cannot simply disregard linguistic information as some sort of noise; faced with an item to which it can't assign any direct meaning, C_{HL} assumes it instantiates functional structure." I hereby put forward a proposal in which the "functional structure" amounts to a "semantically interpretable structure." These explanations rely strongly on the minimalist hypothesis - from which the motivation for PFI is thus directly descended.

Another way of looking at PFI is as follows. Suppose an explanatory adequate theory of grammar involves some purely formal, grammatical element F. Why is there such an F? In other words, how is F itself explained? Quite easily, assuming that explanatory adequacy has already been reached, one is led to the conlucion that F is just a primitive, biological feature of our mind/brain, to be explained by a combination of obscuring biological factors, phylogenetic or ontogenetic. PFI, in contrast, searches for a *linguistic* expla-

nation in terms of interface conditions. In terms of explanation, it is the first choice to be entertained: the biological hypothesis should be entertained only if the cognitive explanation fails. This is because, currently, such a biological explanation is not in sight.

I will now proceed with the details. Let us consider any lexical element lex as it appears in the LLF representation, as currently defined. Suppose we inspect all of the elements that c-command lex and collect them, or rather some sort of formal tag from each one (see below), to an ordered sequence and associate this sequence, called a *feature vector*, with that element.[10] I will assume that if the element is an operator, such as $\wp$ (predicate operator), d (discourse operator), or T (tense), it is copied as such onto the feature vectors; otherwise symbol "a" is used, as in (48):

(48)



This sequence <$\wp$, a_n, . . . , a_1, $\wp$, T> which is associated with lex^n, encodes information about the relational properties of the lexical element at the LLF level. Symbol "a" is taken to contain the agreement / ϕ-features of the constituent it represents. The notion of a local feature vector, or "L-vector," is defined as containing the two most local symbols in the feature vector of a given element. Thus the L-vector of <$\wp$, a, . . . , a, $\wp$, T> is

[10]C-command is used here in the following sense: A c-commands B if and only if A does not dominate B and every X that dominates A also dominates B (from *Barriers*, p. 8).

<℘, a>.

My hypothesis is that the feature vector, an encoding of the formal interpretable (L)LF properties, can be used to predict the formal properties of the lexical element in question in a way that has many otherwise perplexing properties of language(s) as an automatic consequence. Assume, tentatively, the following rules:

(49) (Categorial features.) For elements with feature vector <a, . . .>, associate feature +N, and for those with feature vector <℘, . . .>, associate feature +V.

(50) (Case.) For elements with feature vector <a, ℘, . . .>, associate feature nominative Case (NOM), for those with feature vector <a, a, . . .> associate feature accusative Case (ACC), for those with feature vector <d, . . .> associate feature semantic Case (SEM), and for elements with feature vector <a, d, . . .>, associate feature genitive Case (GEN).

(51) (Maximal Projection.) For each full projection (phase) at the lexicological level, if several primitive constituents have identical L-vectors or identical indices (identical meaning), then only one of them is interpreted syntactically.

These rules associate formal but semantically interpretable LLF structures with morphologically and phonologically interpretable features. The Strict Lexicalist Hypothesis is rejected in principle, but much of its ideology is preserved: lexical elements are associated with, not assigned or constituted by, formal features, according to the abovementioned rules.[11]

The basic idea of these rules comes from Wunderlich (1997),[12] and from

[11] According to the Strict Lexicalist Hypothesis (SLH), lexical elements appear with their full morphosyntactic features already installed in the Numeration from which the derivation begins. This principle is essential to the claim that uninterpretable features explain the displacement property, and it was originally motivated by some perplexing facts concerning the ordering of adverbs. SLH is not denied or supported by this work. If SLH is true, then the formal features derived from LLF are assigned to the lexical elements at some pre-derivational step, the derivation proceeding in much the same way as has been assumed in the minimalist framework so far, or then these features are checked at the LF level provided that the more simpler LLF could be used to replace LF (which undeniably requires more work). If, however, SLH is false, then the Morphological component could take the feature vectors of lexical elements as its input and provide the correct form to the PF. In that case, formal features are not true syntactic features and drive no syntactic operations.

[12] Wunderlich attributes this idea also to Paul Kiparsky.

some recent ideas proposed in minimalist theory. The point is that a single feature system encodes abstract positions of lexical items and/or their morphosyntactic properties. In Wunderlich's system, this has two binary features, as follows:

[+hr] for "there is a higher role"
[+lr] for "there is a lower role"

The syntactic realization of arguments is restricted to the positions and forms that are expressible with these features. "There is a higher / lower role" that encodes structural information. In Wunderlich's approach, they are associated with the structural Case as follows:

Dative Case	[+hr] [+lr]
Accusative Case	[+hr]
Ergative	[+lr]
Nominative/Absolutive	[ ]

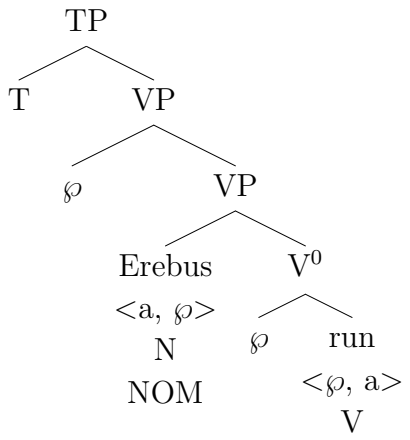
Why should such features exist? Perhaps they do encode the properties of some (abstract) structure: What rules [49], [50] and [51], together with the relevant notion of structure (LLF), contribute to this problem is that *the features, in this case the features of feature vectors, do encode real structure, namely, the lexico-logical form.*

Since lexical elements at the LLF level are void of formal features, they are not "lexical elements" in the traditional sense, but individuated instead by their semantic properties. This abstraction surely has a trade-off in Morphology, stems being abstract, categorial features and other "inflectional" morphosyntactic phenomena (Case, agreement) and at least some derivational ones (e.g., causativity) being reflect of syntactic structure at LLF (feature vectors). This means that the "morphosyntactic features," traditionally understood but including categorial aspects, are not distinct heads (as assumed in the more standard minimalist theory), but formal features derived from the constituents appearing in the feature vector of a given abstract lexical stem, thus implementing the relation Agree of Chomsky (2000b, 2001) and related work. However, formal features do of course mirror the syntac-

tic hierarchy as the feature vectors specifically code it, and it then follows that, as Chomsky (personal communication) puts it, language has "roots that are category-netural, and configurations that determine what we informally called syntactic categories."

To see how the rules work, consider the derivation of simple intransitive and transitive sentences. The feature vector is also shown below each constituent. First, an intransitive sentence is derived:

(52)

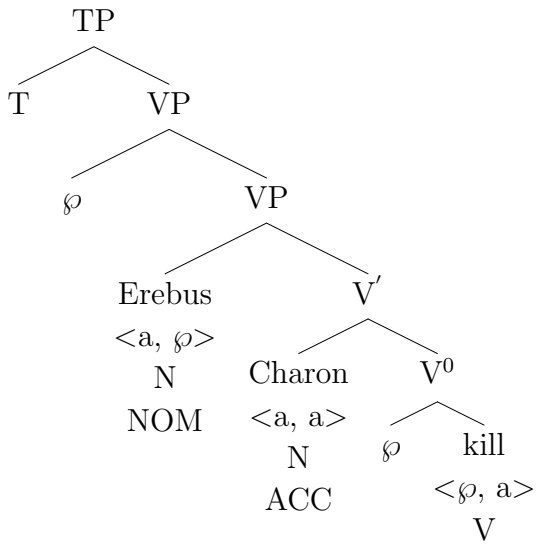


The feature vectors are as follows: *Erebus*= <a, ℘> = N-NOM; *run* = <℘, a> = V, which generates:

(53) Erebus-N-NOM runs-V

A transitive sentence is generated from the following LLF representation (54):

(54)

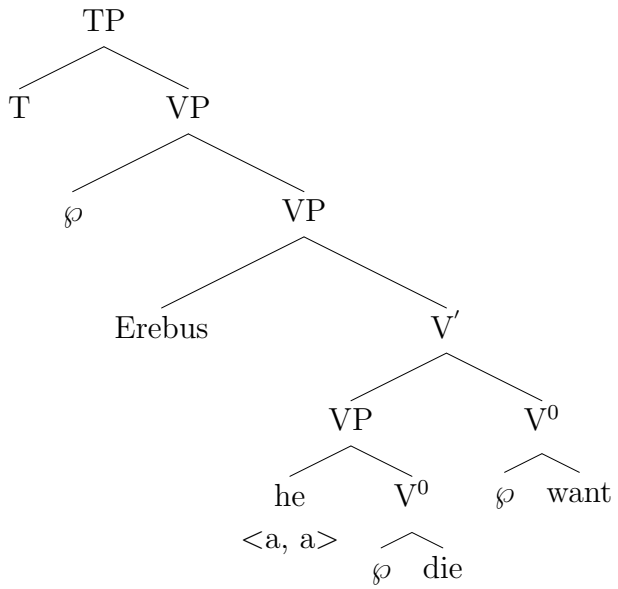


The feature vectors for the NP-arguments are *Erebus* = <a, ℘> = N-NOM, *Charon* = <a, a> = N-ACC, V as above; this generates

(55) Erebus-N-NOM kills-V Charon-N-ACC

The formal categorial and Case-features are assigned thereby, without the need to stipulate them at the LLF level. Now consider the ECM phenomenon, demonstrating that thematic properties and structural Case mismatch in certain environments. This is a curious phenomenon: a thematic subject takes the accusative Case. This follows under the present assumptions, according to which the structural Case depends on the feature vectors of the elements. Suppose that another proposition is embedded as an argument for a predicate. The following lexico-logical form (56) encodes its meaning:

(56)

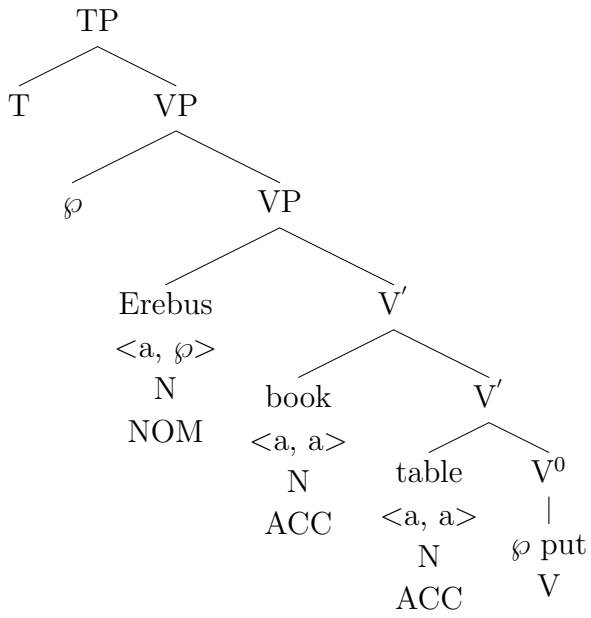


Under the given rules, the derived expression is *Erebus wanted him to die*, where the subject of the embedded clause takes the accusative Case. This leads to the derivation of expressions such as (57):

(57) a. Erebus wanted him-ACC to die
b. Erebus considers him-ACC intelligent
c. Erebus ate the fish-ACC raw

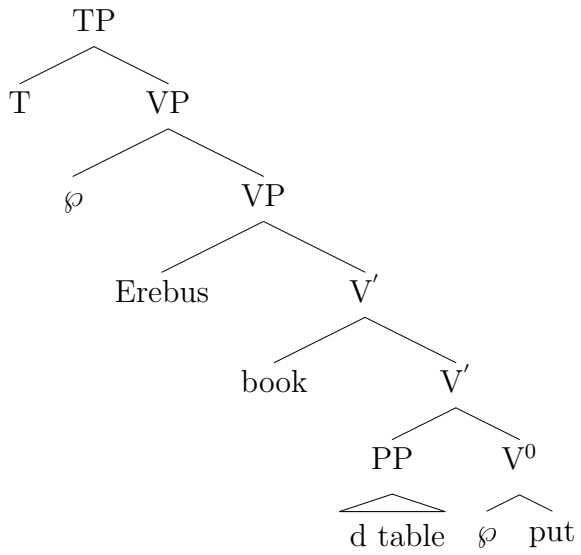
The special problem created by ECM structures disappears whenever a language allows this option. Rephrased in more traditional vocabulary, the matrix verb happens to 'govern' the subject of the embedded infinite clause, because it is nearby and c-commands it in a certain way. There are reasons to assume, however, that the infinite complement clause could contain a defective T node. I will return to this presently. Instead, consider lexico-logical form (58):

(58)



In this case, *book* and *table* have identical L-vectors <a, a> so that according to rule [51, p. 97], one of them is eliminated at the level of syntactic interpretation. Recall that rule [51] prevents C_{HL} from interpreting lexical elements at the LLF level if they have identical L-vectors. It is nevertheless possible to use the discourse operator (d-operator, see section 3.3) as a "last resort" since the feature vector of a d-marked argument differs from an argument that lacks a d-operator, although in that case we must link the argument with semantic Case by [50], generating (60).

(59)



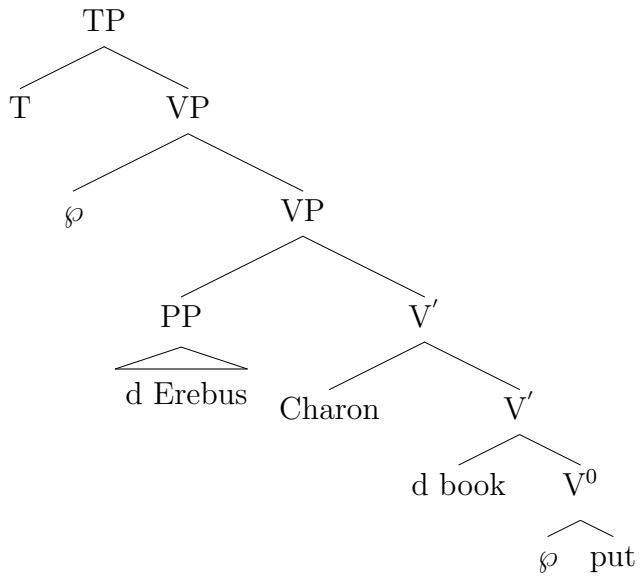
(60) Erebus put a book [$_{PP}$ on the table-SEM]

Indeed, indirect objects are more formal (and interpretable) in structure (or, at least have different properties) than direct arguments. This is necessitated by the linking rule [51], which requires that if there are two objects, then they cannot both have the feature vector <a, a> (ACC). Thus, the rules predict that '— XP XP' is an impossible subcategorization frame (e.g., Palmer, 1994, §3.5.1). This entailment is consistent with other grammatical theories, such as Relational Grammar and Lexical Functional Grammar, none of which allow verbs to have two direct objects.[13]

The present proposal does not rule out constructions with d-marked subjects and "quirky cases," such as (61):

[13]Putting "double DO languages" such as Kinyarwanda aside for now.

(61)



Icelandic (Zaenen, Maling, & Thrainsson, 1985), Japanese (Ogawa, 2001, §2.4.4), Spanish (Fernández-Soriano, 1999) as well as Hindi (Mohanan, 1990), seem to realize this possibility through dative or PP subjects, and there are other languages as well that make the phenomenon quite robust. The following examples come from Chilean Spanish and Icelandic (Holmberg & Hróarsdóttir, 2003):

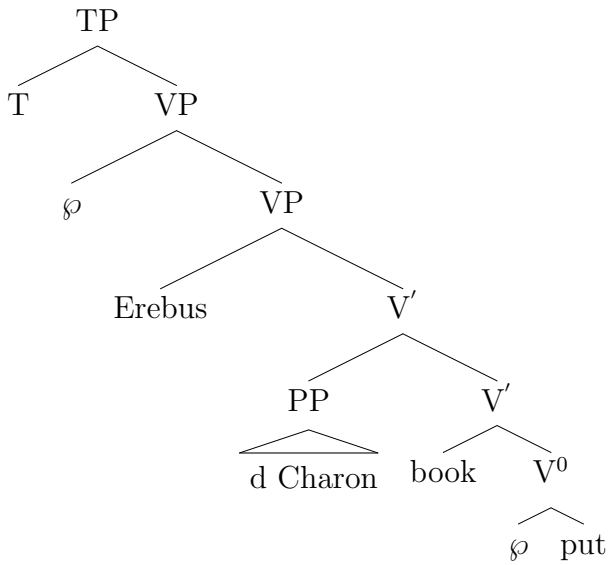
(62) A Juan le quiere gustar Marta
A Juan-SEM clitic wants like Marta
'Juan wants to like Marta'

(63) Hennar var saknað
Her-GEN was missed
'She was missed.'

Nevertheless, significantly, it is also possible to save a ditransitive clause as a "last resort" by d-marking the argument in the middle, creating an IO-DO order aside from the DO-IO order. Both orders are base-generated in Japanese, the former obeying dative marking (d-marking) and the latter obeying the d-argument (thus, IO is PP). Moore and Perlmutter (2000) argue that both cases exists in Russian: dative surface subject and the dative surface direct object, although they also show that the direct object datives have some subject properties: this is an important feature to which I return later in section 4.6 when discussing causatives. Figure (64) illustrates

d-marking in the middle argument:

(64)



Furthermore, d-marking the "middle argument" (DO-IO) is essential in explaining the properties of causatives, in which, as is well known, it is often the direct object causee that alters its grammatical properties when more arguments are added (Comrie, 1976), and d-marking accounts for such changes in grammatical properties quite correctly: the accusative causee is demoted to the status of oblique argument. I will return to this phenomenon more fully later in my analysis of causative constructions in Chapter 4.

One consequence of the linking rule [51] is that it *explains* the Case Filter of the GB Theory, according to which all overt noun phrases must receive Case and appear in a position that receives Case (at s-structure; see Chomsky, 1981b, p. 49ff). If lexico-logical form feature vectors are linked to the structural and semantic Case, and this mapping is unique since identical feature vectors are not interpretable syntactically ([51]), then the Case Filter is predicted: each noun (i.e. a conceptual constituent in the A-position at the lexico-logical form) at s-structure must receive Case (i.e. have a unique feature vector attached to Case).

This could be considered from a somewhat more informal perspective. The purpose of these rules is to associate a propositional thought, or linguistically relevant aspects of it (LLF), with formal features such as category and Case. However, it is assumed that the process is limited in the sense that

only certain formal features count, making two expressions indistinguishable under further processing if those features are identical. This is one and a particularly strong way of operationalizing the idea that language processing is modular.

According to the *Theta criterion* of GB theory each argument in the d-structure, or members of each A-chain at the LF level, can receive only one theta role, but each theta role of the argument structure of a predicate must be assigned to some argument. This condition follows: each LLF argument is assigned only one theta role (either from being an a-argument, or by being d-marked). Furthermore, theta roles are assigned from nowhere else.

Another principle that has gained in importance in recent years is Baker's "guiding principle," the *Uniformity of Theta Assignment Hypothesis* (UTAH), which states:

> Identical thematic relations between items are represented by identical structural relationships between those items at the level of d-structure. (Baker, 1988, p. 46).

According to UTAH, a substantial part of syntax is to be understood as a consequence of semantics (of the lexical entities entering into the representations). As Baker noted, this principle can be made substantial only in the context of some substantial theory of "thematic relations" which he nevertheless left "at a rather intuitive level." Clearly, however, the empirical content of UTAH depends on such a theory (see Pesetsky, 1995, among others). In the present proposal UTAH is maintained in the lexico-logical form alongside the aspectual theory of thematic roles (§3.4), but at the same time it is maintained that such thematic roles are not part of the lexical items or of their meaning.

There are some differences, however. As in the present case the θ-theory is a consequence of the individuation of two or more arguments, the principle is vacuous in the case of just one argument. This argument does not have to be distinguished, or individuated, at all. Thus, were there only one argument at the LLF level with an intransitive predicate, there would be no stable 'thematic position' corresponding to that argument.

One may ask why languages seem to obey something close to the X-bar theory. This question is not meaningless, and is considered in the literature

from several different angles.[14] The present hypothesis is that the X-bar theory is essentially correct because there are no syntactic features at the LLF level, so that the rules operating at that level are insensitive to them. There are no categorial features there either, because the lexical elements are individuated by their category-neutral semantic properties ('concepts'). Rather, the categorial and Case features are given in the syntactic interpretation by the above rules. Since lexical elements at the lexico-logical form are free of syntactic features, such features being associated with them only in the process of linking, we may use 'X' at that level to denote a variable ranging over syntactic features - these are assigned independently by rule [49], hence are not part of the lexical items themselves.

There has been some discussion about why many grammatical relations, if not all of them, have something to do with the notion of c-command (Brody, 2002, §3). Regardless of whether the approach is derivational or representational, the idea is to reduce the notion of c-command to some more primitive notions, such as to properties of the operation Merge. In the present case, the reason why the c-command emerges is essentially reduced to the principle of Full Identification: the relational properties of lexical elements at the LLF level are coded in terms of feature vectors based on the c-command, while the c-command seems to be the only reasonable choice available. Another possibility would be to look "downwards," but this strategy is ambiguous since the nodes branch at every level, the root nodes notwithstanding. Upwards-looking is unambigous: each node has exactly one node that dominates it.

Central to the minimalist theory is the so-called EPP feature (Extended Projection Principle). This was originally postulated in order to give a description of the fact that finite clauses require, pro-drop languages aside, a phonologically overt subject. Moreover, there is evidence that even in the case of pro-drop languages, the syntactic structure contains a phonologically

[14] According to the standard minimalist assumption, the X-bar theory looks correct because the computational operations Move and Merge are not sensitive to categorial features. Chomsky proposes in Categories and Transformations that notions such as XP and X^0 reduce to the property of projection: a category that does not project is XP, one that has not projected at all is X^0, and the rest are X'. Then the X-bar theory is correct in its essentials because it mimics the distribution of the relevant features determining the construction's ability to project. Kayne (1994) has yet another view, in that he tries to derive the properties of the X-bar theory from the Linear Correspondence Axiom. According to this idea, the X-bar theory may be derived from the properties of the linear ordering of the elements because linear ordering must be mapped to "asymmetric c-command". I will not discuss this proposal here.

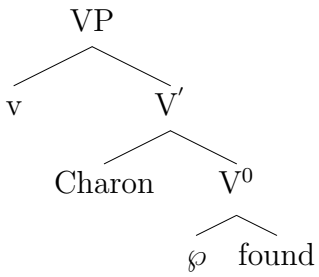
covert subject. A variety of phenomena fall naturally under this principle, among them predicates with expletive subjects (weather verbs), passives (obligatory raising of the object), rising constructions and others. In the minimalist program, EPP is interpreted as a more general condition stating that certain functional heads, T (or I) among them, require an element in their specifier position. The idea is that these functional elements bear an uninterpretable EPP feature that must be deleted in order to satisfy the condition of Full Interpretation, and that moving a suitable probe into such a position suffices to delete that feature. The fact that finite clauses require a subject reduces to the existence of the EPP feature of a finite T. The problem here is that, although the postulation of an EPP feature seems to capture an imporant generalization, its own presence is a mystery and a source of puzzlement.

I have expanded the discussion on the EPP feature in two sections: one to do with the obligatory presence of subjects in "normal finite clauses" and the properties of personal passives, and the second to do with expletive structures (§6.1). I will begin with fairly "normal" finite clauses.

Current rules already entail something very close to the EPP principle. Suppose that only full projections are well-formed at the LLF level, a natural assumption since they form semantically coherent bundles of features. Consider any finite clause: it has a V^0 element which is c-commanded by T. In between, there must exist at least one argument to compose a full projection out of V^0. If there are n arguments, n always > 0 according to the above reasoning, and one of them, the topmost one, must have the nominative Case since its feature vector is <a, $\wp$>. This is so because its feature vector "sees" the predicate operator $\wp$ of the finite TP projection. Nothing of the sort necessarily holds in infinite clauses, since they lack (finite) TP projections (I will return to "defective T" later on). Thus, a significant part of the EPP principle, leaving more exotic constructions aside for now, is simply an inherent consequence of the rule system developed here.

Let us now consider personal passives. They, too, seem to fall under the EPP: a passive is something that lacks a thematic subject, and where the patient behaves as if it were the syntactic subject. Indeed, passives and rising constructions have been characterised by their "suppression" of the logical subject. Suppose, then, that the lexico-logical form allows the suppression of the logical subject: the "vacuous argument" v is installed at the top of the full projection, replacing the agent of the event, as shown in (65)

(65)

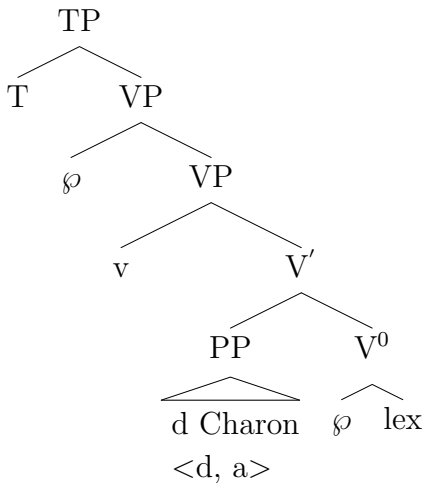


The vacuous argument v accounts for the intuition that passive verbs are still, in some sense, transitive verbs, being "detransitivized." It is just that they are "defective arguments." Assume that v is defective and not visible to the feature vectors of the constituents below it, hence it behaves as a "non-argument." Given that v is invisible in the determination of the feature vectors for *Charon*, *Charon* = <a, ℘> = NOM ([50]), the same feature vector as with v, so again one of them must be deleted due to [51] in syntactic linking. Assuming v is deleted, then we derive (66).

(66) Charon-NOM was found

The problem of "grammatical function changing" disappears, at least in the case of basic personal passives. This phenomenon simply follows from the rules given above. Moreover, if a predicate has d-marked arguments, then the presence of a vacuous argument does not change its syntactic properties, since the feature vector remains <d, a>. We predict that PPs and datives do not 'passivize,' which is the correct result. The following tree (67) represents a passive structure with the dative argument:

(67)



The original explanation for the EPP feature thus applies to passive constructions if we assume that the LLF may contain arguments that are invisible "from below." They are semantically present (representing the presupposed agent), but syntactically invisible. They are syntactically invisible because of the idea that syntactic properties reduce to the notion of a c-command at the LLF level, and "defective arguments" are invisible from this perspective.

One important, although not uncontroversial, generalisation of the GB theory is the "Burzio's Generalization":

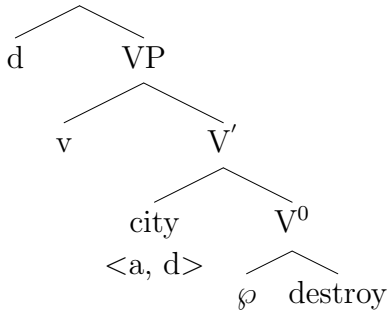
> (Burzio's Generalization.) A verb that governs an NP Case marks this NP structurally iff the verb has an external argument.

This generalization now follows, since to have an accusative Case and hence feature vector <a, a> entails, in the typical case (excluding ECM structures), that there is also another, aspectually more prominent argument (the "external argument") present in the lexico-logical form.

More interesting, and otherwise rather perplexing, consequences follow immediately and in a 'natural' way. Suppose we have a proposition, such as *the barbarian's destruction of the city*, as a d-marked argument of a predicate, such as *was awful*. We can then insert a vacuous argument into the position of *barbarian's*. The genitive subject *barbarian's* is replaced by the vacuous argument v, the patient "rising" for the reasons explained above. Yet in this case, the raised constituent has feature vector <a, d>, linked with the genitive and, not the nominative Case. This is the correct result:

(68) a. The barbarian's destruction of the city

b. The city's destruction by the barbarians



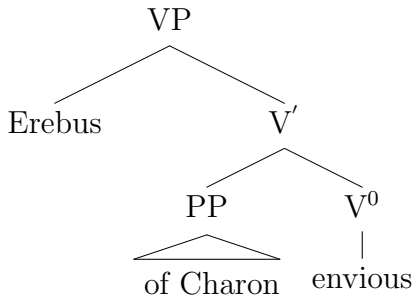
Furthermore, assuming that these genitive gerunds result from d-marking without tense, we of course expect that they are never tensed, as indeed is the case. Thus, we see again how rules [49]-[51] conspire to generate the correct surface forms. Note that these results *also* follow as such if we assume that, instead of being defective argument v, the subject of nominal passives is simply missing. The cost is that one must stipulate that nominal sentences are d-marked arguments, as evidenced by the fact that their subjects appear to take the genitive Case (or at least not the nominative or the accusative). The question of exactly why they are d-marked, namely whether the explanation is semantic or syntactic (e.g., comparable to the treatment of the last resort in the case of dative arguments), is not discussed here.[15]

[15]The present proposal is in agreement with the proposal that DPs are, basically, propositions with predicates and arguments, but often without tense (e.g., Bernstein, 2001, Grimshaw, 1990). However, there are reasons to be sceptical about the proposal that the above-mentioned constructions with genitive subjects of DPs are directly comparable to clausal passives. Longobardi (2001, §1.2) mentions four reasons. First, there are lexical restrictions on whether the process may apply (*The perception of the problem, *the problem's perception*). Secondly, the subjects of passivized DPs lose their controlling ability, while in the case of clausal passives they do not (*the sinking of the ship to collect the insurance, *the ship's sinking to collect the insurance; the testing of such drugs on oneself, *this drug's testing on oneself*). Third, the preposition expressing the subject in clausal and nominal passives may be different. Finally, clausal passives have their own verbal morphology, whereas nominal passives do not. According to Longobardi, these four properties make it plausible to compare nominal passives to middle constructions rather than to clausal passives (e.g., expressions such as *senators bribe easily*). In Finnish, with its rich verbal morphology, practically any verb can be made into a "middle" by adding a specific suffix, which turns a transitive verb into an intransitive one. In that case, one argument may be completely amiss at the LLF level. But note that, if that were the case, the present theory would still predict the correct properties: defective argument v is just an invisible argument from the perspective of the elements below it. If nominal passives are middles and not nominal passives *per se*, then this means that such an argument is also invisible from the LLF perspective. This could explain the lack of subject control.

Consider a predicate that has two readings, one non-aspectual such as *envious*, one aspectual such as *envies*. We assume that they correlate with the same constituent in its lexico-logical form (representing 'enviousness,' §2.1). Thus we have *Erebus envies Charon* according to the rules so far, yet there can exist only one aspectual argument for the non-aspectual counterpart. Thus we can form *Erebus (is) envious* (accounting for the presence of the copula later).

Yet the lexico-logical form can also have two arguments (as nouns and adjectives in general have arguments), but due to [51], one of them must be d-marked as a "last resort," generating *Erebus is envious of Charon*, *of* being the reflection of the underlying d-operator (a PP) at the LLF level.

(69)



Thus, the problem of as why nouns and adjectives need *of* support disappears entirely: non-aspectuality together with rule [51] entail this property.

Vacuous arguments can be inserted into the position of objects (Patients) as well. That there is a category of "null objects" cannot be doubted (e.g., Huang, 2000, §2.2.2). What happens, from a syntactic point of view, if a vacuous argument is inserted at the object position? The present rules predict no change: there are no constituents below "object" v at the lexico-logical form position for which this insertion would have corresponding syntactic implications. Thus we might have pairs such as the following:

(70) a. Erebus eats bread
b. Erebus eats

Non-aspectual predicates can have at most one thematic a-argument. This includes adjectives, nominals and some verbs. I predict that passivization is not possible, since, as expected due to the fact that the vacuous argument occupies a thematically derived structural position at the LLF level,

the vacuous argument is thematic (Agent). In other words, non-aspectuality entails the inability to passivize. This seems correct (so examples in Quirk, Greenbaum, Leech, & Svartvik, 1971):

(71) a. * is being tall Charon
b. * two hundred pounds is weighted by Erebus
c. * five dollars is cost by this pen
d. * Amy is resembled by Sue
e. * four is equalled by two plus two
f. * Erebus was resembled by Bill

Inherently non-aspectual predicates cannot take two thematic arguments, when inherently means "always non-aspectual." Moreover, unaccusatives seem to correlate in their content with the crucial aspectual property at hand when they represent states (Grimshaw, 1990, §2.5.3, Levin & Rappaport, 1995, §3.3). It is predicted that non-aspectual unaccusatives never take only one a-argument. This is the case, as can be seen below (cf. Fillmore, 1968, Lakoff, 1970):

(72) a. the mail arrived
b. * the mailman arrived the letter
c. the mail arrived at its destination
d. * the letter was arrived by the mail
e. the war emerged
f. * the emperor emerged the war
g. * was emerged by the war

I also predict that they could not passivize, lacking two or more a-arguments. This is indeed one property of unaccusatives representing states (Burzio, 1986, Levin & Rappaport, 1995, Perlmutter & Postal, 1984). Furthermore, some of them adopt special adjectival forms, supporting further the fact that they, like adjectives in general, do not have an internal event structure: *recently arrived quests*, *collapsed lung*, *rotten apples*, *vanished treasure*. On the other hand, if a non-aspectual verb has two arguments, which is surely not impossible, then the other argument is predicted to appear in the semantic Case (PP) as a "last resort":

(73) a. Erebus stood on the ground

b. * Erebus stood ground

c. * the ground was stood on

Note that non-aspectual verbs cannot have only one aspectually individuated argument, but discourse arguments are not banned. The other possibility would be to use a non-thematic argument and, indeed, these predicates easily appear with expletives (*there arrived three men*). Expletives are discussed in §6.3. This is important, since, again, unaccusatives are claimed to be predicates that cannot assign the accusative case, hence they appear in an oblique case. According to the present proposal, however, these properties follow from non-aspectuality.

Properties of lexical elements individuated from the more 'surface level' are notoriously hard to formulate, since they are presumably to be explained by invoking the interaction of several principles and, what is worse, the lexicon is naturally the place for a considerable amount of idiosyncracy concerning language use, pragmatics, and so forth. Therefore, the above remarks should be taken as tentative: I expect there to be exceptions, and I will return to the matter more closely in what follows.

The following data shows that we lack an essential property of the vacuous argument, central to the analysis of passives. In each case, a constituent has been "raised" from an embedded full projection:

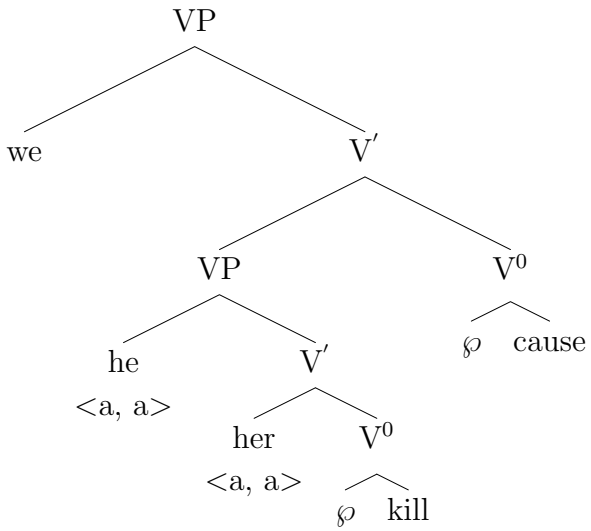
(74) a. Charon seems intelligent

b. Charon was expected to be found

This is an astonishing feature of natural language(s). The distance between Charon and v may be arbitrarily long, but the proposed mechanism that generates 'raising' in personal passives is extremely local, in fact clause-bound. Yet it is related to the EPP phenomenon, as these constructions, too, lack a subject that must be probed from the underlying structure

In pursuing one possible explanation, note first that (75) violates [51]:

(75) we caused [him to kill her]

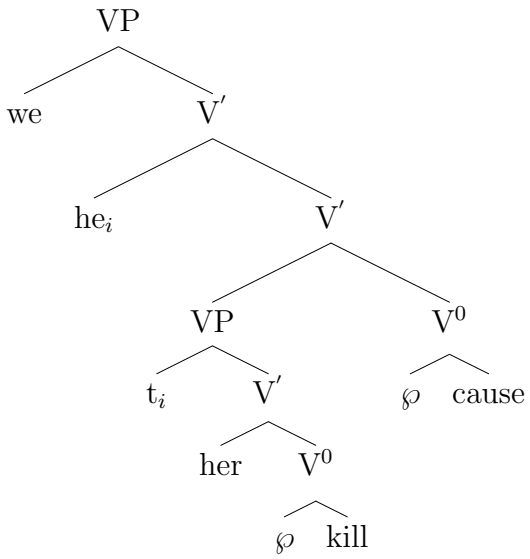


In this sentence, *him* and *her* have identical feature vectors (<a, a>, ACC). Yet they both "pass" at the syntactic-linking level since (75) is well-formed, contrary to what is expected according to the present theory and rule [51], which prevents the C_{HL} from interpreting syntactically two constituents inside the same FP with identical feature vectors. I suggest that this is related to the fact that *he* is still rises in the corresponding passive and has certain other "object properties" (cf. Postal, 1974):

(76) he was asked to kill her

This indicates that *he* would be part of the upper FP, as assumed in Lasnik (1999, §2, 1999b, Lasnik & Hendrick, 2003, §3.2.2) and Bowers (2001). Assume, for now, that this is a matter of extremely local or minimal A-movement which I call "restructuring": the accusative subject of the embedded infinite clause rises to be part of the upper FP, as shown in (77):

(77)



The target position is simply stipulated. More importantly, note that two assumptions converge: assuming that *he* is part of the upper FP, it can satisfy [51] and passivize. Consider the effect of the rule as shown in (78)(a-d), where the lexico-logical form (a) is linked with syntactic properties (d):

(78) a. v expect [v found Charon]
b. v was expected [Charon to be found]
c. v was expected Charon$_i$ [t$_i$ to be found]
d. Charon$_i$ was expected [t$_i$ to be found]

The rules that generate personal passives plus restructuring entail long-distance movement in a truly minimalist spirit: the element moves in a strictly cyclical fashion, one clause boundary at a time rather than in one long step.

But what is restructuring? It might be an economical Last Resort operation: when there is an escape hatch for an argument otherwise deleted by [51], the escape hatch is used.[16] The other possibility would be to assume that arguments rise because of some morphological requirement. My own hunch is that, given that these speculations are not completely off the mark in the first place, restructuring is a consequence of some currently unknown,

[16]One type of evidence relevant to this issue is the fact that in some languages (e.g., Spanish), both the matrix predicate and the embedded predicate agree with the raised (restructured) element. This suggests that the restructuring might not be movement, but copying, as suggested in the minimalist program.

albeit deep and important, aspect of the construction of LLF representations from the syntactic point of view.

Restructuring was described as a process in which the subject of an infinite complement is moved to the matrix clause, taking the role of the object. This definition, although correct for present purposes, is unsatisfactory because it relies on the surface properties of expressions rather than on the LLF properties. Clearly the phenomenon of restructuring belongs to the LLF level if it is used to explain long-range displacement.

To give a first approximation, but one that is sufficient for the purposes of the present study, restructuring may occur at the LLF level whenever there is no additional structure between the embedded proposition and the matrix proposition. Thus, for the present purposes, the presence of T or a d-operator dominating the proposition could be said to block the application of restructuring. Otherwise, the constituent is assumed to be able to move. Note that what prevents restructuring is additional structure (T, d) between the embedded and the matrix clausees: a d-marked argument in the embedded clause, although it does not arise in personal passives, may do so in the case of restructuring (see Frampton & Gutmann, 1999, §5.2 for examples of this type of "quirky case" in Icelandic, and Fernández-Soriano, 1999, for Spanish).[17]

In sum, and to return to the explanation of personal passives, the vacuous argument can be inserted in any argument position at the LLF level. The following sentences are typical examples of subject and object replacement, respectively:

(79) a. Charon was found
b. We ate

There are two other possibilities. If we assume that the LLF structure contains two direct objects, the vacuous argument could occur in the middle position. This still leaves two accusative direct objects, of which one must be deleted according to rule [51], ruling such constructions out. If any of these arguments is d-marked, it remains intact and does not alter its grammatical properties as a consequence of the presence of the vacuous argument.

[17]There is evidence that the presence of a dative (experiencing) subject in a rising construction may block restructuring. This is important, fact but irrelevant to the discussion that follows.

This construction survives, but there would be no syntactic trace of the vacuous argument because the feature vector of the lower d-marked argument would detect no change. Rather, what emerges is (79)(b) with a d-marked argument, such as *Charon ate for pleasure.*

A declarative sentence contains another A-position, however, that of the T node. I have assumed that the T node is the locus of the event argument. So far I have not examined the question of whether a T node might contain something with similar properties to the vacuous argument, resulting in a "defective tempus." There is some evidence that this is possible, for we are searching for a structure in which an embedded clause would have tempus, while behaving syntactically as if the T node were not there. This type of sentence is found in Finnish, among other languages (see Boskovic, 1996, Chomsky, 2000, 2001, pp. 6-9):

(80) minä näin Merjan lähteneen
I-NOM saw Merja-GEN/ACC left-PAST
'I saw (that) Merja has left.'

