

# Chapter one

## Minimalist Expectations

### *Preliminary Assumptions, with a Review of Some Familiar Notions*

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#### 1.1 A point of departure

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Just about everybody has something to say about language: poets, comedians, philosophers. This has been going on since we have had records, which is not surprising if we think that writing is an invention presupposing some serious thought about the topic of language. Our contribution to this enterprise is merely as linguists – of a specific orientation. Our teacher Noam Chomsky has paved the way to a study of those aspects of language that seem amenable to a scientific scrutiny, and we are just building on that tradition. Some intellectuals feel that this perspective is not sufficient to entirely capture language as a phenomenon. We agree, but have never attempted to understand ‘the ultimate essence’ of linguistic matters. Our task is more modest: treating language as a natural phenomenon (just as particle interactions or cell growth are, for instance), what could science tell us about it?

Of course, to put that general question into focus, we must make some idealizations, as any scientist would. Thus for instance we decide to study the language faculty in its universal properties, rather than concentrating on the way it manifests itself differently in various languages. We study those aspects of language that may be thought of as ‘structural’, as opposed to merely ‘behavioral’. We concentrate on those central properties where, as a matter of fact, our investigations make explanatory progress, as opposed to going into the myriad peripheral phenomena that can be merely collected and analyzed. These idealizations separate the study of universal grammar from descriptivist linguistics, the examination of linguistic competence from that of performance, and the concentration on general principles from the taxonomic study of grammatical constructions. Linguistic practice over the last half century suggests that these are productive moves that have led to much successful research, part of which will be discussed here.

Within a class of studies of the core universal grammar of the human linguistic competence, this particular text will be centered around the notion ‘minimalism’. This is first of all the most recent direction that Chomsky has

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taken in his tireless studies of the human language faculty. But to us there is much more than fashion here. In our view, the general goals now made explicit in the Minimalist Program (henceforth MP) were more or less implicit in Chomsky's investigations literally from the start, and MP in most of its variants is a logical culmination of an ambitious research program that began in the early 1950s, although perhaps it was too revolutionary at the time to be fully understood. In other words, we have no trouble saying that, in fundamental respects, Chomsky probably has not changed his vision, even if he may have changed his mind here and there. The context has, however, dramatically changed, and now some disciplines around linguistics (some which did not even exist five decades ago) are prepared to hear how truly bold Chomsky's assertions were concerning the nature of language.

In that respect, before we delve into specifics a disclaimer is surely in order. Explicitly, MP is nothing but a program, a way of looking at linguistic issues from an intriguing perspective. That program can materialize, and in fact has grown already, in various related though not always identical, or even similar, ways. We have been hesitant to finish this book in large part because we did not know which version of the theory would come to dominate the others, after the usual Darwinian competition which fuels scientific progress. To this date we find no clear winner. This will necessarily mean that this book will often have the tentativeness of a work in progress, which we hope the reader takes as an occasion to join in on the discussion. At the same time, we will try to be honest in separating what we think is solid conclusion or general strategy within the program from what may just be accidental execution, much of which we will need simply to move to the next page.

### 1.2 **A grammar and a dictionary**

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Let us move into our subject matter by considering some basic desiderata of MP within the context of reviewing a few notions from the Principles and Parameters (P&P) framework, the approach that gave rise to minimalism. We may begin with interactions between the lexicon and the computational system of human language ( $C_{HL}$ ), two major aspects of the classical and current theories. Of course, these two aspects correspond to the traditional dictionary and grammar of a language, but the exact form that each of these was given depended on various theoretical factors.

For instance, well into the late 1960s, grammars were taken to be essentially sets of rules to construct phrase-structure and manipulate it in some limited ways (transformations). It was basically a desire to limit what were taken to be possible rules at the time, in order to better explain how children uniformly acquire languages so rapidly, that eventually led to the general notions of *principles* and *parameters*.<sup>1</sup> Grammatical procedures were progressively taken to reduce to a few mechanisms that the interaction of very abstract conditions allows. Thus, for example, instead of separate transformational rules for question formation, topicalization, or relativization, the grammatical device

was argued to consist of a single movement transformation obeying general constraints; in turn, specific linguistic variations were taken to be determined by directionality and locality parameters. In the limit, traditional rules become a mere taxonomic apparatus, which ideally can be shown to deductively follow from the more basic principles and parameters (see Chomsky 1981, Lasnik and Uriagereka 1988).

Likewise, the notion of what is (in) the lexicon has changed over the years, in part also as a consequence of the shift from a  $C_{HL}$  based on numerous language-specific rules to a more principled one. Nonetheless, debates on how much work the lexicon and  $C_{HL}$  should each assume are prior to the change in focus we have just mentioned. In early versions of transformational grammar, of the type of ‘The Logical Structure of Linguistic Theory’ (LSLT) (Chomsky 1955), the lexicon did hardly any work at all, whereas  $C_{HL}$  was very busy. The same was true of Lees’s (1960) *The Grammar of English Nominalizations*. In particular, lexically related words were taken to be transformationally related through  $C_{HL}$ . For example, only a verb (like *destroy*) would be listed in the lexicon; then, through a variety of largely idiosyncratic transformations, one could obtain lexically related items (like *destruction*, *destructive*, etc.). Chomsky argued in ‘Remarks on Nominalization’ (1970) (RoN) that  $C_{HL}$  should not have that much descriptive power and, instead, he put most of that power into the lexicon; that was the lexicon’s major expansion. Viewed from the perspective of history, that seems like the right move; but since then, the debate has been going on, with issues not having been entirely resolved.