(81) minä näin Merjan lähtevän
I-NOM saw Merja-GEN/ACC leave-PRESENT
'I saw (that) Merja has left.'

In both cases, the subject of the embedded clause appears in the Case of the structural object (here the genitive: for more on the Finnish case system, see §6.3), and rises in the corresponding rising construction:[18]

(82) Merja näyttää lähtevän
Merja-NOM seems leave-PRES
'Merja appears/seem to leave.'

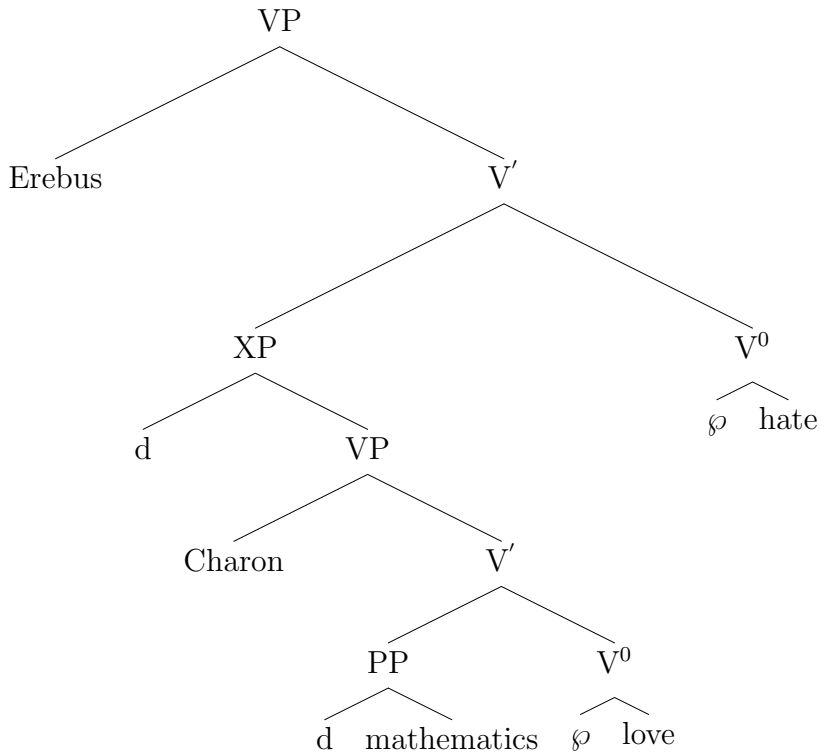
Normally, the presence of a tense prevents restructuring (ECM), but here it is obvious that restructuring plus an accusative Case marking may occur even in a sentence that is tensed. There is thus evidence, in Finnish and

[18]Chomsky assumes that a defective T does not have a "tense structure" (Chomsky, 2000, p. 105). In that case, I would have to assume that these Finnish examples are not instances of defective tempus, but CPs with null C and a full tense. This assumption appears problematic in the current framework, and in the light of theory-external evidence as well (e.g., the lack of agreement inside the embedded sentence), whereas the assumption of defective tempus entails exactly the correct properties. Therefore, I assume here that defective tempus contains features such as PRESENT / PAST, in much the same way as the Finnish impersonal covert expletives have features such as [+pluar][+rational].

elsewhere, that we may not wish to rule out the possibility of "defective tempus" and hence maximal generality in the distribution of v at the LLF level, in other words, that the vacuous argument may indeed occur at any argument position.[19]

Assuming that the d-marking of a complex constituent results a non-aspectual reading of whatever is represented by that constituent, certain principles interact to transform the form of the sentence. The relevant lexicological form is represented below:

(83)



Several factors have an effect here. First, since d-marking is associated with non-aspectuality, I predict the gerund form of the verb. Then, according to [51], the second argument must be d-marked as a "last resort." Finally *Charon* takes the genitive Case since it has the feature vector <a, d>. This

[19]This observation, if true, is clearly important in explaining the nature of restructuring itself. Primarily, restructuring must not be blocked by the "defective T," although it is by a normal full T. This is because if the defective T is analyzed as a vacuous argument, such a node is invisible to the arguments inside the embedded proposition. Hence it also follows that the "triggering structure" of restructuring must lie in the embedded structure.

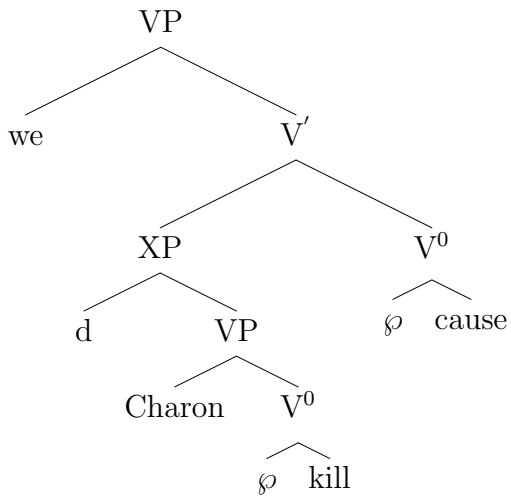
lexico-logical form, meaning that 'Erebus will hate the idea that Charon loves mathematics,' is then linked to expressions (84a-c).

(84) a. ? Erebus hates Charon's loving/love of mathematics
b. Charon's killing was awful
c. Charon's leaving from the solar system surprised me

According to my semantic intuitions, this is correct. Note that I assume here that it is the gerund nominal that is productive enough to warrant this generalization at the level of syntax. Other nominal forms do not seem productive enough, so they merit separate treatment following which the conclusions, if executed in the present context, are currently open (e.g., **the stories' amusement of him*, Chomsky, 1970). I return to this issue later, since it is crucial in understanding the nature of redundancy in the lexicon, here especially as it is related to the study of causatives.

I also predict an interaction between ECM constructions and sentential nominals: assuming that sentential nominals are non-aspectual, I expect them to lack ECM structures due to the presence of d-marking at the top of the embedded clause. This is shown in (85):

(85)



This turns out to be the desired result, since ECM structures indeed cannot be found inside NPs:

(86) a. Erebus believes Charon to be intelligent
b. * Erebus' belief Charon to be intelligent

The following data further supports the conclusion that, as assumed in the above argument, genitive gerunds do not have an internal event structure representing propositions in a fully abstract sense:

(87) a. Erebus watched Charon leave twice
b. ?? Erebus watched Charon's leaving twice

Here, (b) is a genitive gerund, incapable of taking an aspectual adverb twice. Now consider

(88) a. i. Erebus wants Charon to leave twice
ii. ? Erebus wants Charon's leaving twice
i. Erebus wants Charon to leave here
ii. ? Erebus wants Charon's leaving here
i. Erebus wants Charon to leave now
ii. ? Erebus wants Charon's leaving now
i. Erebus wants to leave
ii. * Erebus wants leaving

These aspectual adverbs modifying the complement clauses produce odd sentences if there is no internal event structure present, as assumed. A similar pattern is to be found in other languages. In Japanese, for instance, the complement clause is marked with *koto* if it represents an "abstract concept" formed "out of the proposition," while it is marked with *no* if it is used to represent a "concrete event" (Kuno, 1973, p. 221).

According to this theory, sentences are much like nominals in their structure (e.g., Ogawa, 2001). Whereas most minimalist theories explain their differences by the presence of nominal affix or a nominal head in the nominalized clauses, in the present case the difference is located in the differences in the corresponding feature vectors. These elements in the feature vectors (i.e., d-operators) have morphological consequences that show up as nominal affixes.

In sum, and returning to the EPP principle, I have argued that there is no EPP principle as such, but that its overt manifestations are the results of several interacting principles. This seems to me a significant departure from the standard minimalist theory, but I think it is an improvement: the EPP principle has been a mystery. At the same time, much of the evidence that

has been explained directly by invoking it, such as A′-movement, remains to be explained.

So far I have been occupied in presenting and exploring a few of the main theoretical assumptions taken up in this work, in addition to the atomistic hypothesis entertained earlier, and in embedding these assumptions in the core of minimalist theory, guided by the strong minimalist SMT hypothesis. These assumptions were the three rules [49]-[51] that were used to derive, but not to stipulate, the formal features of the lexical elements, and which, when used together, entail a number of grammatical properties, including the Case Filter, personal passives, Burzio's Generalization, UTAH, *of* support in the case of nominal and adjectival predicates, and aspects of other principles such as EPP. These phenomena are certainly quite real, but the suggestion is not that one would wish to introduce dramatic changes to minimalist architecture itself, but that a somewhat deeper unification might be possible in terms of [49]-[51]. Thus, the lexical elements that appear at the LLF level do not have all their morphosyntactic properties in place, as stipulated in the Strict Lexicalist Hypothesis: rather, these features emerge as the syntactic interpretation proceeds from LLF towards PF. This is important, for it makes the lexical component abstract enough to allow me to attempt to combine minimalism with the atomistic lexicon - with which this thesis is concerned.

Rules [49]-[51] are not intended as all-explaining, but rather as abstract principles that may lie behind the surface aberrations of natural language(s). Other grammatical constraints might be explected to be in operation at other levels, especially in the morphological component, and I will turn to them in detail in the subsequent sections. Moreover, many aspects of C_{HL} have simply been omitted for now, including head movement and other grammatical aspects that have putative ramifications in word ordering.

LLF is targeted to represent a formulation of 'propositional thought' insofar as its linguisticially relevant properties are concerned. Rules [49]-[51] thus have an intuitive interpretation, as follows. The formal features of lexical elements such as Case and category are seen as encoding (for the speaker) and revealing (for the hearer) the structural and hence also the semantic properties of thought. Principle [51] is intended to capture the fact that the interface between the language faculty and thought contains a bottleneck: when thought is 'formalized' for the purposes of the operation of the language faculty, the resources are very limited, and many elements of thought cannot survive the filter.

3.7 The empty lexicon and subcategorization

The received view is that the lexicon has been conceived of as a collection of features: phonological, syntactic, semantic and pragmatic. For instance, the lexical item for the verb *run* could be individuated as follows:

run:

/run/	phonological features
V	formal features
run(Agent)	argument structure
'. . .'	semantic decomposition

The arguments put forward in previous sections suggest that the semantic features of the lexical items ought to be unstructured primitives, at least insofar as they are visible to the computational processes available in the 'language faculty.' Let us assume that they are primitives, thus there are no decompositions present. A lexical entry for run could thus look like the following:

run:

/run/	phonological features
V	formal features
run(Agent)	argument structure
+run	semantic feature

[+Run] is a semantic feature, capturing the content of *to run*. This content is best understood as being the 'phenomenon of running' as understood by human beings. Let us now turn to the syntactic features. There is no trivial way of predicting the syntactic class of the corresponding expression at the surface from the semantic feature [+run], as *running* (N, A), *to run* (V), *runs* (V) all denote, in some sense, the same phenomenon of running, and we do not want to lose this generalization. What running is, or whatever it is that we are acquainted when with we know what running is, has nothing to do with distributional and morphological facts about language. In other words, if it were possible to communicate by telepathy, features such as V,

N and A would not necessarily play any decisive role in such communication. If this is the case, where do these syntactic features emerge? Given the minimalist background, this problem must be solved.

The most straightforward hypothesis is to assume a plethora of lexical items corresponding to the stipulated syntactic features. According to this scheme, there is a separate lexical item for the verb *run* and the noun *running*. This entirely stipulative strategy is not at all satisfactory, since the differences between verbs (*runs*) and nouns (*running*) would appear to be entirely mysterious - even more so in the case of the minimalist program, which seeks to eliminate all "imperfections." I have assumed that these features emerge in the course of syntactic linguistic computation (§3.5), and hence they are not part of the lexical elements. Thus, if *run*/*running* are morphemes in the surface language, they both correlate with the same lexical entry containing both phonological and semantical features at the LLF level. According to this hypothesis, the lexicon truly contains what is required by "virtual conceptual necessity" alone, the pairing between sound and meaning:

run: /run/, [+run]

The syntactic features have vanished altogether from the lexicon, assuming that this move proves successful. Morphological and distributional facts, reflected in the categorial and structural features, are explained as a by-products of the linguistic computation. General UG priciples account for the further semantic properties.

In the context of the more standard minimalist theory, these highly minimalist assumptions of the "bare lexicon" might look impossible in practice, due to the well-known fact that at least some subcategorization restrictions seem operative at the lexical level, and there is some evidence that such restrictions are best explained by assigning semantic features to the lexical elements (s-selection). Although some general aspects of subcategorization already emerge from the principles set out above, these idiosyncratic properties, while surely represent an important aspect of any theory of grammar, do not. A typical example is given below:

(89) a. Erebus gave a book to Charon
b. * Erebus gave a book

The lexical element for *give* thus requires certain arguments that seem to

be semantically motivated, while the atomistic "bare lexicon" explained above seems utterly incapable of capturing these facts, precisely by virtue of its assuming no semantic structure inside of the lexical elements. Furthermore, I have assumed that there is not much syntactic structure in the lexicon either.

However, this argument is mistaken in that as soon as one drops the assumption that lexical elements are constituted by their categorial features, as assumed here, this objection loses much of its appeal. Nominalized predicates, represented here by identical lexical elements with verbal predicates at the LLF level, are not subject to such restrictions:

(90) a. Charon's giving of money to Erebus was astonishing
b. Charon's giving of money was astonishing
c. giving is a virtue

The surface forms *giving* in (a-c) results from the same lexical element *give* at the LLF level, hence *there is no more no reason to stipulate these restrictions as part of the abstract lexicon.*

I do not claim that such restrictions do not exists: rather, they do not belong to the lexical component of the grammar which, therefore, does not need to be constituted by semantic features. These restrictions emerge at some later point when the lexical element has been assigned its categorial feature +V, possibly as late as in Morphology (as evidenced from the fact that often such restrictions concerning e.g. causativization or subcategorization found in English do not extend to Finnish). There may exist semantic explanations of such restrictions, say in terms of event structures, whole propositional frames or extra-linguistic aspects: what is important to the present argument, and what is relevant here, is that such explanations need not, and plausibly do not, invoke lexical decompositions at the LLF level.[20]

If the presence of certain complements can be explained away by assuming that lexical elements at the LLF level are abstract category-netural roots, the fact that *when* such arguments are present they are not arbitrary cannot. Example (91) illustrates why.

(91) a. Erebus' giving a book to Charon was unexpected

[20]There is a residuum of data that cannot be explained along these lines. For example, certain predicates simpy cannot take sentential or propositional complements (**Erebus sleeps that he is married*).

b. *Erebus' giving a book from Charon was unexpected

Thus, the verb *give* does not allow one to combine it with a prepositional phase headed by *from*. From the perspective of the present theory, certain predicates can be combined with certain d-operators, but not with others, and this seems to be a matter of semantics. However, even if a verb such as *give* is associated with the d-operator *to*, this relation does not have to, and presumably cannot, represent a *decomposition* of *give*. It is again a matter of interaction between *give* and the other element involved in the event represented by *give*.

3.8 Conclusions

A theory of lexico-logical form (LLF) was outlines in this chapter. Lexical items appearing at the LLF level are atomic and correspond to primitive morphemes at the surface level. They were assumed to represent properties. A system of linking rules was then suggested within the minimalist framework. This shows that a compositional atomistic theory may be linked to linguistic representations with rules that are not too complicated, but in fact the contrary seems to be the case.

Admittedly, showing that these assumptions would gain comparable or even better explanatory force than any previous minimalist theory would require going into details of linguistic data beyond the scope of this thesis, in fact beyond the timeframe of a single lifetime. Nevertheless, in the next chapter, I will investigate in detail the properties of lexical, morphological and analytic *causatives*, applying the theory to that area. I will then turn again to many other grammatical properties in Chapter 6.

Chapter 4

Causatives

4.1 Introduction

Causatives represent a bona fide case of a linguistic phenomenon that threatens to tear apart the atomistic lexicon: practically all analyses of this phenomenon involve decomposition, and when they do not, they do not seem to be explanatory, relying on stipulative, or at best descriptive meaning postulates. There is a good reason for these choices: decompositional analyses trace the origin of many linguistic properties, both syntactic and semantic, of the causatives to their shared conceptual constituent CAUSE. This is an attractive line of inquiry to start with, since this constituent has meaning on both sides of the LF, affecting syntactic and semantic computations. On the other hand, those who deny that such elements are real, linguistically or otherwise, have been relying on meaning postulates, but essentially due to the lack of a better theory.

However, in truth, there is no inference from the presence of CAUSE to a *biclausal explanation*. These two assumptions, although closely related, are independent of each other. According to Pesetsky's (1995) theory, for example, the element CAUSE is, from a syntactic point of view, a 'preposition' and hence the structure is not *syntactically biclausal*. Pesetsky's position is close to the one presented here in that the element CAUSE is an affix that rises to check the causative affix of the verb, adopting the Strict Lexicalist Hypothesis. Similar questions arise in the case of Larsonian vP-shells. In the present Case, causativity affects grammar indirectly: first, aspectual individuation of arguments is interpreted causally according to C-I system, whereas it correlates with morphological changes in the lexical item itself.

Specifically, I will argue that there is a third possibility that does not assume any kind of linguistic causal element. In sections 3.2 and 3.3 I developed a thematic theory that relied on aspectual properties of the arguments / participants in an event denoted by the predicate. The role of the θ-theory changed: it was no longer a description of the thematic roles (θ-grids) of the lexical elements, but more a theory that was used to distinguish arguments / participators in an event. Therefore, it is part of the UG and, more importantly, a theory of how predicates and their arguments interact in complex constructions. It is the whole propositional frame that involves thematic roles, not any of its lexical elements taken in isolation. Causativity is seen as a subtype of aspectual order, aspectuality provided by the UG, causativity by the C-I system. This means that, from a linguistic point of view, there is no underlying element CAUSE, yet it is easy to see that the existence of certain meaning postulates is predicted. For instance, among other inferences, the sentence *Brutus killed Caesar* implies that Brutus preceded Caesar in the event of killing.[1]

4.2 Causativity

A typical causative construction expresses the notion that "someone causes something to occur," giving rise to specific inferential relations between the respective morphemes. As an illustration, consider the following versions of the Finnish verb *juosta* ('to run'):

(92) juostiin kotiin
ran-PASS-PAST home
'People ran home / there was a running home.'

(93) juoksin
ran-PAST-1SG
'I ran.'

(94) juoksutin koiraa
ran-CAUS-PAST-SG1 dog-PRT
'I made the dog run.'

[1]Russell was aware of the problem that this analysis leads to infinite regress, since precede is a verb that seems to presuppose its own meaning in the individuation of its arguments. Thus, the fact that Caesar precedes Brutus in the event of killing entails that Caesar preceded Brutus in the event of preceding. I do not attempt to deal with this problem here.

(95) juoksututin koiraa
ran-CAUS-CAUS-PAST-SG1 dog-PRT
'I made the dog run / I made somebody to made the dog run.'

(96) juoksutettiin koiraa
ran-CAUS-CAUS-PAST-PASS dog-PRT
'A dog was made to run.'

From the perspective of form, (92-96) are morphological variations of the same 0-morpheme *juoSTA* ('run'). Items (94-96) are causatives since, from a semantic point of view, they involve the sense of someone's causing somebody to run. In (96), the causer is vacuous, hence it is a 'causative passive.'

Item (95) represents what has been called "indirect causation," whereas (94) involves "direct causation." The difference is that, in the case of indirect causation, there is sensed of something being between the causer and the causee, so that the causer is not directly responsible for the changes affecting the causee, whereas in direct causation they engage in more direct physical contact with each other.

Causative morphology is productive in Finnish, admitting relatively few morphological gaps, at least compared to English. Turkish, Quechua, Matses and Tarascan are similar to Finnish in that there are few causative morphemes, exploited in an iterative and virtually gapless way. English is more restrictive, containing only a few morphological causatives such as *mechanic-mechanize, large - enlarge, sure - ensure, red - redden.*

Verbs, nouns and adjectives all undergo causativization, even in passive forms when they are verbs (but not, of course, in all languages), thus for each of the verbs listed above, perhaps apart from (a), there is also a corresponding specific causative nominal. The nominal form of (c), for instance, is illustrated in (97).

(97) juoksuttaminen on tylsää
running-CAUS is boring
'Making somebody run is boring.'

Further elaboration is possible, since it is also possible to construct agent and patient nominalizations from deverbal causative nominals:

(98) juoksuttaja
run-CAUS-AGENT
'the one who causes running.'

(99) juoksutettava
run-CAUS-PATIENT
'the one who is caused to run.'

I call these causatives "morphological causatives," which are, in fact, one of the most extensively studied linguistic phenomena in the world's languages. Their diagnostic properties include (i) the appearance of a special causative morpheme, or several morphemes, (ii) a monoclausal structure at least on the surface level, and (iii) causative interpretation. I will illustrate later how each of these items raises interesting problems.

First, a few comments on each of these conditions are in order. There are various kinds of "special causative morphemes" mentioned in condition (i), ranging from the more fusional (or inflectional) to the agglutinative. Problems arise when the causative affix is highly fusional and perhaps even irregular. Thus, some linguists include a fourth criterion that the morphological causative must be a member of a productive system, as there are causatives satisfying conditions (i), (ii) and (iii) that are nevertheless irregular and hence, in some sense, unclear instances between the morphological and the lexical causatives (see Shibatani, 2001, for a review). As they are irregular, they must be listed separately in the lexicon. Shibatani (2001) also offers some evidence that these irregular morphological causatives tend to pair with lexical causatives in their semantic properties, as lexical causatives, together with irregular morphological causatives, imply more direct causation than morphological causatives (more below). Making a definite choice on this matter is not altogether essential, but in the context of the present theoretical framework it seems appropriate to include this fourth criterion in the list.

In many cases, a morphological causative word begins a 'life of its own,' attaining special meaning which, in some cases, might be quite far from the possible original causative. The following monoclausal (criteria ii) examples come from Finnish, and each word appears with causative morphology (criterion i):[2]

(100) olettaa, pidättää, tuulettaa, hivuttaa
'presuppose, withold/arrest, ventilate, move slowly.'

Yet it is unclear whether these satisfy criterion (iii), the causative meaning; it is also unclear whether they have any kind of root predicate form. In

[2]I thank Urpo Nikanne for raising the issue.

some cases, it seems evident that the morphological form, even if identical to the causative morphology, is not related to causatives, as is the case with predicates such as *saavuttaa* 'reach.' In many cases, a word that might originally have functioned as a fully transparent causative has assumed special rules of use. I will return to this matter in section 4.8. Nevertheless, care must be taken in applying crieria (i-iv) when inferring LLF properties from the surface form. It is not to be expected that a completely transparent set of inferentail rules can be given since, especially towards the lexical level, irregularities and other peripheral (in the sense of not being part of the core grammar) and not so peripheral properties begin to take effect.

"Causative interpretation" (iii) should be taken as a specific type of 'cognitive perspective,' as Pinker (1989) put it: "Viewed with a sufficiently sharp microscope, there is no such thing as direct causation: when I cut an apple, I first decide to do it, then send neural impulses to my arm and hand, which cause the muscles to contract, causing the hand to move, causing the knife to move, causing the knife to contact the surface of the apple, causing the surface to rupture, and so on" (p. 86). In some cases the causative chain is rather long and intricate, as in sentences such as *the president of the United States grows bananas in Guatemala.* Thus, 'what causes what' in a causative construction might not in reality have as much to do with (physical) causation as with how we conceptualize the situation and what aspects we perceive as 'salient' (e.g., Langacker, 1987).

Productive morphology is not a necessary feature of causatives, as we have alternatives such as *Erebus killed Charon* and *Erebus caused Charon to die.* Morphologically unrelated causative pairs are called "lexical causatives." Their diagnostic properties are (i) the lack of appearance of a causative morpheme and irregularity of form, (ii) a monoclausal structure on the surface level, and (iii) a causative interpretation. One major research problem concerns the conditions, i.e. the nature of the represented event itself, under which a causative verb can be, or is most typically, lexicalized (e.g., Haspelmath, 1987).

In addition, there are causative phrases themselves (that is, phrases on the surface level), such as *Erebus made the ship sink*, *Erebus caused the ship to sink*, *Erebus made Charon give a present to James* and *Erebus had Charon give a present to James.* In some such cases the cause appears as an adjunct, marked by the by preposition, for example, *the ship was sunk by Erebus.* If causation is explicitly marked by a causative word at the surface level,

such as *had*, *make*, *cause*, *let* or *help*, I call such constructions as "analytic causatives." The diagnostic syntactic property of an analytic causative is that it is biclausal at the surface level. I assume that English "bare infinite causatives" such as *Charon made the ship sink* contain clausal complements, in this case the *ship sink* (Felser, 1999, among others).

The terms "morphological causative," "lexical causative" and "analytical causative" come from Comrie (1985). The classification is simplified, but widely used. Furthermore, properties of the "d-structure" (in terms of GB theory) are not used diagnostically in this study: rather, whether morphological causatives are biclausal (decompositional) in d-structure represents exactly what is controversial. Thus, I will use the surface properties of causative constructions to diagnose the type among lexical, morphological and analytical causatives. I do not claim that these categories are 'real' rather than convent heuristics or, presumably, only taxonomic artifacts equal to notions such as nouns and verbs.

Indeed, the diagnostic properties of morphological, lexical and analytical causatives may be in conflict. One example comes from Chichewa. In this language, analytic and morphological causatives coincide at least morphologically in the sense that the same causative appears as an (movable) affix in both constructions. Nedyaldov & Silnitsky (1973) concluded that, "In a number of languages there are transitional cases where the causative morpheme can function both as a causative affix and as an empty causative verb" (p. 6, cited in Baker, 1988, p. 151). French (and other Romance languages) have similar peculiar 'mixed properties.'

After studying a number of languages, Song (1995) presented a somewhat more detailed typology of causatives. She assumed that causative constructions could be analysed as consisting of two abstract particles, Vcause and Veffect, the former being the "causative particle" and the latter being the verb for the effect. The typology was then based upon the syntactic-surface-closeness properties of Vcause and Veffect, ranging from "complete fusion" (lexical causatives) to "complete separates" (analytical causatives). "Morphological causatives," with their various versions, lie in between in this classification. Some of the properties of Song's typology coincide with those described above, yet it singles out some additional classes, the "AND-type causatives" and "PURP-type causatives."

There is an interesting semantic relation between analytic, morphological and lexical passives. The 'directness' of the causation increases (rather

smoothly and iconically), from analytic causatives via the morphological to the lexical ones (Bouchard, 1995, pp. 104-108, Comrie, 1985, §2.2, Fodor, 1970, Gergely & Bever, 1986, Shibatani, 2001, Song, 1995). This phenomenon has been exploited quite extensively in showing that lexical or morphological causatives cannot be decomposed into analytical ones due to the lack of strict synonymy. One cannot analyze, or exhaust, the meaning of lexical or morphological causatives in terms of phrasal ones.

Not all causatives involve direct or indirect causation: sometimes the relation between the causer and causee (or the caused event) is permissive ('let') or assistive ('help'). There are languages in which the causative morpheme carries all these meanings, and languages where some other features of the sentence may alter the force of the causal bond (Comrie, 1985, §2.2, Talmy, 1985).

In addition to the fact that causativity is a common property in the lexicons of a wide variety of otherwise unrelated languages, as a semantic property it seems to correlate with certain grammatical effects, implying that, presumably, there is something in it that interacts with the syntactic computation inside the language faculty. According to those who support the decompositional explanation, that something is the lexical semantic feature CAUSE in its various forms. Let us call this as the "lexico-centric explanation": the origin of the semantic and syntactic properties is traced to the properties of the lexicon. Looked at from a slightly different perspective, scholars who prefer decompositional analysis of causatives claim, in effect, that there is something in lexical and morphological causatives that resembles analytic causatives, namely, they are decomposed to a similar representation. There is, of course, the substantial question of determining what "similarity" amounts to here, but it often looks as if the putative semantic representation underlying lexical and morphological causatives is the *same* representation that underlies the analytic causative.

4.3 An atomistic explanation of causatives without meaning postulates: a first sketch

In this section I will outline my solution to the problem of explanatory adequacy in the case of causatives, giving the technical details only in the next section. The problem, to recap, is to explain the systematic and productive

syntactic and semantic properties of causative constructions in a minimalist framework that assumes lexical atomism and rejects the view that lexical, morphological and analytic causatives are mapped onto identical semantic representations.

According to Dowty (1979), among others, explaining the widespread phenomenon of derivational causative morphology requires one to postulate a causative decomposition in which new lexical elements may be derived from previous ones by combining them with a phrase expressing causation, and hence supplying them with a semantic decomposition. The alternating grammatical properties of the resulting causative expressions are then explained by invoking the corresponding differences in their respective lexical elements. For example, a causativized intransitive, such as *kill*, allows but does not necessarily force a new argument (external subject), whereas the subject of the original intransitive (*die*) is demoted to the status of direct object. Thus, semantic changes in the lexical element induce changes in the morphology, syntax and semantics of the whole phrase.

There is an alternative to this explanation, which somewhat reverses the cause and its effect and has what I believe is the desired effect of solving the conflict between lexical atomism and linguistic explanation. If the lexico-centric approach stipulates that the grammatical properties are a consequence of lexical decomposition, one could as well reverse the reasoning by proposing that it is the grammatical properties - i.e., properties of the whole expressions or 'propositional frames' or its logical syntax - that induce the surface alterations to the appearances of the lexical elements, not only in the case of the structural Case and agreement, for example, but also in the case of what has been traditionally explained by invoking semantic features. According to this hypothesis, causation is not part of the lexical elements, not represented in virtue of predicate CAUSE, but is part of the *logical syntax of the whole proposition.*[3]

Consider a proposition involving two participants, Brutus and Caesar, and a relation, killing. These participants, or their roles in the respective propositions, must be distinguished from each other, as *Brutus killed Caesar* certainly differs in its truth-value from *Caesar killed Brutus.* As explained in section 3.4, this problem has traditionally been adddressed by postulating thematic roles and their syntactic linking, meaning that it was Brutus who

[3]To be accurate, causativity is a consequence of the logical syntax of the whole proposition, provided by the C-I system, thus it is not, strictly speaking, 'internal' to it.

was in the role of being the 'Agent' of the killing, whereas it was Caesar who was the 'Patient,' assuming that it is *Brutus killed Caesar* and not *Caesar killed Brutus* that is true. On the basis of this observation alone, however, there is no reason to suppose that this information is part of the lexical elements *Caesar*, *Brutus* or *killing*, since this explanation is perfectly general and applies to a wide enough range of linguistic expressions to suggest that there is a general principle involved, although the details are, as is well known, moot. The role of that principle is to *distinguish* the roles of the participants in the proposition they are parts of (who is the killer, who is the one being killed), and hence it is a principle concerning essentially *a complex construction and not a single lexical element taken in isolation.*

What is the principle involved? As I already mentioned in section 3.4, I believe Russel (1940) was the first to suggest the correct answer, especially relevant to the analysis of causativity. Russell, being concerned with the above-mentioned asymmetry between *Brutus killed Caesar* and *Caesar killed Brutus*, wrote:

> The two sentences "Brutus killed Caesar" and "Caesar killed Brutus" consist of the same words, arranged, in each case, by the relation of temporal sequence. Nevertheless, one of them is true and the other is false. The use of words for this purpose is, of course, not essential; Latin uses inflexions instead. But if you had been a Roman schoolmaster teaching the difference between nominative and accusative, you would have been compelled, at some point, to bring in non-symmetrical relations, and you would have found it natural to explain them by means of spatial or temporal order. Consider for a moment what happened when Brutus killed Caesar: a dagger moved swiftly from Brutus into Caesar. The abstract scheme is "A moved from B to C," and the fact with which we are concerned is that this is different from "A moved from C to B." There were two events, one A-being-at-B, the other A-being-at-C, which we will name x and y respectively. If A moved from B to C, x preceded y; if A moved from C to B, y preceded x. Thus the ultimate source of the difference between "Brutus killed Caesar" and "Caesar killed Brutus" is the difference between "x precedes y" and "y precedes x," where x and y are events. (Russell, 1940, pp. 35-36).

Note that causativity is only one subcase of the aspectual individuation proposed by Russell, since whenever one causes something to occur, it precedes it as well. Yet Russell's principle of aspectuality was intended not as a *lexical* principle, but as a principle of how propositions with identical lexical elements are distinguished from each other. *Causation is not in the lexical elements, taken in isolation, it is in the interpretation of the aspectual order of the arguments inside a proposition.* Thus, it is part of the logical syntax of the sentence.

Thus, insofar as this aspectual theory provides the most general way of capturing the difference between two propositions with identical lexical elements (see next section), it also provides a way of explaining causativity and thematic properties without postulating them to be parts of the lexical items. They are semantic properties that take effect at the level of phrasal combination, not at the level of isolated lexical elements, hence they originate from principles of UG, not from idiosyncratic information constituting the lexical items.

For example, the fact that *red* does not allow two arguments, but *redden* does, could be explained thus: one would assume not that there are two different lexical elements *red* and *redden* with their respective subcategorization frames and semantic decompositions that explain their relations to each other - *redden* containing a feature CAUSE - but rather that there are two identical lexical elements inside of two differing 'propositional frames' having corresponding morphological reflexes, thus transforming *red* into *redden.* This means that *redden* is not a lexical item with CAUSE added to *red.* It is rather a variation of *red* inside a different propositional frame together with the aspectual theory of the individuation of the added arguments, with its semantic import. More generally, putative semantic features emerge from the sentential context of the lexical elements insofar as they are grammatically relevant, and from conceptual-intentional performance systems (C-I) insofar as they are not.

This explanatory strategy is not alien to contemporary theories of universal grammar, for there are many grammatical phenomena that are determined by such structural conditions, such as structural Case, agreement, binding, government, c-command and the like. Even in the minimalist grammar, if the Strict Lexicalist Hypothesis totally contradicts this type of explanation at least terms of in rhetoric (as it also contradicts the strong minimalist hypothesis), linguistic expressions are constructed by matching and eliminating

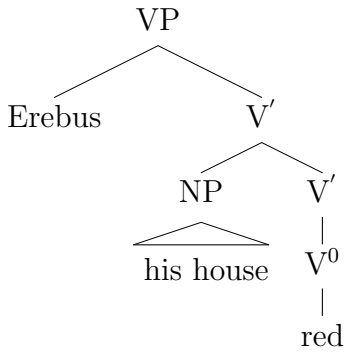
mutual features between linguistic objects. I have now departed from SLH by proposing that the morphosyntactic features of lexical elements might emerge from their feature vectors, beginning with the category and structural Case. Why can the same not be true of semantic features?

Recall that the problem here was that if we drop the assumption of lexical decomposition, then it becomes notoriously difficult to explain the appearance of the 'analytic' inferential relations between lexical elements. For example, it is then hard to see why there are so many productive and systematic causative relations between lexical elements, and why these relations appear to have significant grammatical reflexes. One such inference is expressed by (101)(a-b), in which (a) seems to entail (b).

(101) a. Erebus reddened his house
b. the house is red

Suppose that (b) is taken to express the basic case of something's being red, and the 'deep structure' of sentence (a) to involve the same lexical element red with one extra argument added. This is expressed more simply in structures such as (102).

(102)



Since there are two arguments, they must be distinguished from each other in order to explain the asymmetry between (101)(a) and (103).

(103) his house reddens Erebus

Following Russell, let us assume that, in terms of the relevant semantic properties of (101)(a) qua linguistic, the arguments are individuated by their temporal order of participation in the event of something's being red, first Erebus, then the house. This implies that Erebus participates in the event of the house becoming red before the house does, but *the inference is not based*

on the semantics of the lexical elements: it is based on the whole propositional frame. It does not yet imply that Erebus *caused* the house to become red: that inference is based on the conceptual-intentional system in filling the necessary link between Erebus and the house's being red, so that Erebus preceded the house in the event of the house being red because he must have caused it to become red.

There are other details that are provided by the C-I, such as the fact that Erebus is an intentional Agent, the originator of the event. However, these details are part of the pragmatic performance systems, we expect them to be, at bottom, too obscure to serve as the basis of linguistic generalization. True, such pragmatic properties can be grammaticalized in a language, but that is a different issue altogether: it concerns *what* can be expressed by whatever means, not *how* the C_{HL} operates with whatever it aims to express.

Although the proposal is similar to the one put forward by Hale & Keyser (1993) (henceforth H&K), there are important and illuminating differences. First, insofar as I understand their proposal, in H&K's case, *red* is turned into *redden* via Verb Raising: the element rises and adjoins the empty v-element dominating the inner VP structure. The details are not important insofar as this means that there is a causative decomposition: the v-head has a causative content, expressing an abstract causative participle. Thus, in H&K's case, the element CAUSE is a *linguistic participle* appearing in the phrase-structure tree itself, whereas in my case it is part of the semantic interpretation provided by the C-I. In the latter case, there is no linguistic causative decomposition, since *red* is turned into *redden* by a morphological rule that simply detects the existence of an extra argument. If causative predicates are decomposed at *some* level, it is not a *linguistic* level. Still, we can show why causativity takes part in word-formation.

Secondly, the meaning of (101)(a), or its deep structure, is not expressed by the analytic causative phrase (104).

(104) Erebus caused the house to become red

As is well known, there are both semantic and syntactic difficulties with this view, to which I will return later (Cooper, 1976, Falk, 1991, Fodor, 1970, Shibatani, 1973, Wunderlich, 1997). In the present case, phrase (104) has a different deep structure altogether: it involves a linguistic predicate *cause* with its own sentential complement, whereas a morphological causative (101)(a) does not have either. On the other hand, what I would like to cap-

ture is the fact that (101)(a) entails (104). This entailment follows since sentence (104) indeed approximates the way in which the C-I interprets (101)(a), although they express different 'propositions.' Causativity (temporal precedence) is, as Russell posited, a feature that is *internal to the constitution of a proposition* even if the proposition does not contain the predicate *cause* itself.

Consider the comment by VanValin & LaPolla (1997), in a footnote, on the status of their causative semantic decompositions: "It is important to keep in mind that semantic representations, even informal ones, like 'cause to die,' are not equivalent to the English sentence X caused Y to die. Rather, the claim is that semantic representation of kill involves causation, a change of state, and a result state" (p. 654, note 6). What, then, is the semantic analysis of *X caused Y to die*? Does it involve causation, a change of state and a result state? It does - hence the problem is not solved. To avoid the problem of decompositional analysis one would have to explain why the meanings of words and the meanings of the corresponding phrases are not represented identically at the semantic level. In the present theory, this is because causativity is part of what happens inside a basic clause or a basic proposition with a predicate and its arguments (aspectual individuation with causativity provided by C-I), and not between two clauses.

This feature brings up a familiar philosophical puzzle, discussed by Frege, Russell and Wittgestein, among others. Since this thesis is not targeted as a primary philosophical work, I will settle here for merely explaining, not investigating, the problem, but I still feel that VanValin & LaPolla, for instance, might try to reply along these lines so as to warrant a short discussion on the matter in the present context as well.

Consider, to begin with, Shibatani's recent diagnosis of the monoclausal / biclausal controversy. He writes: "Most of us agree that causation is a relation between two events [...] The question is whether we need a level of abstract syntactic representation between the event structure consisting of two sub-events connected by a causal relation and superficially simplex causative constructions, typically manifested by morphological causatives" (Shibatani, 2001, boldface characters removed). Thus, the question is, if this diagnosis is correct, not whether causatives are semantically/metaphysically decomposable, but whether they are also 'linguistically' decomposable. To be sure, there are some problems here concerning what Shibatani means by "syntactic," i.e. whether he is concerned with the language faculty or Mentalese,

or both; but we may put this question aside for now. Clearly, causative situations are "semantically decomposable," if by that term we mean that they have some kind of parts, say chains of events, since everything we refer to can be seen under closer inspection as containing such 'parts.' Any causative situation, for instance, consists of several interconnected nodes of energy transfer, some kind of metaphysical or physical primitives notwithstanding, and some of these interconnections might be constitutive ingredients of the event.

Thus, as pointed out by Shibatani, the real question whether there exists a linguistic (this thesis), or perhaps conceptual (Fodor), decomposition. Because I now have to admit that, from a semantic or metaphysical point of view, causative propositions might have internal decomposition, VanValin & LaPolla could reply that their logical metalanguage, as its expressions "are not equivalent to the English sentence[s]," represents precisely such logical decompositions. In other words, their "semantic representations of *kill* involve causation, a change of state, and a result state" in the metaphysical sense (§2.3).