From the viewpoint of the MP, one of the most important questions that arises when studying the relation between the lexicon and  $C_{HL}$  is the *redundancy* existing between the two. It was found that a lot of the work that  $C_{HL}$  was taken to do, the lexicon was independently doing. The desire to get rid of this sort of redundancy when doing work in syntax has been a very pervasive force since the 1980s, and it is one of the most central practices in the minimalist methodology. In general, when two (or more) conditions within the theory are doing overlapping work, we take that as an indication that we are missing something. No one is sure why language should be built in such a way that its properties are not *overspecified*, but we have indirect evidence that this is the case. Whenever we revise the theory in such a way that this redundancy is avoided, we seem to arrive at a theory that covers more facts and covers those we already know about in a simpler, more natural fashion. We will be seeing several relevant illustrations in the pages that follow.

The redundancy that concerned Chomsky in the 1970s was between the sort of lexicon he argued for in *Aspects of the Theory of Syntax* (ATS) (1965) and the  $C_{HL}$  of the time, which made crucial use of phrase-structure (PS) rules. In ATS, Chomsky had argued that a lexical entry must contain syntactic information about whether, for instance, a verb is transitive, intransitive, ditransitive, and so on (so-called *subcategorization*). In the late 1970s, however, it was noted that this information is redundant with the corresponding information resulting from PS rules.<sup>2</sup>

For example, we have subcategorization frames for verbs like *sleep*, *eat*, *give*, and so on, together with PS rules telling us that a verb phrase must consist

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of (a) a verb, or (b) a verb and a nominal expression, or (c) a verb and two nominal expressions:

- (1)a.  $VP \rightarrow V$             b.  $VP \rightarrow V NP$             c.  $VP \rightarrow V NP NP \dots$

It was by rules like these that *phrase-markers* (PM) (of the sort we usually represent through ‘tree’ diagrams) were built, up to the mid-1970s. But the information involved in (1) was also needed in the lexicon, or else we could not have prevented a verb like *sleep* from forming a PM in terms of a rule like (1b), yielding (2a); or a verb like *eat* from being involved in (1c), with (2b) as a result:

- (2)a. \*John slept the bed  
b. \*John ate Mary a cake

Then, however, the lexical entry seems to be providing all the necessary information to rule out the examples in (2), while still allowing the corresponding good cases. For instance, given the subcategorization information of *sleep*, *eat*, and *give* we know we can only generate examples of the sort in (3):

- (3)a. John slept.  
b. John ate a cake.  
c. John gave Mary a cake.

What is more, should we find a verb with a new subcategorization frame that we did not already know about, we would simply add it to the lexicon, and the corresponding rule to  $C_{HL}$ . Thus:

- (4)a. *put* <\_\_ NP PP>            (correspondingly:  $VP \rightarrow V NP PP$ )  
b. *say* <\_\_ S>            (correspondingly:  $VP \rightarrow V S$ )  
c. *persuade* <\_\_ NP S>            (correspondingly:  $VP \rightarrow V NP S$ )  
etc.

So at least with respect to complements as in (4), it truly seems that there is nothing that PS rules are telling us that subcategorization frames do not independently code. Then the question – assuming the minimalist logic – is which information one should get rid of. And it really seems as if it is the PS rules that must go, because if we were to eliminate this information from the lexicon, we would again lose the distinction between *eat* and *sleep*, and all the other verbs.

The reason this sort of problem was not seen earlier might have been related to the metaphor that we need PS rules to ‘build trees’. Of course, unless something deeper is clarified, notions such as ‘tree’ or even ‘build’ are entirely metaphorical. In fact, all that is really needed for linguistic theorizing is a classification of syntactic representations of a certain, intuitively phrasal, sort; and yes, one standard procedure for doing this is through PS rules to literally build PS representations. But there are many other ways of achieving these

representations: for instance, as in Lasnik and Kupin (1977), an article presenting a procedure which is in fact incompatible with standard PS rules.<sup>3</sup> In systems of that sort, all relevant phrasal dependencies must be coded either lexically or, as in McCawley (1968), in terms of general conditions on phrasal admissibility (more on these in chapter 2).

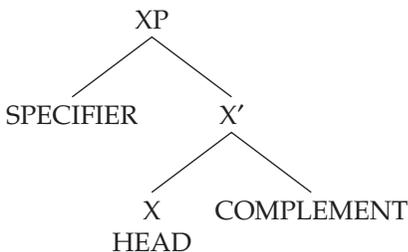
Interestingly, there is still a little work for PS rules to do. As we have just seen, subcategorization is only about complements, and indeed lots of PS rules are about complements – those are the redundant ones. But there are some PS rules that are not about complements; for instance, PS rules introducing information about ‘specifiers’, whose information cannot be expressed in terms of subcategorization frames. Curiously, there is one type of information pertaining to specifiers which is, to this day, quite hard to deal with; this is what was called the *Extended Projection Principle* (EPP) in the P&P framework. In plain English, the EPP’s demands are clear:

- (5) *Extended Projection Principle*  
A sentence S must have a subject.