Similarly, one could argue that the concept of three represents the number three, which has some kind of metaphysical decomposition since it is, for example, certainly a prime number. Fodor, too, agrees that such decompositions are real - and there seems to be no point in denying their reality of such decompositions.

Finally, since I have argued, and even more importantly explained in some metalanguage, that the causative structure emerges from the aspectual dimension concerning the participants of a proposition, I may also need a metalanguage, preferably a logical metalanguage with explicit model-theoretical semantics, to explicate these assumptions even if they are, by hypothesis, internal to propositions.

The problem is not, to repeat, in assuming linguistically irrelevant metaphysical decompositions, but in the claim that the proposition-internal event dynamics cannot be expressed by phrases, for I am committed to the claim that monoclausal causatives and biclausal causatives are not synonymous. There is a semantic difference between these two types of causatives, which is what makes the present issue crucial linguistically. This is implied in the present theory by the fact that different LLF representations generate these two types of causative expression.[4]

[4]Hence, I must conclude that there is something in the outer limits of our language

Thus, a biclausal logical analysis of lexical / morphological causation, say in terms of VanValin & LaPolla, within one proposition must be somehow inadequate, representing, necessarily, an analysis of causation in an analytic, not lexical / morphological, causative. Perhaps such analyses approximate the internal event dynamics of propositions but, to borrow Wittgenstein's terminology, the rest of it must only "show" itself in the language. Dowty seems to advocate occasionally similar treatment, writing that his decompositional analysis is only a "rough approximation of the correct meaning of the complex word," and suggesting that "the 'real' interpretation being obtainable only from independent definition or contextual information" (I must confess to being unable to grasp the intended sense of the "real interpretation"). Fortunately, we need not succumb to this philosophical morass here, suffice it to note that it is not at all clear what such logical metalanguages are supposed to do in the theories *if they are not representations of the corresponding biclausal sentences.*

I think Fodor and Lepore land in to the same muddle concerning the status of logical metalanguage decompositions in their latest book. Consider the following sentences containing the predicates *want* and *drink*:

(105) a. Erebus wants a beer
b. Erebus wants to have a beer
c. Erebus drinks a beer

The problem is that (a) seems to be nearly synonymous with (b), yet (c), which is structurally similar to (a), is not synonymous with *Erebus drinks to have a beer*. Pursuing a semantically atomistic lexicon leads one to take both (a) and (b) to contain the same predicate *want* at the LLF level. According to Fodor & Lepore, 2002, pp. 112-119, the predicate *want* requires a propositional complement (b), whereas sentence (a) is interpreted as (b) by an interpretation rule associated with the predicate *want* that is able to interpret 'want X' as 'want to have X' (as understood in the material mode). Such a rule must detect the logical form of the predicate's host, then, meaning that although one could still claim that the lexicon is semantically empty, it is otherwise complex, containing information about the host's logical form.

which appears, strictly speaking, to be unanalyzable: the internal constitution of a proposition; a language, when made to work upon itself, will end up consuming all of its resources before succeeding completely because, in order to *say* something, the proposition is the minimal unit, hence presupposed in the saying.

For instance, the *beer* in *want a beer* is not the logical object of *want*, but a logical object of *have*, and the opposite is true of *drink a beer*. Why we not say, then, that *want* contains the semantic feature [+to have]? Fodor and Lepore comment on this as follows:

> Our discussion has not assumed that there is a level of logical form at which, for example, 'want a beer' is represented as WANT TO HAVE A BEER. On our account, all that happens is that wanting to have a beer (material mode) is assigned as the denotation of the expression 'wants a beer'. Our treatment is, however, compatible with positing an explicit level of logical syntax should there prove to be any reason to do so; we have no views on the matter for present purposes. (Fodor & Lepore, 2002, p. 116)

The authors may be claiming here that *want* is not decomposed from *have* plus the relevant logical form because they take this decomposition to be a 'metaphysical,' not a representational, fact. They seem to claim, if I read this passage correctly, that *wants a beer* simply means 'wanting to have a beer,' without suggesting that this meaning is represented by any kind of complex representation. What they cannot deny, it seems, is that what is needed in analyzing the meaning of *wants a beer* is the biclausal sentence *wants to have a beer*, although they may deny, in the characteristic material mode, that this synonymy is of *linguistic origin*. A similar interpretation of the 'metalanguage' may thus be available for VanValin and LaPolla, and the rest of the decompositional theorists.

If we keep it in mind that the designation being 'metaphysical' need not imply such meanings to be mind-independent (as argued in §2), we could claim some plausibility here, for often reading the literature on lexical semantics strikes us as if we were reading trivial facts about the world, only under new terminology such as "semantic traits" or "semantic fields," rather than lawful claims on how our mind and language work.

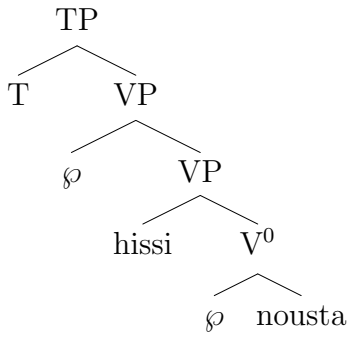
Assuming that morphological and analytical causatives can be dealt with as proposed here, we are left with lexical causatives, which are discussed in section 4.7.

4.4 An atomistic explanation of morphological causatives: the minimalist framework

My aim in this section is to investigate whether the LLF together with a theory of linking suffice to explain the semantic, syntactic and morphological properties of causatives. I will show that the principles laid down in the previous chapter suffices to explain the basic properties of causatives. I will also discuss some of the consequences of adopting these assumptions, some of which are rather problematic. Causation is assumed to be part of the logical syntax rather than being part of the lexical elements.

Let us begin by considering the Finnish predicate *nousta*, 'to rise.' Suppose that rising is instantiated in an elevator, taking T = past. The relevant LLF structure is shown in (106).

(106)

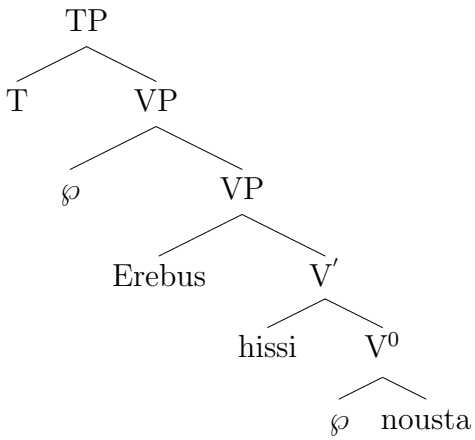


According to the rules given so far, this LLF representation generates the following expression:

(107) hissi nousi
elevator-NOM rise-PAST-SG3
'The elevator rise.'

Suppose I introduce another argument, say *Erebus*. This new participator is predicted to appear as the 'originator' of the event of the rising of the elevator, generating (109):

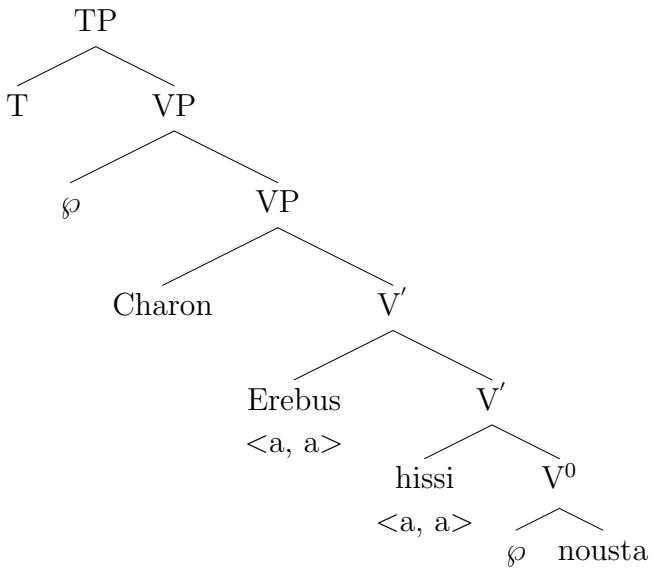
(108)



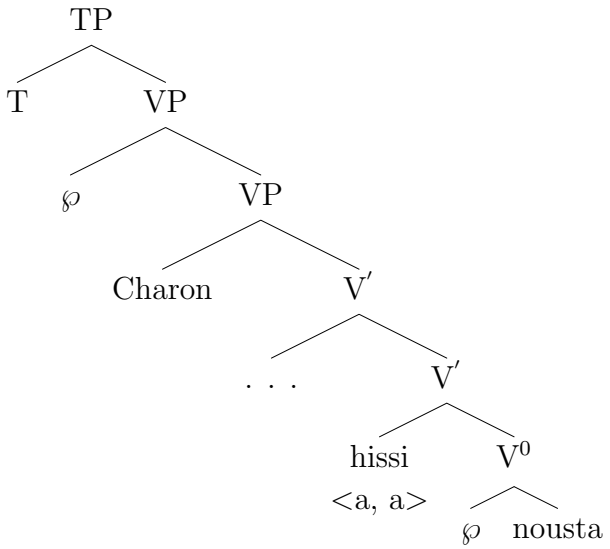
(109) Erebus nosti hissin
Erebus-NOM lift-PAST-SG3 elevator
Erebus lifted the elevator.

Now suppose a third argument is introduced. Given the individuation of aspectual arguments, this new originator must participate in the event before Erebus. I have assumed that this is how aspectual arguments are individuated: *if* there is a particular causative bond of some particular strength (permissive, causative), then it is supplied by C-I. Consider the underlying lexico-logical form of the above situation with three arguments. Since there are three arguments, they cannot all pass [51]. The constituents *hissi* and *Erebus* have identical feature vectors, namely <a, a>, a case prohibited by [51]:

(110)



One of the arguments must thus be deleted at the syntactic interpretation. Since the choice of deletion is free, assume that *Erebus* is deleted. This has been the general pattern on several occasions. The resulting expression is (111).

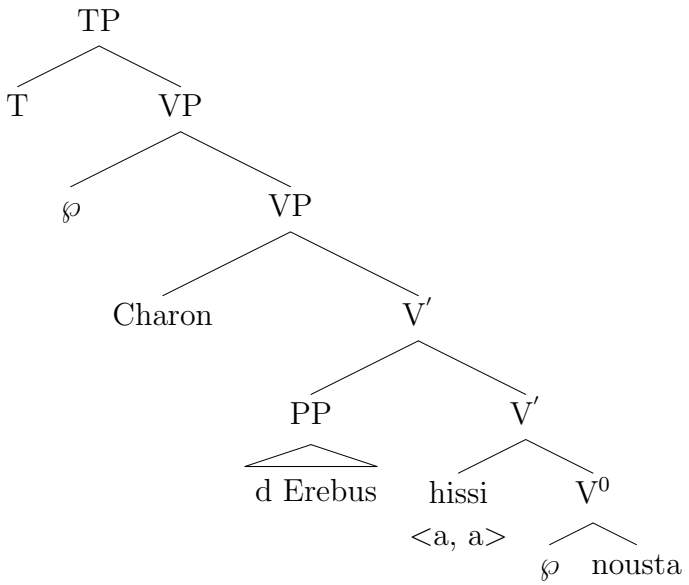


(111) Charon nostattaa hissin
Charon-NOM lift-CAUS-SG3 elevator
'Charon caused somebody to lift the elevator.

The sentence means that 'Charon causes somebody to lift the elevator.'

There is a clear sense of something being 'between' Charon and elevator, as is explicit in the lexico-logical form, the person or thing who actually brings about the rising. This is my explanation of the difference between direct and indirect causation, in that the latter has been argued to contain an 'intermediate agency' between the causer and the causee, now explicitly represented at the LLF level but deleted when further processed according to C_{HL}. This structural property is thus able to trigger the relevant morphological markers, as in many languages, like Finnish, the distinction between direct and indirect causation is signaled in the morphology.

Syntactic rule [51] prevents a syntactic interpretation (111) unless further structure, such as d-operator, is involved. Indeed, a slightly different lexico-logical form may escape the limitations of [51], namely, if *Erebus* is d-marked. If the constituent is equipped with a d-operator, its feature vector becomes different from <a, a> (§3.6). The exact semantic Case alters from language to language: the above proposal predicts only that it may surface as an oblique or indirect argument, having corresponding syntactic and morphological properties. Finnish does not have indirect arguments, so it appears as an oblique argument. The predicted form is shown below.



(112) Charon nostatti hissin Erebusilla
Charon-NOM lift-CAUS-PAST-SG3 elevator Erebus-SEM
'Charon caused Erebus to lift the elevator.'

This phenomenon is not specific to Finnish; rather, according to Comrie, "One feature that does seem to be common to languages with morphological causatives is that it is always the causee that has to change its syntactic relation in order to fit in with the increased valency" (Comrie, 1985, p. 336). With certain reservations, this is a reasonable generalisation (some systems with different properties are investigated in the following). Exactly this happens above: the causee is eliminated, due to [51], but it escapes automatically if it is d-marked. None of these facts need to be stipulated as independent properties of language, let alone causatives. The addition of a new causer to the FP results in an illegitimate structure with the causee and the patient having identical L-vectors, so that one of them (causee) is d-marked or appears as an adjunct. Consider the following causative constructions from Turkish (Marantz, 1984, p. 261):

(113) Ali Hasan-i öl-dür-dü
Ali Hasan-ACC die-CAUS-PAST
'Ali caused Hasan to die.'

This expression is derived in a similar manner as the Finnish examples, raising no further problems. *-dür-* is a causative morpheme in Turkish (together with *-t-*). *Hasan* appears in the accusative Case, as predicted from its feature vector. The causee, if there was one, has disappeared, due to [51]. However, Turkish allows indirect objects to appear in the dative Case, as in the following:

(114) disci mektub-u mü-dür-e imzala-t-ti
letter-ACC director-DAT sign-CAUS-PAST dentist-NOM
'The dentist made the director sign the letter.'

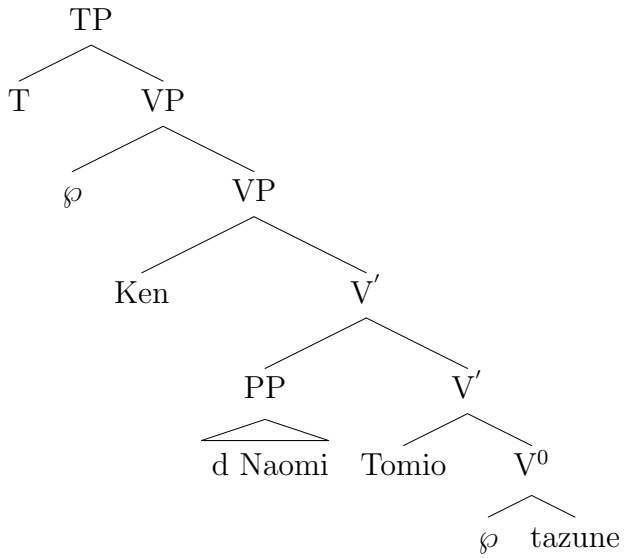
In such cases, the present proposal predicts that the dative Case is the result of d-marking an aspectual argument. Turkish differs from Finnish in this respect, since Finnish does not seem to have a dative Case (Nelson, 1998). There is good support for this predication, as it seems that one of these arguments needs to be a d-argument rather than a direct object of the verb. In Finnish, the causee appears in the adessive Case (SEM, oblique argument). This is what is predicted if d-marking, or moving into the position of a d-argument, is necessary due to [51]. Furthermore, this fact is already clear from the hypothesis given earlier: double objects and datives are d-marked at the lexico-logical level. A similar pattern emerges in Japanese,

which has a causative affix, -*sase*, attached to the verb. If there are only two arguments, no syntactic changes of structure are predicted. This is the case, as can be seen below:

(115) Naomi-wa Ken-wo kuukoo ko-sase-ta
Naomi-NOM Ken-ACC airport come-CAUS
'Naomi had Ken to come the airport.'

(Assuming that Japanese causatives are morphological, not analytical causatives.[5]) Since Japanese has the dative case, marked by -*ni*, a causative with three arguments affects the causee, turning it into a dative. This is a result of d-marking at the lexico-logical form:

(116) Ken-wa Naomi-ni Tomio-wo tazune-sase-ta
Ken-NOM Naomi-DAT Tomio-ACC visit-CAUS-PAST
'Ken made Naomi to visit Tomio.'



The generalisation is that, if there are only two arguments, the direct object may appear either as the *wo*-accusative (<a, a>) or as in the *ni*-dative (<d, a>); if there is a direct object *and* an indirect object, then they both cannot appear as *wo*-accusatives, but rather the indirect object appears

[5]Gunji (1987) argues that the morpheme -*sase* is an independent transitive verb and that the similarity of causative constructions with ditransitive verbs is illusory. I do not deny that this analysis could be true, since, obviously, analytical causatives exists. I simply assume that this is not so, but not much depends on this assumption concerning this particular example.

in the *ni*-dative. These facts are predicted from the principles given so far, and we therefore do not have to stipulate them.

Causatives are important from the perspective of grammar precisely because, in semantic point terms, they represent the basic case of increasing the valency of the predicate. Thus their grammatical properties, by operating in tandem with their causative interpretations, are valuable sources of evidence of a theory of grammar: they represent the core case of what happens when too many arguments are introduced into the linguistic structure. A general argument overflow thus occurs, which motivates principles such as [51] and, in doing so, implies many other grammatical phenomena, such as the Case Filter, as a side effect. One could say that causatives reveal a unifying theme underlying many grammatical phenomena.

This elimination of the causee, typical of transitive causatives, may result if one of the three arguments is d-marked by the predicate, or appears as a d-argument. This prediction is correct, as shown by (117) and (118).

(117) Merja juoksi kotiin
Merja-NOM ran-PAST-SG1 home-SEM
'Merja ran home.'

(118) Pekka juoksutti Merjan kotiin
Pekka-NOM ran-CAUS-PAST-SG1 Merja-ACC home-SEM
'Pekka made Merja run home.'

Kotiin is an oblique argument in the semantic Case. *Merja*, the causee, appears in the surface structure in the accusative Case. This is possible since *kotiin* is a d-argument. These facts are in agreement with the present theory.

Causativity is an extension of the general principles introduced in the preceding chapters, causative interpretation being a special case of the aspectual individuation of arguments. Causatives are, in other words, a natural consequence of the rules so far, and in no sense a "special class" of predicates. Thus, my usage of the term "causatives" differs from when the term is reserved only for more restricted types of construction, say morphological causatives, or when there is an explicit causee added to a basic transitive clause (Dixon, 2000). In the present case, there are no causatives per se, but 'causativity' exists as an implied way of distinguishing arguments inside any clause containing more than one argument, taken as an extension of aspectuality provided by the conceptual-intentional system.

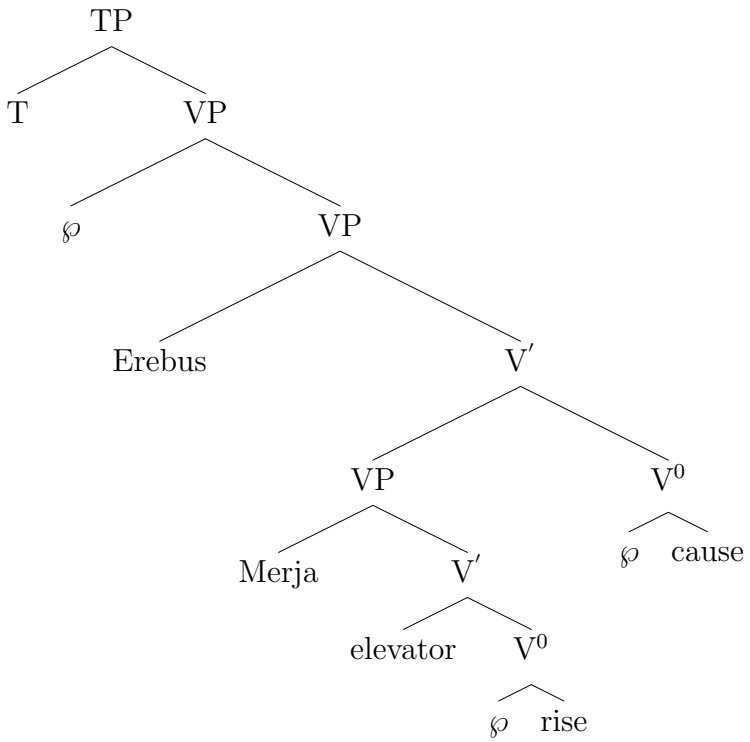
If we look at the properties of causatives from a theoretical perspective, it is clear from these examples that principle [51] does the crucial explanatory work. Verbs with less than three arguments are immune to [51]. I predict that the causatives of intransitive verbs are never subject to radical grammatical alteration, and is that the subject of the root verb becomes the direct object of the causative derivation, the causer becoming the next subject. This is exactly what happens, as noted by Baker: "There is one important domain in which the unity of causative constructions can be observed relatively directly: the causatives of intransitive verbs. Regardless of their differences in the causatives of transitive verbs, all the languages discussed [. . .] treat intransitive verbs similarly; the causee consistently acts like the direct object of the matrix clause" (Baker, 1988, p. 197). From a somewhat different perspective, we might also expect that when [51] begins to take effect, a plurality of mechanisms of avoiding it as a 'last resort' may enter, such as d-marking and d-arguments (cf. Comrie, 1976).

It is also possible to explain why morphological causatives differ in their meaning from analytical ones. Let us compare the lexico-logical representations of the following two expressions, in which the analytic causative appears to be a rough English gloss of the Finnish morphological causative:

(119) Erebus nostatti hissin Merjalla
Erebus-NOM lift-CAUS-PAST elevator-ACC Merja-SEM
'Erebus had Merja lift the elevator.'

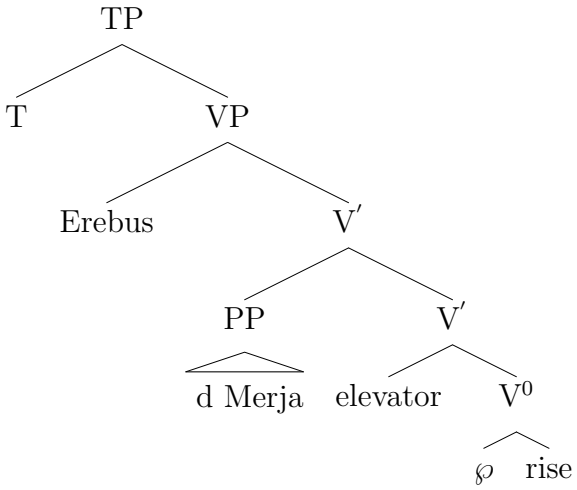
The English gloss is not an exact paraphrase of the Finnish morphological causative, since Finnish has a corresponding bi-clausal analytic causative as well, approximated as *Erebus laittoi Merjan nostamaan hissin*, 'Erebus put Merja to raise the elevator.' This biclausal sentence is represented by the LLF structure (120):

(120)



This generates its properties correctly, and it correponds closely to the English gloss of the Finnish morphological causative. On the other hand, a morphological causative has a different lexico-logical representation, as shown in (121).

(121)



There is no grammatical element CAUSE in the latter lexico-logical form,

although there is in the former.

One interesting question that could well be explored is the relation between the feature [+non-aspectual] and causativization. If a predicate is non-aspectual, it must have at most one a-argument (expletive structures notwithstanding). The prediction is that one could not form a causative of such a verb, since there is no way of creating new a-arguments as new causers. This is true of Japanese (Kuno, 1973, pp. 149-150) and of Finnish. Some Finnish examples are given below:

(122) Pekalta puuttuivat kyvyt
Pekka-SEM lack-PAST abilities
'Pekka lacked ability.'

(123) *Merja puututti Pekalta kyvyt
Merja-NOM lack-CAUS-PAST Pekka-SEM abilities
'*Merja caused Pekka to lack ability.'

Of course, were it possible to change the predicate from the non-aspectual to the aspectual, then causativization would be possible if accompanied by a corresponding change in the aspectual class of the predicate.

So far I have predicted that the causee, or whichever of the objects undergoes a syntactic change due to [51], can be interpreted if d-marked, or if appearing as a d-argument. Which one is actually the case? The question is not empirically empty.

The former belongs to the category of the "inherent Case" of GB theory, and is therefore determined at d-structure with clear thematic content, being assigned by the predicate, being non-optional (for that predicate). D-arguments, on the other hand, do not have these properties. In Finnish, for example, the causee in a causative construction does not have any of the properties of d-marking or an "inherent Case." Instead, it is an oblique, adjunct, and hence must be a d-argument. Baker reaches a similar conclusion, and generalizes it to all causative constructions (Baker, 1988, p. 192): "Case-marking of the causee is idiomatic, varies from language to language, and does not correlate with the typical thematic content of its realizing semantic Case."[6]

We would legitimately ask whether it must be the causee that is eliminated in causative constructions. Although this seems to be the core case,

[6]Except perhaps for languages such as Gilyak, in which the semantic Case realising the deleted causee does not appear anywhere else, see Comrie, 1976.

there are exceptions to this rule (Comrie, 1976). Nothing in what is proposed here prevents the direct object of the source predicate from being eliminated. Gibson (1980) argues that causative rules are separated into two classes, depending upon the choice of the elimination. An example is provided by Marantz, 1984, §7.2.2 (from Chi-Mwi:ni):

(124) mwa:limu Æ-wa-àndik-ish-iz-e wa:na xati
teacher SP-OP-write-CAUS-T/A children letter
'The teacher made the children write a letter'

-ish- is a causative morpheme. The causee, *wa:na* (children), carries the properties of a direct object, as shown by the passive construction (adding a passive morpheme to the causative verb):

(125) wa:na wa-àndik-ish-iz-a: xati na mwa:limu
children SP-write-CAUS-PASS-T/A letter by teacher
'The children were made to write a letter by the teacher.'

The passive construction is not well formed if the indirect object, letter, is raised (for more examples, see Baker, 1988, pp. 162-166). Given the present assumptions, the elimination of an argument depends on [51]. As such, the rule does not determine which of the syntactically non-eliminated arguments is deleted, so there is conceptual room for constructions similar to the Chi-Mwi:ni example. The fact that [51] alone does not predict which of the identical arguments is eliminated weakens its empirical content. On the other hand, this was assumed due to the fact that the matter seems to be subject to language-specific variation. One should then explain the source of the variation. Baker, for instance, claims that it derives from differences in Case theory.

Comrie (1976) argued that there exists a "paradigmatic case" of causative construction that may be used as a 'default case' in analysing various exceptions. This paradigmatic causative consists of four properties (quoting directly):

> (i) There is no syntactic restriction on the formation of causative constructions [. . .] no matter how many arguments a given noncausative verb has, there will also be an equivalent causative verb with one more argument;
>
> (ii) doubling on the syntactic positions subject, direct object, indirect object is forbidden;

(iii) where restrictions on doubling require some constituent to be removed, it is always the embedded subject that is so removed, either by being omitted or by being demoted down the hierarchy [subject, direct object, indirect object, oblique object];

(iv) when the embedded subject is demoted down the hierarchy, it is demoted stepwise, i.e., always to the next-highest position in the hierarchy that has not already been filled.

Property (i) follows in the present proposal since there are no restrictions on the addition of LLF arguments from which we derive most of the properties that are traditionally attributed to syntax (syntactic category, Case, passives). The bottleneck is principle [51], which regulates syntactic interpretation from LLF into the d-structure. Principle (ii) follows the rules of linking as well, and is thereby explained. Comrie also notes that oblique arguments can be doubled, a natural consequence of the rules so far. Properties (iii) and (iv), on the other hand, are not covered by the rules so far: [51] does not determine which of the identical arguments is deleted at the syntactic interpretation, and what position they take. I currently see no motivation for (iii) and (iv), hence, if I am correct, they must remain descriptive statements, lack of independent explanation.

This completes the argument to the point that the present theory is able to account for the core syntactic and semantic properties of causatives without decomposition. The key properties of causatives are automatic consequences of the linking rules. It is also possible to explain why morphological causatives differ in meaning from analytic causatives. Stipulative meaning postulates are not needed, since they arise from the θ-theory with some properties provided of C-I. Causatives are considered from a morphological point of view in section 6.4.

4.5 Complex morphological causatives: a syntactic view

By the term "complex morphological causatives" I mean causative constructions that are not transparently morphological, as Finnish causatives are, but which involve a perplexing set of properties that, at first, appear as a mixture of the morphological and the analytical causatives. I will attempt

in this section to apply the present theory to the analysis of these complex causatives.

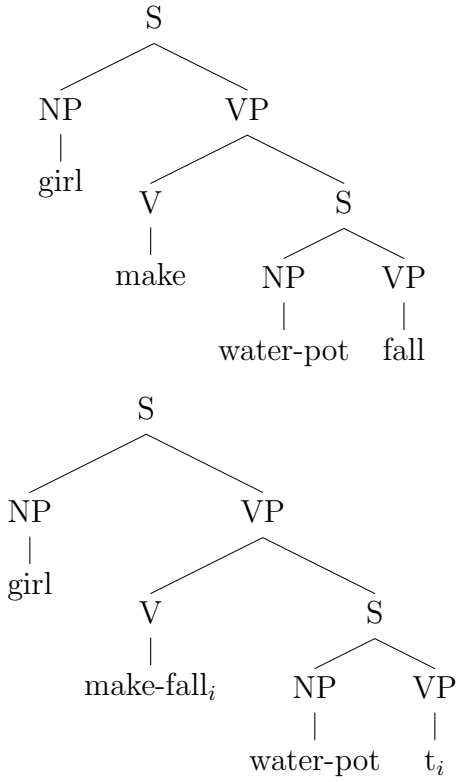
To give a simple example, causative particles may behave as if they were agglutinative, resembling corresponding free (analytic) morphemes yet attaching to the verbal stems. This is illustrated below (From Baker, 1988, p. 148).

(126) mtsikana ana-chit-its-a kuti mtsuko u-gw-e
girl AGR-do-make-ASP that water-pot AGR-fall-ASP
'The girl made the water pot fall.'

(127) mtsikana anau-gw-ets-a mtsuko
girl AGR-fall-made-ASP water-pot
'The girl made the waterpot fall.'

In the biclausal sentence (126) there is a separate causative particle -*its*- that appears to be attached to the verb stem in (127), which looks monoclausal on the surface level. According to Baker, if we wish to maintain UTAH, these sentences must have identical d-structures: the thematic roles of the constituents are identical. This transformation is shown below:

(128)



Baker points out that this analysis allows him to take the causative particle *its* to have only one thematic structure, applicable in all relevant constructions due to an identical d-structure and UTAH. This is surely an advantage, more so when it is recognized that the phenomenon is very general across the world's languages, extending beyond causatives to applicatives and other similar constructions. However, the analysis, if true, is decompositional: it assumes that the monoclausal causative is, on a deeper level, identical to the analytical one.

Supposing we reject the decompositional, transformation approach here, without any reason and only for illustrative purposes, then *gw-ets* verbs would reflect properties of the feature vectors of the non-causative predicates, as explained earlier. Its properties are predicted based on the linking rules. The fact that the causative particle appears in a virtually identical form in both analytical and morphological (monoclausal) structures would be a general property of the morphological component of such languages, but accidental with respect to their narrow syntax. That such a mechanism is general is undeniable: other verbal constituents may be attached to the verb stem via similar processes, as shown below (from Chichewa, cited in Baker, 1988, p. 155):

(129) ndi-ka-pemp-a pamanga
I-SSP-go-beg-ASP maize
'I am going to beg for maize.'

(130) kati madzi banu dza-man-e-ni ine
if water your come-refuse-ASP-PAST me
'If it is your water, come and refuse me'

(131) ku kasungu si-ku-nga-chok-er-e bangu woipa
from Kasungu NEG-PRES-can-come-APPL-ASP people bad
'Bad people cannot come from Kasaungu'

In each case, the source structure is similar to the case with causatives: a verb from the embedded clause seems to be attached to the matrix verb and, assuming transformational analysis, the trace of the embedded verb appears to be governed by the matrix verb, hence satisfying the ECP. Baker also cites evidence that, when these morphological forms exist, they do not violate the ECP: "The observed variation does not cross certain well-defined

boundaries. In particular, polyadic verbs may incorporate a verb out of their sentential objects, and some monadic verbs (always nonagentive) may incorporate out of their sole sentential argument, but these are the only possibilities" (Baker, 1988, p. 161). In other words, according to Baker, verbs never incorporate out of subjects or adjuncts: a hallmark of the ECP (Lasnik & Saito, 1984; for conflicting view, see DiSciullo & Williams, 1987, §3). The key difference between the above proposal and Baker's is thus where to locate the explanation of these general mechanisms, i.e. in the morphological component (in my case) or in the narrow syntax (Baker's proposal).

So far I have offered no reason whatsoever to even question the transformational version. Let us now to turn to the relevant evidence. First, as shown by Baker (1988, §4.3.1), it appears that the argument structure of these verbal complexes behaves as predicated by the present theory, in that "the subject of the base verb [...] surfaces as an oblique in a prepositional phrase" (p. 163). The direct object of the embedded clause appears as the direct object of the causative construction, being raised in the passive and triggering object agreement in the matrix verb. This (typical) property of causatives is predicted by [51], and was explained thoroughly in section 4.3, but only at the expense of assuming that these structures are monoclausal. In Baker's words, "The Case patterns seen in [Verb Incorporation] constructions are almost always Case patterns seen with solitary underived verbs" (Baker, 1988, p. 208). As I will show later, this aspect entails certain complications to the transformational analysis (see also Baker, 1988, §4.3.2 for more discussion), so that one might at least entertain the possibility that these morphological causatives could be analysed on a par with Finnish morphological causatives, being monoclausal on all linguistic levels (as argued e.g., in Achard, 1998 and Mohanan, 1983).

Much the same is true of Romance causatives. Although they have some peculiar properties, they behave as if they were monoclausal at LLF. Consider (132)(all examples come from Kayne, 1975, §3 unless otherwise indicate).

(132) on a fait sortir Jean de sa chambre
'They made Jean come out of his room.'

Jean appears postverbally with respect to both *fait* ('cause') and the infinite verb *sortir*. The problem is, again, that it is not clear whether they form biclausal or monoclausal structures. For instance, although from the morphophonological point of view, *faire* behaves like a single word, being

inflected and defining a domain of stress assignment, from a morphosyntactic perspective, it behaves much like a bound morpheme, being comparable to passive, anticausative and causative bound morphemes even though there is a word boundary present.[7]

Strong evidence in favor treating this construction as monoclausal on every linguistic level comes from the fact that the syntactic realisation of the argument structures of *faire* causatives can be explained by assuming that *faire* + V is a single predicate in its lexico-logical form. If the embedded V had two arguments, its complement would be d-marked in order to comply with [51]. This appears to be exactly the correct prediction, since in such cases the postverbal object appears with the dative preposition *à*, making it PP rather than NP :

(133) a. il fera boire un peu de vin [$_{\text{PP}}$ à son enfant]
b. * il fera boire un peu de vin [$_{\text{NP}}$ son enfant]
c. 'He'll have his child drink a little wine.'

If, on the other hand, the embedded verb is intransitive, or has a PP complement, no change in predicted. This predication, too, is confirmed:

(134) a. il a fait partir [$_{\text{NP}}$ son amie]
b. * il a fait partir [$_{\text{PP}}$ à son amie]
c. 'He had his friend leave.'

As further examples of the same pattern, consider the following:

(135) on fera boire du vin à Jean
'We'll have Jean drink some wine.'

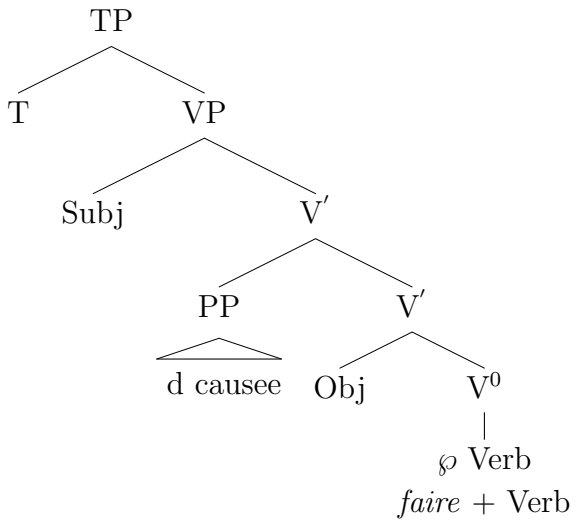
(136) *on fera boire à Jean
'We'll have Jean drink.'

[7] Almost every imaginable and reasonable hypothesis concerning the syntactic analysis of *faire* causatives has been proposed. To name a few, Zubizarreta (1985) assumed that some Romance *faire* constructions actually have two syntactic analyses, one monoclausal, the other biclausal. She also refers to works by Vergnaud and Burzio, unavailable to me, in which it is assumed that *faire* takes a VP complement, suggesting an analysis falling somewhere between monoclausal and biclausal hypotheses. DISciullo & Williams (1987) present a similar analysis, in which *faire* constructions, as well as other constructions, involve two analyses ("coanalysis") depending on how the attachment of an affix is interpreted. According to this view, faire is ambiguously both "the first member of a compound verb and a main verb taking a complement" (p. 91).

(137) on fera boire Jean
'We'll have Jean drink.'

Boire is a predicate that takes either one or two arguments: again, if there are two arguments, the dative *à* is necessary, but not otherwise. If the argument structures of the complex *faire* constructions behave as if they constituted a single clause, then the best hypothesis for their underlying LLF representations would be the following:

(138)



In some cases, *à* may be replaced by the preposition *par* ('by'), as in the following:

(139) elle fera manger cette pomme par Jean
'She'll have that apple eaten by Jean.'

There ought to be a semantic difference between the choice of PP, given that this form is a result of SEM in both cases. This is so: the preposition *par* implies that the causative link between the causer and the causee is more indirect than it is with *à* (cf. Kayne, 1975, p. 239, Zubizarreta, 1985). The same patter is found in many languages. In general, a sentence in French cannot have two direct objects, so that this data is readily explained by assuming that we are looking at a single sentence with the fused verb *faire* + V. The key problem of why the "subject of the embedded infinite sentence" appears post-verbally disappears entirely, since the causative particle and the infinite verb form a single LLF predicate. Similarly, there is no problem in

explaining why the embedded sentence cannot contain an auxiliary verb or the negative particle - the supposed sentence itself does not exists.

The *faire*-construction is productive, much like causatives in Finnish and Turkish, as illustrated below:

(140) son fils a fait sauter le pont
'His son made the bridge blow up.'

(141) elle a fait faire sauter le point à son fils
'She had her son make the bridge blow up.'

Here we see the effect of the *à* rule, a consequence of [51]. This data, again, suggests that *faire* + V correlates with one underlying LLF predicate. This would then also account for the fact that no PRO may appear as the subject of the embedded verb.

More evidence in favour of the monoclausal hypothesis comes from cliticalization. Clitics cannot come between the causative particle and the verb, but they move in front of the whole construction as if it were a single verb. The following examples illustrate this (cf. Kayne, 1975, §3.3 and §4.1):

(142) on fera boire du vin à Jean
'They'll make Jean drink some wine.'

(143) on lui fera boire du vin à Jean
'They'll let Jean drink some wine.'

The same is true of nouns, which must be postponed after the whole complex *faire* + V. Consider the behavior of the reflexive pronoun *se*. *Se* must have a clause-mate antecedent. The following examples illustrate this (from Aissen, 1974b, p. 336):

(144) Jean a dit que Marie$_i$ s'est$_i$ insultée
'Jean said that Marie insulted herself'

(145) Jean a convaincu Pierre$_i$ de se$_i$ mutiler
'Jean convinced Pierre to mutilate himself'

Se is coreferential with *Marie* in (144), with *Pierre* in (145). Importantly, with respect to *faire* causatives, *se* behaves as if the *faire* + V represented a single verb: *se* may take the causer as its antecedent (see also Aissen, 1974b, §3).

The following data, leading towards the same conclusion, comes from Italian (from Zubizarreta, 1985):

(146) Piera$_i$ ha fatto rasare se stesso$_i$ da Maria
'Piero made Maria shave him.'

This is the paradigmatic case in Italian. In French and Spanish, *se* can also take the direct object of the causative complex as its antecedent, and the same is true of many other languages.

The permissive participle *laisser* is peculiar in that it behaves optionally as forming either a complex verb, comparable to *faire*, yet allowing a situation in which the syntactic cohesion is not as tight as in the *faire* construction (Comrie, 1976, pp. 298-299, Kayne, 1975, §3.4). An essentially similar situation arises in Italian. Interestingly, Comrie notes that the effect of demoting the embedded subject to an indirect object, as predicted if the causative verb complex is one predicate in its lexico-logical form, appears if and only if the causative *laisser* and the main verb are fused into a complex verb. This suggests that, in a construction with a tighter relation between the causative particle and the main verb, the complex verb corresponds to a single predicate in its lexico-logical form. This also suggests that the *faire* causative, which behaves similarly with respect to its arguments, corresponds to one predicate at the lexico-logical form. Let us consider some data from French to illustrate this basic point (Kayne, 1975, §3.4). With *laissera*, the two possible constructions are illustrated in (147)-(148).

(147) il laissera son amie réfléchir
'He will let his friend think.'

(148) il laissera réfléchir son amie
'He will let his friend think.'

In (147), the subject of the embedded sentence is pre-verbal, in (148) it is post-verbal. Both embedded infinite verbs are intransitive. Now consider what happens if a transitive verb is substituted for *réfléchir*:

(149) a. il laissera son amie manger les gâteaux
b. il laissera manger les gâteaux [$_{\text{PP}}$ à son amie]

The *á* insertion is obligatory only in the latter case. Thus, the position of the subject correlates with whether the *à* insertion is required or not, exactly

as predicted, were the (b)-type sentences generated by taking *laissera* + V to correspond to a single LLF predicate. Moreover, if the patient were to be replaced by a clitic pronoun, it would appear in the accusative Case in (a)-type sentences, and in the dative in (b)-type sentences.

These sentence types differ in other respects as well. The (b)-type sentences do not allow a negative particle in the putative "embedded sentence," hence one could argue that this is because the embedded sentence does not exist in the first place: the sentence is uniclausal at all linguistic levels. According to Kayne (1975, p. 231), the negative participle, when inserted into the (a)-type sentences, results in a much better sentence.

The hypothesis that the *faire* causative is monoclausal can be tested by trying to combine it with a predicate that is inherently non-aspectual and involves vacuous arguments, such as *seem*. This would lead to an ungrammatical expression, assuming that the *faire* particle represents increased adicity, impossible in the case of *seem* and similar predicates. The following sentences (glossed as 'His pained expression makes Jean/him seem to be suffering') illustrate this outcome in both French and Finnish:

(150) *son expression painée fait sembler souffrir Jean

(151) *hänen tuskainen ilmeensä näytätti Jeanin olevan
his-GEN painful expression seem-CAUS Jean-ACC be
tuskissaan
in-pain
'His painful facial expression caused to Jean seem to be in pain.'

Although all these effects could be explained by postulating transformations and d-structures - as is often the case in empirical inquiry - it would presumably be a still more complex explanation still, taking the *seem* to constitute its own sentence.

One such explanation runs as follows (from Kayne, 1975, §3.7). First, it is assumed that the complement of *seem* is a sentence, while the Subject Raising transformation brings the embedded subject into the position of matrix subject, which is empty. According to this explanation, it must then ensued that the *faire* construction has the capacity to block this transformation and, moreover, that it applies before SR. These are all complexities that arise if the putative embedded sentence is treated independently of the matrix predicate *faire* (I will return to these problems in §4.6). Moreover, as predicted by the above account in terms of syntax-semantics linking, other inherently

non-aspectual predicates, such as *appear* and *know*, behave similarly, as do modal auxiliary verbs such as *must* (*pouvoir*) and *might* (*venir de*); this data falls into place within the monoclausal account.

Although there is some evidence that *faire* + V could be treated as a single LLF predicate, this hypothesis is not without its difficulties. One problem concerns the status of *faire*: why does it appear as a separate morpheme? Why can certain adverbs appear between *faire* and V? Why is it the *faire* that is inflected for tense and person, and not V that is infinite?

Basically, I will adopt the hypothesis proposed by Aissen (1974b, see also Aissen, 1979, §6.2), according to which *faire* is to be categorized as an auxiliary. Taking only some of the relevant evidence at this point, let us consider the perplexing feature of the Romance *faire* causative that certain quantifiers are allowed to appear between the causative *faire* and the infinite verb. One such quantifier is *tout*, 'everything,' as illustrated below:

(152) il fera tout sauter
'He'll make everything blow up.'

Assuming that *faire* is auxiliary, we might expect the same to be true of English grammatical participles. This is indeed so, as shown in (153)(a-c) below.

(153) a. they had gone home
b. they had all gone home
c. they all had gone home

What we see here is that the auxiliaries indeed behave similarly with respect to *faire* causatives. Crucially, some adverbs may occur between *faire* and the infinitive in French, but this is also true of auxiliaries and main verbs, as in *il est certainement parti*, 'He has certainly left,' in which the adverb certainly intervenes between the auxiliary *est* and left. The same is true of the negative particle *pas* (Aissen, 1979, §6.2.4).

There are other considerations that suggest a striking similarity between the causative particle and auxiliary verbs. Aissen (1979) showed that the Spanish causative participle *hacer*, comparable to French *faire*, behaves in the same way as auxiliaries in yes-no questions. He also notes that causative participles and auxiliaries behave in the same way with respect to certain deletion rules. If asked whether one wants to go to the movies, one could answer 'I do,' deleting the infinite clause that would otherwise be repeated.

However, in French this deletion cannot leave behind a construction with the just subject and an auxiliary. Now, the same is true of the causative participle *faire*, as shown by (154c)(a-c), 'Do you always make Marie cry?' (a),

(154) a. faîtes-vous tout jours pleurer Marie?
b. oui, je la fais tout jours pleurer
c. *oui, je la fais

Finally, it is the *faire* causative which inflects for tense and person, exactly as auxiliaries do. Thus, there is evidence that *faire* is comparable to an auxiliary, although now it must be admitted that no theory of auxiliaries has been presented in this study. I will return to this matter in Chapter 6.4, in which I claim that most auxiliaries are Last Report options for providing hosts for inflectional features.

There is a lot of data on Romance infinitives that appears to be non-diagnostic between the biclausal and the uniclausal hypothesis presented here in terms of LLF. An explanation of roughly the same complexity often seems to be possible given either one of the assumptions. To take an example, *faire* infinitives with *par* causees are subject to same restrictions as the passives with *par* subjects (Kayne, 1975, §3.5). According to the present theory, they are both adjuncts and d-arguments, hence the consequence is somewhat expected. However, it is also possible to construct a transformational and bi-clausal explanation of this fact (Kayne, 1975, §3.6). In some respects, the present proposal appears to be too impoverished to allow careful argumentation. This is the case with the complex properties of *tous*. Although the basic properties are as they are expected to be, a more careful argument would require a theory covering the placement of particles such as *tous*. These problems aside, a strong case for treating Romance causatives as monoclausal in terms of LLF can be made, and it was shown above that their basic properties then fall into place. This, if true, strengthens the argument in favor of an atomistic lexicon.

4.6 Biclausal analyses of causatives: some problems

So far I have sought evidence in favor of the monoclausal, atomistic hypothesis. This section furthers discussion on the problems of biclausal proposals, especially those invoked in the context of transformation grammar. I will argue that, independently of the atomistic hypothesis, biclausal analysis is problematic both semantically and syntactically.

Presumably the first analysis of causatives executed in the context of transformational grammar comprised the generative semanticists' treatment in terms of a causative deep structure. According to this theory, the derivational source for *Erebus broke the vase* is [$_S$ *Erebus cause* [$_S$ *the vase broke*]] . The verb of the embedded clause was raised to be incorporated into the matrix verb, forming a constituent 'cause-broke,' which was then lexicalised to form $broke_{tr}$. This analysis was thus biclausal in a very transparent sense: lexical/morphological and analytical causatives were, on a deeper level, identical.

However, as pointed out by Fodor (1970) and others,[8] this analysis is not without problems. There is an asymmetry between the *break/broke*, *kill/cause to die* cases, exemplified in the following sentences:

(155) a. Erebus caused Nyx to die and it surprised me that he did so
b. Erebus caused Nyx to die and it surprised me that she did so
c. * Erebus killed Nyx and it surprised me that she did so

Assuming that killing is represented as 'cause to die' at the d-structure (or LF), asymmetries like these cannot be explained by relying upon the d-structure (LF).

Moreover, lexical items are not synonymous with their phrasal equivalents. By exploiting advantage of this, it is possible to produce odd sentences according to the above-mentioned transformational operators, such as *Erebus caused the glass to melt on Sunday by heating it on Saturday* and **Erebus melted the glass on Sunday by heating it on Saturday.* The same mismatch of content holds for *kill*, hence the following pair:

(156) a. *Erebus killed Nyx on Sunday by stabbing him on Saturday

[8] See Shibatani (1973) and Wunderlich (1997).

b. Erebus caused Nyx to die on Sunday by stabbing him on Saturday.

These sentences exhibit 'iconicity': the more there is syntactic 'fusion' between the verb and its causative particle, the more direct is the causative bond (§4.1 and Haiman, 1985).

A third problem concerns the distribution of instrumental adverbials. Consider (157).

(157) a. Erebus contacted Charon by using a telephone,
b. Charon believed Erebus contacted Charon by using a telephone

In this sentences the subject of the instrumental adverb is the same as the subject of the clause it modifies. Hence *Erebus caused Bill to die by swallowing his tongue*, which is ambiguous in terms of who has swallowed his tongue, yet the same ambiguousity is lacking in the derived alternative, *Erebus killed Bill by swallowing his tongue*. Again, it is impossible to explain this difference by relying upon the properties of the d-structure if they are identical.

Finally, consider the following sentences:

(158) a. Smith caused the students to sit on the floor on purpose
b. Smith sat the students on the floor on purpose.

The former is (again) ambiguous in the way that the latter is not. These examples illustrate the fact, derived from the causative analysis, that syntactic complexes (seem to) contain more syntactic and semantic material than derived lexicalised forms, therefore allowing ambiguities that the latter do not tolerate. In any case, the explanation of such ambiguity could not be formulated by relying upon their respective d-structures, although one could attempt such an explanation in terms of s-structures or some extra-linguistic interpretational mechanisms. Such an attempt seems unattractive, however, because the relevant ambiguities appear to be structural in terms of the location of the adverb, and hence clearly visible in the d-structure (or LF).

Finally, lexical and analytical causatives also differ in the thematic properties they assign to the subject of the sentence. Thus, one could say (159a) but not (159b).

(159) a. a change in molecular structure caused the window to break

b. * a change in molecular structure broke the window

Indeed, I think Wunderlich (1997) was right in insisting that "the paraphrase is about two different situations, whereas the use of a single verb requires one coherent situation." *Cause to die* expresses a relation between a thing and an event of dying, making room for quite exotic mechanisms intervening between, whereas *kill* denotes a single event, in some sense.

These problems are also pertinent in the case of more 'logical' analyses of causative constructions. There are many such analyses available, one of the best being that produced by David Dowty (1979). I will now consider this proposal in the light of the above-mentioned problems. Consider the following statement of the problem:

> It is still somewhat controversial in present linguistic theory whether or not operators such as CAUSE and BECOME are really necessary in analyzing the meaning of basic lexical causatives, but I think it cannot be denied that such operators (or semantically equivalent theoretical constructs) have a place in a language-universal theory of DERIVATIONAL MORPHOLOGY. One natural language after another exhibits morphological processes for producing new causative and inchoative verbs out of adjectives, nouns, and noncausative and noninchoative verbs. (Dowty, 1979, p. 234, capitals from the original.)

As I have shown, it is precisely the productive and systematic "derivational morphology" that poses the most interesting problem for the analysis, beginning with meaning postulates. Dowty proposed a word-formation rule for the derivation of causatives. Suppose α is an intransitive predicate. Then we may form a new transitive predicate $\mathrm{F}(\alpha) = \alpha$ + affix, such as if α translates to α^* in the intensional logic of PTQ, then $\mathrm{F}(\alpha)$ translates into

(160) $\lambda \mathrm{P}\ \lambda x\ [\ y\ [\ \vee\ \mathrm{M}\ [\ \mathrm{M}\ (^{\vee}x)\ \mathrm{CAUSE}\ [\ \mathrm{BECOME}\ [\ \alpha^*(y)\ ]\ ]\ ]\ ]\ ]\ ]$

This represents the meaning of *kill*. BECOME is a tense operator, CAUSE is a sentential connective. In this analysis, the meaning of CAUSE is analysed along the lines proposed by Stalnaker: f CAUSE j is true in some world i if and only if they are both true in i, and furthermore, in that possible world which is most similar to i except that if f, then also j. There exist a number of improvements to this analysis, but in this case we need not

be concerned with them insofar as they are all biclausal, involving sentence operators. Other operators concern the notion of a 'possible world' that is assumed in this system, but they are inessential to the problem at hand.

What is absolutely essential is the fact that the sentence means, approximately, that 'x causes y to become α.' Here the decompositional formula (160) is composed from a logical metalanguage with explicit model-theoretical semantics, rooted in Montague's compositional semantics. The potential advantage of this rule is that (160) appears to explain something: it produces new causative predicates from the old ones, as this seems to be possible in many languages. The logical formulae then provide the approximated meaning of the derived word.[9] Nevertheless, the theory is decompositional in this sense: the meaning of the new word is supposed to be decomposed into the phrase (160).

What is important here is that, interestingly, Dowty nevertheless prefers the 'lexicalist hypothesis,' and claims that (160) generates new lexical entries rather than clauses. Suppose that we adopt the above-mentioned 'lexicalist hypothesis.' Then, consider the translation of the phrase 'x caused y to α,' such as *Erebus caused Charon to die*, into the same logical metalanguage. If we wish to avoid the problems associated with decompositional analysis, the translation of *Erebus caused Charon to die* cannot then be (160) (with *Erebus* and *Charon* substituted by y and x, respectively). However 'x caused y to die' seems to translate to (160) - that is what (160) says. The meaning of new words and phrases have identical semantic representations, which, of course, constitutes a biclausal, decompositional analysis.

Dowty considers several possibilities in understanding the relation between (160) and lexical words such as *kill*. The first solution is to translate *kill* in terms of (160), or to take *kill* as a syntactic abbreviation of (160). The second method would be to lay down strong meaning postulates in terms of biconditionals. Since, as Dowty notes, *kill* and (160) are not synonymous (see above), he considers the possibility that the meaning postulates could be one-way only. He then recognizes the problem I discussed in section 2.4: one-way

[9]Dowty is aware of the problem that it still looks as if the above formula (160) does not exhaust the meaning of the items (i). He assumes that it is a "rough approximation of the correct meaning of the complex word," suggesting that F(α) could still form an independent lexical entry in the grammar, "the 'real' interpretation being obtainable only from independent definition or contextual information." I think the idea is that the above rule is an idealisation, although I do not fully understand the meaning of "independent definition" here. I will put this problem aside for the moment.

meaning postulates presuppose, rather than analyse, the meaning of *kill*. Furthermore, its validity and apparent 'analyticity' is left without explanation. Why is there not some other formula than (160) instead? Dowty suggests that we may continue with laying down more and more one-way meaning postulates rather than approaching the meaning of the "whole word" at once. I can see no explanatory benefit of that process, however, unless we suppose that the meaning of *kill* can be analysed completely, in the end, in terms of other words, that is, if *kill* has, after all, a complete definition. Admittedly, if that were the case, then all the problems of compositionality would disappear at once (§2.1), and I would conclude that my approach could have been misguided from the start. It is a crucial premise in the present work that it is more or less correct idealization to say that morphemes cannot be defined by using other words.

Biclausal analyses also have some syntactic problems. Cooper (1976) first showed that certain Bantu causatives behave as predicted in the proposal put forward here: whenever there are too many arguments at the lexico-logical form, one must appear either as d-marked, or in a more traditional taxonomy, as an oblique argument. If we try to explain this phenomenon in terms of transformations, such as Verb Raising, we would need corresponding mechanisms for manipulating the noun phrases and their "emergent" Case properties. This mechanism is difficult to come by if we accept transformational analysis since, according to Cooper, which Case actually appears depends in an irregular manner upon the resulting complex causative form. In other words, what seem to control the properties of nouns are the subcategorization facts of causative verbs. A similar problem arises in other, more modern, biclausal analysis: how can the matrix verb govern and control the Case properties of the noun(s) in the assumed embedded sentence?

In developing a typical biclausal analysis, Burzio assumes that the dative *a Giovanni* originates at the subject position of the embedded clause. The d-structure would be as follows:

(161) $[_{S}$ Maria $[_{VP}$ ha fatto $[_{S}$ Giovanni $[_{VP}$ riparare la macchina]]]]

Then the lower VP rises between the upper VP and S to form

(162) $[_{S}$ Maria $[_{VP}$ ha fatto $[_{VP}$ riparare la macchinai$][_{S}$ $[_{NP}$ a Giovanni$[$ t_i]]]]

Giovanni is turned into the dative in the process. How, and why does this happen? It looks mysterious under this transformational analysis (cf.

Burzio, 1986, pp. 233-234 and note 2, p. 307). To take another example, Burzio (1986, §4.1.3) first argues that verbs in Italian do not assign Case across clause boundaries (under normal conditions), thus they lack ECM structures. Consider the following pair:

(163) a. *Maria ha fatto Giovanni riparare la macchina
b. Maria ha fatto [$_{VP}$ lavorare] [Giovanni t]
c. 'Maria has made work Giovanni.'

How does *Giovanni* pass the Case filter in (b), assuming the analysis given above? No such problems emerge if we assume the present uniclausal hypothesis. If my analysis is correct, then the adicity operator that produces causative predicates at the lexico-logical form is reflected syntactically by attaching the verb *fare* (or a particle corresponding to it) to the main verb, whereas in many languages the syntactic cohesion is more tight (cf. Song, 1995). Furthermore, [51] readily explains the following violations:

(164) ?? faccio scrivere una lettera a Giovanni a Maria
'I will have Maria write a letter to Giovanni.'

(165) Maria fa ripararla la macchina Giovanni
'Maria had Giovanni fix the car.'

In the present hypothesis, *a Maria* is an ordinary nominal argument so that [*a Giovanni a Maria*] violates [51]. Given no new assumptions, the d-structures of the causatives are thus predicted to be

(166) [$_{XP}$ Maria [$_{X'}$ ha fatto riparare [$_{YP}$ la macchina [$_{NP}$ a Giovanni]]]]

and

(167) [$_{XP}$ Maria [$_{X'}$ ha fatto riparare [$_{YP}$ la macchina]] da Giovanni]] .

Falk (1991) presents similar criticism of the biclausal transformational analysis proposed by Baker (1988), noting that Baker's proposal, like that of Burzio, requires a language-specific mechanism to alter the Case properties of the causee.

Transformational analyses also lead to the syntactic problem that, for some reason, transformations with a simple sentence as their antecedent structural analysis cannot be applied to the embedded causative constructions. Thus, there are no constructions in which, assuming biclausal analysis,

passive transformation has been applied to the embedded sentence before the verb raising takes place. This holds for most such transformations (cf. Aissen, 1974b, §2, 3). According to the present proposal, this is so because there are no biclausal structures: the problem disappears. The data falls into place without any complications: it does not make sense to 'passivize an embedded sentence before causativization.'

Transformationalists are forced to stipulate that Verb Raising transformation is precyclic, meaning that it takes place before other transformations. However, this assumption, although already rather stipulative, runs into some empirical problems. Aissen (1974b) shows that Reflexivization must be allowed to appear before Verb Raising in French, contradicting the claim that Verb Raising is precyclic. In the following examples, the reflexive clitic *se* refers to the underlying subject of the embedded clause so that it must have been applied before the Verb Raising moved it into the position of the object of the matrix verb (Aissen, 1974b, §5.1):

(168) voilà ce qui a fait se$_i$ tuer votre ami$_i$
'That is what made your friend kill himself.'

(169) Paul essaiera de faire se$_i$ laver les mains a mon ami$_i$
'Paul will try to make my friend wash his hands.'

These problems disappear if we jettison the assumption that the Romance causative is a result biclausal d-structure with Verb Raising. Another, similar problem again occurs in French, in *faire* causatives and their transformational analysis. According to biclausal transformational analysis, the causative particle *faire* takes a sentential complement at least at the d-structure. Then the order of the verb and its subject is reversed in the embedded clause via a transformational vehicle (see above), and in some cases a special transformation, say *á* insertion, inserts a dative particle. In more precise terms, the transformation applies to the structure '. . . faire NP V (NP) . . .' and turns it into '. . . faire V NP . . .', with the possible *à* insertion if the second NP is present, omitted for now. Now consider the fact that the transformation is recursive: it may apply to its own output, as illustrated below:

(170) elle a fait faire sauter le pont à son fils
'She had her son make the bridge blow up.'

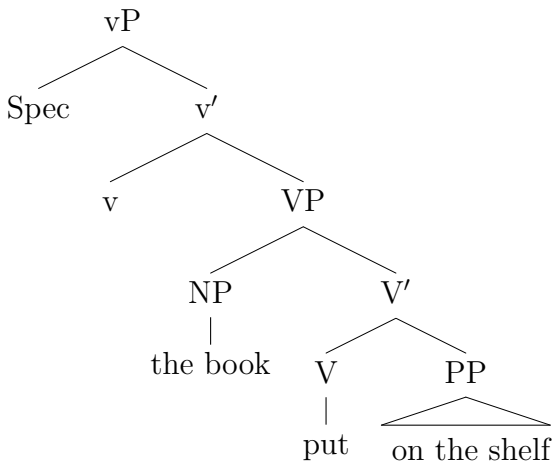
In deriving this sentence, the transformation that inserts a second *faire* must treat the complex *faire sauter* as one verb, otherwise *à son fils* would

not end up in the correct post-clausal position. Hence Kayne assumes that the input to the transformation is rather '. . . faire NP V* (NP). . .', where V* stands for arbitrary long string of verbs (Kayne, 1975, §3.8). Thus, transformation is applied cyclically so that the resulting verbs are 'merged together' as if they would represent one predicate for the purposes of further causativization and structural operation. We could avoid this complication - transformation that merges separate predicates from distinct clauses cyclically into one - by taking this evidence at its face value, taking the causative complex to be one predicate V*.

In sum, the properties of the argument structures of these causatives constructions can be explained in the light of the present hypothesis without invoking complex notions of deep structure and related transformational analysis. As noted by Falk (1991), these transformational devices might be unnecessary complications, in terms of both their syntactic and their semantic properties.

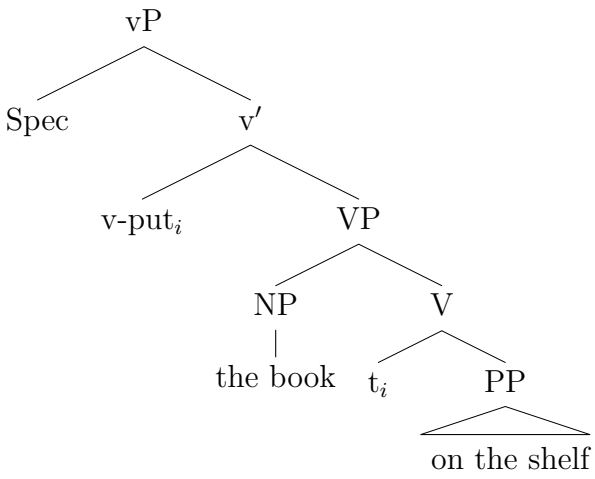
Could this path be pursued with a view to eliminating the causative 'light verb -projection' vP or related heads that encode transitivity? From the beginning I have presented the minimalist theory without vP projections (§3.3), although many linguists today share the assumption that syntactic structures ought to be binary (Chomsky, 1995, §4, Kayne, 1984, Larson, 1988, Radford, 1997, §9). This is the standard minimalist assumption. Given the binary branching condition for syntactic structures, assumed here as well, verbs with three arguments result in a hierarchical rather than a flat, representation. Recent analyses suggest that causatives are made up of VP shells of the following kind, as shown in (171).

(171)



The upper v is a light verb position having a 'causative meaning.' PUT is a predicate that does not surface in English. Rather, as a result of X^0 movement, *put* moves up to adjoin or incorporate the light verb:

(172)



This generates *Charon put the book on the shelf.* According to Hale & Keyser (1993), sentence (173) below, with a denominal predicate, is also formed via successive X^0 movements:

(173) Charon shelved the book

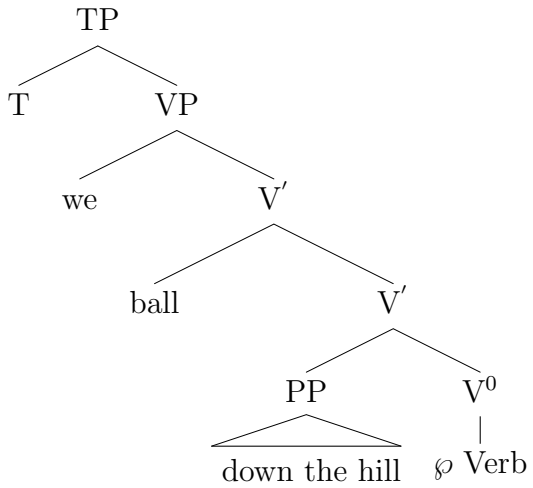
(174) $[_{vP}\ [_{v}$ v-shelve$_i$ $[_{VP}\ [_{NP}$ the book$]\ [_{V'}\ t_i\ [_{PP}\ t_i\ ]\ ]\ ]\ ]\ ]$

My proposal in terms of the lexico-logical form (LLF) in fact comes quite close to analysis in terms of the vP shells. In both cases, the relevant structure is binary. Although there is no structure in my proposal that is comparable to the d-structure vP shell, the lexico-logical form plus the d-structure seems to contain relevant information to link it with the correct surface expression. It is thus a substantial question whether the above abstraction is correct in terms of the vP shell, or whether less is, in this case as well, sufficient.

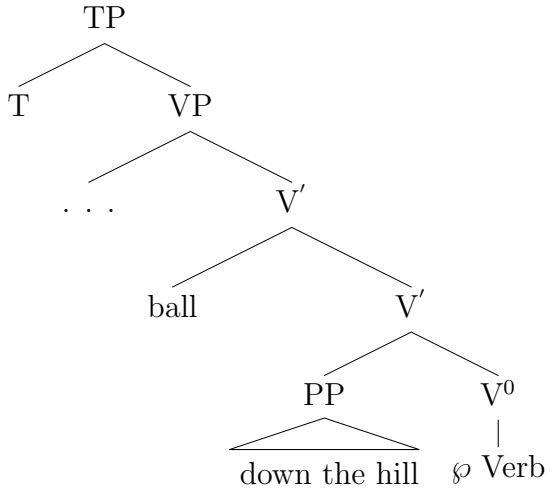
One motivation for the postulation of a more intricate vP structure representation is that it provides a mechanism for combining Baker's UTAH straightforwardly with 'ergative' predicates. We are virtually forced to assume the vP shell analysis by presupposing a certain version of UTAH, binary

branching, and the traditional theory of argument structures. What is problematic here is, I think, Baker's formulation of UTAH. The individuation of thematic roles by means of Agent and Patient is problematic. This challenges the claim that an empirically principled UTAH can be formulated along these lines, associating unclear notions such as 'Theme' with certain structural positions. As I have argued previously, a more general approach would be to subsume thematic roles with their aspectual roles. I have shown that it is possible to maintain UTAH by using aspectual arguments at the lexico-logical form without syntactic features such as N and V, thereby avoiding the postulation of complex vP shells. Note that the constituent *ball* does occupy the same position in the lexico-logical form and hence it has identical thematic properties in both constructions:

(175)



(176)



Thus, pairs such as (177a-b) do not pose special difficulties.

(177) a. we rolled the ball down the hill
b. the ball rolled down the hill

In sum, UTAH with binary branching does not necessitate anything like vP shells, it does so only given the meaintenance of all the conventional, but I think dubious, assumptions concerning the d-structure.

Further, the analysis in terms of vP shells leads to difficulties with semantics, although I think this point is not particularly strong. The trouble is that the d-structure representation of *we rolled the ball down the hill* is not synonymous with *we made the ball roll down the hill*, a reading obtained directly from the vP shell analysis. This is clear from the following data:

(178) a. we made the ball roll down the hill by letting Peter roll it
b. * we rolled the ball down the hill by letting Peter roll it

On the other hand, the proposal given here avoids these problems, since it is not assumed that s-structure expressions with one predicate correspond to analytical causatives with two predicates on any level of linguistic representation.

This problem could nevertheless be avoided by distinguishing the semantics of vP clauses from the semantics of analytical causatives since, in the standard neo-Larsonian analysis, an analytical causative is not mapped onto the same LF-representation than an ergative transitive predicate. I will return to this matter presently.

One empirical problem concerning vP shells is that, in certain languages, verbs that are assumed to occupy the light-verb position can be causativized productively. Following the vP shell analysis, we should then postulate another light-verb position, perhaps even two or more. I have assumed that one could do this at the lexico-logical level in terms of unrestricted adicity (there are no semantic limitations on the number of arguments a predicate can have). Another possibility would be to locate the productive causativity on the lexical level, introducing new lexical elements for each such new causative form. This strategy would obviously miss something important, locating the same phenomenon at two levels, namely, the lexical and the syntactic (v-heads).

Another problem is the fact that Finnish causative forms may also appear systematically in nominal forms (§4.1), suggesting that we should construct

recursive xP shells. This leads up to the present proposal and its lexicological forms which, if rephrased in X′-theoretical terms, are just such $x\mathrm{P}^n$ shells in their simplest possible form: argument shells that can be expanded aspectually and which appear either in the V or N -form, depending on their position in the LLF. According to the the standard minimalist theory, we would be obliged to expand the transformational analysis to NPs. This is not impossible (e.g., Longobardi, 1994), but requires some rethinking - which to some extent had led me to consider the present alternative.

One argument in favor of vP shells concerns the process of Incorporation, which, together, explain certain morphosyntactic features of causatives and other ergative predicates. The embedded verb is assumed to raise and incorporate the v head with its associated morphological consequences. If there are no vP shells, there are no landing sites, or empty heads, to which V incorporation could apply. This issue is discussed more fully in section 6.4, in which an alternative analysis is proposed. Note that I have remained sceptical on whether the relevant word-order facts (e.g., Object Shift, the placement of adverbs, negative particles and auxiliaries, all linked to each other in a complex manner) should always be taken to count as evidence of deep, structural assumptions in narrow syntax in the first place (that is, evidence of structural LLF positions), favoring a model that puts some explanatory burden onto Morphology, phonology and hence linearization (Chomsky, 2001, p. 30f, Moro, 2000, Zwart, 2001). Thus, a consequence of my abandoning of the vP shell analysis is that a variety of facts concerning word order are left without explanation. Whether this is a serious problem, or rather calls for more work, depends on whether an explanation in terms of other components of grammar can be found.

I have argued for a theory in which morphological and lexical causatives are represented as monoclausal at the LLF level, differing from analytical causatives in this respect, since the latter are represented by biclausal deep structures (LLF). It is not entirely clear, however, whether vP analysis is "linguistically biclausal," and hence whether it suffers from the problems of biclausal analysis. In terms of metalanguage used to analyze the meanings of vP shells, the vP shell is biclausal, given that it comprises two events, one causing the another, plus a separate predicate (the v-head), which has a causative meaning (e.g., Hale & Keyser, 1993). However, the vP analysis of an ergative predicate is not necessarily identical to the LF-structure representing an analytical causative, which seems to involve more structure: a

finite matrix clause with an embedded infinitive headed by a deflective T:

(179) Erebus made [T_{def} the ship sink]

Thus, the difference between the present proposal and vP theory is very subtle: one could argue that vP theory already offers at least part of the solution to the original problem, namely the formulation of a theory of causatives that does not take analytical causatives and morphological causatives onto map into identical 'semantic representations.' If this is the case, then whether it is the present theory or the vP theory that is more true is a question of overall elegance with respect to linguistic data, and is not to be argued on the basis of independent considerations concerning causative decompositions. From a slightly different perspective, one could adopt vP analysis into the present theory as a small complication in the structure of a full projection at LLF, changing the rest of the rules accordingly. For reasons already given, and for other reasons, I would not pursue this strategy, but I do not rule it out either.

Thus, it turns out that the present proposal resembles recent proposals in terms of vP shells: both assume UTAH and binary branching. The difference is that the linking rules of the former do not generate vP shells, and the correct surface forms directly. I have maintained the general idea of vP shells, even if I have tried to express the same properties in somewhat more general terms. The process of incorporation is addressed in Chapter 6.4.

One strong argument in favour of biclausal analysis relies on the properties of reflexives. The basic facts are as follows. In many languages, a reflexive needs an antecedent that is its clausemate and appears in the 'subject position.' Such reflexives may take the direct object as their antecedent in some causative constructions, suggesting that at some linguistic level, i.e., that at which reflexives are bound to their antecedents, this direct object appears at the 'subject position.' It is the biclausal d-structure that makes this generalisation possible.

Let us begin by considering the data first. Consider the data presented in Marantz (1984, citing a study of M. I. Abasheikh). Chi-Mwi:ni contains the reflexive *ru:hu-*, 'self,' the distribution of which is constrained in the above-mentioned sense: it may appear attached only to the direct object of the verb, taking the external subject as its antecedent and requiring the antecedent to be a clausemate of the reflexive (for similar properties of Turkish, see Aissen, 1974a, unavailable to me but cited in Falk, 1991, pp. 65-66, in which similar

evidence is cited from Tamil). The relevant constraints operate in English analytic causatives as well, as illustrated below (Marantz, 1984, p. 267):

(180) a. Elmer made himself lock the porcupine cage
b. Elmer made Hortense help herself to the pâté
c. * Elmer made Hortense help himself to the pâté

Chi-Mwi:ni causatives behave similarly although they are morphological (at the s-structure), suggesting that they could be explained in analogy to (180)(a-c) at the d-structure level. A corresponding violation in Chi-Mwi:ni is shown in (181).

(181) *mi ni-m-big-ish-iz-e Ali ru:hu-y-á
I SÅ-OP-hit-CAUS-T/A Ali myself
'*I made Ali hit myself'

I made Ali hit himself is well-formed, although the Chi-Mwi:ni causative is not biclausal at the s-structure. Thus, if the verb is a morphological causative, then the direct object may be the antecedent of *ru:hu*, suggesting that the direct object of this verb is an external subject at some linguistic level, in this case the d-structure. This is easily explained by biclausal analysis, suggesting that *Ali himself* is a clause. Baker draws similar conclusions based on similar data. Similar evidence in favour of biclausal analysis also comes from Chamorro (Gibson, 1980, cited in Baker, 1988, p. 211). In Japanese NPs in the object Case of underived verbs cannot be antecedents of reflexives. Yet specifically in causative constructions, such NPs can be the antecedent of reflexives, as shown in (182).

(182) Erebus-ga Charon-ni zibun-no uti-de hon-o
Erebus-NOM Charon-DAT self-SEM house-in book-ACC
yom-(s)ase-ta
read-CAUS-PAST
'Erebus made Charon read the book in her own house.'

Similar properties are found in Georgian, at least (Anderson, 1992, pp. 269-279, Harris, 1982). Zubizarreta (1992) argues that these causatives have mixed properties: they also behave much like morphological causatives (due to their effects upon the syntactic realization of the causee), but due to the above property, they also resemble biclausal causatives (at the d-structure). In effect, the main verb and the causative verb have independent argument

structures that combine to yield the desired effects. Kuno (1973, §25) argues for the transformational biclausal analysis of causatives based on the properties of *zibun*.

However, if we search for a possible monoclausal explanation within the LLF theory, we should be aware that causatives, as well as other clauses, sometimes involve a semantic parameter describing the amount of 'control' the causee exercises in the causative situation. For example, in Japanese, the accusative Case expresses a lower level of control ("non-subject properties"), whereas the dative Case expresses a higher level ("subject properties")(see Shibatani, 1990, cited in Song, 1995, pp. 5-6). Accusative objects with their lower level of control over the situation cannot serve as the antecedent of the reflexive in causative constructions:

(183) *Erebus-ga Charon-o zibun-no uti-de korosi-ta
Erebus-NOM Charon-ACC self-GEN house-in kill-PAST
'Erebus killed Charon in her own house.'

Moreover, as pointed out by Falk (1991), under certain semantic conditions, *zibun* also appears to be bound to a non-subject in a non-causative (from Kitagawa, 1980, see also Huang, 2000, §3.3.3). If this is so, then the distribution of *zibun* does not point to a biclausal analysis of causatives, but *calls for an independent semantic parameter of 'subject control.'* Huang sums it up: "Antecedents for long-distance reflexives in Chinese, Japanese, and Korean can also be some non-subject argument, provided that this argument represents the 'source' of the proposition or the 'experience' of the mental states that is being described" (Huang, 2000, p. 192). According to Langacker's theory (1991), datives tend to represent the active participants in the event, whereas accusative objects are non-active.

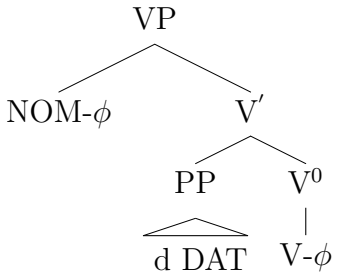
An interesting piece of data comes from Bemba (a Central dialect of a Bantu language, Givón, 1976). The underlying subject, the causee, of causative form of a transitive verb appears in the dative Case if it is a human agent, otherwise it is in the accusative with the previous direct object taking the instrumental or locative Case. Thus we again see the same pattern: agentives, or control arguments, appear in the dative Case.

Furthermore, as noted by Shibatani (1985), several languages have a pattern in which one of the arguments of the non-causative predicate takes the dative Case, yet retains some properties of the subject, such as (i) appearing in the position of the subject, (ii) entering into agreement with the verb

and (iii) serving as the antecedent of a reflexive. This particularly applies to noncanonical verbs expressing human experiences and feelings. We would, in fact, expect this construction when the predicate is non-aspectual, as it clearly is in Japanese (Ura, 2000, §4.1). Some scholars have thus argued, contrary to morphological evidence, that constituents in the dative Case are the real subjects of such sentences. Such constructions have also been referred to as "Dative Subject Constructions." My proposal does not rule out such constructions: the subject could well be d-marked, therefore appearing in the dative.

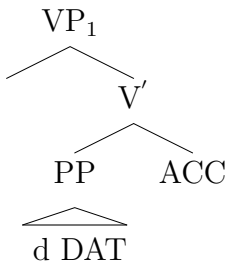
Consider data from Tamil. Tamil is a nominative-accusative language, which occasionally allows dative subjects. In these constructions, the other NP argument appears in the nominative Case. Interestingly, the dative argument has properties (i) and (iii), whereas the nominative enters into agreement with the finite verb (ii)(see Ura, 2000, §1.2; for similar data, see Holmberg & Hróarsdóttir, 2003). This cluster of properties is expected in the present theory. If the nominative patient takes the nominative Case and not the null accusative (correlated with the presence of a vacuous argument), then such a nominative NP must appear as the topmost argument at the LLF level, since if it were below the dative argument it would appear in the accusative Case. The same is true of Japanese and Korean non-aspectual verbs with dative subjects. Thus, if the nominative is at the top of the structure, it is predicted to induce subject agreement in the verb:

(184)

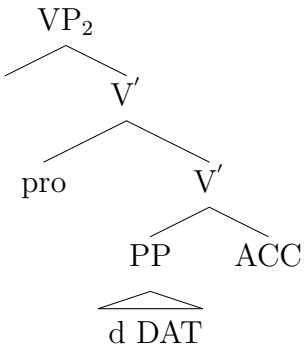


In some, but not all, Tamil Dative Subject Constructions the Patient takes the accusative Case rather than the nominative Case. On the basis of this fact alone, the underlying LLF representation is predicted to be (185) or (186):

(185)



(186)



This is so because the accusative Case is associated with feature vector <a, a>, meaning that in both constructions the accusative object cannot be the uppermost constituent of the respective FP. In this construction, then, ACC does not agree with the predicate, as predicted, but neither does the d-marked dative subject DAT. Rather, a default third-person singular neuter agreement always appears. On the other hand, as predicted, if the Patient takes the nominative Case in Tamil Dative Subject Construction, then it also induces agreement. Exactly the same is true of Icelandic (Falk, 1991; Holmberg & Hróarsdóttir, 2003) and, for all I know, of Old English as well. These facts are predicted on the assumption that it is the topmost arguments at the LLF level that induce agreement in these languages: hence the generalization that, if there is no covert or overt expletive, it is the argument appearing in the nominative Case that agrees with the verb. I will return to these assumptions at the end of this section, but for now, I will take them for granted. Note, however, that in terms of binding and control, dative subjects are subjects. Hence, it must be concluded that binding and control are not determined entirely by the LLF structure: rather, they are determined, at least in part, by semantics, which only correlates with structure in standard cases. This further supports my contention that the binding of reflexives could not be used as a decisive argument against monoclausal explanations.