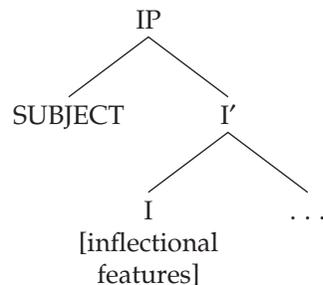
The question is what this simple statement (which in the end is asserting that  $S \rightarrow NP \dots$ ) could follow from.

One might want to make (5) follow from the *projection* procedure of *X'-theory*, the general current substitute for PS rules. This, in the old days, was not such an easy task. Thus, in RoN (where Chomsky put forward the initial version of *X'-theory*) S was not a projection of anything, since there was nothing obvious at the time that could support the projection of a sentence. In later years, the virtues of another (usually unstated) minimalist desideratum became relevant; this is what we may call the drive for *symmetry*. From this point of view, sentential structures should look like other familiar structures, even if this involves making the move of postulating some abstract category that supports the projection of S. The theoretical advantage of this proposal is that now a claim about a subject, as in (5), is really a claim about the specifier of an *X'* structure, of the familiar sort in (6):

- (6)a. PROJECTION



- b. For S



But have we now deduced (5)? Probably not, since other specifiers appear to be optional. Take, for instance, the application of the schema in (6a) to clauses headed by C (or Comp), as in Chomsky’s (1986a) extension of Bresnan’s (1970)

insights. The specifier of C may be occupied by a moved Wh-phrase, but this is not necessary, since most clauses do not involve Wh-movement. Similarly, consider the extension of (6a) to determiners, as in Brame's (1982), Szabolcsi's (1983), and Abney's (1987) 'DP hypothesis'. In something like *Rome's destruction*, we take *Rome* to occupy the specifier of the head D, whose lexical instantiation is arguably the genitive clitic 's;<sup>4</sup> but of course one can also say *the destruction of Rome*, with *the* occupying the D head position and nothing in its specifier. So the structural characteristics of the specifier, in and of themselves, will not predict (5).

The sort of redundancy we have seen between the lexicon and PS rules is, as it were, external to the lexicon. Another interesting redundancy found in the late 1970s was in fact internal to the lexicon. To see this, consider the sort of lexical entry that was relevant already in ATS:

- (7) *Lexical entry*  
 [Phonological representation]  
 Categorical information: +/–N, +/–V  
 Subcategorization frame: <\_\_ . . . >  
 [Semantic representation]

The phonological and semantic representations are what allow English speakers to distinguish *use* from *hit*, from *eat*, and so on (even if they are all transitive verbs). Both of these kinds of interpretive representations are complex. We put aside phonological considerations, but note, concerning semantic relations, that at least notions like *agent*, *patient*, *goal*, and the like, are involved as what are usually called *theta-roles* (see chapter 7.1). Particular verbs have particular theta-roles that they associate with their arguments by virtue of their lexical semantics.

There surely are universals in these aspects of semantic representations. For instance, if a verb has an agent theta-role, this role must associate to a subject, regardless of whether the verb is *eat*, *hit*, or whatever. That is, as far as is known there is no hypothetical verb in any language whose subject is a patient and whose direct object is an agent.<sup>5</sup> If one comes up with a significant set of generalizations of this sort, one might be able to describe how thematic structure projects onto syntactic structure, on a general basis.<sup>6</sup> Suppose this were possible. Now the redundancy question re-emerges: which should one get rid of, thematic or subcategorization (i.e. syntactic) information?

The logic of the situation provides an answer. If one were to get rid of thematic structure, one might not know what the semantics of a particular verb is. That is, at our present level of understanding, saying that a verb requires an NP complement, for instance, does not tell us whether this complement is to be understood as an object (*I broke the vase*), a beneficiary (*I married the lady*), a goal (*I reached the summit*), an instrument (*I turned the key*), or any such thing. On the other hand, (at least some) explicit syntactic information would seem to be derivable from more basic thematic information. Ideally, knowing the thematic structure of a given verb is akin to knowing everything there is to know about the sorts of constructions where this verb

can appear. For instance, *hit* will have to appear in a syntactic structure <subject, verb, object> because *hit* has thematic roles to assign to a subject and an object; it would be semantically incoherent for *hit* to appear in a structure without a subject or an object, and thus the fact that it requires an NP complement will follow. In contrast, *assert* will require the syntactic structure <subject, verb, proposition>, as its complement must be propositional, and thus appear in the syntactic format of a sentence. There will also be verbs like *persuade* requiring both an NP and a sentence (*we are trying to persuade [the reader] [that this is the case]*), and intuitively this should also follow from its lexical meaning, as was the case with the meanings of *hit* or *assert*.<sup>7</sup>