If my explanation in terms of (185-186) is on the right track, then I would expect to find evidence showing that the Patient is located in a structurally higher position in the DAT-NOM construction (185) than in the DAT-ACC (186) construction, given that both of are possible. Ura (2000, §4.2.4, attributing this observation to Vaijayanthi Sarma) presents evidence that this is so. Whereas sentential negation can take precedence over the Patient in the DAT-ACC (185) construction, it cannot do so in the DAT-NOM construction (186), thus indicating that the Patient does appear in some lower position in the former. Further evidence supporting this theory comes from passivization, whereas it is a key feature of the present theory that dative arguments do not rise, although after the insertion of v this may, but need not be, the sole argument of the verb. The following data comes from Icelandic (Ura, 2000). In (187) and (188), an intransitive dative Patient raises but maintains its Case properties, as expected; in (189) and (190) the dative argument in the ditransitive sentence behaves similarly, but the accusative rises and takes the nominative Case:

(187) Paul bauð ykkur
Paul invited you-DAT
'Paul invited you.'

(188) Ykkur var boðið
You-DAT were invited
'You were invited.'

(189) Olafur sagði mer Pessa sögu
Olag-NOM told me-DAT this story-ACC
'Olag told me this story.'

(190) mér var sögð Pessi saga
Me-DAT was told this story-NOM
'I was told this story.'[10]

The verb agrees with the nominative. These are exactly the expected properties. Both Japanese and Korean allow a subject-related reflexive to be bound by the dative argument, and they allow the dative argument itself the power to control (Ura, 2000, §4.3). Falk (1991) on the basis of similar observations, suggests that dative arguments in Japanese causatives are not indirect

[10]"P" replaces 'the thorn' in these examples; see the original source.

arguments, but are "in some sense, a secondary subject[s]."[11] Despite the properties of the *zibun*-reflexive, he also cites the fact that Japanese floating quantifiers, allowed in the case of subjects and direct objects but prohibited in the case of indirect objects, are nevertheless allowed in connection with a dative causee. Moreover, there seems to be evidence that phrases that are normally controlled only by subjects may controlled by the causative dative (Falk, 1991, p. 64, citing Dubinsky, 1985). Finally, Falk also notes that a pronoun can be bound by the causer in a Japanese causative construction, a potential violation of Binding Condition B of in GB theory, which requires that the pronoun cannot be bound wihtin a minimal governing category. However, this problem is avoided if we assume that the dative object is an 'accessible subject' in the sense of Binding Condition B: it moves the causer out of the minimal governing category for the pronoun.

My hypothesis is, then, that the 'subject properties' of causees in causative constructions - and, as I have shown, in other constructions as well - are due to special subject d-marking. It need not be argued that a suitable operator exists; one candidate is illustrated in (191):

(191) Erebus was killed $[_{\mathrm{d}}$ by Charon]

The *by*-preposition argument clearly determines the subject in some sense. Assuming that the same operator is used as an aspectual argument, turning the constituent into a dative rather than an adjunct would create such an argument with apparent subject properties. A clear example is provided by Spanish:

(192) Juan hizo arreglar el carry a Maria
'Juan had Maria fix the car.'

Here Maria is d-marked as the subject, appearing with the preposition *a*. These subject properties, and their relation to d-marking, become strikingly obvious when the *a*-dative is replaced by the corresponding discourse argument in a construction with a reflexive clitic:

[11] Falk's proposal on the notion of "secondary subject" is quite far-reaching: he argues that the structure of Japanese causative constructions is [[NP causer] [I′ I VP][NP causee]]. This structure has, literally, two subjects. Wali (1981) cites further evidence of subject properties of dative causees in causatives. According to him, it is universally valid that the dative causee cannot be coreferential with the causer, although the direct object causee can be. Thus, the dative object can not be replaced by a reflexive bound by the causer. This suggests that the dative object holds an exceptional structural position, if compared to other objects.

(193) Juan hizo [afeitar-sei a los muchachosi]
'Juan had the boy shave themselves'

(194) Juani hize [afeitar-sei por el barbero]
'Juan had the barber shave himself (Juan)'

In the former, the d-marked constituent *a los . . .* serves as the subject, entering as the antecedent of the reflexive. This interpretation is impossible in the latter, thus the reflexive refers to the causer.

Many languages allow d-marking to code properties of 'control' directly in the subject position. For example, in Manipuri (a Tibeto-Burman language from north-east India with semantic case marking) and Folopa (from a Teberan family of Papua New Guinea), there exists a separate semantic case marking (d-marking) for subjects that gain "control" of the event (Dixon, 1994, §2.2). That this is an instance of d-marking is reasonable since for many verbs in Manipuri and Folopa, the agent may appear in this form, but also in another form, corresponding to 'lack of control.' Thus, it is not in doubt that there exists a d-operator that can raise the 'controlling ability' of an argument in an event (Lyons, 1968, pp. 350-65). My suggestion is that this operation might underlie the curious binding properties of causatives, since there, too, direct objects that can be bound appear to be d-marked. In fact, given the monoclausal analysis, there is hardly any other possibility.

Further evidence in favor of d-marking with 'subject properties' is to be found in Romance causatives. Consider again the behavior of the reflexive clitic *se*. In French and Spanish *faire*-causative constructions, this clitic can appear as attached to the embedded verb, taking the object or the dative NP as its antecedent:

(195) On a fait se$_i$ raser Pierre$_i$
'We made Pierre shave himself.'

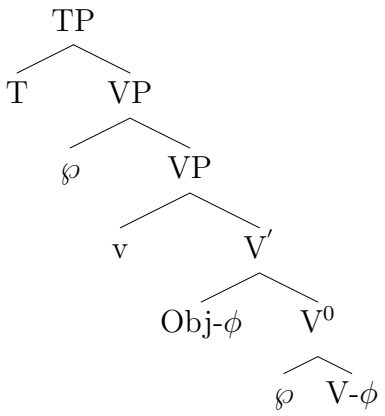
This is not possible in Italian, however. Thus in French and Spanish, the patient NP of the *faire* causative must possess some crucial subject properties, since the antecedent of the *se* clitic has been traditionally captured as the d-structure subject. It turns out that this property also correlates with the impossibility of passivization (Zubizarreta, 1985, p. 268). Given the above assumptions, this is crucial evidence in favor of d-marking: d-marked arguments do not undergo grammatical function shifting due to the fact that

the insertion of a vacuous argument does not change their feature vectors (<d, a>).

Before ending this section, I will return briefly to the syntax of Icelandic and Tamil dative-subject constructions. I have assumed that dative subjects are not "subjects" from the syntactic point of view, though they may be subjects in more pragmatic/semantic terms. Some evidence in favor of this assumption was presented: the nominative patient behaves in many respects as if it were the syntactic subject, inducing agreement with the finite verb. When it appears in the accusative form, such agreement is not observed, and a default agreement appears. The price of these assumptions is that many properties associated with subjecthood, e.g., reflexivization and subject control, do not hold solely for the topmost argument at the LLF level and cannot be used as a diagnostic tool for that position: the dative might carry these properties as well. Thus, the fact that the dative passed many such tests of subjecthood (e.g., Boeckx, 2000, Zaenen, Maling & Thráinsson, 1985) does not indicate, in my theory, that it appears in the topmost LLF position and hence it indirectly implies that it ought not to appear at [Spec,TP] either. This could be considered a serious difficulty in terms of my proposal.

However, the generalization that it is the topmost argument that induces agreement with the finite verb, although quite correct from a heuristic point of view is, obviously not strictly correct according to my theory. If personal passives are created by inserting a vacuous argument in the subject position at the LLF level, then the finite verb apparently agrees with the *patient*, as shown in (196):

(196)



Now note that the weak vacuous argument, observed in personal passives, is invisible to the constituent below it, and hence, it does not appear in their

feature vectors either. I have assumed that it is precisely the ϕ-features that appear in the feature vectors, not the constituents themselves, for assuming that the constituents themselves should be allowed to be part of the feature vectors would heavily complicate the morphological component.

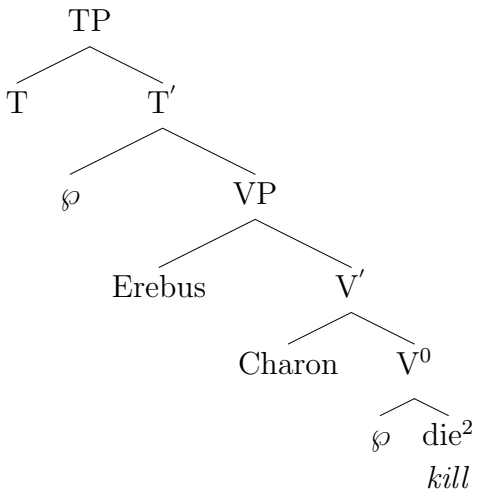
The null hypothesis seems to be that the predicate always agrees with the subject, as this notion is defined from the perspective of the predicate's own feature vector. This is entirely reasonable, since agreement has to do with feature vectors, not LLF positions. The weak vacuous argument is not a subject from the morphological point of view, but the patient is. This fact is relevant to the ongoing analysis of dative subjects in that in these constructions it is the nominative patient that induces agreement with the finite verb. However, given the above remarks, one cannot infer from agreement alone that the argument entering into the subject agreement appears at the topmost LLF position. This is because the subject position could be filled with vacuous argument and we would still be observing normal subject agreement, given that the patient appears right below the vacuous argument.

4.7 Lexical causatives and lexicalization

Lexical causatives are, by way of approximation, words that do not bear any trace of a separate causative morpheme, but are still strongly felt to have a causative meaning. This is illustrated in (197)(a-b).

(197) a. Erebus killed Charon
b. Erebus persuaded Charon

The predicate *kill* is perceived to be related to the fact that if Erebus killed Charon, then Erebus caused Charon to die. This type of inference is a core feature of (197)(a-b). Given the theory so far, the question is whether *kill* is a lexicalised form of die[2], 'cause to die':



This is clearly what is predicted by the present proposal, although 'cause to die' is to be understood linguistically in the sense of 'x precedes y' together with whatever more robust interpretation is supplied by C-I. If this is so, then we could explain why *kill* involves 'causative tone,' viz. much of the same reason as any causative does.

This solution removes the ad hoc character of meaning postulates. Meaning postulates can be stipulated freely, and there would be no explanation for their appearing in certain cases, and not in others.

Secondly, we avoid the pitfalls of biclausal analysis. Biclausal causatives and their corresponding lexicalist versions are not synonymous, and the explanation runs into technical difficulties over scope phenomena, and so forth. Here, causality does not involve any sentential source. "Causative tone" is a result of the cognitive principles regulating the assignment of arguments at the level of syntactic representation. This can be illustrated by comparing analytical and lexical causatives in the context the present theory. Analytical causatives involve two predicates, whereas a lexical causative involves only one. This, of course, leaves room for syntactic and semantic differences, ideally just the required amount.

As another piece of evidence, consider a language (like Finnish) in which the morphology of causative forms is quite productive. We may thus ask whether we could form a morphological causative from *die* instead of the lexical one, and whether these are synonymous. These appear to be two options available to the language faculty. It turns out that where there is a lexical causative, the morphological one is ungrammatical (or very odd):

(198) Pekka kuoli
Pekka-NOM die-PAST
'Pekka died.'

(199) ?Merja kuoletti Pekan
Merja-NOM die-CAUS Pekka-ACC
'Merja caused Pekka to die.'

The verb *kuoletti* in (199) has a special meaning, 'to eliminate something in an abstract sense.' Similar data is cited by Givón, 1976, §5.1: in Bemba, a Bantu language, there is no morphological causative *cause-to die*, since this "target slot" is already occupied by the lexical causative *kill* (see also note 6 on p. 336 for an analysis of what happens - basically just what is seen in the case of Finnish). This supports the hypothesis that lexical causatives and morphological causatives are derived from basically the same mechanisms. However, it also shows that we must be able to distinguish *tappaa*('kill') from *kuolettaa*('to eliminate something in an abstract sense') at some point in order to guide the morphological component to produce the corresponding differences; so far their LLF representations are identical.

On the other hand, it must be mentioned that there are languages that allow the same intransitive verb to correspond with both lexical and morphological causatives. Thus, in Japanese the intransitive verb *ori-*, 'come down,' can be transformed into a morphological causative *ori-sase*, 'cause to come down,' while there also exists a verb *-oros*, meaning 'bring down'. In that case, much as in Finnish, the morphological component must have two options for interpreting one and the same LLF representation, one morphological and the other lexical, presumably associated with some slight difference in meaning in Japanese, too.

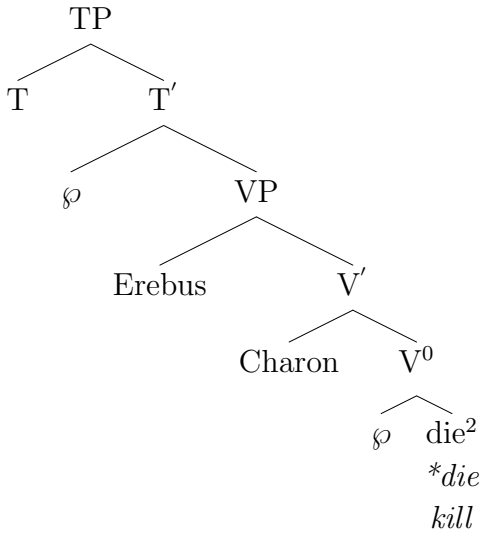
A more interesting case is reported by Valenzuela (2001). A lexical causative verb in Shipibo-Konibo can be detransitivized by adding the suffix *-t*. Interestingly, these derived intransitives can be further turned into morphological causatives by adding the productive suffix *-ma*. The derived elements, then, double the lexical causatives so that, from the semantic side, they correspond to a single LLF representation. The prediction is that these new forms should carry a different meaning from the doublets, as is indeed the case as they tend to code more unusual and elaborate causative situations.

As yet another piece of evidence, it has been observed that children produce causative forms of *die*, as they tend to produce such form from any intransitive predicates (from Pinker, 1989, p. 24):

(200) a. he's gonna die you, David

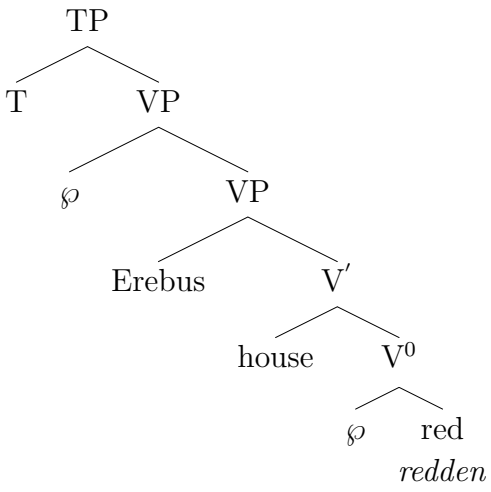
b. Eve's gonna die it

These are ungrammatical only in the sense that the morphological form of *die* is wrong (i.e. *kill*). The child is thus overgeneralizing the rules of the UG, needing only to substitute *kill* for *die*[2] to obtain the correct forms. But then we know that the ungrammatical forms cease to exists in his or her idiolect (Hochberg, 1986, Pinker, 1989, §7.1.3). Thus we might speculate that the morphological component overrides the general rules, replacing morphological causatives with lexical ones:

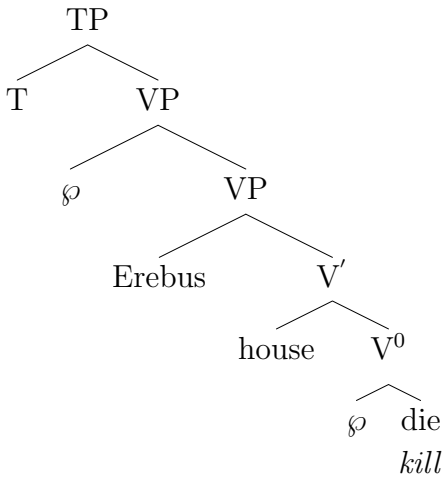


Thus, the Lexicalization Hypothesis (LH) predicts that transitive verbs are, roughly, formed from their intransitive counterparts, and that this process has consequences for both types of interpretation: conceptual-intentional systems (causativity, agentivity), and the morphology-PF surface forms (*red*, *redden*). Sometimes a separate morphological affix is used, as in the case of morphological causatives, while at others a whole new lexical item is used. These two options are illustrated in (201) and (202) below.

(201)



(202)



However, as noted above even if the lexicalist version (*kill*) often replaces the morphological one (**die-CAUS*), in some cases both forms co-exist with a slight difference in meaning. This is the case with the Finnish verbs *tappaa-kuolettaa*, of which represent 'elimination,' but the latter in a more abstract sense. However, the latter is a morphological causative formed from *kuolla* 'die'. The question is how to represent this difference.

This phenomenon is not exceptional, but part of the 'core grammar,' hence it is not really useful to try to explain it in terms of obscure facts concerning pragmatics. Thus, words such as *kill*, *assassinate* and *murder* all involve causation and dying, yet they cannot be synonymous. They seem to express different semantic concepts. This leads to the following dilemma. Suppose we take *murder* to be a distinct concept of *kill*. Then the fact that

murder involves 'cause to die' would be left unexplained, requiring us to rely on meaning postulates. If we rely on meaning postulates, we could do so in the case of *kill* as well, so that much of the original favor of the proposal would be lost. In any case, we would then fall short of a theory of any kind, since the appearance of such meaning postulates would be stipulative. On the other hand, if we assume that the causative meaning of *murder* derives from the same source as the causative meaning of *kill*, then we would predict that they were synonymous, with identical deep structures '*die*(x, y)'. This cannot be so, since *murdering* and *killing*, although close in their meaning, are not synonymous.

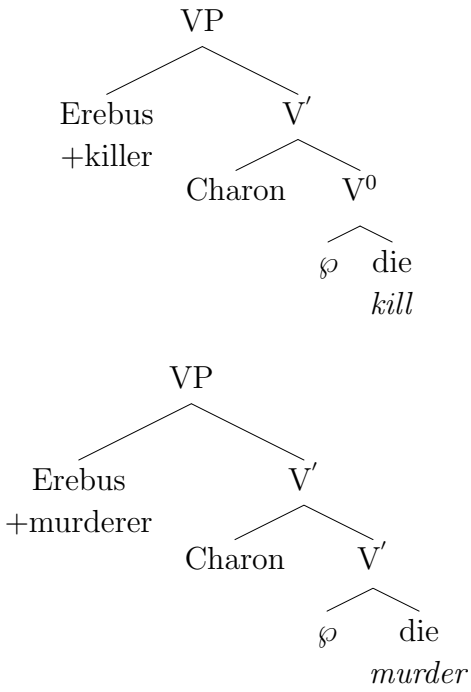
Consider the difference between killing and assassinating, as a concrete example with which to begin. In both cases, the Patient is subject to the same course of events: she or he dies. The difference is located in the activities of the Agent. The atomistic lexicon contains the item *die*, which has one semantic feature, say DIE, representing dying or death. The result is killing by associating a subsequent participator, an Agent, with the dying in a certain (causative) way. Thus the fact that dying becomes killing is not only to do with the lexical item DIE, it also has to do with the way another participator becomes involved in the event. The difference is located in the "interaction" between the two constituents. Much the same is true of *assassinate* / *kill*: assassination involves an Agent who kills the subject 'on contract' and/or for 'political reasons,' and killing does not necessarily involve such an element. Hanging differs from shooting (to death) in that the instrument (the d-argument) differs. If this is so, however, we do not need a separate lexical item for *assassinate*: instead, we need to vary slightly the presuppositions involved in the participation of the Agent. Again, the difference is in the 'composition rule,' not in the lexical elements.

From a different perspective, the Agent of the assassination is individuated in a slightly different way than the Agent of the murder or killing. These differences, assuming that the above analysis is correct, nevertheless affect the morphological component, turning *kill* into *murder* / *assassinate*. Such phenomena are widespread in human languages: different lexicalised verbal forms are associated with various pragmatic aspects of the agents and objects, as "languages lexicalise different aspects of a state of affairs and vary in what they require a speaker to code about it" (VanValin & LaPolla, 1997, p. 89). Moreover, I observed earlier that the same LLF representation, as it has been defined so far, may result in two lexical words, so that, in any case,

some additional source of semantic-pragmatic information must be assumed as part of the theory.

This is especially true of causatives, which are often sensitive to pragmatic factors such as the agent's 'control over the situation' and the like. These pragmatic differences are often grammaticalized, hence there must be a way to allow them to affect linguistic computation.

To summarize the argument so far, if morphological and analytical causatives are produced by transparent generative mechanisms, 'lexicalization' requires that some additional input is fed into the LLF. One hypothesis is that such information comes through the ϕ-features of the arguments, and is then reflected in the morphological component:



This hypothesis is in line with my more broader hypothesis that many syntactic, morphological and even semantic properties of lexical elements are not intrinsic to the lexical elements themselves, but emerge from the propositional frames that constitute them. From a linguistic point of view, such 'propositional frames' are LLF representations. This hypothesis allows me to entertain lexical atomism by removing the explanatory burden from the lexicon to the overall clausal / propositional structure. Moreover, the theory does not violate the atomistic hypothesis, since now the fact that there is no definition of the verb *kill* is explained by the fact that there is

no definition of the semantic feature *killer*. Yet, it can be shown that to kill means also 'cause to die' - just the result I wanted.[12]

One problem with this specific hypothesis is that these lexical forms, such as *murder* and *assassinate*, can be nominalized without *any* arguments. This is shown in (203).

(203) a. killing is wrong
b. murdering is wrong
c. assassinating is wrong

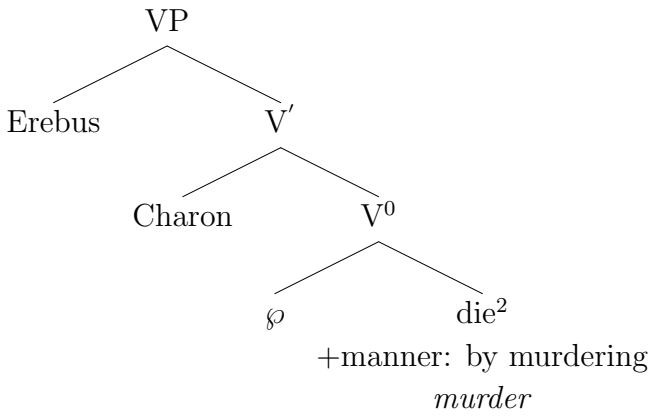
If these forms are differentiated by virtue of an agreement-type argument-predicate relation, then in these cases there are presumably no arguments to induce such changes. Moreover, if semantic features such as [+murdered]/[+killer] are part of the agreement features of arguments, why they do not induce something like 'inflectional changes' to the word, but rather than changing it fundamentally? In other words, why does 'lexicalization' not look *at all* like an instance of inflectional morphology?

To further understanding of the problem, let us consider the semantic feature that represents the *valency* of a predicate. We could ask whether it is the valency that forces the required LLF structure around the predicate (a bottom-up explanation), or whether the LLF structure induces the valency to the predicate (top-down explanation). The matter is not empirically empty. The latter hypothesis means that there cannot exist a predicate with two argument positions without the relevant structure. Nevertheless, there is grammatical evidence that a predicate with a valency > 0 can appear with any number of arguments:

(204) a. dying^1 was scary
b. killing^2 was scary
c. Charon's killing^2 was scary
d. Charon's killing^2 by Erebus was scary

[12] A certain resemblance to the "contextual" theories proposed by Frege, Russell, Quine and Davidson must thus be acknowledged, yet these authors, for different reasons than the ones given here, went far beyond in their "contextualism" by proposing that (certain or all) words do not have meaning at all in isolation, but only acquire meaning when they appear, and in virtue of their appearance, as constituents of truth-valuable sentences. My view is similar in that many properties of words, although not, of course, all, depend upon their 'context,' namely causativity, thematic roles, categorial features and structural Case, among other things.

This, in turn, means that when a lexical element is attached to a valency, the required structure is created only when it is necessary (when the predicate is merged with $\wp$ operator, which makes it unsaturated). The morphological component does *not* reflect the LLF structure (feature vector), but it does reflect valency as an associated (but not constitutive) lexical semantic feature. Since lexicalization follows the same pattern as valency in all relevant respects, I am inclined to assume that the semantic features of 'manner' that distinguish killing from murdering are attached to the lexical root elements even if they have something to do with the participation of the Agent (or Patient):



In other words, lexicalization means that the root lexical element is associated with an additional semantic feature. Note, however, that this semantic feature now represents murdering, as a distinct event from killing, and the theory thus still relies on the atomistic lexicon. The answer to the question of what it means to murder somebody, is not 'cause to die', but 'cause to die by murdering.' It is just that murdering is a 'manner of causing to die' that cannot be expressed *completely* in any way other than as "X murders Y."

Another way of looking at this proposal is to say that the facts that make the lexicon *semantically primitive* may still be complex from the perspective of *form* (LLF). What is primitive in the lexicon can be tunneled into it via several routes (events, manners, or instruments).

4.8 Homonyny, polysemy, lexical redundancy rules and language use

Lexical elements, causatives included, have semantic properties that I have left unexplained, raising questions in the present atomistic context. To begin with, although most Finnish T(T)A causatives are causatives in that they satisfy the diagnostic criteria given earlier, they often have more intricate semantic properties in addition. For instance, the morphological causative verb 'cause to jump', *hyppyyttää*, can be used in the following sentence:

(205) Pekka hyppyyttää Merjaa kaupoissa
Pekka jump-CAUS Merja shops-SEM
'Pekka causes Merja to go to the shops.'

The verb has a special meaning, hard to translate into English, implying that Pekka is somehow being unfair in causing (asking) Merja to go to shops. I have mentioned earlier that lexical words can begin a 'new live' when they attain such features, or special meanings and special use. In this case, the special meaning exists aside with the normal causative 'cause to jump': Merja does not literally jump to the shops, but probably goes there to hand Pekka's business. Thus, it could be a causative, but the root predicate at the conceptual level (or at the LLF level) is, insofar as its meaning is concerned, not JUMP. Many possibilities remain.

As discussed earlier, we need to distinguish the productive and systematic aspects of language from the idionsyncratic, non-productive aspects. Problems related to the usage of individual words could potentially cause confusion in this respect. Lexical atomism follows from compositionality (productivity and systematicity), in a manner explained earlier. It thus represents an idealization in allowing a finite number of exceptions. In other words, as soon as the productive and systematic representational generative capacity is in operation, it can digest additional properties as long as such properties come in finite numbers. A single word, for instance, can begin a "life of its own." Sometimes complex expressions have this property, hence idioms such as *kick the bucket*. Nevertheless, the lexicon is a finite stock by nature, and thus an easy candidate for hosting such information. It is clear that it really *does* attract a lot of it. It is thus to be expected that the lexicon of a language is only indirectly related to the stock of concepts, primitive or complex, masked by a complex array of idiosyncratic and language-specific information. Taken

from a slightly different perspective, the lexical meaning relevant to the level of concepts is that which is "abstracted": it is the core meaning that is relevant in terms of language as a whole. In other words, the meaning that is relevant when the lexical element contributes something to the meanings of its infinite range of hosts.

Returning to the case of *hyppyyttää*('cause to jump'), we could seek for an historical explanation for the usage of words related to jumping, which not implausible because *hyppyyttää* in this sense has a synonym *pompottaa* ('cause to bounce'). This is the case of homonyms: one word, two concepts.

In some cases, certain words that are either identical or different in their phonological properties resemble each other in meaning enough to warrant one to say that they are not homonymous, but polysemous. The problem here is that, assuming that two polysemous words express two different primitive concepts, it is difficult to explain why they are related to each other.

Such an explanation is not altogether impossible, however. There are three possibilities within the atomistic hypothesis. The first is to rely on non-constitutive connections between the two concepts: it may not be the concepts that are similar, only what they represent.

However, there is another possibility. It is not prima facie clear if any such items express two different concepts that are 'related in meaning', or whether they express one concept with a more abstract meaning. Consider the Finnish causative nostattaa, 'to rise', and its use in the following sentences:

(206) Pekka nostattaa kapinan
Pekka rise-CAUS revolution
'Pekka caused a revolution to rise.'

(207) Pekka nostattaa myrskyn
Pekka rise-CAUS storm
'Pekka causes a storm to rise.'

(208) Pekka nostattaa hissin
Pekka rise-CAUS elevator
'Pekka causes an elevator to rise.'

In addition, it is common that even temperatures and stock prices can rise. One might be inclined to argue that these meanings are related to each other, and an advocate of the atomist hypothesis could further claim that they involve the same abstract concept RISE, disambiguated in a given

context. This would, of course, explain why they are related in meaning (Fodor, 1998, pp. 49-56).

The third possibility, used especially in the present proposal, is to rely on the fact that several elements at the LLF level may interact to yield different morphological forms of words. Thus, words such as *kill*, *murder*, *assassinate*, *hang*, *shoot* (to death) are clearly polysemous. They appear polysemous, according to my proposal, because they have been constructed from the same LLF predicate DIE-*die*, with slightly different presuppositions concerning the participation of the agent, patient and the instrument. Such information must be present as features at the LLF level, and then these features are turned into corresponding morphological forms.

Finally, and most importantly, the operation in the language faculty is, to a certain extent, subject to one's voluntary control. One could *use* its resources by, for instance, allowing it to output certain morphological forms. These activities are guided by the C-I system as a whole, and hence they are virtually unrestricted: there are no constraints on how one can use a certain word. In the context of this study, one could, at will, construct an LLF representation in any situation. Thus, it is not altogether impossible that society might sustain certain stable patterns of language use. These stable patterns of social conventions could account for the correct intuition that certain words are used in special situations or special contexts, and hence that they are associated with 'semantic features.' A root lexical element at the LLF level, being interpreted truth-conditionally by some external system, might well constitute a concept; but there are other usages of these formal elements as well. Thus, it is possible to use the word or lexical element, expressing the concept JUMP in contexts that are not related to jumping, or which are restricted to special contexts. This would constitute part of the theory of "choice of action," determining how C_{HL} is put into use, or part of the more extensive "situational semantics," to use Bouchard's term (1995, §1). But, as Chomsky put it, even if the "problem of choice of action is real," there is no theory of such action in sight, and the whole phenomenon is "mysterious" and thus excluded from the "narrow study of mechanisms" (Chomsky, 1995, p. 227). My own suspicion is that, in addition, most of such curious facts are not explainable (in the naturalist sense of the term), although they can be described and provided with some kind of historical account.

Consider the following question: What is it to know Chinese? What are the possession conditions for the concept CHINESE SENTENCE? What is the meaning of the word *Chinese*? Yet the question might be misleading in that, according to many authors, there is no such concept as CHINESE: "In the empirical study of language, it has long been taken for granted that there is nothing in the world selected by such terms as 'Chinese,' or 'German,' or even much narrower ones" (Chomsky, 2000, p. 155). English, Chinese and German are best described as "non-natural categories," determined in part by principles and primitives of the Universal Grammar and hence, ultimately, by the innate and genetically determined endowment of the human language faculty, plus a great deal of socio-cultural noise influencing through the senses. This makes categories such as CHINESE and ENGLISH "sociological and political construct[s]" (Lasnik, 2000, p. 2). This means that the meaning of the term *English* (or *English sentence*) is not constitued (only) by the concept ENGLISH, but also involves some deeper aspects of our mind (UG) plus some 'socio-cultural noise.' It could be that a large part of the putative semantics of *English* is determined by the usage of that *word*. What I am suggesting, then, is that this *might* be so with many other ordinary language words, such as *doorknob*.[13]

These assumptions entail that the interface LLF representation has two kinds of "semantic interpretation." One of these associates an LLF representation with the property or a proposition it represents. The lexical element *red* is associated with the concept RED, hence the phenomenon of redness (whether individuated in terms of I-semantics or E-semantics). This kind of interpretation should be a suitable descriptive tool in an appropriate model theory, the model being a collection of either some internal or external realms of objects, or possibly both. I have assumed that it is semantic properties in this sense that are used to *individuate* the lexical elements at the LLF level. Thus, a typical LLF representation contains lexical elements such as RED rather than *09834*. Nevertheless, LLF representations are formal objects in terms of the operation of C_{HL}, and such formal elements are, to some extent, subject to one's voluntary control. This results in the second notion of in-

[13]Part of the reason I am fond of this proposal is that it appears to solve some mysteries concerning 'mad dog concept nativism.' For much the same reason as there is no reason to assume that the concept ENGLISH is innate, there is perhaps no reason to assume that the concept of DOORKNOB is. Moreover, the proposal explains why lexical concepts cannot be defined: viz., for much the same reason as *English* cannot. If this is compared to Fodor's theory, some amount of intentional realism must nevertheless be compromised.

terpretation, which has to do with langauge *use* rather than *representation*. It does not matter in terms of systematicity or productivity what semantic feature such processes associate with the lexical elements, since pragmatic interpretation need not be productive and systematic - it may, and should, be highly idiomatic.

4.9 Conclusions

I have argued in this chapter that it is possible to develop a theory of causative constructions in a minimalist framework such that the lexicon is taken to contain what seems forced by virtual conceptual necessity: atomistic semantic features plus idiosyncratic phonological matrices. Causativity, as a semantic property, is factored into two components, aspectual θ-roles and C-I. θ-grids that have traditionally been located in the lexicon are derived from the principles of UG with some contribution from C-I. These modifications are also guided also by the desire to formulate semantics in a manner that is abstract enough to be relevant to syntax (Bouchard, 1995).

It is no longer a mystery why certain characteristic syntactic, semantic and morphological properties of causatives tend to hang together. These properties follow from the minimalist principles of the UG in a way that seems inevitable.

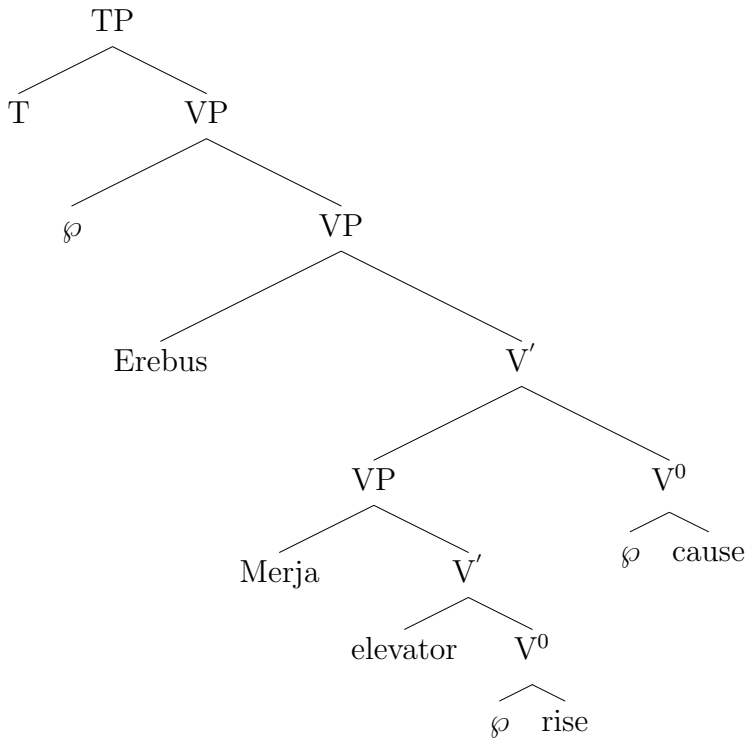
The LLF theory presented in this work is intended to solve the long-lasting problem of 'uninterpretable formal features,' the presence of which in the Numeration appears mysterious. These formal features reflect semantically interpretable relational properties of lexical items at the LLF level, supervening their feature vectors. This involves categorial features as well as the abstract Case. It was also possible to derive certain core principles of the GB theory, such as the Case Filter, Burzio's Generalization, one version of UTAH, the theta Criterion, and the heuristic validity of the X-bar theory and the EPP principle. PP support in the case of adjectival arguments was shown to depend on a cluster of properties, such as non-aspectualilty, needing no stipulation. ECM constructions pose no special problem, and were derived immediately. Similarly, properties of personal passives emerge automatically without stipulation, even in some more complex cases involving nominalised sentences and genitive subjects. However, on balance these properties must be derivable in any theory, since they are so central to the UG.

Although the theory I present assumes a truly minimalist lexicon in that it

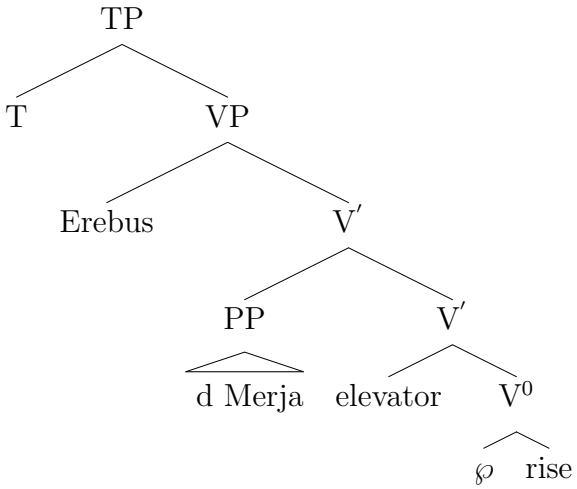
contains only what is required by "virtual conceptual necessity" - a phonological matrix and one semantic feature - and hence agrees with Fodor's atomistic lexicon, there is an important difference. According to Fodor (1975), lexical elements such as *kill* and *die* both express primitive concepts, but the fact that killing entails dying is not explained. I could say that, according to Fodor, there is, as understood in the material mode, something in killing that implies dying; but it is not part of the concept of killing. Why is this so, and why are such implications commonplace among the world's lexicons? This is what I believe to be the most challenging problem of explanatory adequacy. Those who assume lexical semantic decomposition have pointed out, correctly I believe, that the explanation must rely on the properties of our mind/brain. However, that alone does not lead us to assume that the explanation must rely on the *concept* of killing, since there are other mental faculties and entities expect concepts that could, in principle, determine such connections - say Kant's "pure intuition." For instance, the fact that three is a prime number does not, probably, constitute the concept three, yet the fact could be - and it is not argued here that it must be - constituted by our mind/brain, say as some 'number faulty.' The details are not important, and the fact that there are non-concept constitutive necessities is at least consistent with the view that the explanation of such necessities emerges from the mind/brain.

According to my proposal, the meaning of *Erebus killed Charon* is not synonymous with *Erebus caused Charon to die*, because these sentences expresses different LLF representations, and hence different meanings altogether. Thus, the first LLF representation (209) is typical of a biclausal causative, the second (210) is a representation of a monoclausal causative, whether lexical or morphological.

(209)



(210)



There is no conceptual or linguistic element CAUSE present in (210), although they both contain the lexical element, or concept, DIE-*die*. This explains why lexical and analytical causatives do not have the same meaning, and why the problems with scope, for instance, emerge (§4.6). Further, the present theory also explains why these patterns, both syntactic and semantic, are so productive and systematic; the mechanisms are built to the operation

of C_{HL} and its interaction with conceptual-intentional systems.

The toughest question, then, is this: what is the meaning of *kill*? According to Fodor's 'disquotational theory,' *kill* means killing (Fodor, 1998a). My theory departs from this suggestion, if only slightly. First of all, there is no separate entity, the meaning of *kill* or the concept of killing, that corresponds to this morpheme, for it was produced by a generative mechanism sensitive to both LLF representations and Morphology (see section 6.4 concerning the role of Morphology). The morpheme occurs, or is intelligible, only in certain 'propositional frames,' much as suggested by Frege, who postulated for other reasons that predicates are unsaturated entities and hence incomprehensible in isolation. Furthermore, as noted above, the meaning of *kill* cannot be given by the corresponding analytic phrase, *x caused y to die*, for this sentence expresses another proposition, or in fact an amalgamation of two propositions. Thus, to repeat, what, exactly, is the meaning of *kill*?

I believe the meaning of *kill* can be given only by saying that it is the activity that occurs when Charon, for example, kills Erebus. The sentence *x kills y*, expressing the relevant propositional frame, in virtue of its explicit derivational history, immediately "shows" that Charon must then be dead. Causativity is part of the 'logical syntax' of a single sentence rather than an aspect of its lexical elements.

Many problems concerning this solution remain, and some are discussed in Chapters 5 and 6.

Chapter 5

Conclusions

According to the theory put forward in this study, the lexical component of grammar (Numeration) contains only what is forced by virtual conceptual necessity: atomistic semantic features (following Fodor, 1998a) plus idiosyncratic phonological features. This explanation is similar to that given by Bouchard (1995), adopting certain modifications from Chomsky (2000b, 2001) but with a greatly simplified lexicon. All semantic and formal structure is removed from the lexicon and predicted based on the general principles of the UG.

The core idea of my proposal is that formal features in the lexicon can be predicted from their relational properties in the syntax-semantic interface, their lexico-logical form (LLF). Such an approach simplifies many components of grammar and offers a natural explanation of many otherwise problematic but interesting phenomena, such as displacement properties (passives, long-range raising), causatives, empty elements, X-bar theory, and others. Characteristic to my theory is that passives, for instance, follow automatically from simple linking rules and do not need to be characterized by any construction-specific properties. These favorable consequences suggest that we can pursue minimalist goals by eliminating imperfections from the lexicon. The Strict Lexicalist Hypothesis was simplified so that, although formal features can be associated with the lexical elements in situ at the LLF level, they are not stipulative, but are rather consequences of syntactic linking.

As the formal features of lexical elements can be predicted along the lines of their relational LLF properties, it was argued that the principle of semantic compositionality leads to similar conclusions concerning the semantic structure inside the lexicon. I have argued that there is no semantic structure

inside lexical items, and what look like semantic constraints can be explained by other means.

Specifically, argument structures are largely predictable with an abstract θ-theory of the UG, whereas the residuum of the s-selectional restrictions are apparently best explained by relying upon Morphology that assigns morphological structure to the the lexical items based on their relational LLF properties. Part of the explanatory burden was eliminated by assuming that lexical elements lack categorial features.

Whether this strategy can be pursued so as to eliminate all s-selection and c-selection cannot be demonstrated in a single study, however. This hypothesis was guided by the minimalist goal of reducing language-specific variation to the Morphology whose properties are supposed to be more easily learned. However, the θ-theory that could explain the thematic properties of the arguments of lexical items must be more abstract than it has appeared in recent literature, and many semantic properties that are often seen as part of the lexicon and linguistic computation are better viewed here as products of the conceptual-intentional (C-I) system. For example, notions such as 'Agent' and 'Patient' are partly pragmatical (C-I), and only partly part of C_{HL}.

Several issues and problems were brushed under the carpet here, sometimes consciously, quite possibly sometimes not. My primary concern has been to show that the atomistic lexicon provides the starting point for a linguistic analysis under the rules of minimalist grammar, such that it is explanatorily adequate, and has the potential to further simplify minimalist theory, and to reveal interesting generalizations about the organization of human language(s). The basic principles were laid down, and some core data was discussed, especially data that was relevant to the study of causatives. The theory is being tested by computer simulation, as it is fully formal and easy to implement on a computer. Focusing on a somewhat narrower topic is not likely to be illuminating in the long run, however, though it must be assumed to be a necessary but unfortunate restriction for a single study. I would now like to point out some significant weaknesses in my account, mostly concerning the omission of a great deal of important data.

The development of the minimalist theory from GB theory was crucially influenced by the properties of long-distance displacement phenomena, covert and overt, such as wh-movement, Quantifier Raising and V-movement. More specifically, it was evidence concerning invisible landing sites (ECP) that

suggested that an element can only move a certain highly restricted distance and for certain highly restricted purposes. This seemed to be so in the case of NP-movement and V-movement. Of particular importance in these speculations was, and still is, evidence concerning word order and its variation in various languages across the world, which is considered an important syntactic fact, and part of the reason to postulate specific hypotheses concerning narrow syntax. One problem is, thus, that I have, first of all, ignored such evidence almost completely, and secondly, assumed that word order might not be revealing in terms of the study of narrow syntax (see, in particular, §3.5, 6.1 and §4.8).

Ignorance is not necessarily a deficit as such, quite the contrary, but in the present case one might wonder whether only ignorance of complex data could have produced the putative simplifications to the theory or, indeed, it could be that "what appear to be improvements in one area may turn out to raise problems elsewhere" (Chomsky, 1981a, p. 3). The burden of proof is on my side, requiring demonstration that the putative simplifications and other assumptions proposed here do not lead to difficulties, but result in simplification in these other areas of data as well.

The hallmark of true explanation in comparison with mere description is the fact that some putative explanatory principle may be motivated and supported by independent evidence. This has been the case with most assumptions put forward in this study. Thus, systematicity and productivity have been motivated by reasons external to the syntactic and semantic properties of linguistic causatives, leading to the atomistic lexicon which, in turn, leads to an alternative explanatory strategy concerning causatives. The derivation of formal features in the lexicon, along with many other assumptions that have a minimalist flavor, was motivated by the strong minimalist hypothesis, which, in turn, could have been motivated in part by some evolutionary hypothesis concerning the origins of human language. Empirical, linguistic evidence for the minimalist hypothesis, such as the fact that elements seem to make the 'shortest moves possible,' should be viewed more as a hint that makes minimalist speculations at least plausible in the first place.

The rule for deleting constituents with identical feature vectors (maximal projection) or otherwise identical formal properties (binding theory) was motivated by the assumption that the language faculty is modular, in a strong sense. Some principles lack independent motivation, however, and could be seen only as descriptive, which is an obvious defect.

The most problematic of these, was the phenomenon of restructuring, which involved, roughly, the movement of an element from an embedded infinite clause into the matrix clause, thus explaining the ECM constructions and many phenomena that would otherwise contradict my assumptions. I was incapable of finding any independent motivation for such a curious phenomenon, and more so when I recognized that in order to explain various phenomena concerning binding and the assignment of morphosyntactic features to lexical elements, for example, I had to assume that this movement took place before any other derivational aspect. This makes restructuring a relatively deep property, yet not deep enough to be motivated semantically. The possibility remains that, from a theory-internal perspective, something fundamental is missing; of course, from a theory-external perspective this is obviously the case.

Certain phenomena which that clearly contradicted my assumptions were not addressed properly, but only mentioned. Real double-object languages, if they exists, plainly contradict rule [51], which requires that an XP cannot contain two arguments with identical morphosyntactic properties (L-vectors). I was unable to solve this puzzle. From a descriptive point of view, these languages represent the minority, and in many cases the argument has been put forward that their usage of double objects is in part illusory, a result of the confusing morphology (see §3.5). However, it was impossible to state with complete certainty that this would be so in every case, hence I believe there might be a real problem here.

Two possible solutions present themselves at once. The first is to stipulate a parametric difference, although such a difference must again be motivated in part by independent considerations, requiring an extensive and careful study of these languages one-by-one. A 'parametric difference' that distributes the number of direct objects among the world's languages would not explain anything. The other solution points to the fact that, from both theory-internal and theory-external perspectives, something important is amiss. I still believe that the general pattern might be right. Only a certain number of arguments are interpretable syntactically, for some reason, as was clearly illustrated in the case of causatives in which such overflow is transparent, but it may be that a serious mistake has been made.

I have argued that LLF representations are semantically interpretable, yet no concrete interpretational procedure together with an explicit model-theory was presented. There were two reasons for this.

First, presenting such details would have enlarged the study considerably with material that was not essential to the argument at hand, and would merely have added more technical niceties. Some earlier versions of this study nevertheless included sections on semantic interpretation, in which I largely followed Bealer's theory of concepts and qualities (1982), adding nothing original. The theory is first-order, taking intensional entities, such as the property of redness, as primitives into the model, and producing a specific predicate for instantation corresponding to the operator $\wp$.

The second reason for such an omission of semantic detail is that I was not able to develop or borrow a convincing theory of quantifiers - precisely the aspect of model theory that is most non-trivial, and most discussed in the literature which offers a range of opinions and theoretical possibilities. The whole complex of issues to do with the syntactic and semantic properties of quantificational expressions was simply not addressed in this study. Yet I would suggest that this omission is not so harmful to the argument at hand, since it is hard to see how data on quantifiers could be relevant enough to the issue of the atomistic lexicon to warrant the considerable lengthening of the material and of the time taken in discussing such topics. Another and separate treatment is thus called for.

Ergative languages, omitted here, present an interesting challenge to current rules of syntactic interpretation (see Dixon, 1994). At first, syntactically ergative Case marking - i.e. a system in which the object of a transitive clause and the subject of an intransitive one have identical morphosyntactic features - seems impossible in the present context, which necessitates that the subject of both intransitive and transitive sentences are identical in their morphosyntactic properties due to their identical feature vectors (<a, $\wp$>). This case is fairly interesting and much studied, but these aspects were not discussed here again due to the desire to concentrate on the essentials.

My weakest and most vulnerable hypothesis, as I see it, concerned the derivation of lexical formal features (categorial, Case) in agreement with STM and the idea that the lexical component might be more abstract than has been assumed previously. Thus, I rejected the Strict Lexicalist Hypothesis. There is no direct argument in favor of this proposal, only indirect ones: these assumptions, together with the rules of linking, could be used to deduce several other principles of grammar. Furthermore, this hypothesis 'converges' with, or supports, the atomistic theory. The problems inherent in these principles (e.g., the Case theory, Burzio's Generalization), however, are also

problems for the present proposal. I could merely reply that counterexamples are not usually sufficient as such for the rejection of some principle or other: they often call for independent explanation.

One complex problem concerns the ordering of the various rules. For instance, restructuring has an immediate effect on the feature vectors of various constituents, which then influence the operation of further rules. One problem is the question of timing between, say, restructuring and binding, and another is the question of the recursive application of restructuring. These assumptions and the interactions of various rule components have a wide variety of empirical consequences. In the worst case the interactions cannot produce the correct empirical consequences, while in the best case, an optimal arrangement is found that conforms to the empirical data. The matter remains open, but must be addressed urgently. Before these questions are resolved in one way or another, they leave crucial problems unsolved.

Finally, I would like to add that if these assumptions appear too problematic from the linguistic point of view, there is a weaker interpretation such that they do not represent any attempt to change the more standard assumptions as they are formulated in the context of current minimalist theory. One could interpret LLF structures as 'conceptual,' and not 'linguistic,' assuming that the linking rules are not rules of grammar per se, but rules of connecting semantic representations to linguistic ones. In that case, the linking rules provide the Numeration, given a semantic representation (LLF): lexical elements with the morphosyntactic features assigned to them. This weaker interpretation would still satisfy the goals of this study, namely, to present a linguistically relevant atomistic theory of concepts. It is just that, if we assume that LLF structures are conceptual and not linguistic, they become detached from the plethora of useful linguistic evidence, becoming almost completely invisible to rational, empirical inquiry. Not much is known about non-modular cognitive structures and processes.

In conclusion, I have pointed out a number of problems and inadequacies in my proposal in order to set some broad guidelines for future research, given that my assumptions were on the right track in the first place.

Chapter 6

Appendix: The minimalist framework and the atomistic lexicon II

The previous chapters presented the basic assumptions of the theory and applied them to the study of one important body of linguistic evidence, namely, causatives. The central idea of the theory was to propose rules of linking that would map a 'propositional thought' (LLF), in terms of its linguistic propeties, to its surface form. The existence of an atomistic lexicon was a crucial assumption in the theory. In this appendix I will continue to explore the grammatical consequences and properties of the atomistic theory.

6.1 Numeration and linearization

In what is currently the most influential version of the minimalist theory, the derivation of linguistic expressions begins from an array of lexical choices, called Numeration. According to the Strict Lexicalist Hypothesis, these lexical elements are already attached to their relevant morphosyntactic features (e.g., agreement, Case, category) already at Numeration, rather than assigned in the course of derivation. Whenever such a feature is uninterpretable it must be eliminated in the course of the computation by finding a suitable local match in order to satisfy the principle of Full Interpretation (§3.5). In some cases this elimination process entails the displacement property as a side effect. The remaining problem is then to explain why such formal features emerge in the Numeration, or why they are included in the lexical

elements, in the first place.

According to the present proposal, formal features are not stipulated in the array of lexical choices, but are derived from the structural LLF properties. This has left the status of Numeration and the notion of "derivation" essentially open. There are several possible lines of inquiry one may take at this point, but fortunately the choice is not so significant for the issue at hand. Nevertheless, these choices should be made explicit in order to reveal the purpose of such minimalist explorations in the context of the working hypothesis of this thesis.

I have followed Fodor's insight in arguing that systematicity and productivity are not consistent with any kind of conceptual role theories with statistical features, and on the other hand that there is much evidence that lexical elements cannot be decomposed either by invoking necessary and sufficient semantic features (definitions). In Fodor's view this is a question of the nature of lexical concepts, but from the minimalist perspective it is not obvious that a theory of concepts, even if it uses the notion of "lexical" in its formulation, is relevant to the computation in language faculty. I think it is relevant, but in any case it is an empirical matter. Suppose, however, that it is the "level of concepts" - by which I mean a more abstract level of representation than the linguistic levels - on which systematicity and productivity operate, and that they henceforth constrain only a theory formulated at that more abstract level. In that case, the LLF representation would represent a non-linguistic level of semantic representation, which is *interpreted* or *linked* linguistically.

In this scenario, the LLF is either connected to the LF or it is used to generate the Numeration, for as I have shown, the structural properties of LLF may be used to assign formal features to the lexical elements, from where the derivation is usually thought to begin. This scenario would still satisfy my initial purposes in that it shows how an atomistic theory of concepts could be used in an explanatory framework, its conceptual primitives and structures being linked to the syntactic ones via rules that are intuitive and simple. In other words, it would be possible to leave the minimalist theory almost intact by proposing that an LLF theory is a theory of concepts (a "language of thought").

There are two principal reasons to deny this weaker hypothesis, although nothing that follows will depend on the matter. Chomsky (2000b, pp. 175-176) considers this possibility, pointing out that assuming yet another se-

mantic level would add some "extra layer of complexity." This is true: without very strong empirical motivation, we ought not to assume new levels of representations. Secondly, some LLF properties are motivated by linguistic concerns, and thirdly, it is not evident that the C-I system consists of representations and local rules that manipulate them (derivationally or representationally). It seems that this system is too "holistic" or "complex" to benefit much from the computational approach (Chomsky, 1957, 2000c, Fodor, 1983, 2000a). I will remain as neutral about these possibilities as possible; the data is still too impoverished to settle the issue one way or the other. Note that this is not a question of whether some kind of "language of thought" exists, but has more to do with the theoretical status of the LLF representations.

Another and stronger, hence also more interesting, hypothesis is to speculate whether LLF is internal to the language faculty, or whether it is understood at the "interface level," its properties replacing the notion of LF, or some aspects of it. In that case, the rules that assign formal features to the lexical elements would be syntactic or linguistic, with the sole purpose of linking LLF representations to elements that are phonologically interpretable, in other words, code semantic properties in terms of what is interpretable at the PF level according to the principle of Full Identification (not Full Interpretation). Still, the principle of Full Interpretation would not be violated, since LLF is semantically interpretable and PF requires properties that are interpreted at that level. Then, assuming that the hierarchical relations are not thus interpreted, they must be deleted, leaving an array of phonologically interpretable elements, say lexical elements with their morphosyntactic features assigned. This corresponds to the concept of Numeration, except that its place in the architecture as a whole is changed from *the pre-syntactic phase to the post-syntactic phase.*

Morphology appears between syntax and phonology (cf. Zwart, 2001), so that if we assume that Morphology feeds PF, meaning that every element leaving Morphology must be phonologically interpretable, then clearly some features - for example, ACC in English - are not. What happens is that either the derivation crashes, or these features are not implemented by "movement" or some other relevant condition.

This proposal already entails much of the approach that relies on Kayne's LCA (1994), since if linear order is assigned to the output of the morphological component, as proposed by Chomsky, then this process can be sensitive *at most* to the feature vectors of the elements (the feature vector being the

input to the Morphology): the feature vector of a constituent contains the nodes (or their agreement features) that c-command that constituent. There is an another feature in my proposal that converges towards the LCA: I abandon the X-bar theory, and attempt to predict linear order by relying upon feature vectors.

Certain word order properties can be captured by allowing agreement (Morphology) to guide linearization. It is well-known that rich morphology tends to "free" the linear order, and poor morphology freezes it, although this is best understood as a strong tendency. One part of the execution of this idea has been the whole notion of the "abstract Case," which can be expressed morphologically, but by other means as well. Thus, in English the relation between the verb and the direct object is strict adjacency - a property best captured at linearization - presumably due to the loss of overt morphology.

It seems that the V2 phenomenon follows this pattern quite strictly. In German, verbs in finite root clauses, or rather whatever agrees with the subject (a verb or auxiliary), must occupy the "second position" in the sentence (in quite a literal sense), whereas this requirement is not in force in embedded finite clauses if these clauses have an overt complementizer. This had lead many scholars to assume that these V elements must occupy C (Koopman, 1984, Koster, 1975, Rizzi, 1990). This explanation runs into a trouble with V2 languages such as Yiddish and Icelandic, in which V2 constraints apply to sentences with overt complementizers (Diesing, 1990). As a consequence, some scholars have assumed that there is yet an additional position available between CP and TP. English, too, has been said to constitute a "residual V2 language" due to the fact that the V2 constraint seems to be effective in questions. Finally, the same pattern - the second positioning of whatever it is that agrees with the subject - can be illustrated in the case of VSO-languages, a variation of the V2 language. The following example comes from Welsh (Sproat, 1985):

(211) gwelodd Siôn ddraig
saw Siôn dragon
'Siôn saw the dragon'

(212) gwnaeth Siôn weld draig
did Siôn see dragon
'Siôn did see the dragon.'

(211) illustrates the basic word order of Welsh, in which in the periphrastic

construction (212) it is the agreeing element that takes the first position. If these word-order facts are indeed correlated with agreement, then we might expect to find different behavior in non-finite clauses. Welsh shows this pattern: infinite clauses obey the SVO rather than the VSO order.

Another type of evidence that is important in this connection is the positioning of negative particles and adverbs. In general, there seems to be variance in how "high" the verb can rise in a sentence. This appears to be conditioned, to a significant extent, by morphological properties. In French, the main verbs in finite clauses are said to undergo "raising" over the negative operator - and over whatever appears in the vicinity (some quantifiers and adverbs). (213)(a-b) is a contrasting pair.

(213) a. Erebus did not go
b. Jean ne partit pas

Descriptively, these properties follow by from the choice of linear order between negation and the predicate, depending on the tense/agreement properties of the predicate. Such a process is already in place in the case of the predicate and the complement. Now consider instead the fact that, as noted by Pollock (1989, §6.4), the lack of "Verb Raising" in English can be attributed directly to the disappearance of its rich verbal morphology: there is also diachronic evidence of these. (For similar evidence from a variety of languages, see Thráinsson, 2003). Chomsky, too, proposed that Verb Raising takes place for morphological reasons, an essential feature of the minimalist program. Again, it might thus be a good generalisation to try to explain why some linearization (positioning of the verb with respect to other major constituents), and not some other, takes place by relying upon morphological properties. Bobaljik (2002) argued, along similar lines, that Holmberg's Generalization could be explained by relying upon the adjacency requirement between inflectional features and their host in the phonological component (linearization). This strategy would be in agreement with my proposal, in that it removes some of the explanatory burden away from the narrow syntax and correlates it with inflectional agreement.

Consider this in the light of the fact that the negative particle has morphological effects on the predicate, as it appears in its feature vector. In English, this prevents the verb from being inflected. From the diachronic data, and following suggestions made by Pollock and Chomsky, it thus looks as if the verb is able to distance itself from the subject as a consequence of,

or in correlation with, the lost agreement morphology - in other words that a rich morphology binds these elements together. This could explain why French infinities behave much like English finite sentences, allowing more space between the verb and the subject.

By way of further exploration of the consequences of this hypothesis, note that, in Finnish, the negative particle agrees with the subject, behaving much like an auxiliary. In English it does not, but English never realises a sentence Subj-not-V, whereas Finnish does. Thus, (a) is ungrammatical in English, paralleled in the Finnish example (b) with agreement taking place in the negative particle:

(214) a. Erebus not leave
b. Erebus ei lähde

The English sentence becomes grammatical if we insert a dummy participle and generate *Erebus does not leave*. The explanation is straightforward in terms of Morphology: negative operators block person and tense agreement in English, thus the dummy participle is used as a last resort (§6.4). Given that the hypothesis was that this morphological process could lock the word order between the predicate and negation, consider the following sentence.

(215) sinä et ole ollut lähdössä
you-NOM not-SG2 be been-PERF leave-PROG
'You have not been leaving.'

Although Finnish word order is relatively free, it does not allow variation here:

(216) *sinä ole ollut lähdössä et
you-NOM be been leave-PROG not-SG2
'You have not been leaving.'

(217) *sinä et ollut ole lähdössä
you-NOM not-SG2 been be leave-PROG
'You have not been leaving.'

(218) *sinä et ollut ole lähdössä
you-NOM not be leave-PROG been
'You have not been leaving.'

This supports the hypothesis that word order is fixed by Morphology: a negative particle that agrees with the subject does not generally tolerate being pulled out of its vicinity, the same phenomenon that occurs in English and French.

As mentioned above, these tendencies are strong enough to have led Chomsky to suppose that Move and Agree are related to each other. This seems correct. If it could be hypothesized that the present theory of LLF could replace minimalist LF, and therefore that Numeration could be understood, not as part of a narrow syntax, but more as a post-morphological stage with morphosyntax derived from the feature vectors, I would claim that these word-order properties are not, in their explanation, part of a narrow syntax, but more of the surface phenomenon of assigning linear order based on features otherwise unrealizable morphologically. This is basically the suggestion made in Chomsky (2001), that head movement is not part of a narrow syntax, but it is part of phonology (see also Zwart, 2001). This is in line with Chomsky's proposal in that certain surface properties that are sensitive to interpretation (topicalisation, theme-rheme) are not part of a narrow syntax, but belongs to the phonological component (Chomsky, 1995, §4, see e.g., p. 220).

If this is so, then it follows that some aspects of semantic interpretation, notably those that are sensitive to surface order (e.g., topicalization, Object Shift), must be tunneled somehow into the process of linearlization. This could be done either by attaching appropriate features to the representations at the LLF level, letting them guide linarization, or by supposing that the C-I, which must be responsible for such shifts in meaning, can access linguistic computation at other interfaces besides LLF or Numeration. In the latter case, one might violate the Inclusiveness condition by proposing that the derivation is more dynamic, allowing extra-linguistic intervention, or by introducing the relevant features to the Numeration (these are by no means two clearly different theses, and may be only notational variants). Such effects are implemented standard minimalist theory in terms of formal features, and hence they occur at Numeration (Strict Lexicalist Hypothesis and Inclusiveness). In the present case, there are no formal features, but nothing prevents the introduction of formal, or even semantic features at the LLF level insofar as they do not constitute the lexical elements, but are associated with them freely and are thus based on "contingent truths" which, by virtue of inflecting C_{HL}, "have syntactic reflexes" (Fodor, 1998, p. 64). In

any case, what matters here is that the LLF contains lexical concepts that, due to the requirement of systematicity and productivity, are not constituted of semantic features (statistical or definitional): this suggests that a theory of such non-constitutive features is independently called for. In other words, LLF satisfies systematicity and productivity and is connected strongly, to use as neutral a phrase as possible, to linguistic computation. Part of its appeal as an explanatory level is reduced to its ability to serve in that role: the more it can be used in explaining linguistic evidence the better, but it is not assumed that it would, could, or should explain it all. Considering what is known about language, it seems impossible to provide convincing arguments in favour of any more specific theory of such features, inducing surface manipulations. Thus, I cannot provide answers to these questions here. Needless to say, the hypothesis that the explanatory burden is moved from narrow syntax towards linearization and PF is far-reaching and cannot be maintained without much empirical support. It must be reduced to the status of speculation in this connection.

6.2 A note on double object constructions

So-called double-object constructions could be argued to pose a challenge to the present theory, which bans them outright. Sentence (219)(b) is a double object construction that corresponds syntactically and semantically to the more typical construction (a):

(219) a. Erebus gave a book to Charon
b. Erebus gave Charon a book

These constructions present an interesting challenge for any theory of argument structure, especially in the present case since these constructions seem to violate principle [51] requiring all arguments inside the same FP to have a unique but different feature vector. Dativization is not general in English, and is subject to morphophonological, syntactic and semantic restrictions (Levin, 1993, §2, Pinker, 1989). If a language does not have dative arguments, then it does not have this sort of alteration (like Finnish). On the other hand, there are languages other than English that do have dative, in which case the alteration seems to be subject to similar contrasts, suggesting that UG principles are in operation (Ura, 2000, §7).

Let us first look at some of the structural properties of these constructions. Consider the following very interesting example:

(220) a. interviewing Nixon gave Mailer a book
b. * interviewing Nixon gave a book to Mailer

These examples are peculiar in that only the double object construction is grammatical. Suppose that the surface order of the arguments reflects their dominance order at the lexico-logical form, *Mailer* being the direct argument of *give*. What (220)(a) says is that there was an interview that caused Mailer to write a book. Thus, the temporal order of the participators in the event of giving in this propositional content is 'interview > Mailer > book.' It is this order that entails (220)(a), according to the present proposal. This also explains why (b) is ungrammatical: this ordering of the arguments violates the principles of the individuation of a-arguments, given the above meaning. On the other hand, if the subject is volitional, then of course there is no such problem, as shown in (221):

(221) Nixon gave a book to Mailer

This explanation works *only if we assume that the s-structure order of the arguments in (220) reflects the order of arguments at the lexico-logical level*. In other words, we must assume that (220)(a) is not a "transformational version" of (b), and rely upon the most simple hypothesis. Furthermore, it appears that quantifier scopes in double object constructions are fixed, as argued by Barss & Lasnik (1986), Hornstein (1995, §7.2) and Pesetsky (1995, §5.1.2) and it therefore follows that the surface order fixes the quantifier scopes. Under the present proposal, the fact that quantifier scopes are determined at the lexico-logical form supports the view that the double object construction and its dative counterpart differ in lexico-logical form. Moreover, the binding properties of double object constructions indicate that the dative argument is not part of its own small clause, but behaves as if it were part of the same FP as the verb and other arguments (Pestsky, 1995, §5.5.1.2).

Finally, there are languages in which, if the indirect theme argument is d-marked, and the direct Goal argument takes the accusative Case, the double objects construction seems to be the only possible arrangement of arguments for certain verbs. If this is so, then the account in terms of transformations seems especially odd. For these reasons it would seem to be correct to assume

that (220)(a-b) are represented differently at the LLF level, and that (a) is not a transformational source of (b).

The problem is then how to explain the fact that such a construction can survive [51]. I predict that the indirect object - either one of the accusative objects - is d-marked by the predicate, appearing in [+N][+SEM], although this fact is not realised morphologically. Presumably, it is then realised by some other means, say by word order. This hypothesis is in agreement with Marantz's (1984) proposal, suggesting that the argument structure of the predicate is changed between the double object construction and its homophone. It is also in agreement with Pesetsky's (1995, §5) proposal that the indirect object is supplied with a phonologically null element. For a review of the passivization of double object constructions, see Ura (2000, §7).

A small number of languages seem to allow the doubling of the direct object (Arabic, Matses, Mongolian, German, Dutch, Sanskrit, Swahili), while a somewhat larger number allow the doubling of the indirect object (Punjabi, French, Italian, Turkish; cf. Comrie (1976). To the best of my knowledge, doubling is not obligatory. Rather, as in English, a dative or oblique construction exists side-by-side with the more marginal double object construction.

The data cited in Comrie (1976) suggest that informants also tend to prefer the 'normal forms' to the double object construction, and there is considerable hesitation in accepting the latter. This suggests that we could put these marginal cases on a par with English double object constructions. (In some cases, like Kinyarwanda,[1] this explanatory scheme looks problematic.)

[1]According to Kimenyi (1980), datives and direct objects are "formally indistinguishable" in Kinyarwanda. This language has at least the following properties that are peculiar to it: (i) datives and benefactives, like direct-object NPs, undergo reflexivization, passivization, and incorporation; (ii) datives and benefactives, like direct-object NPs, are connected to the verb without prepositions, and occur in sentences without explicit direct objects; (iii) datives, like direct-object NPs, lack case marking, while benefactives are case marked; (iv) oblique arguments, such as instrumentals, locatives, can be raised to the status of a direct object: in some cases the existing direct objects undergo some syntactic change, such as deletion, but mostly the deleted preposition of the oblique argument is suffixed to the main verb when dropped from the constituent, and there are constraints that prevent such raising in some cases, e.g. locatives are not raised if the verb does not have a direct object already; (v) raised oblique arguments receive all syntactic properties of direct objects; (vi) the words order of datives, raised objects, and direct objects, all coming after the verb, is crucial and fixed; (vii) oblique arguments cannot be passivized, but they can be passivized if they first undergo the process of begin raised to a direct object: non-aspectual verbs may also have direct objects. These properties suggest that the doubling of the direct object in Kinyarwanda might be possible. Some scholars disagree, however, and I will not attempt to deal with this controversy here.

If so, one of the double direct or indirect objects should have inherent Case or indirect-object properties, interacting with the formation of passives as in the case of English.

Although data from each language must be examined carefully and separately, this could well be the general pattern. To take an example, Swahili takes a double object construction according to morphological criteria. A ditransitive sentence contains two direct objects and no visible prepositions or affixes. However, only one of these objects inflects the verb. This object rises further in the passive construction. The other object does not passivize, which is a direct consequence of d-marking. In general, as predicted, the object does not behave identically with respect to syntax (Comrie, 1976). Exactly the same is true of Hebrew (Wali, 1981, p. 292), Korean and Dutch (Kozinsky & Polinsky, 1993), but perhaps not of Sanskrit, Kinyarwanda or Matses (Fleck, 2001). A controversy remains concerning whether these languages are truly double object languages or not.

As far as double object *causatives* are concerned, Wali (1981) argued that the same patterns holds: "A preliminary comparison drawn from widely unrelated languages indicates that the behavioural properties of the IO causee differ considerably from that of the true IO. It appears that, contrary to expectation, languages consistently differentiate between the causee IO and the true IO, thus suggesting a need for critical re-evaluation of the proposal to classify the causee IO in the object category in the deep sense" (Wali, 1981, p. 297).

6.3 Expletives and impersonal passives

I have argued that a considerable part of the EPP principle is already satisfied given the present rules, including the fact that personal passives involve movement of the patient to the positions of the syntatic subject. Expletive structures are part of the EPP phenomenon, however, in a sense that, were there no subject for some reason, an expletive must then appear. I will argue in this section that the present rules of syntactic interpretation explain a number of perplexing properties of expletives and impersonal passives. The key assumption will be that, alongside the vacuous argument postulated in the case of passives, there exists another type of vacuous argument that differs from the former in that it is 'visible' to the feature vectors of the constituents below it at the LLF level. This vacuous argument is an "expletive." Thus,

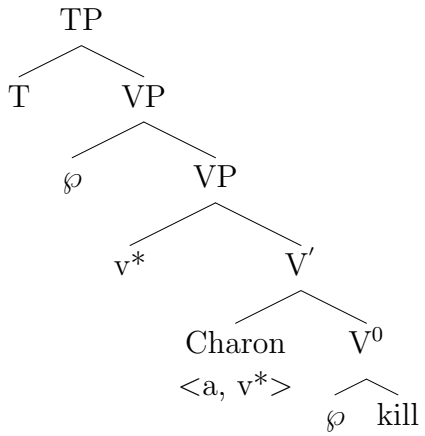
expletives are semantically vacuous arguments that are almost (normal) argument from a syntactic point of view. Finnish impersonal constructions are also discussed and it is argued that they contain covert expletives.

Expletives are related to the vacuous argument and hence to passives. First, there is overlap in their semantic properties, and seem to be "null arguments" in some sense. Secondly, there is some overlap in their syntactic properties, as it is well known that expletive structures, especially those containing *there*, are related syntactically to personal and impersonal passives. The most interesting difference is that the expletive *there*, but not the expletive *it*, requires a local, correlating nominative patient, called its "associate." This is illustrated in (222).

(222) there arrived three men

Suppose there are three distinct types of vacuous arguments instead of just one: the weak vacuous argument v, which was used in the case of personal passives above; a strong vacuous argument, symbolized as v*, which differs from the weak v in that it is visible to the feature vectors of the constituents below it at the LLF level, and finally, that the phonological content of weak v is *it*. From semantic side they are all null arguments, so that these stipulated differences are purely formal. Finally, suppose that the new feature vector <a, v*> corresponds to a structural Case we may call the *null accusative*, symbolized as ACC^0. The following LLF representation (223) illustrates the basic configuration with v*, a transitive predicate and a direct object:

(223)



At this point I will take the term "null accusative" as a new term, reflecting the fact that we do not yet have a name for the structural Case corresponding to the feature vector <a, v*>, and that a relation with the more traditional notion of null accusative is implied, as shown presently. The basic properties of expletive structures, and their close relation to passives, now seem to follow.

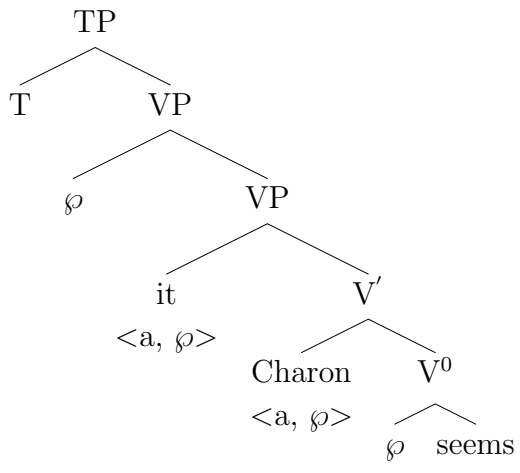
If a predicate has only one argument and it is vacuous, it is pronounced as *it*, as in (224).

(224) a. it rains
b. $[_{VP}$ v $[_{V^0}$ ℘ rain]]

The fact that a verb requires an expletive if it does not have a thematic arument follows from the assumption that only full projections are well-formed at the LLF level; the expletive fills the position of the argument. The fact that weather verbs do not tolerate thematic arguments instead of expletives is presumably part of their semantics.

It also follows that the *it*-expletive may never appear in a transitive construction with an NP argument, but it requires a clausal complement (**it seems Charon*). This is because if the same clause contained both expletive the *it* and a nominal argument, they would have identical feature vectors <a, ℘>. Figure (225) illustrates this:

(225)



Remember that v was defined as a 'no-argument' and hence it is invisible to the constituents below it, in this case *Charon*. The present proposal predicts that predicates with pleonastic subjects and accusative objects should

be completely absent. Thus, no verbs like KILL, as in the sentence **it kills Erebus*, ought to appear (cf. Chomsky, 1981a, p. 148, note 113 and references therein). However, if the object of the verb is d-marked (a PP, indirect object), the subject could be *it* (or equivalent), as in (226)(from Frampton & Gutmann, 1999, p. 25):[2]

(226) Það glampað á sverðið
it gleamed on sword
'The sword gleamed.'

Consider next (227).

(227) Erebus seems to have hit Bill

If *Erebus* appears in situ then the complement must be tensed and requires a complementizer: *it seems that Erebus hit Bill.* (227) does not violate [51], since *Bill* has the feature vector <a, a> and appears in the accusative Case. On the other hand, inserting a subject into the infinite clause then results in a violation of [51], as correctly predicted:

(228) a. * Erebus seems James to have hit Bill
b. * it seems James to have hit Bill

What happens if a rising construction is embedded in some construction as an argument? Several modules of the grammar then interact. First, if the predicate is non-aspectual, then the embedded clause inside the rising clause must be d-marked. It then follows that no "restructuring" and raising is expected, for the same reason that datives do not generally rise. This predication is correct, as is shown below:

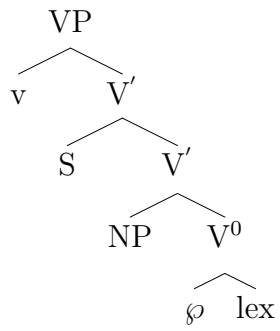
(229) a. Charon seems [to be intelligent]
b. * Charon's$_i$ semblance [t$_i$ to be intelligent]

By the same line of reasoning, *it seems to a strange man that it is raining* is grammatical, but **a strange man seems to that it is raining*, **to a strange man seems that it is raining* are not, the PP being the d-argument: the d-marked argument does not undergo "restructuring." Note that I have so far treated sentential arguments as distinct from nominal arguments in that

[2]Frampton & Gutmann conjecture that the expletive in these Icelandic sentences is comparable to English *it*, which I take for granted here.

they are not assigned a Case or any categorial features. Therefore, they are not subject to principle [51]. Putting more detailed exploration of this aside for now, and merely assuming it to be a descriptive generalization, I suggest that it is then possile to construct and interpret the following structure (230) syntactically with three arguments:

(230)



In this structure the vacuous argument is invisible to the feature vectors of the elements below it, yet if sentences (S) are exceptions to rule [51] in that they are not assigned Case or category, then all three arguments can be interpreted syntactically. This type of sentence is indeed possible, as illustrated below:

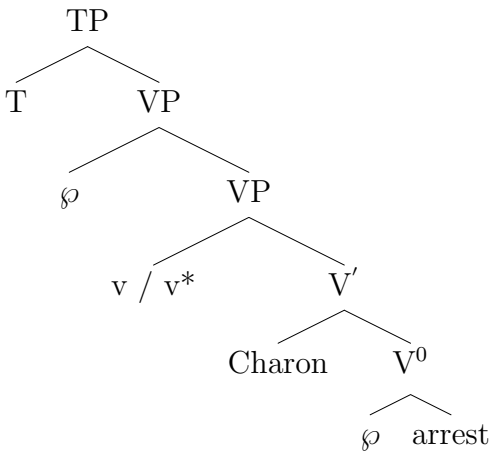
(231) it struck me that Erebus would kill Charon

The *there*-expletive, on the contrary, can appear in transitive constructions with a zero accusative "associative," as in (232b). Since the *there* expletive involves a quasi-argument v* that lacks a thematic role, (232b) is taken to be a transitive construction (Belletti, 1988, Bouchard, 1995, §5, Bowers, 2002). The *there*-expletive is related to the passive, as illustrated by the following examples:

(232) a. a man was arrested
b. there was a man arrested

The difference between (a) and (b) is that in (a), the vacuous argument is weak, and in (b) it is v*. The meanings are very close, since both contain a vacuous argument. (233) illustrates both types of sentence:

(233)



Expletive constructions provide some support for the "restructuring" A-movement discussed earlier in the case of long-range displacement. Consider (234).

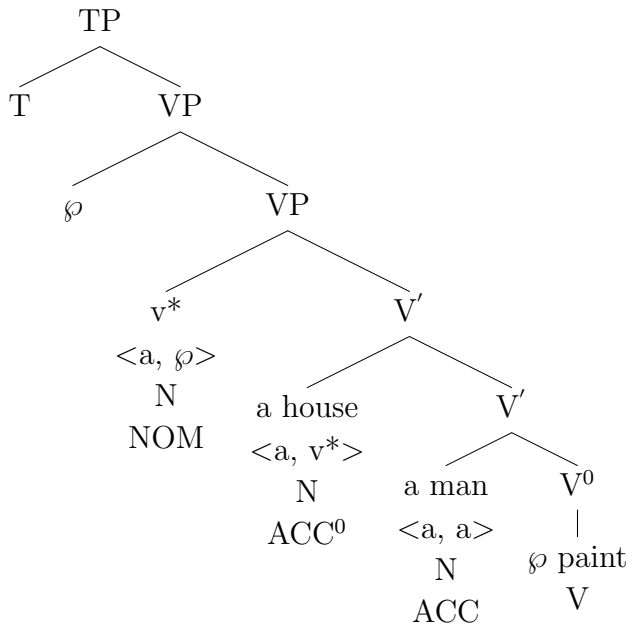
(234) a. * there is likely there to be someone in the room
b. * there seems there to be a problem

These "perennial troublemakers" are ungrammatical, and the reason follows from the LLF properties of expletives and restructuring: in both cases the embedded v* rises to be part of the matrix clause, but then two strong vacuous arguments appear in that clause, hence there are two synonymous constituents with an identical index, so that according to [51], one of them must be deleted. The resulting sentences are as follows:

(235) a. there is likely XXX_i [t_i to be someone in the room]
b. there seems XXX_i [t_i to be a problem]

For much the same reason, **it seems Charon intelligent* is not grammatical: *Charon* rises through restructuring, but then the matrix clause has two arguments with same feature vector, and this is banned according to [51]. Transitive expletive structures (Chomsky, 1995, §4, Jonas, 1996), which are marginal in English but grammatical in many languages (e.g., Icelandic, see also Rohrbacher, 1999, §5.4), emerge naturally since in such constructions all three arguments have different feature vectors:

(236)



(237) *there painted a house a man

Even more exotic constructions result when *there* (V) is combined with vacuous argument v, resulting in what might be called a "double passive."

(238) *there have some cakes been baked

[there-NOM [v-ACC^0 [cakes-ACC^0 [bake]]]]

[v-NOM [there-NOM [cakes-ACC^0 [bake]]]]

These are grammatical in Icelandic and Faroese (Jonas, 1996). In both cases, it is predicted that *cakes* should appear in the zero accusative (ACC^0).