If matters are as discussed, two predictions are made. First, imagine a verb with no theta-roles to assign to an object; that would be an intransitive verb – which there are plenty of. But second, imagine a verb with no theta-roles to assign to a subject, like *rain* or other weather verbs, or *raising* verbs like *seem*. Now these verbs can appear with a grammatical ‘dummy’ subject – which is curious in and of itself – but never without it:

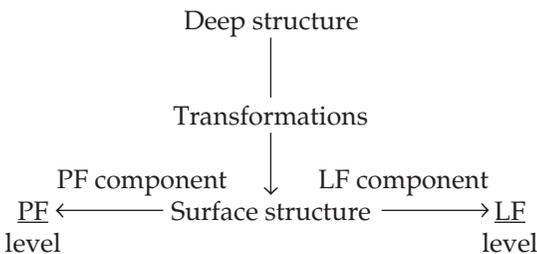
- (8)a. \*(it) rained last month.  
 b. \*(it) seemed that John was a liar.

Evidently, something else is going on in (8) which the mapping of syntactic structures from theta structures cannot predict. This is why we need the EPP in (5), which unfortunately is largely redundant with Theta Theory as we know it.

### 1.3 Levels of representation

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A third desideratum of minimalism is provided by Occam’s Razor. As with the other desiderata, this one need not be correct; however, research guided by this tendency has been fruitful. The traditional P&P model included the representational levels of Deep structure (which is where thematic structure was coded in *Lectures on Government and Binding* (LGB) (Chomsky 1981)), and Surface structure, which served as an intermediate link between Deep structure and the interpretive (Phonetic Form and Logical Form) levels:



The MP eliminates levels that are irreducible to the *conceptually necessary* claim that language interfaces sound and meaning.

In passing, we note that the terms Deep and Surface structure led to misunderstanding from their earliest use. There was nothing ‘deep’ about Deep structure, nor was there anything ‘surfacy’ about Surface structure. If there is a ‘deep’ level, in the sense of coding the representations that are closest to ‘thoughts’, that is probably LF, and if there is a level which is ‘surfacy’ in the sense of coding information which eventually comes out to the environment and has to be picked up in terms of learning, that is the level of PF. Then again, there appears to be no good reason to call thought ‘deep’ and speech ‘surfacy’; they are just two aspects of the natural world, somehow manifested through the minds and bodies of humans. In any case, since the terms Deep and Surface lead to unnecessary confusion, we will drop them from now on (as did Chomsky already in LGB), referring to Deep structure as D-structure and to Surface structure as S-structure, in a technical sense.

MP’s razor isn’t really about eliminating confusing names, though: it proposes eliminating the very levels of D-structure and S-structure, keeping only to PF and LF. The latter are usually referred to as *interface* levels because it is through them that the system relates to other aspects of mental reality: the ‘inner’ intentional/conceptual components and the ‘outer’ sensory/motor components.<sup>8</sup> The reductionist position is that – since logically speaking no interfaces beyond these ought to be needed – in fact no other levels of representation exist. One aspect of that idea was first explored in the 1960s by generative semanticists, who wanted to eliminate Deep structure for empirical reasons that we will not review. MP too has empirical reasons (which we return to in chapter 7.2) to argue against that level of representation. But more generally the minimalist desire is to conceive language as a ‘perfect’ linking between sound and meaning. A priori we know that in language we have crucial interfaces with (at least) those two very different domains; the syntax that links them can be thought of as some *elegant solution to the linking problem*. In a nutshell, that is the minimalist thesis.

One should also ask whether any interface levels as such are really necessary. Strictly speaking, what is required is that language be a relation between sound stuff and meaning stuff. However, quite a different thing is to affirm that codings for sound and meaning have to be expressed in terms of *interface levels of representation* such as PF and LF. It is reasonably possible that, instead of forming a unified syntactic structure to interface the interpretive components of the linguistic system (a level), these components are multiply accessed in the course of the derivation. If this were the case, we would have PF and LF *components*, but not the corresponding *interface levels*, since there would not be anything like a unified, complete PF or LF object that the system presents as a single interface point.<sup>9</sup>

It may be worth clarifying a terminological point that is not immediately obvious. There is a debate in the literature, starting in the 1970s, about whether in particular the LF part of the system is necessary. Actually, the terms of the debate were not stated correctly, since it really was a debate about whether *S-structure* exists – not about LF. LF (either as a derivational component or as a level proper) is the syntactic interface with the semantics, by definition; so not having this part of the system would immediately entail no semantic

interpretation, which is clearly not what we see in human language (although it might be true of something like the mental representation of musical or mathematical structures). Then of course you may decide on the rest of the architecture for the model: whether you have S-structure or some other intermediate level, whether LF feeds, bleeds, or is parallel to PF, and so forth. Those are real questions, but whether there should be PF or LF components of the system, and perhaps even unified corresponding levels, is hardly a question, given what we know about our subject matter.<sup>10</sup>

It is worth mentioning, also, that S-structure had a weak conceptual motivation basically from the time it was proposed. As we have seen, PF and LF are, if not obvious, at least very reasonable levels. So is, to some extent, D-structure – if one is going to have a level or component to interface the lexicon (which may be debated). But S-structure was always a purely theoretical level, interfacing with nothing external to the language faculty. Of course, conceptual matters aside, we may well have seemingly strong empirical arguments for the existence of this level. However, for a minimalist this is not the end of the debate, given the central role in the program of the theoretical desiderata outlined above. To illustrate this point, we present an analogy from another science.