Let us consider more carefully the interaction between rising predicates, such as *seem*, and expletive structures. Interesting interactions emerge. A rising predicate, such as *seem*, is one which has, by virtue of its very nature, a vacuous argument v as its subject (hence it is related to passive structures) and, due to the non-aspectuality of the predicate, no direct object arguments (dative subjects and objects are possible, however; see Holmberg & Hróarsdóttir, 2003 for a discussion on Icelandic rising predicates with quirky subjects and objects). Due to its relation with the passive, if an object argument is restructured in the same clause with this predicate, it takes the

nominative Case, and either the raised argument or the vacuous argument is deleted at the syntactic interpretation. Suppose that v is deleted, thus we derive

(239) a man$_i$ seems [t$_i$ to be here]

If the embedded subject is *there*, then this element raises and replaces the vacuous argument:

(240) there$_i$ seems [t$_i$ to be a man here]

However, assuming that the subject of *seem* is necessarily v, it is not possible to generate (241) where the *there* expletive emerges from nowhere:

(241) *there seems [a man to be here]

The same is true of a structure with two *there* expletives (**there seems there to be a man here*), although there are already several rules that are broken here. Given that, with rising predicates, *there* must emerge from the embedded clause if it is to emerge at all, we can explain why (242) is ungrammatical:

(242) *there seems [that a man is here]

In this case, too, there is no possible origin of the *there* expletive, since nothing can rise by being restructured from a finite clause. On the other hand, it seems to be the case that any clause, whether a matrix clause or an embedded clause, that has a *there* expletive as its subject, must contain an associate:

(243) a. * there sleeps
b. * there thinks that a man is here
c. * there seems to a strange man that it is raining outside

On the other hand, if no constituent appears with the vacuous argument in the matrix clause, then the vacuous argument is pronounced (*it*):

(244) a. it rains
b. it seems that a man is here

It is now possible to explain (245) below is ungrammatical:

(245) *it seems to be a man here

In this sentence the subject position of the embedded clause, as assumed above, is filled with a strong vacuous argument:

(246) *it seems there to be a man here

Yet this expression is ungrammatical as well, since, according to the rules of restructuring, *there* rises so that rule [51] is violated in the matrix clause, both expletives having the nominative Case. The correct from is therefore

(247) there seems to be a man here

I have argued that expletives might be overt realizations of two types of vacuous arguments, one weak, the other strong. They are thus covered by the EPP, which is itself explained as an inherent feature of the present rules.

Let us consider next Finnish impersonal passives. These constructions are important since, as I will now argue, they represent an intermediate case between personal passives and expletive structures. More specifically, I will argue that Finnish impersonal passives contain covert expletives as their subjects. Finnish impersonal constructions are also important because they are relevant to the question of whether EPP, and hence the present proposal, is too strong a condition for the formation of finite clauses, because they are constructions that, apparently, seem to lack any kind of subject. According to the present theory, this is predicated to be impossible: such constructions must involve a covert subject of some kind.

"Impersonal passives" have been characterized by the demotion of the subject without the promotion of the object (e.g., Comrie, 1977). The putative, impersonal passive form has the following properties that differ from personal passives and Finnish rising constructions:

(i) no agreement between the NP of the patient and the passive verb;

(ii) patient NPs appear to be in the nominative Case, although pronouns take the accusative Case;

(iii) expletives appear freely in these constructions;

(iv) in colloquial Finnish passive verbs appear with the first plural pronoun with an apparently nominative patient (*me löydettiin koti*, 'we-NOM found-PASS a home-NOM');

(v) both intransitives and transitives passivize freely (for a more detailed investigation into these properties, see Holmberg & Nikanne, 1993, 2002, Nelson, 1998, Vainikka, 1989, 1992).

Some examples are given below.

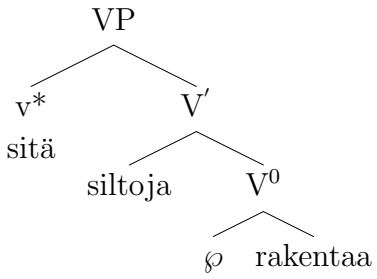
(248) (sitä) rakentaa sillan kuukaudessa
there builds-SG3 a-bridge-ACC month-SEM
'one builds bridge in a month.'

(249) (sitä) rakennettiin silta
there build-PASS-PAST bridge-NOM
'A bridge was built.'

(250) (sitä) löydettiin hänet
there found-PASS-PAST him-ACC
'He was found.'

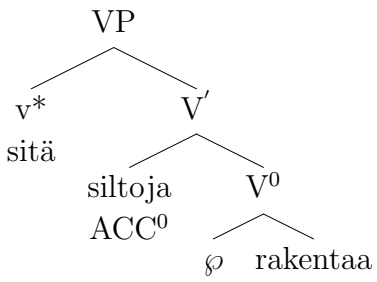
These properties of Finnish passives and other impersonal constructions are problematic for the standard GB theory to deal with, for obvious reasons, leading some authors to parametrize the whole Case theory (Vainikka, 1989). Given the current assumptions, however, there is no need to make such radical hypotheses. As Finnish is a pro-drop language, one explanation could be to assume that the Agent of the construction is "impersonal" pro, optionally visible as an expletive (iii)(Perlmutter, 2001, made a similar proposal concerning impersonal constructions in Russian; this paper is unavailable to me, but is cited in Bowers, 2002).

(251) (sitä) rakennetaan siltoja
there builds-PASS bridges-ACC[0]
'Bridges are built.'



Current rules do not, in fact, predict agreement between the patient NP and the verb (i). Then, if there is an expletive (covert or overt) in Finnish, the curious property (ii, iv) could be explained in terms of why pronouns take the accusative Case while nominals take the nominative Case: this may be how the zero accusative ACC^0 (<a, v*>) is realized in Finnish, as shown in (252).

(252)



If this were the case, then we might expect to find corroborating evidence from other areas of linguistic phenomena. Before discussing the supporting evidence further, I will briefly consider the basics of the Finnish system of structural and inherent Case (Holmberg & Nikanne, 1993, Nelson, 1998).

According to morphological criteria, the direct object is marked either in the genitive Case (-*n*) or by the partitive Case (-*a*), the latter implying that the object is indefinite, or that the action toward the patient has not finished. When the object is plural and not partitive, or when it is a pronoun and not partitive, the suffix becomes -*t* instead of -*n*. Hence it is not appropriate to say that the accusative Case is a genitive Case tout court, for there is a separate genitive Case for plurals. Both objects satisfy the syntactic criteria of direct object e.g., by undergoing a shift of grammatical role in rising constructions. Thus, we may say that the structural accusative Case is realized by the endings -*n* and -*t*, with the partitive Case optionally added with its semantic import, while there exists a separate genitive case in Finnish, which is not accusative. The true genitive Case and the accusative Case appear to be homophones in the singular, presumably due to the diachronic process that has transformed the older accusative ending -*m* into -*n*. Finally, it has been argued that in some constructions, Finnish accusative objects are realized morphologically as nominatives.

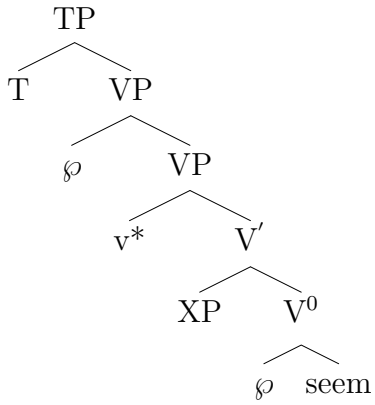
Keeping these basic facts in mind, let us turn to the evidence for the covert expletive in Finnish impersonal passives. First, consider the surprising fact that Finnish rising predicates (such as *seem*, *näyttää*) have their own

impersonal forms. This curious fact is illustrated in (253).

(253) talot näytettiin(/näyttiin) rakennetun
houses-NOM seem-PASS build-PASS
'Houses seem to have been built.'

This means that, if normal rising predicates have vacuous argument v as their subject, forms such as (253) must contain some additional structure. The hypothesis that v is replaced with strong v* in such a construction provides just the right kind of explanation: it turns the rising predicate (*näyttää*) into the impersonal passive form (*näytettiin*, *näyttiin*). Thus, the LLF structure of these constructions could be as follows:

(254)



Another interesting property of Finnish impersonal passives is the fact that when the sentence is negated, the Patient takes the partitive case. This property is typical of structural objects in all clauses. Thus, negation does not allow the partitive Case in raising construction (255), meaning that the partitive Case is controlled, at least in part, by structural and not thematic properties. Example (256) is a finite declarative negative clause with a partitive object:

(255) Pekka ei näyttänyt kuolleelta
Pekka-NOM not-SG3 looked dead
'Pekka did not appear dead.'

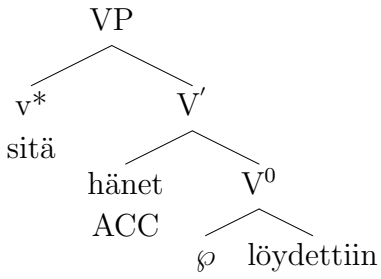
(256) Pekka ei nähnyt Merjaa
Pekka not-SG3 saw Merja-PRT
'Pekka did not see Merja.'

Now, as mentioned above, the patient takes partitive Case in negative impersonal passives:

(257) Pekkaa ei löydetty
Pekka-PRT not-SG3 found-PASS
'Pekka was not found.'

This means that, from a structural point of view, *Pekka* is a direct object in (257), not its subject. If this is so, what is its structural subject? The present theory, which proposes that Finnish impersonal passives are expletive structures with covert expletives, provides an answer: the subject position is occupied by the covert expletive. This explains why the visible argument *Pekka* behaves as if it were an object, in this case in the zero accusative Case ACC^0. Pronouns behave similarly, appearing in the accusative Case (ACC^0), as shown in (258).

(258)



Finnish impersonal passives, like many other constructions, allow the subject to appear either pre- or post-verbally:

(259) talo rakennettiin
house-NOM build-PASS-PAST
'The house was built.'

(260) rakennettiin talo
build-PASS-PAST house-NOM
'The house was built.'

As observed by e.g. Burzio (1986), for example, this property is typical of pro-drop languages more generally. The following example illustrates the shifting of the position of the subject in Italian:

(261) molti studenti arriveranno
many students arrived
'Many students arrived.'

(262) arriveranno molti studenti
arrived many students
'Many students arrived.'

The similarities between pro-drop languages in general support the fact that Finnish impersonal passives are *there* expletives with a covert (dropped) subject. For instance, in English *there* constructions the verb agrees with the patient. This is not so in Finnish, in which verbs do not agree with the patient - a fact that could be used against the above analysis. Yet, as Burzio (1986, §2) pointed out, this difference could be traced to the pro-drop parameter as Italian allows similar expletive constructions without agreement. What is perhaps more relevant, in Italian the verb could optionally agree with the impersonal *si* ('one'). Thus, it might be the case that, in the Finnish impersonal passive, as in Italian, the verb agrees with the covert expletive (or a standard impersonal agreement appears), not with the patient. Even English has similar cases, although only marginally acceptable (*there's many people*).

Another test of this proposal concerns ECM structures. Recall that, in these cases, the subject (Agent) of the embedded clause behaves as if it were the object of the matrix verb:

(263) we saw him die

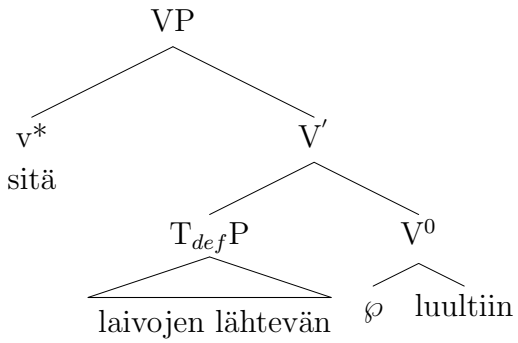
How do Finnish impersonal passives behave when they occur in these constructions? The data indicates that the impersonal passive verb induces the ECM phenomenon, but does not allow the subject of the embedded clause to shift its grammatical properties:

(264) laivojen luultiin lähtevän
ships-GEN thought-PASS leave
'The ships were thought to have left.'

The reason why the subject of the embedded clause takes the genitive (accusative) Case follows from the fact that it originates in the subject position of the embedded sentence and thus has feature vector <a, a> (ACC) instead of <a, v*> (ACC^0). It does not, and cannot, occupy the subject position of the matrix verb because, if these arguments are close to the truth, that position is again occupied by the covert expletive.[3]

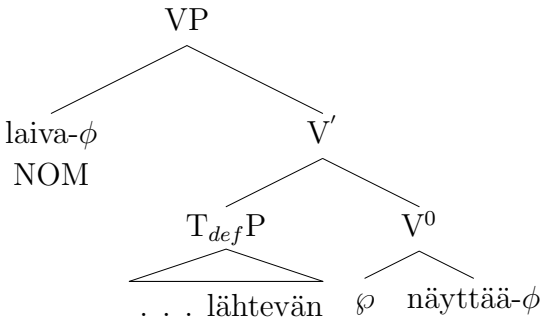
[3]One could argue against the present hypothesis by questioning the idea that the gen-

(265)



If, in contrast, the matrix verb is a rising predicate, the argument occupies the subject position and induces subject-verb agreement to the matrix verb:

(266) laiva näyttää lähtevän
ship-NOM seems-SG3 leave
'The ship seems to be leaving.'



These facts thus support the hypothesis that Finnish impersonal constructions involve a covert expletive subject. The previous example (264), repeated here as (267), is also interesting from the perspective of the Finnish case system.

(267) laivojen luultiin lähtevän
ships-GEN thought-PAST-PAST leave
'The ships were thought to leaving.'

Here, the subject of the embedded sentence appears in the genitive Case which, as argued previously, is the characteristic realization of an accusative

itive argument represents the real ECM phenomenon, and thus the standard structural accusative Case. Vainikka (1989, 1992), for instance, takes the genitive subject to be a Case assigned by the head verb to its [spec, VP] position, taking the complement clause to be a VP (for a conflicting view, see Maling, 1993).

Case in Finnish. This is in line with the hypothesis that (267) is, basically, an ECM construction. Nevertheless, the genitive NP is in the plural and, surprisingly, appears in genitive plural. This is untypical of accusatives in Finnish which, in more typical declarative clauses, do not appear in the genitive plural. Hence I used the more conservative ending GEN rather than ACC.

Finnish contains other types of construction that seem to involve covert expletives which, if this analysis can be maintained, further supports the present proposal. Consider the following pair of sentences:

(268) ihmiset juoksevat kadulla
people-NOM run-PL1 streets
'People run on the streets.'

(269) ihmisiä juoksee kadulla
people-PRT run-SG3 streets
'People run on the streets.'

The former sentence is a typical declarative finite clause, having nominative subject and subject-verb agreement. The latter is not typical, however: the "subject" appears in the partitive Case, whereas the verb is no longer thus inflected. Note that the verb in Finnish impersonal passives does not agree with the patient, much as in the above cases: we could say that it agrees with the covert expletive or with a null argument of some sort. This corroborates the above examples if it is assumed that the latter sentence also involves a covert expletive of some kind, much like the impersonal passive. A proper gloss into English would thus be *there run people on the street*. Furthermore, if the "subject" of these constructions is singular, then it takes the genitive and hence the accusative Case. I conclude that, as with impersonal passives, the nominative in these sentences is actually the direct object, the subject being a covert expletive or a null argument of some sort.

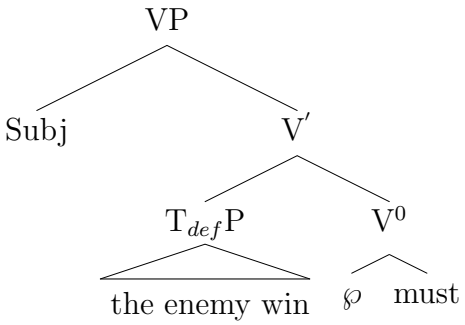
A somewhat more complex, but also quite telling, pair of examples is given in the following:

(270) vihollisen täytyy voittaa
enemy-GEN must win
'The enemy must win.'

(271) vihollinen täytyy voittaa
enemy-NOM must win
'One must conquer the enemy.'

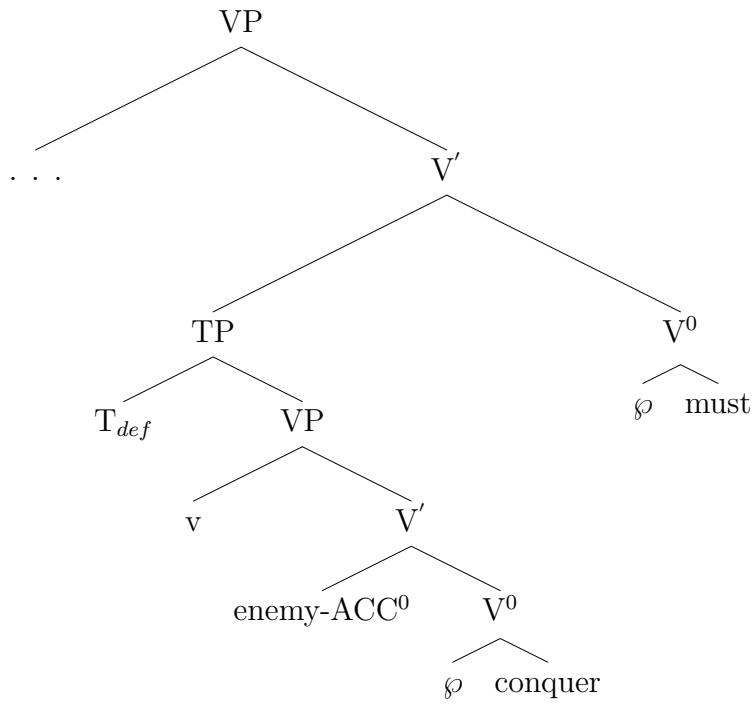
The sentences are identical, apart from the different Case of the word *vihollinen* ('enemy'). The surprising fact is that in sentence (271), the word *vihollinen* ('enemy') is the structural object of the sentence by all tests of objecthood (e.g., partitive Case tests). It is clearly not the object of *täytyy* ('must'),however, amd this sentence could therefore be an ECM construction in which the nominal *vihollinen* ('enemy') has been restructured. Let us assume so: in that case, the enemy is the structural subject of the predicate *win*, whereas the object of must is the whole proposition 'the enemy wins,' and the enemy takes the accusative Case from its structural position in the combination of two propositions:

(272) [must [the enemy-ACC win]] (ECM)



Here I assume that the modal operator *täytyy* ('must') is not located in a T node, but that it occupies a verbal position and takes a clausal complement. If we assume the above analysis for now, what is the subject of *must*? Again, we might speculate that there is a covert null subject, an expletive, of some kind. This would explain the fact that the predicate *must* - which, if this analysis is correct, is semantically a modal sentence operator - does not agree with the *enemy*, but it does still inflect for tense. This analysis is supported by (271). Here, there is clearly a covert subject somewhere, since the sentence means that some unspecified person must conquer the enemy, hence the enemy/*enemy* is not the thematic or structural subject of conquer/*conquer*, but its thematic and structural object: it is the enemy who one must conquer. Where is the unspecified subject in the structure? Clearly, it is the agent of conquer who is supposed to conquer the enemy. Suppose there is, again, a null argument of some kind, presumably a covert version of *there*. This then explains at once why the enemy appears in the nominative Case, since it would be the zero accusative object of *win*:

(273) [must [one/there conquer the enemy-ACC^0]]



Again, if the object of *win* is a pronoun, it appears in the accusative Case, exactly as predicted were it a zero accusative.

I have engaged in a rather somewhat detailed discussion of Finnish impersonal passives in order to show that their curious properties could be explained within the present framework by assuming that they involve covert expletives. This section was not intended as an extensive study of Finnish impersonal passives, however, merely as a review aimed at demonstrating how the present assumptions work in the case of concrete and often compelling data, often making it look less complex. For other and often conflicting analyses, but with a somewhat different theoretical background, see Holmberg & Nikanne (1993), Nelson (1998) and Vainikka (1989, 1999), among others.

According to the EPP principle, a finite clause must have a subject, whereby "subject" means a constituent that agrees with the verb and takes the nominative Case. In the present theory, EPP holds, expect for constructions with quirky case subjects. Finnish impersonal constructions provide an apparent counterexample to this generalization, and this now follows from the above assumptions as well. If it is true that impersonal constructions involve a covert expletive, then the EPP principle can be maintained here as well.

These assumptions concerning expletives as a whole can be further tested by investigating their relations with empty elements and the theory of binding. This is discussed in section 6.5.

Allow me to summarize my observations so far. I have explored the following possibilities of inserting a vacuous argument into an A-position at the LLF level with the following options:

type of vacuous argument	deletion by [51]	Result
v	v	personal passive
v	patient	*it* construction
v*	-	*there* construction

The key properties of the expletives seem to emerge from these simple assumptions, together with the rules of syntactic linking described previously, although of course a much more detailed survey is needed than is possible in a single presentation.[4]

Consider the status of the Case Filter in the GB theory. One problem with the GB theory has been to explain the existence of the Case Filter itself: why do all overt nominals have to take a Case? As such, it appears rather odd and unprincipled, although a powerful requirement. One explanation concentrates on unifying the Case Filter with the θ-theory in terms of the *Visibility Hypothesis*, requiring that in order to be visible for θ-theory at the LF level an argument in the A-position, or an element of its chain in the A-position, must take a Case.

From the LLF perspective and that of the theory of linking, the *Visibility Condition* seems close to the truth. Here, Case is associated with properties of feature vectors that, together with certain exceptions that emerge automatically, encode structurally encoded thematic information at the LLF level. We could say that feature vectors thus represent semantic information, but in a 'defective' way.

However, expletives pose problems for the Visibility Hypothesis. They appear in a position to which the nominative Case is assigned, but they a

[4]If expletives are vacuous arguments with slightly different syntactic properties, then this means that, if I may use more traditional vocabulary, expletives are base-generated inside VPs (Dikken, 1995, Groat, 1999). They are external subjects, albeit without semantic content.

lack θ-role. In the present proposal, the Case Filter is explained in terms of [51], which requires that each argument inside an FP must have a unique Case (unique L-vector). Note that this is very close to the Visibility Hypothesis, since each FP position is an A-position, with a thematic role. There is therefore a close connection between thematic roles and Case (but not one-to-one, given ECM). The same problem arises again here, although in a somewhat different form: the expletive *there* appears as an argument at the lexico-logical form, hence it is predicted to have a thematic role, which it clearly does not have. Furthermore, the explanation requires that non-aspectual predicates (which do not even passivize) could nevertheless have a vacuous *there* with agreement features. Does the dummy expletive v have thematic content or not?

The correct hypothesis seems to be that we must distinguish the existence of an aspectual thematic role from the existence of the corresponding structural position at the LLF level. Thus, in **Erebus arrives three men*, *Erebus* is left without semantic interpretation, it has no role in the event, although it may exist as a formal element at the lexico-logical form. No such expression is ever generated, however, since there is no coherent meaning to be projected. The only expression that makes sense in this position is the semantically dummy expletive *there* (a vacuous argument visible to feature vectors). This is why non-aspectual predicates must allow two or more structural properties, but prevent their aspectual interpretation.

According to my hypothesis, expletives (vacuous arguments) are base-generated inside VPs, although I am forced to admit non-thematic positions inside VPs as well. There is some empirical evidence to support this position: *there* expletives possess strikingly similar properties to those of vacuous arguments of passives that, on the standard account, appear as empty positions inside VPs. I have also shown that many of their essential properties follow of the basis on these assumptions. Furthermore, although expletives in English lack thematic content, Finnish impersonal constructions, arguably instances of null expletives, seem to possess some thematic properties, breaking the absolute connection between expletives and thematic roles. Perhaps VPs do contain non-thematic arguments other than thne vacuous arguments of passives, (Dikken, 1995, Groat, 1999). Furthermore, there is no conceptual problem in assuming that expletives can "raise," in this case out of the VP to adjoin T$'$. Rising, whether in terms of restructuring or Move/Agree, is a potential operation for the expletive.

6.4 Morphology: derivational and inflectional

The morphological component has so far played an important albeit implicit role. Several major and also to a certain extent controversial assumptions concerning the operation of this component have been made, including the fact that it is the task of Morphology to convert *red* into *redden* when the former lexical element, representing 'redness,' appears in a suitable propositional frame at the LLF level. Some features that are taken to be part of syntax are thus moved to Morphology. This was one principal reason for rejecting the decompositional hypothesis, which assumes that the lexical element *redden* contains the semantic feature CAUSE. As mentioned at the beginning of this study, one way of developing the atomistic hypothesis is to move some of the explanatory burden into Morphology.

On balance, the present proposal maintains many features of the more traditional minimalist assumptions concerning the morphological component, among them the assumption that morphology operates after syntax by converting morphosyntactic features into something interpretable at the PF level.

Some thirty years ago, the generative semanticist advocated a transformational theory of the lexicon, the main idea of which was to capture the (derivational) regularities, both morphological and semantic, in syntactic terms. More specifically, new lexical items, when they were regularily related to their root forms, were produced by transformational means. Causatives were one paradigmatic example of this explanatory strategy. The key point was that the formation of regularly connected lexical elements was syntactic (and semantic). vP shell analysis has, to some extent, resurrected these ideas. Much the same is true of the present hypothesis, although the details differ.

The Lexicalist Hypothesis (see Chomsky, 1970, Jackendoff, 1975), in contrast, holds that these lexical regularties, or most of them, are not to be explained syntactically (that is, by using the syntactic engines available at the time), but should be explained 'at the lexical level.' Jackendoff, for instance, proposed that they could be captured by lexical redundancy rules, which were supposed to relate otherwise full lexical entries to each other, and in doing so also to predict what would be possible words in natural language. These lexical entries were inserted into the syntactic tree as such.

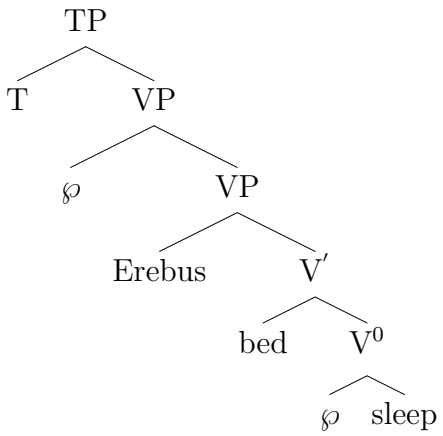
I have argued that there are two sources of irregularity at the lexical level:

first, the morphological component is relatively autonomous and is situated post-syntactically. It uses whatever information is provided as its input, and its own available resources, and produces an output when such exists. This, of course, is not a morphological theory: it is only a statement of the basic architecture following the proposal given in Halle & Keyser (1993), for example. The second source of irregularity is C-I, which can give, in addition to a truth-condition literal interpretation, various contextual features to any of the lexical elements that appear at the LLF interface.

Within this framework, "lexical redundancy rules" do not constitute a single natural class, they dissolve into several components - hence the name "distributed morphology" would be appropriate here as well. One part is syntactic and is constituted of the local feature vectors. The morphological component then uses this information and produces an output, following whatever principles it contains such that they can be realized phonologically. In some cases, no form is generated. More concretely, many morphological gaps are argued to result from these principles (or properties) of the morphological component, and not, specifically, from the putative lexical semantic features. This assumption is not explanatory, and what is worse, it is empirically empty, but I suspect that a great deal of such restrictions are really empty in that they are not explainable by applying general principles in the first place. The rest should be explained by relying upon autonomous morphology, perhaps conditioned by obscure pragmatic facts. On the other hand, the proposal also allows for idiomatic semantic information, especially concerning the proper use of words, since the operation of C-I is also partly independent of the mechanisms of C_{HL}. There is thus no one-to-one mapping between morphology and meaning: such a transparent relation occurs on the more abstract level (LLF), being abstracted from both complete semantic and complete morphological / phonological interpretation.

In this section I will make these morphological assumptions more explicit, and compare them to the more standard assumptions of minimalist theory, exploring the consequences and detecting potential problems. To begin with, consider (274):

(274)



This LLF produces an ungrammatical sentence due to the fact that the main predicate *sleep* is "intransitive":

(275) a. * Erebus sleeps the bed
b. * Erebus sleepize the bed
c. * Erebus sleepize Charon to the bed

According to the present hypothesis, there is a gap in the morphological component of English - in particular, there are no gaps in semantics (LLF) or syntax (d-structure), and no variation in the principles of the UG. Sentence (275a) is semantically understandable, on a par with *Erebus opens the bottle*: there is nothing logically or semantically wrong in thinking that sleeping would take the bed as its direct object, since there are surely many ways in which sleeping 'affects' the bed, as opening a bottle affects the bottle.

To cite another example, the verb *to bleed* is ungrammatical in its transitive form in modern English (**Charon bled Erebus*, but also *the government bleeds the taxpayer*), but it was accepted in early English since 'causing to bleed' was a standard medical practice in those days (for a similar and very illuminating example, see Soto, §3.4). To borrow some terminology adopted by Chomsky, the fact that *sleep* does not take the direct object reflects the way in which language and its lexical items open 'perspectives' on the world (cf. Chomsky, 1993a, 2000c). In Finnish, (275) would come out as (276).

(276) Erebus nukutti sängyn
Erebus-NOM sleep-CAUS-SG3 bed-ACC
'Erebus made the bed sleep.'

This is a grammatical sentence, categorized as "morphological causative" (§4.2). Thus, the morphological component is likely to follow, at least to a considerable extent, pragmatic principles. I doubt if there are any metaphysical, logical or semantic (truth-conditional) laws that could explain the transitive and intransitive uses of predicates such as *to bleed*, and it seems that when such principles are sought, they turn out to be increasingly pragmatic in their nature, and hence part of the C-I system.

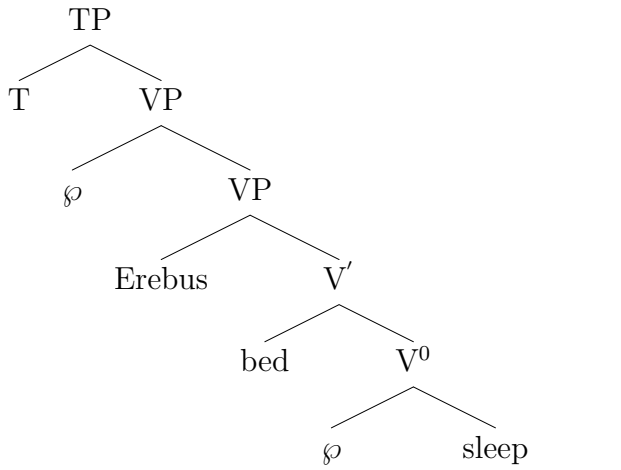
One especially illuminating example comes from Matses causative nominals (Fleck, 2001). Matses is a very productive language in terms of its causative morphology, and much like Finnish, it allows causatives to be nominalized so that the nominals refer to the causer of the event. These nominals can further be used as predicated when accompanied by a copula (a realization of +V, I will discuss this matter presently). One morphological suffix that codes these causative nominals is *-anmës*. This particular ending is not very productive, however, and as summarized by Fleck, "What all nominalizations with *-anmës* seem to have in common seems to only be describable in terms of a rather complex set of variables, with a definition of the specific function of *-anmës* reading something like: 'the referent of the nominalization is one that non-volitionally, indirectly and often mysteriously causes helpless victims to enter an undesirable, enduring state" (p. 406). Interestingly, he also argues that, if we want to be able to use it correctly in explaining Matses causative morphology, this description must be understood against the cultural background of native speaker/hearers and not against that of Western civilization. Thus it makes sense to capture such restrictions in terms of pragmatics, taking effect in Morphology.

There is also independent evidence to support the claim that notions such as "intransitive" or "transitive" belong to Morphology, although here the exact meanings of these terms become blurred due to the fact that predicates or lexical elements at the LLF level are not identified on a morphological basis. First, I have assumed, in accordance with the minimalist framework, that variation among languages is to be explained by relying upon the morphological component. Since there is much variation in the availability of causative forms, it is to be expected that the differences between languages are not attributable to the narrow syntax. Secondly, as remarked earlier, when there are restrictions in the morphological component, children tend to overgeneralize the rules of the UG and overcome such restrictions, learning them only later. The present proposal also explains the well-known fact that

most three-place verbs are causatives: they emerge automatically in the C-I when aspectual arguments are added.

This strategy has consequences for the theory of subcategorization. Consider again the predicate *sleep*. If the predicate appears with just one argument, the rules generate *Erebus sleeps*. If there are two arguments, it depends upon the Morphology whether this component has a form for the predicate *sleep* with two arguments. If it does not have the corresponding form, as in English, nothing is generated. In some cases, Morphology does provide the correct form, as in the case of *mechanics* - *mechanize*, *large* - *enlarge*, *sink* - *sink*. This option is not available in English for *sleep*. In Finnish, in contrast, this form is generated, and a grammatical sentence occurs. The difference between these two languages is thus explained in terms of Morphology, keeping the narrow syntax intact, as is assumed in the minimalist framework presupposed in this study. Sentence (277) illustrates this difference:

(277)



This is not to say, however, that all derivational morphology can be explained along these lines: this claim would require a separate study, and whatever the end result would be is currently very unclear. Nevertheless, this strategy does provide some hope for keeping within the atomistic lexicon by moving some of the explanatory burden of morphosyntax from narrow syntax and semantics into Morphology. This is a good strategy on independent grounds as well, I believe, because of the uncertainty surrounding the

identity and diagnostic properties of most putative semantic features (see Fodor, 1998a, pp. 49-53, 57-64).

This explanation does not presuppose that Morphology produces a so-called 'morphological causative.' In some cases the morphological form of the word is not changed at all:

(278) a. the boat sank
b. Erebus sank the boat

These causatives are often called "labile." Questions of this type also belong to the theory of Morphology, given the proposal put forward in this work. There is no presupposition here, either, that an increase in valency always results in a causative, whether morphological or lexical. Some languages have specific forms, such as applicatives, which are associated with an increase in the valency of a verb but restrict the individuation of any of the new arguments to applicatives or instruments, for example - a hallmark of the specific d-marking of such arguments. I will not attempt to review and discuss these operations here.

Consider, again, the status of the so-called Strict Lexicalist Hypothesis, part of the more standard minimalist framework. According to this hypothesis, morphological and formal features are part of a narrow syntax, since they are brought into the derivational procedure at Numeration. C_{HL} is then obligated to erase them, since otherwise the resulting syntactic object would not satisfy the condition of Full Interpretation. In the present theory, the only formal features that are part of the narrow syntax are lexical elements and their relational properties, feature vectors. The difference is that relational properties at the LLF level are semantically interpretable: hence the problem of 'uninterpretable formal features' does not arise, and we could claim to be one step nearer the strong minimalist hypothesis. The morphological component implements these features by altering the word forms, hence it is quite correct to say that they belong to the separate study of morphosyntax, not to narrow syntax: there is a fairly sharp difference. From a more functional perspective, the purpose of these morphosyntactic features is to code meaning and meaning-related properties of thought into expressions, and thus enhance communication, so that the principle of Full Identification is satisfied:

> (Principle of Full Identification, PFI.) Every syntactic formative of a sentence must have a corresponding element in the seman-

> tic representation. Every formative of a semantic representation must be identified by a morphosyntactic element in the sentence with which that representation is associated. (Bouchard, 1995, pp. 93-4.)

The present theory approximates this principle quite closely, and is clearly construed in the same spirit.

My proposal could also be fruitfully compared to the older minimalist theory, as proposed in Chomsky (1995, §2). According to the first version of this minimalist theory, "first version" referring to the above-mentioned paper, inflectional and some of the derivational morphology (agreement, structural Case) are explained by moving elements to appropriate [Spec, Agr] positions which, because the V and T heads could incorporate them, are called Agr complexes. According to this theory the Agr elements (both AgrO and AgrS) are stacked over the VP, which contains the thematic argument structures of the head V. Roughly, the clause is divided into one part recording the thematic argument structures (VP) and another part recording the formal features.

In his subsequent work, Chomsky (1996, pp. 194-197) argued that the stack of formal heads was redundant, the formal features being assumed to originate from the lexical elements themselves. Thus, this stack collapses and becomes part of the lexicon (thoroughly so in Chomsky, 1995, §4). What was previously explained in terms of the extra VP heads was now explained by the double-Spec -hypothesis, for instance, and the idea that lexical heads must check and erase their formal features in the process of the derivation of linguistic expressions. Both solutions are suspicious in terms of the strong minimalist hypothesis, however, for they involve formal features that are non-optimal.

In my work, I have returned somewhat to the framework of the earlier theory, moving formal features out of the lexicon back to the structure, but this time the structure that takes the burden of generating formal features is semantically interpretable (Full Interpretation). The relevant structure is encoded in terms of feature vectors, which mirrors the LLF structure.

Let us now widen the perspective and consider the phenomenon of agreement, returning to causatives later on. Since the appearance of *Syntactic Structures*, a transformational vehicle was used to explain agreement (both inflectional and derivational). In the first version of the theory, agreement

was implemented by its own inflectional node, whereas in more recent versions, I is split into tense and AGR, which take on their own heads and project. Following this trend, AGR was split into AgrS and AgrO. As explained earlier, the minimalist theory continued this tradition by proposing that agreement is implemented by Agree, which is a more static local relation between two syntactic elements.

I have suggested and argued that formal features supervene upon the relational properties of the constituents at the LLF level, thus they supervene upon their feature vectors. So far, this has involved formal features such as categorial features and Case. I now conjecture that this is how inflectional morphology, and a least a substantial part of derivational morphology, ought to be explained. This seems to be the null hypothesis. If this is correct, then Agree is a relation between a lexical item and elements appearing in its feature vector. These elements do not rise, and the elements in the feature vectors do not fall (affix hopping), but something like a minimalist 'feature match,' or more appropriately 'feature percolation,' appears. There is no head movement for the purposes of inflection, but inflection is driven by morphophonological requirements (e.g., Bobaljik, 2002, among others).

According to these assumptions, Morphology is an independent component: its input is a lexical element (index) of its feature vector, for which it targets a representation implementable at the PF level. For example, English plurals, third-person-singular verbs and possessives are realized by the same morphological or morphophonological element. This fact is not related in any way to narrow syntax: on the contrary, Morphology uses whatever resources it can (Aronoff, 1994).

Consider the feature vector of the predicate, containing the symbol 'a.' What is it? It cannot be the whole constituent, since that may be of arbitrary complexity imposing too much parsing power on Morphology. Let us follow the hypothesis presented above and call them "agreement features," including ϕ-features such as [+person][+gender][+number] and others, which are derived from the elements themselves and thus are "interpretable" by them. Then we could take'a' to be shorthand for agreement rather than "argument," replacing AgrS, AgrO and even AgrIO. I would further stipulate that some agreement features are [+strong], meaning that they must be overt at the PF level. Tense and aspect, residing in T, are typically strong. In that case, the feature vector of an element also contains among its properties related to Case and category also information concerning agreement.

Morphology is thus sensitive to different formal features of 'a,' that is, ϕ-features [person], [gender] or [number]. The asymmetry in interpretability of these features arises automatically, since the features themselves originate in the A-positions where they are interpretable, but they appear in the feature vector of the predicate that bears no content relation to them.

The whole feature vector is clearly too wide a notion to capture a local relation such as agreement. This was assumed in the case of categorial and Case-features, which supervene upon L-vectors. As far as agreement is concerned, let us define the notion of "ψ set" and use it as a constraint on (inflectional) Morphology:

(279) A ψ set of a lexical element at the LLF level consists of all symbols inside two full projections: morphology is sensitive to the ψ set of a lexical element.

According to [279], no language should

(i) have an inflectional dependency between constituents whose distance is blocked by the juncture of two full projections; tense/aspect can only inflect its own predicate; tense/aspect never inflects any of its arguments; inflections never cross complementizers; there is no inflection from tense/aspect of the infinite complement;

(ii) no inflectional relation from predicate to its arguments can exist, only vice versa;

(iii) the subject can inflect objects and predicates, but objects cannot inflect subjects;

(iv) inflecting elements must c-command inflected elements in their lexico-logical form.

To the best of my knowledge, these are correct. A strong feature must appear in the ψ set of an element that can take the required inflections. If no such element is found, then we may assume that a suitable dummy grammatical participle is created as a "last resort" (e.g. Bouchard, 1995, §5.3.1). Consider a sentence such as *this is a book*. Since [tense] is strong and book does not inflect for tense (as a morphological property), a dummy grammatical participle, a copula, is created. However, *book* cannot inflect

for +V. This, again, requires a dummy participle that can realize the feature +V. Thus, consider a representation in which '[this [℘ book]]' appears as an embedded proposition. In that case, there is no need to implement matrix T, since T is not in the ψ set of the predicate *book*: note the two FP labels between *the book* and T. Nevertheless, *book* must still realize the feature +V, since its feature vector is <℘, a>. A copula is thus created, as expected, without tense (a), but not if the predicate can realize feature +V (b):

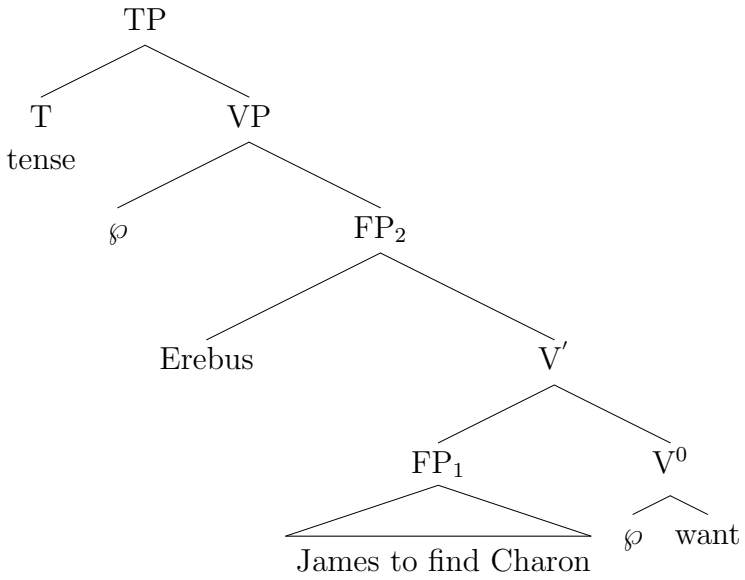
(280) a. I want this to be a book
b. I want this to run

Consider the following sentence and its LLF representation:

(281) Erebus wants [James to find Charon]

The predicate *find* of the embedded proposition is in a predicate position, yet [tense] is not in its ψ set due to the two intervening FP projections:

(282)



So far, I have assumed (implicitly) that this situation creates an "infinite verb." According to this explanation, infinite verbs are those surface forms of tense -inflecting predicates (verbs) that lack tense. We thus derive the following:

(283) a. Erebus wants James to find Charon

b. James wants fish raw

The reason why *raw* and *to find* do not inflect for tense follows from [279] and the associated notion of ψ set: tense is separated from them by two junctures of full projection. In the case of *to find*, the predicate itself could inflect for tense, while in the case of *raw* it could not. When these predicates are moved to a position in which they cannot escape inflection due to [279], the difference becomes visible, as shown below:

(284) a. Erebus loves Charon
b. the fish is raw

In (b), the copula is created as a "last resort," since adjectives in English do not inflect for tense.

Consider next the category [+aspect], resident as a feature at T. There are agreement features as [+progressive], among other possibilities. This feature turns a predicate in English into its gerund form. However, the gerund does not inflect for tense, person or number. This, again, creates contradictory requirements so that a dummy grammatical participle, a copula, appears, and this inflects for tense, number and person, as follows:

(285) Charon is running
Charon-NOM is-PRES-SG3 running-PROG

Suppose that the predicate does not inflect for [+progressive]. This is characteristic of adjectives (and of infinite verbs). Then I predict that the copula ought to appear and take these inflections - hence [be+ing nasty]. *Being nasty* does not inflect for tense, person or number, and neither does the gerund or the adjective (in English) if there is tense, the conditions force a new grammatical participle, a copula, to take these inflections. The result is shown in (286).

(286) Erebus is being nasty
Erebus-NOM is-PRES-SG3 being-PROG nasty

If this construction appears in the A-position in its lexico-logical form, then according to [279], there is no inflection of tense or aspect. The leftmost copula should be missing: *Erebus's being nasty bothers me*, **Erebus's is being nasty bothers me*. The generalization is that if the verb form does not inflect for T in a LLF with T, a functional word is needed:

(287) a. * Erebus own a house
b. * Erebus owning a house

Now suppose that tense = [+perfect]. This is associated with two morphological effects: first, the VP inflects for [+perfect] when possible (*ran*, *been running*), and second, there is a separate and independent perfect auxiliary, have, potentially inflecting for number and person. Let us assume that this auxiliary is a dummy grammatical participle, created at some point after the lexico-logical form. The following surface forms can be derived:

(288) a. Charon has-PERF-SG3 run-PREF
b. Charon has-PREF-SG3 been-PERF running-PROG
c. Charon might-SG3 have-PERF been-PERF running-PROG
d. James imagined [Charon to have-PERF been-PERF running-PROG]

In (d), the embedded infinite have does not inflect for person or number since the subject has been restructured (*Charon seems to have been running, they seem to have been running*).

I have been assuming here that strong features are inflected, or "hopped," to borrow the terminology of *Syntactic Structures*, in a specific order: first progressive, then perfective, then tense, number, person and gender. It is generally known that the order matters. Tense and various forms of aspect are independent of each other, so that I take them to consist of independent features or operators, stacked above the proposition. For instance, bare infinitivals are generally assumed to be tenseless (a), yet, according to one hypothesis (Felser, 1999, §2.6), they do have progressive forms (b):

(289) a. Erebus saw Charon leave
b. Erebus saw Charon leaving

The fact that bare infinitivals do not inflect for the perfect or tense provides further support for the ordering of the V features (tense/aspect), so that we can readily classify them as clausal complements lacking [+perfect] and [+tense], headed by an aspectual element containing [+progressive](Felser, 1999, §3.4), and presumably residing in the defective T (ibid., §3.6). Note that a radically different LLF structure emerges if by d-marking the complement clause is given a non-aspectual reading:

(290) a. Erebus saw Charon leaving

b. Erebus saw Charon's leaving

Furthermore, it is assumed here that all strong features must be inflected. This, together with the assumption that all affixes are strong inflected features, entails that there cannot exist 'stranded affixes'.

The present proposal maintains the original minimalist hypothesis that agreement is explained by implementing a local, static relation Agree, in this case between an element at the LLF level and the properties of its feature vector. What is different in my proposal, however, is that auxiliaries (dummy participles) are not part of the narrow syntax, but emerge as late as at Morphology. Hence they are not syntactic heads, nor do they have projections. In fact, they do not exist at the LLF level but emerge for the purpose of inflection. I thus predict that they are closely attached to their host element (i.e., to the element the inflectional features of which they implement), or to another position in a sentence readable at this stage of derivation (such as at the end of the sentence), forming a highly integrated or 'fused whole' with their hosts.

For instance, I have suggested that the argument structures of Romance *faire* causatives behave as if they correspond to a single predicate at the lexico-logical form, given the fact that there are some well-known complications concerning the distribution of certain grammatical participles: for example, the verbal complex *faire* + V seems to consist of two fused verbs. However, this can be explained if we assume that *faire* is a particle created as a 'last resort,' comparable to auxiliaries.

Grammatical particles like copulae are created when a constituent itself cannot inflect for a strong feature. In *faire* + V causatives, it is the causative particle *faire* which, like other grammatical extensions, inflects for tense, number and gender. The predicate itself appears in an infinite form, which is some kind of verbal form not inflected for tense. Thus we may speculate whether, for some reason, the predicate inflects for a strong feature, say increased adicity at the lexico-logical form, and the morphological component assumes a grammatical participle, in this case *faire*, in order to realise the required strong inflections. It is this participle that inflects for tense as well. This hypothesis, with its relevant visible structure, is illustrated below:

(291) a. on a [(fait sortir) Jean de sa chamber]
b. elle a [fait (faire sauter) le point à son fils]

The verb has undergone an "extension" in terms of the grammatical participle *faire* that ought to take all inflections: it is then not present at the LLF level. Indeed, this is just the case. In example (b), this process has been applied twice: the sentence means 'She had her son make the bridge blow up.' If this hypothesis is correct, then the *faire* participle is to be categorized as an 'auxiliary verb,' comparable to grammatical participles such as perfective auxiliary *have* (see Aissen, 1974b, 1979, §6.2). Note, however, that the *faire* participle is not a syntactic head, nor does it have its own projection at the LLF level - indeed, there is no such constituent at LLF. It is expected that the infinitive verb and the *faire* participle form a highly integrated or 'fused constituent' in syntactic terms. This is indeed what happens in the case of Romance *faire* causatives (§4.4). How integrated the hosts and their auxiliaries are depends on Morphology: they are not totally integrated since some elements can intervene.

In order to test this hypothesis further, we would need a more accurate theory stipulating which linguistic phenomena are to be explained by relying upon LLF properties, and which emerge as a consequence of the syntactic derivation/interpretation of the expressions. There are, nevertheless, several phenomena that certainly belong to the LLF domain and which can be used as a diagnostic test for the present hypothesis.

One such phenomenon is the formation of interrogatives, which involves the replacing of an element at the A-position or predicate position with a wh-element or a variable. The notion that dummy auxiliaries are not represented at the LLF level thus gains more support from the fact that they cannot be replaced by wh-elements, or appear as answers to interrogatives. Other evidence, more direct perhaps, concerns the fact that LLF is motivated semantically, whereas most dummy auxiliaries are what their name implies, that is, semantically 'dummy.' When they do appear to carry some meaning, it does not seem to be intrinsic to these elementt's themselves, but depends on some other constituent in the LLF representation, such as aspect or tense. Thirdly, on the descriptive level, auxiliaries really do seem to be motivated by the process of inflection, since they gather around the predicate, not around the arguments. Moreover, they are nearly always homonyms of verbs (e.g., *be*, *have*), the very lexical elements that can inflect for tense and aspect.

The order of morphological affixes tends to mirror the syntactic dominance hierarchy, in this case encoded in terms of the feature vectors. In this spirit, we could incorporate Baker's Mirror Principle into the present

theory without invoking the notion of rising and adjunction. According to Baker's theory, hitherto adapted to many versions of the minimalist theory, the difference is explained by relying on the properties of the transformational process, where the head verb (X^0) rises to incorporate the functional heads, picking up or checking/deleting the morphological affixes along the way. The strongest form of such a theory would stipultae that morphology is actually a strict mirror image of the syntactic hierarchy. This is what Baker assumes. The guiding idea is the Mirror Principle:

(292) (Mirror Principle.) Morphological derivations must directly reflect syntactic derivations.(Baker, 1985, 1988)

The Mirror Principle imposes a strict order on morphological inflection (/derivation). In the present case, morphological affixes and their order are linked directly with the ordering of agreement features in the ψ set of the constituents: the order in the ψ-set happens to be a perfect mirror of the syntactic dominance hierarchy. In other words, we could assume a strict mirroring principle at the level of linking as well:

(293) (Mirror Principle*.) Morphological processes must directly reflect the order of agreement features in the ψ sets (i.e., hierarchical structure at the lexico-logical level).

It is also true that Baker's Mirror Principle does not appear to hold in its strictest form. Nothing forces strict hierarchical mirroring in the order of inflectional affixes, although this hierarchical information is still present in the feature vectors and, as we have seen, means that order is crucial.

Baker's theory of syntactic head incorporation is motivated by a wealth of data concerning Noun Incorporation, in which part of the phonological content of the noun (an argument) is attached to its verb (predicate), leaving behind either an empty position, some phonological material, or a copy (Chomsky, 1995, p. 202-205). It appears that any of the predicate's arguments can be incorporated in this way, although considerable language-specific variation exists. Agglutinative languages use this technique by default. Although I will not discuss this phenomenon here in detail, it does not present an obstacle against excluding the transformational explanation of incorporation, nor does it present a forceful case for transformational analysis. In the present case the required relations (Agree) are already there, with much of the spirit in place as well, namely, that this may indicate that being sensitive only to

the ϕ-features of arguments is just a morphological option, not a requirement - some languages use no features at all (Chinese) and some use richer material (Agglutinative). This is in agreement with the minimalist thesis that variation among languages is explained by relying upon Morphology.

I will now turn to how these assumptions concerning Morphology work in connection with causatives, taking some aspects of Tarascan (a Mexican Indian language) causative morphology as a detailed case under study (all of the examples and analyses are from Maldonado & Nava, 2001, and all of the page numbers refer to that paper). I chose this language because its causative morphology is particularly rich, resembling that of Finnish, thus given the present assumptions, the richer and more productive the morphology, the more transparent the evidence it might bring to bear upon narrow syntax.

Tarascan has, first of all, several basic causative morphemes (I will omit one here). The most basic one is -*ku*, which introduces an agentive participant having "direct contact" with the patient. Hence it turns intransitives and statives into transitives and actives. According to the present theory, -*ku* encodes an increase in valency when the process is morphologically productive. Its resources are exploited when it is used once, following which other suffixes take over (more on this below). Another causative morpheme is -*ka*, which behaves, in this context, like -*ku*, except that it involves, "in the vast majority of cases," a "higher degree of causation entering thus the domain of indirect causation," such that "the causer's initiation and the causee's activity and volition are more transparent" (p. 167). From a syntactic point of view, -*ku* resembles -*ka*; thus it, too, encodes the increase in valency. Yet we need some input to Morphology in order to decide which of the endings is actually used. One criterion would be the lexical element itself, requiring -*ra* in the case of some lexical elements, -*ka* in the case of others, the distinction being controlled by some pragmatic condition or other. In this case, this does not suffice, since the same verb might take either ending. On the other hand, both suffixes might simply be ambiguous between the two typical readings, as indeed is often the case (p. 168). In that case, no additional input to Morphology is needed, both readings being available. However, it seems from the analysis given in Maldonado and Nava that, although the ending -*ta* is ambiguous between more direct and more indirect readings, -*ku* is not. Thus, the locus of the difference could be in the θ-theory and its overt syntactic realization, which is consistent with the pragmatic differences cited above: the role of the Agent and the Patient changes, in a typical case, in

agreement with the change in suffix.

When *-ra* is applied to a transitive verb, an indirect causative construction emerges (pp. 168-169). A transitive verb already has two arguments, and when the third is introduced, a genuine indirect causative results, according to the present theory. Much the same is true of ditransitive verbs (p. 169). Thus, *-ra* encodes the increase of valency up to four arguments. The productivity of Tarascan causatives does not end here, however. When the resources provided by *-ra* or *-ku* are exhausted, the morphological component uses the new specific ending *-tara*, again as a means of encoding increased valency. From a theoretical point of view, this suffix is completely productive, allowing for double suffix forms such as *-tara tara*, although of course such forms are rarely used.

Several comments on this scheme are in order. First, although morphology encodes valency, there is no one-to-one mapping from the former to the latter, since the addition of specific morphemes, and hence the number and nature of these morphemes, depend on the level at which lexical causatives, or lexical transitives, occur. What is expressed by the morphological causative might well be lexicalized, and vice versa (e.g., *let die - kill*). The level of lexicalization thus becomes an important notion in Morphology, a level from which productive morphology begins to expand the resources of language in coding valency. Thus, it makes a lot of sense in the present case to focus on the pragmatic conditions involved in the process of lexicalization, setting up the "lexical level" with respect to the valency of predicates.

Secondly, as Tarascan causative morphology is fairly rich, I take it to represent a near "pure case" of the implementation of valency increase at the LLF level. Thus, in other languages in which morphological resources are more limited, these restrictions are to be coded into the morphological component and whatever pragmatic or sui generis morphological or morphophonological principles might be in operation. The reason for arriving at this hypothesis is that I have followed the ideology of minimalist theory, which attempts to locate variation among languages into the morphological component rather than the narrow syntax. Such properties are apparently easier to learn, although this does not mean that the morphological component does not operate according to some universal principles and primitives, such as the putative rule [279].

To summarize, I have assumed that the morphological resources of a language, or languages, are highly constrained, and that much of the explanation

constructed in terms of lexical semantic features could, and perhaps should, be explained by relying upon these constraints. The constraints, in turn, might reflect more pragmatic principles and properties, hence knowledge of the world. Agreement was explained by reducing the relation of Agree to relations between a constituent and elements in its feature vector.

6.5 Types of empty categories

I have encountered an argument purporting to show that certain binding theoretical considerations favor the biclausal analysis of causatives. In some cases, the causee in a causative construction was demonstrated to have 'subject properties.' Although I have argued that these subject properties could be due to d-marking, it was left open as how to explain the curious binding properties in connection with such structures. This requires a sketch of how the theory of empty elements can be reformulated in terms of LLF. Such a redefinition is provided in this section, together with a discussion of core cases of data. However, this is not intended by any means to provide a complete study of empty elements: it demonstrates how the basic data can be explained by relying upon LLF properties and rules of linking. Moreover, I will not go into great detail about the formulation of the ECP principle in this context.

According to standard GB theory (Chomsky, 1981, 1982), NPs are of two features, [+pronominal] and [+anaphor]. Their distribution was regulated by the following binding conditions:

A. An anaphor is bound in a local domain
B. A pronominal is free in a local domain
C. An r-expression (-anaphor -pronominal) is free

An element α could bind element β just in the case in which α is in an A-position, α c-commands β and α and β are coindexed. The notion of "local domain" appearing in the binding conditions A-C can be defined in terms of the governing category, as follows: α is a governing category for β if and only if α is the minimal category (the smallest NP or IP) containing β, α is the governor of β, and the SUBJECT is accessible to β. "SUBJECT" refers to AGR, and it is "accessible" if their co-indexation (reference borrowing) does not violate any grammatical principles. These constitute jointly what

Chomsky (1986a) defined as the "complete functional complex." Suppose that there is a fourth class of NPs that are both pronominal and anaphoric. This creates contradictory requirements, since such an element satisfies both binding condition A and binding condition B, both being free and bound in terms of governing category. However, such an element can appear in positions in which it is not governed, and this characterisation seems to apply to the empty pronominal PRO that, then, seems both anaphoric and pronominal. Thus we can actually deduce the distribution of PRO.

Let us look these properties from the perspective of the present proposal. According to linking rule [51], if two constituents have identical indices, then only the other can be interpreted syntactically. Let us assume that this is the c-commanding element. The surviving c-commanding index is called an "antecedent" of the deleted one. In essence, this means that I will try to resurrect the old idea of *Equi* transformation, which deletes nominal elements under identity, thus all its problems are inherited here as well. Neverthelss, I think it is, in part, the correct idea. The deleted category is called an empty category (EC) or a gap. Then the following relations hold between the antecedents and the corresponding gaps:

> (i) a gap must have an antecedent at the lexico-logical form, otherwise EC is not licensed at all;
>
> (ii) the antecedent must c-command the gap at the lexico-logical form.

This seems descriptively correct, so that the basic properties of empty elements can be deduced from [51]. If this proposal is close to the truth, then, as Zwart put it, "Anaphoricity is not a lexical property of certain expressions, but a feature that arises in a certain syntactic context," so that "anaphoricity is a property acquired in the course of derivation, rather than a lexical feature which is present from the outset" (Zwart, 2002, p. 274). My proposal differs from Zwart's, however, in that I do not assume that the anaphor must be merged with its associate, and agrees with the more standard treatment in that it resembles the copy/deletion theory of traces.

What happens when a synonymous element is deleted by this operation? It seems that at least three mechanisms are in operation, depending upon how close the antecedent is with respect to the empty category (binding conditions A-C above). In other words, omitting traces, sometimes EC is

PRO, sometimes a reflexive anaphor, and sometimes a pronoun. In other words

(294) (1) The antecedent of a reflexive must be found inside the same full projection (the same "complete functional complex" in terms of binding conditions A-B), (2) the antecedent of PRO must not be found from the same full projection, not beyond two full projections, and it must be dominated by a full projection (a node labelled FP), (3) a pronoun otherwise.

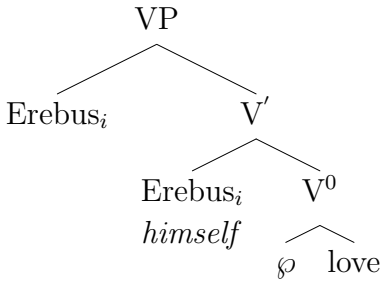
Reflexives and pronouns are "pure ϕ-features," meaning that the deletion leaves the Agr labels and the ϕ-features intact. At this point, I regard these conditions as merely stipulations without explanation, and return to the matter at the end of this section. I will first show how these rules work on the descriptive level, beginning with the distribution of reflexives.

Consider (295).

(295) *Erebus$_i$ loves Erebus$_i$

Here we should apply [51]; given that [294](1) is satisfied, *Erebus* must be replaced with a reflexive, *Erebus loves himself* (reflexives are affixed to the verb in some languages, Lidz, 1996). This is shown in (296).

(296)



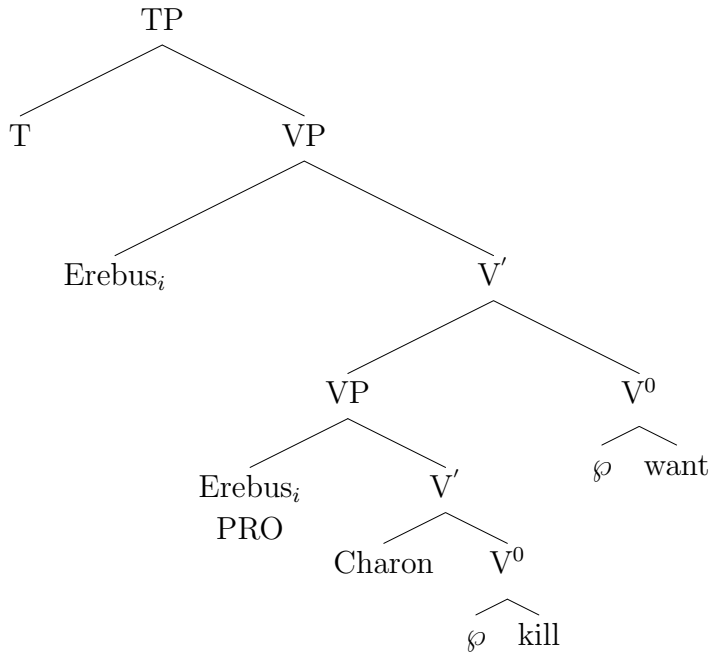
The LLF contains two instances of *Erebus*, whereas the syntactic linking converts the lower *Erebus* into a reflexive. This is an extension of the idea that syntactic linking is not "complete": elements with identical formal properties (*Erebus* - *Erebus* in (295)) or identical L-vectors cannot be linked syntactically.

On the other hand, **himself$_i$ loves Erebus$_i$* is ungrammatical due to (ii). For the same reasons, *Erebus believes Charon's description of herself* is grammatical, but **Erebus believes Charon's description of himself* is not. The

latter can, however, be generated as *Erebus$_i$ believes Charon's description of him$_i$*. On the other hand, **Erebus believes that himself is intelligent* violates condition [294](1).

Now consider **Erebus$_i$ wants Erebus$_i$ to kill Charon*. This again violates [51], but this time [294](2) is satisfied so that *Erebus* is replaced by a gap, resulting in *Erebus wants to kill Charon*, as shown in (297).

(297)



For the same reason, **Erebus wants him to kill Charon* is ungrammatical. *PRO loves Charon* is ungrammatical, since it violates (ii): there is no antecedent for PRO, but why can it not appear in the place of the object, **Erebus$_i$ kills PRO$_i$*? The reason, in this case, is that there is no boundary of full projection between PRO and its antecedent, thus condition [294](2) is not satisfied. PRO must also be dominated by FP.

Consider, then, the subject of a finite clause. Here, too, PRO cannot appear, as is shown by the ungrammaticality of **Erebus thinks that loves Charon*. There are two full projections between the PRO and its antecedent, hence [294](2) is again not satisfied. Rather, [294](3) is satisfied so that we predict the appearance of the pronominal, *Erebus thinks that he loves Charon*. Finally, to take a slightly more complex case, consider *Erebus arrived PRO exhausted*. Here again, [294](2) is satisfied, (1) and (3) are not, hence the

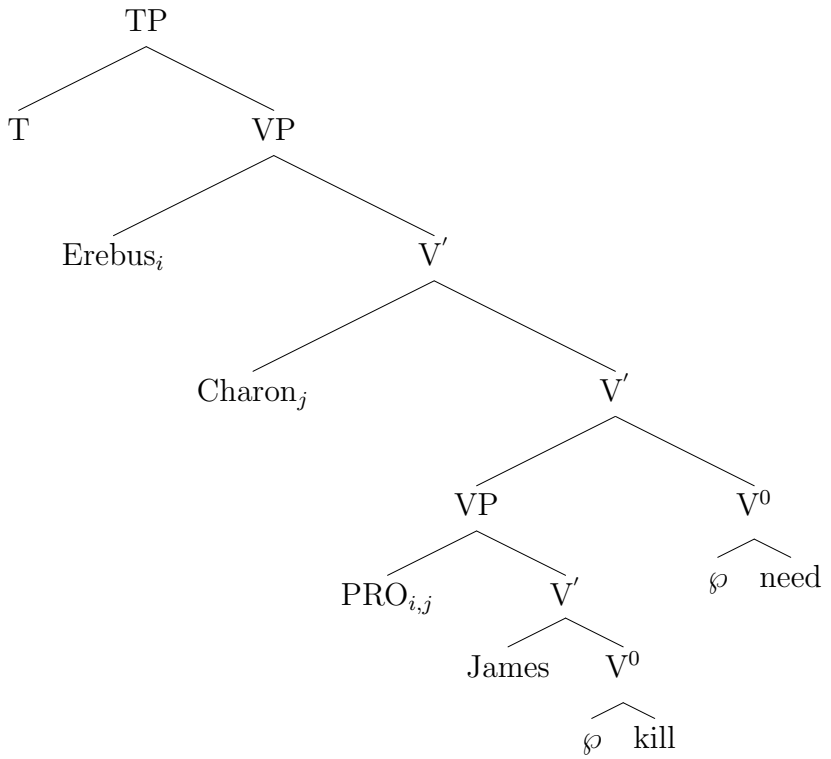
ungrammaticality of **Erebus arrived him exhausted, *Erebus arrived himself exhausted*.

The elements mentioned in [294] actually appear in virtual complementary distribution, as predicted by [294], showing that empty categories have an "underlying essence" (identical indices) with various surface manifestations (Chomsky, 1982, p. 34).

Note that by "reflexive" I do not mean "reflexive anaphors" in the widest, cross-linguistic sense, and rather refer to a narrow subset of reflexives, most closely those of morphologically complex anaphors (e.g., the SELF-anaphors of Reinhart & Rauland, 1993). This rule does not explain the distribution of empty pronouns (pro), but they seem to fall under some rule that differs from [294] in many fundamental ways. Moreover, the distribution of pronouns falls into an "elsewhere" category here, which is descriptively too general since suc distribution is more limited in that, rule [294] only prevents the antecedent of a pronoun being too close. It appears that the distribution of pronouns is also controlled by a mixture of discourse-oriented factors (cf. Chierchia, 1995).

There are cases in which a PRO may have several antecedents, as in *Erebus needs Charon to kill James*. Both *Erebus* and *Charon* satisfy [294](2), thus the sentence is automatically ambiguous, as shown in (298).

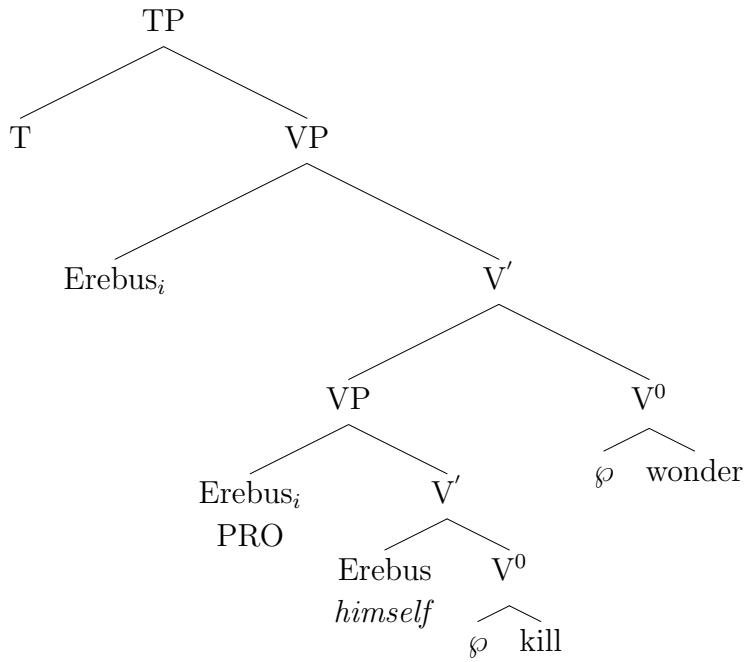
(298)



However, **Erebus needs to kill James* is not possible, since condition [294](2) is not satisfied, assuming that PRO is not the argument of *need*. Rather, [294](1) is satisfied to generate *Erebus needs himself to kill James*. For the same reason, **Erebus needs Charon$_i$ to kill PROi* is not grammatical, but *Erebus needs Charon$_i$ i to kill herself$_i$ i* [294](1) is.

Many structures involve several empty categories, such as *Erebus wonders whether to kill himself*. There is no problem here, given the stated conditions for covert pronouns at lexico-logical form. Thus, in the above example, *Erebus* appears three times at LLF. In each case [51] is applied, resulting in the correct expression (PRO for the subject *Erebus*, a reflexive for the object *Erebus*). The correct surface form is generated thereby, as shown in (299).

(299)



As these examples show, the generalisation of [51] with [294] is descriptively sound: lexico-logical forms are linked with the basic properties of empty elements. Reversing the reasoning, we might thus hope to use [51] with [294] as a diagnostic property for detecting the boundaries of full projections at the lexico-logical form. Consider the interpretation of the pronoun in the following examples:

(300) a. Erebus thinks he's smart
b. Erebus thinks highly of him
c. Erebus considers him intelligent

In (300)(a) the pronoun could have *Erebus* as its antecedent, a reading which is only possible in (b-c) if the pronoun is reflexive. This suggests that the pronoun in (b-c) is closer to the subject than in example (a). In the case of (300)(b), the pronoun and the subject clearly occupy the same full projection, *think highly* being a single complex predicate. In (a), the pronoun occupies a different full projection, which is shown by the fact that an overt complementizer is possible (*Erebus thinks that he's smart*). Why is *him* inside the same full matrix projection as the subject in (c), even though it is clearly the thematic subject of the embedded sentence? The reason is to so with restructuring: it moves to the matrix clause, showing that deletion takes

place after restructuring, thus making it a very 'deep' property of language. On the other hand, the pronoun in (a) does not rise, since restructuring does not cross T boundaries.

Consider *Erebus$_i$ believes Charon's description of him$_i$* once more. The fact that the embedded clausal nominal contains a pronoun (*him*) indicates that there is a boundary of two full projections between *him* and *Erebus*. Much the same is true of *Erebus* and *Charon*, since we have *Erebus$_i$ believes his$_i$ description of Charon* and *Erebus$_i$ believes his$_i$ description of himself$_i$*. Again, a pronoun appears. Thus, there must be one 'invisible' full projection α, a fact that is supported by the corresponding tensed sentential counterpart *Erebus believes that Charon describes him*:

Erebus believes $[_{\text{FP}}\ \alpha\ [_{\text{FP}}$ his description of Charon]]

The existence of α is relevant to the analysis of derived nominals (cf. Chomsky, 1970). Clearly, we are assuming here that they have a "sentential source": whereas *Erebus's describing of Charon* represents an abstract proposition, we must assume that *Erebus's description of Charon* involves events, as clearly is the case if semantic intuitions are to be trusted. Then α should be taken as an operator, such as T or a determiner, which turns an intensional entity into an extensional one. However, more work is needed here, somewhat independently of the properties of empty pronouns.

The sentence *the book which$_i$ you filed without PRO reading t_i* is a more complex example. Given that the distribution of pronouns is used to diagnose the structural distance at the LLF level, this entails that there must be one, but not two, full projection(s) between PRO and *you*. (The gap t produced by the wh-constituent (or wh-operator) is not discussed here due to the lack of a theory of wh-constituents.)

The theory of empty elements interacts with other components of grammar. Care must be taken in separating the passive rule, resulting from the insertion of a vacuous argument, from the rule concerning deletion of elements due to identical indices. Empty categories or 'gaps' result from [294] when two arguments have the same index; a passive is created in accordance with [51] when the vacuous argument and the object have the same feature vector. The following is a minimal pair in this respect:

(301) a. Erebus expected to hurt himself

b. Erebus was expected to hurt himself

At the lexico-logical form:

(302) a. Erebus$_i$ expected [Erebus$_i$ to hurt Erebus$_i$]
b. v expect [Erebus$_i$ to hurt Erebus$_i$]

The gap in (301)(a) results from [294](2), and in (b) it is due to the appearance of the vacuous argument and there is no deletion due to [294](2). In both cases I predict the appearance of the reflexive *himself* in accordance with [294](1). In the GB theory, this difference is diagnosed by the property that the antecedent of PRO assumes an independent thematic role, whereas the antecedent of the passive trace (t_i in *Erebus$_i$ was expected t_i to hurt himself*) does not assume such a role. This feature is a direct consequence of the present rules, since in (a) *Erebus* occupies two positions at the lexico-logical form with potentially differing thematic roles, one being deleted at the syntactic interpretation, whereas in (b) *Erebus* occupies only one position with one corresponding one thematic role assigned to it.

Some verbs, such as *seem*, to take a vacuous argument inherently as their subject. This difference is visible in minimal pairs such as:

(303) a. your friends hoped to finish the meeting happy
b. your friends seemed to finish the meeting happy

In the terminology of GB theory, your friends have two thematic roles in (a), and with only one in (b). In both cases, *happy* modifies the surface subject your friends, an interpretation that is lacking in *your friends hoped the meeting would finish happy* (from Chomsky, 1995, pp. 38-39). To take a slightly more complex example, consider the following sentences:

(304) a. one translator each was expected to be assigned to the visiting diplomats
b. one translator each hoped to be assigned to the visiting diplomats

Sentence (304a) contains two passive clauses, and hence two vacuous arguments. Semantically, and hence in its lexico-logical form, the subject *one translator each* is the object of assign: 'v expect v assign one translator each (to the visiting. . .).' Then through passivization and restructuring, *one translator each* perlocates up into the structure. In (b), *one translator each* is also the subject of the matrix clause: *one translator each expect (v assign*

one translator each (to the visiting...)).' The embedded v is deleted due to the identical L-vector, but the embedded *one translator each* is deleted due to identical index with [294]. Thus, in the framework of GB theory, *one translator each was expected tt' to be assigned t to the visiting diplomats* and *one translator each hoped PRO to be assigned t to the visiting diplomats* (Chomsky, 1995, p. 39). Assuming that the embedded clause is tensed, [294](3) is applied instead of [294](2), resulting in *one translator each hoped that he would be assigned to the visiting diplomats*. This is similar in meaning to (304b), a consequence of the rules given so far. What are completely analogous to (304a) are sentences such as *Erebus seems to have been expected to leave*.

Rule [294](2) regulating the distribution of PRO interacts with the rule on restructuring in a somewhat complex manner. Suppose restructuring takes effect after the creation of the gap. This produces *Erebus wants PRO to die*, PRO rising. Assume that *Erebus* is replaced with v at the lexico-logical form while the whole construction is embedded as an argument in a larger structure, such as . . . *is awful*. Then v and PRO have identical feature vectors deriving [*to want to die*] *is awful*. This indicates that PRO may "rise" as well (in the sense of rising via linking, not in the GB theoretical sense), creating sentences with the "quasi-agentive" PRO. For identical reasons, inserting the overt impersonal pronoun one instead of v in these constructions predicts d-marking, as in *for one to want to die is awful*, since otherwise the pronoun would not pass [51].

Suppos, however, that we apply restructuring first. Then [294](1) applies instead of [294](2), the antecedent existing inside the same full projection due to restructuring: $Jonn_i$ *wants* $Erebus_i$ $[_{FP}$*to die*], linked with

(305) Erebus wants himself to die

Embedding this structure inside . . . *is awful* results in the deletion of either one of the *Erebus* constituents due to [51], since they would both have an identical L-vector - hence the ungrammaticality of **Erebus to want himself to die is awful*, a violation that can be corrected by d-marking the whole constituents in order to derive *Erebus's wanting himself to die is awful*.

Pertinent to this issue is also the difference between ECM and control structures. If a predicate does not allow a direct object, it would not tolerate restructuring, as in the case the verb *try*:

(306) a. Erebus tried him to leave

b. Erebus tried himself to leave

Then in (306), the embedded argument is simply deleted, fixing its meaning to be synonymous with the matrix subject due to some property of "recoverability." The verb believe behaves exactly the opposite way due to the fact that it allows accusative objects. The interesting fact is that it does not tolerate embedded PRO subjects in English, although in French the opposite is true. This is predictable if we assume that restructuring is implemented before [294], which gives independent evidence that it is so (e.g., binding in *Erebus$_i$ seems to himself$_i$ to be stupid*). Otherwise PRO would be possible, the embedded subject remaining in a suitable position when [294] applies.

Discussion on so-called long-distance reflexives, backward binding and logophoric anaphors is postponed to another occasion (see Huang, 2000, for a review). Quantificational expressions are subject to other conditions, as their "formal identity" does not guarantee synonymy, as shown below:

(307) a. every hippie loves every hippie
b. every hippie loves himself

These are nonsynonymous, so that (b) could not have resulted from (a) by the application of [294]. Much the same is true of the so-called "donkey anaphora," illustrated in (308a-b):

(308) a. every man who owns a donkey beats a donkey
b. every man who owns a donkey beats it

Again, (b) could not have arisen by virtue of [294] since these sentences are not synonymous. Since synonymy is a precondition for [294] at the LLF level, something else must be going on here. What is pertinent to this discussion is the fact, shown by this data, that the notion of "formal index" codes the meaning of the lexical element inside the language faculty involves complexities related to quantificational expressions: *a man* is not synonymous with *a man*, since they may denote different men, although they are identical in their surface properties. Another problem with the "formal index" account is that complex antecedents can and normally do license anaphors. This shows that it is not just the meaning (index), but it is also the formal structure that counts in applying [51] and [294]. These matters require further clarification, which is not attempted here (see also Fodor, 1975, 124-147, for arguments against the present proposal involving structures with quantifiers).

In previous chapters I have examined dative subject constructions of various types, showing that, even if the dative subjects does not appear (arguably) in the topmost position at the LLF level, they can still bind and control. This is a matter of how to apply the [294](A) clause internally when there are several possible antecedents. Among these, the choice of a possible one must thus be based on semantic properties such as aspectual prominence (semantic subjecthood) and the ability to "control" the situation (causatives).

I have shown in this section how the binding theory could be modified slightly so as to formulate binding conditions at the LLF level as it was defined in this study. The material is part of the ongoing project of seeking evidence to support the atomistic theory by demonstrating that these assumptions do not present insuperable barriers when dealing with new data. This discussion on the binding theory also supplies the discussion on causatives in section 4.5 , in which I argued that the binding properties of anaphors in causative constructions do not inevitably lead into a biclausal explanation of them.

If there was a leading idea in the GB theory, aside from the formulation of the P&P-framework, it was the assumption that various locality relations between movement and binding could be unified under ECP or some related principle. Much work that was done in that period, and much of that work which is still carried to the minimalist framework, is understandable from that perspective. I believe this is one of the dimensions on which my proposal differ most. There is no ECP, no unifying locality condition present. Consider various phenomena having to do with empty categories and thus falling under the putative ECP. Restructuring is strictly cyclic, but not motivated independently by any kind of ECP: it is just as local movement as possible. Passives and raising constructions emerge naturally without any help from independent ECP, and their locality, e.g. clause-boundedness, relates directly to the assumptions which are used to deduce them. The locality properties of binding were, so far, only stipulated, and long-distance movement has not been explained in this study, yet it, too, seems to fall under different set of assumptions. Derivational and inflectional morphology was explained without movement, hence they fall naturally out of the scope of ECP: locality was captured by introducing a notion of ψ-vector. It thus looks as if all that was gained in terms of ECP has been replaced only by a diverse set of stipulations.

Yet, it is not impossible that general principles among all these stipula-

tions could be found. I think they have certain similarities to suggest that we still miss important generalizations. Many rules refer to the notion of full projection, which is crucial in constructing the LLF representations. For instance, the relation between PRO and its antecedent closely resembles the notion of ψ-vector: both are restricted by two full projcetions. Single full projections are important in applying the rule [34] as well as the rule of replacing synonymous constituents with reflexives. Restructuring, again a highly local rule, originates from the same position where PRO can occur. Why does the notion of full projection occur in these definitions? Note that well-formed LLF representations must be full projections, so that this condition might, and indeed is expected to interact with the construction of the feature vectors somehow. One might speculate that feature vectors are constructed in "phases" (VP, CP) corresponding to full projections at LLF. Since such phases are "local," so are many syntactic rules which are sensitive to feature vectors. On the other hand, I simply do not know whether this is the correct line to pursue at all, and the question must be left to another occasion.

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