Consider the patterns appearing in flower corollas, among many other domains of plant morphology (so-called *phyllotaxis*). Mathematicians have studied those patterns for ages, and have extremely accurate descriptions of their properties. For example, most of them obey a proportion in their growth which is very well known to architects and sculptors since classical antiquity: the *golden mean* of ‘one plus or minus the square root of five divided by two’. Does nature ‘know’ or somehow code this number? Surely not. Nature codes, in some form or another, the various forces that interact in the growth of a plant, including whatever it is that impels the plant to expand and whatever it is that keeps it glued together as an organism. This results in a dynamic system with certain properties, given the nature of matter and the interactions it allows. The system in question poses a problem of a certain sort, whose solution is in terms of the golden mean, for reasons that do not concern us now (but which have a minimalist flavor, in that they seek the optimal stability of the system).

Returning to linguistics: is S-structure like the golden mean, or like the forces interacting with one another, that *result* in the golden mean? The latter seems more plausible – and more intriguing. LF and PF, together with whatever it is that codes lexical information (be it D-structure or something even more basic) are, as it were, the opposing forces that create a certain dynamic problem; and S-structure may well be the optimal solution that the linguistic system finds. If this picture is anywhere near right, we have to be careful about whatever properties we ascribe to S-structure. Certainly we will find some obvious properties, just as mathematicians studying flowers observed some immediate, rather astonishing properties (after all, that plants should obey a ratio independently found in other organisms, or human architecture, is as extraordinary as it gets in natural science). That, however, should be the beginning of our research, not an end in itself. Not surprisingly, much of the

energy spent by minimalists in recent years goes to figuring out clever ways in which to deduce the observed properties of the system, which are relatively easy to state as S-structure conditions, but much harder to conceive as PF, LF, or purely computational mechanisms.

## 1.4 Syntactic or semantic bootstrapping and functional categories

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Concerning a ‘reductionist’ lexicon, it is useful to consider the two arguments that Pesetsky (1982) gave for the absence of subcategorization. We have already discussed his first argument, regarding the possibility of deducing subcategorization from semantic relations. The second argument is based on language acquisition considerations, and was given in the context of a debate with Grimshaw (1979), who argued that we need both *categorial selection* (c-selection, i.e., subcategorization) and *semantic selection* (s-selection). Pesetsky reasoned as follows.

Chomsky had already introduced in the 1970s the concept of *epistemological priority*. He reminded us that, for any linguistic theory, we need some theory-external notions that are necessary to get the theoretical apparatus off the ground – to get some learning going. That is, the human child has to come to the acquisition task with certain primitives that are prior to acquiring knowledge of language. Pesetsky reasoned that c-selection notions such as NP or VP could not have the status of epistemological priority. On the other hand, notions such as ‘agent’ or ‘patient’ could perhaps be prior to any linguistic experience, thus constituting the basis for a *semantic bootstrapping* on the part of human children. The argument is tantalizing, but it does not obviously go through, for a couple of reasons.

First, there is a big leap in the argumentation. In a sense, even the first step in the argument should be controversial: that the child is capable of analyzing the world in terms of a *language of thought* with notions such as ‘agenthood’, ‘patienthood’, and so on. But let us grant this step. Nonetheless, it is not enough to know that this entity in the world is an agent or that entity is a patient; rather, one needs to know that *some particular noise* corresponds to an agent in some particular sentence. Now, this does not sound so plausible: that prior to any knowledge about English a child should know, when hearing the sentence *John kicked the dog*, that the noise *John* corresponds to an agent. Such a claim does not sound any more or less plausible than claiming that the child knows that *John* is an NP prior to any linguistic experience. The point is not so much the child’s ability to analyze events in the world, but rather the corresponding ability to analyze noise in a sentence, and match this knowledge with the knowledge about events and their nuances. That is a tough problem. One may agree with Pesetsky’s conclusion about what is epistemologically prior, but the question is still how the information managed to get into the lexicon. On the other hand, if this is the question, one may also disagree with

Pesetsky's conclusion, and reopen the possibility that lexical bootstrapping by children be syntactic, instead of semantic.

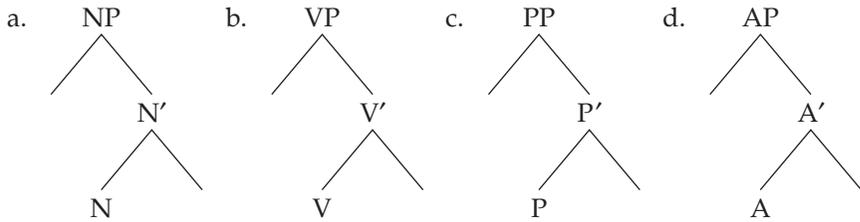
Suppose for the sake of argument that NP, VP and so on are epistemologically prior, and the child can indeed use them to analyze the noise she hears. That reduces her range of options. For instance, if you – at least as a linguist – analyze a VP as involving V and S, you already know that this verb must be one of propositional attitude like *think*, and not a verb whose complement is an object, like *hit*.<sup>11</sup>

The second difficulty with Pesetsky's argument stems from another curious assumption he had to make: that somehow the child is presented with words and their meanings in isolation. This is most certainly not true, at least for verbs, which are almost always presented in the context of a sentence. That is, one might possibly point at Fido and say 'Fido!', and less obviously so, one might do the same with a car or a house (although people sometimes do that when speaking to children); however, no one really goes around pointing at seeings, lovings, digestings, and so on – it is not even clear what one would point at. But if words are not somehow presented in isolation, how exactly is the child going to use a language of thought to analyze them? That is, suppose the child were to hear a simple sentence like *John caressed the cat*. To make the problem even remotely solvable, suppose the child already has knowledge about John and the cat, and has visual access to the event of John caressing the cat. How is the child supposed to know that John, and not the cat, is the agent of this event? All the child sees is a relation between John and the cat, which could be of several sorts, including many in which John is the theme of the action, and the cat is an agent (*the cat tickled John*), or an experiencer (*the cat loved John*), or a cause (*the cat entertained John*), or even an instrument (*the cat served to soothe John's hand*). Likewise, there is no reason why John could not be analyzed as receiving many of those roles: experiencer (*John loved the cat*), cause (*John entertained the cat*), instrument (*John served to scratch the cat's back*). Finally, why concentrate on the patting aspect of the perceived event? Any event of patting is also one of touching, sharing, communicating, standing together in the same space, to name just the obvious. What exactly makes the child focus on the patting aspect? These are mostly questions that we will not solve here, but they should be kept in mind when evaluating any claim about what is epistemologically prior to language acquisition, particularly when this evidence is used for or against the existence of c-selection.

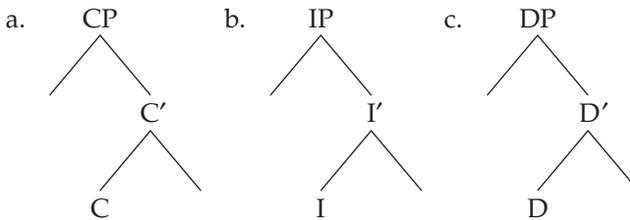
So far, then, we are reasonably certain that the lexicon must contain semantic and phonetic information, and it is unclear whether it has any categorial information. Chomsky's conviction over the years (and as recently as Chomsky and Lasnik 1993, in Chomsky 1995b:31–3) has been that categorial information is so obviously necessary that one does not even need to provide an argument to justify its presence in a lexical entry; but not everyone shares his conviction, and in any case it is hard to find conclusive evidence one way or the other.

One further consideration in these matters was introduced by the 1980s, when  $C_{HL}$  was assumed to obey some general, symmetric  $X'$ -schema (see chapter 2), instantiated as follows:

(9) Substantive structure:



(10) Functional structure:



The divide between substantive and functional (or formal) structure is also traditional: it is nothing but the classical separation between lexical (substantive) items (*motorcycle, overlap, underneath, moderate* and so on) and grammatical (formal) items (like *if, will, the* and so forth). There is lots of evidence for this cut, although nobody has a clear picture of what it really means. The bottom line, though, is that substantive items tend to be long, they compound and get modified, they denote in standard semantic ways, they are theoretically unlimited, and they can be learned every day, basically until we die; in contrast, formal items tend to be short and not even pronounced in some cases, they do not really combine as words, they mean rather ‘strange’ notions (like ‘conditional state of affairs’, ‘future time’, ‘unique individual’ etc.), they are only a handful, and they have to be learned prior to the critical age of puberty – or you won’t speak a human language.

In spite of those important differences, (9) and (10) suggest that the basic skeleton of all categories, be they substantive or formal, is the  $X'$  projection. In other words, even if there were fundamental dimensions separating substantive and formal categories, their structural  $X'$  properties are thought to be conserved constant across these different dimensions; this is what we mean by the desideratum of *symmetry*.

## 1.5 Modularity, Case, and thematic relations

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At the same time, we know that there has to be something more than what (9) and (10) imply, to deal with distributional differences among categories, properties of displacement, the coding of apparent adjacency restrictions, and many such conditions. As hinted at above, something useful that can help decide what other elements are relevant in this respect is Case theory. We will

show how this ‘module’ of the theory works immediately, but before we do that, note that this line of reasoning presupposes a certain way in which the system operates; that is, *modularly*.

Modularity is not, specifically, a minimalist desideratum, although it is one of its presuppositions. All that modularity tells us is that we do not have a monolithic system in our hands – one that offers a direct correlation between mechanisms and observable phenomena. A rule like  $VP \rightarrow V NP$  is pretty much telling us that a noun phrase follows a verb to form a verb phrase. But the minute we start decomposing that sort of phenomenon into its (hypothetical) component parts, there is no given statement that is responsible for the exact way the combination of words looks. Rather, a bunch of different parts of the theory are responsible for the dynamic articulation of the noun phrase following the verb. It is in this subtly dynamic system that it makes sense to raise the minimalist questions about economy that we are considering here. Let us illustrate this with Case considerations.

Being deliberately naive about Case, we can say that some feature  $F$  distinguishes nominals with and without that feature.  $V$  and  $P$  elements are taken to be  $F$  assigners, but not  $N$  and  $A$  elements. This is all intended to help us separate categories of a certain sort, which can or cannot appear in some specific frames. For example, we have *prove the theorem* but not *\*proof the theorem*. More generally, Case theory is a priori helpful in distinguishing different subtypes of categories – for instance, verbs. Another interesting redundancy arises here. Take a verb like *hit*, which needs to assign Case to the object it assigns a theta-role to. It is intriguing that *hit* happens to have Case to assign to this object. Or take *sleep*, which does not have a role to assign to an object, and does not have Case to assign to an object either. Apparently, we now have two explanations for why an object could not associate to *sleep*: this object would not get a role and it would not get Case. That seems redundant.

Curiously, however, there is a reduced set of instances in which it seems as if *sleep* is able to assign Case. For instance:

(11) John slept a long/nightmarish sleep

Not all intransitive verbs behave like this, but a substantial group of them do take what is sometimes referred to as *cognate objects*. This is a typically circumscribed class of objects which do not really provide any referential information, but rather seem to sort out the verb in various ‘adverbial’ ways: John’s sleeping was long, nightmarish, etc. It is possible that *sleep* assigns a kind of role that only the object *sleep* can receive – but this is not a very illuminating theory. A more plausible alternative would be that *sleep* assigns no role to an object; however, if Case is a mere formal property, when one places any kind of nominal in object position – be it referential or not – it will plausibly need Case. If this is the correct analysis, we may take *sleep* to have a Case feature to assign, even though it does not have a thematic feature to assign to an object.<sup>12</sup>

In light of the previous redundancy, we should assume certain ground rules on redundancy. If we have a version of linguistic theory that has some substantive redundancy we do not like, and we restate the theory in such a way

that we get rid of the redundancy, but no empirical gain ensues – then we don't have much of a theoretical gain. However, when we restate the theory so as to get rid of the redundancy, and something else is accomplished (a further fact is explained, or the global architecture becomes drastically more elegant and insightful), then the result is worth the effort. With these ground rules, it is not obvious to us that we want to get rid of the redundancy mentioned above. We won't try to make the Case-assigning property of *sleep* follow from its thematic property because if we did, we would lose the fact shown in (11).

Continuing with our brief overview of Case and theta matters, we may recall another interesting use of these notions in the past. This concerned the redundancy we have already mentioned between subcategorization and some properties of the PS component. At the time the redundancy was discovered, it was so great that the theory not only doubly expressed what kinds of complements a given verb should have, but furthermore where they would appear – right or left of the verb. That is, just as a rule like  $VP \rightarrow V NP$  tells you what kind of complement a verb like *hit* has and where it appears (to the right in English, but to the left in Japanese, in accordance with  $VP \rightarrow NP V$ ), so too a subcategorization frame like  $\langle \_ NP \rangle$  states exactly those two facts, with the Japanese  $\langle NP \_ \rangle$  encoding the opposite fact about order. One could argue that order is a residue of subcategorization that one should not give up in favor of restrictions of a semantic sort. After all, there is no obvious reason to think that semantic information should tell us anything about the ordering of elements, since the English VP *chase the dog* has presumably the same semantics as its Japanese counterpart, even if they are ordered differently. Of course, things could have been different in some other universe, with human language coding grammatical relations in terms of phonetic ordering, just as – for instance – it is relevant in musical terms whether we get a sequence C G or a sequence G C. Yet, it does seem as if, at least in terms of PF appearances, human language allows for two different realizations of the (head, complement) relation, the obvious ones in a linear system:  $\langle \text{head, complement} \rangle$ ,  $\langle \text{complement, head} \rangle$ .<sup>13</sup>

Stowell (1981) tried to eliminate even that residual use of subcategorization to code linear ordering. He tried to show that in fact all PS properties can be deduced from what he took to be deeper properties of the theory. In this instance, Stowell argued, what is relevant is a combination of basic theta properties and a generalization about these theta properties, often called the *Head Parameter*. This Head Parameter is nothing but an explicit statement of the fact mentioned in the previous paragraph:

(12) *Head Parameter*

In human language, complements precede or follow heads.

Of course, (12) is a bit stronger than just allowing any complement to precede or follow any head; the idea is that a language is defined in terms of fixing one of the options that (12), and other similar parameters, allows. Once (12) is fixed, while the child may still need to learn to associate semantic forms to

corresponding phonetic arrangements, she will not need to learn again the relative position of complements vis-à-vis heads in the language she assumes she is acquiring.

In the limit, the program that Stowell and others were aiming to establish tried to reduce human language to statements like (5) and (12). (5) (and other assertions of that sort) were taken to have the nature of universal laws for language, whereas statements like (12) were assumed to allow for variation along certain dimensions established by the parameter – in this instance, different possibilities in ordering. But then a further question emerged: what could be said about the relative order of complements when a verb has more than one, as in *put the book on the table*? Why is that not ordered *\*put on the table the book*?

Stowell argued that Case theory provides an answer to that question, assuming Case is assigned under adjacency: when a verb, such as *put*, selects for a PP which needs no Case and an NP which does need Case, obviously the only order that will satisfy Case requirements would be V NP PP. As one could imagine, this sort of strong proposal quickly became controversial – particularly the part about Case requiring adjacency for its assignment. Empirically, the hardest difficulty the idea faced was posed by instances of ‘double objects’, such as *John gave Mary a book*. How does *a book* get Case under adjacency? A related (equally tough) question seems to be why only the ordering above is possible; that is, why is it ungrammatical to say *\*John gave a book Mary*?<sup>14</sup>

Case was also instrumental to Pesetsky’s argumentation concerning matters of c- and s-selection and how they might be resolved in elegant (non-redundant) ways. Grimshaw observed that verbs like *ask*, which can semantically take a question, can take noun phrases with a question meaning – what she called *concealed questions* – such as *the time*:

- (13)a. She asked what time it was.  
b. She asked the time.

This observation formed the basis for Grimshaw’s strongest argument that we need both c- and s-selection; this is shown by the fact that *wonder* is also a verb that takes an embedded question, but it crucially cannot take a concealed question:

- (14)a. She wondered what time it was.  
b. \*She wondered the time.

Grimshaw argued that nothing obvious in the semantics of (14b) can be wrong, given (14a) and the corresponding paradigm in (13). So according to Grimshaw, *wonder* and *ask* are exactly the same with respect to s-selection (they both select for questions), but they are different with respect to c-selection (only *ask* subcategorizes for an NP, while both *ask* and *wonder* subcategorize for a clause). Needless to say, Pesetsky had to reject Grimshaw’s answer, since he was arguing that there is no c-selection. Pesetsky argued that the phenomenon

need not be explained in terms of c-selection; rather, Case is what is at issue. All we have to say is that *ask* has the feature *F* for Case assignment available to it, whereas *wonder* does not have this feature available. Therefore, the NP object of *wonder* would lack Case, which is illegitimate (the Case Filter, chapter 2.1).

The analysis could end here, but from Pesetsky's perspective there still is an important question to ask: is it plausible to say that a human child knows, prior to the acquisition task, that a verb like *ask* has feature *F*, while a verb like *wonder* does not? Remember, knowing this prior to acquisition really means knowing, prior to hearing the stream of noise that the child is to analyze, what are the formal combinatorial possibilities (in terms of *F*) of different sorts of verbs. Now that seems at least as problematic as the alternative that troubled Pesetsky: that whereby knowledge about NP, VP and the like is taken to be prior to experience.

One other important point to note is that the discussion above presupposes that sentences, unlike NPs, do not need Case, or else by hypothesis (14a) should be as bad as (14b). One can indeed argue that CP does not need (perhaps cannot receive) Case:

- (15)a. [I am [proud [that Mary is here]/\*[the fact]]]  
 b. [I am [proud of [the fact]/\*[that Mary is here]]]

We know that nouns or adjectives (in English) do not assign Case (see chapter 2). That view is directly consistent with nominal expressions (which obviously need Case) not being able to appear as complements of nouns or adjectives (15a); in contrast, entire complement clauses are fine in these contexts.<sup>15</sup> That way we can account for why *wonder*, by hypothesis an element that assigns presumably no form of Case (thus the ungrammaticality of (14b)), can host a (Case-resistant) clause as its complement in (14a). On the other hand, when a clause is in a subject position, it does seem to need Case. Thus:

- (16)a. \*[It seems [[that idea] to be true]].  
 b. [[That idea] seems [*t* to be true]].

- (17)a. \*[It seems [[that 2 + 2 = 4] to be true]].  
 b. [[That 2 + 2 = 4] seems [*t* to be true]].

Observe that the clause in subject position (17) behaves just like the corresponding nominal (16). Just as the NP is impossible in (16a) as the subject of an infinitive (which by hypothesis does not receive Case when it is complement to a raising verb like *seem*: see the discussion around (26) below), so too is a clause impossible in this context (17a). In turn, moving the offending NP in (16a) to the matrix subject position, where it can get nominative Case (16b), salvages the derivation; correspondingly, the same can be said about (17b), where the clause is moved to matrix subject position. So this arguably shows that at least subject clauses need Case – which is conflicting with the evidence in (15) for clauses in object position.

Note also one further wrinkle with the analysis. Suppose we strengthen the claim that clauses do not need Case to the more plausible one that clauses cannot receive Case at all. If so, how does the hypothetical Case feature *F* of *ask* get satisfied in (13a)? No obvious answer to this question comes to mind, which should be kept in perspective when analyzing Pesetsky's account of Grimshaw's facts, and whether his overall analysis on c- and s-selection is ultimately as feasible (given general facts about language) as it is non-redundant.

## 1.6 Elegance and fact

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A major impetus for the theory since the 1990s has been to try and solve those descriptive problems, many of which are still being debated. At the same time – particularly as linguistic theory became more feasible in terms of the picture of language acquisition implied by parameter setting – theorists became increasingly concerned with meta-linguistic matters, such as the form of the principles and parameters being hypothesized.

Regarding parameters, the concern has often been that the proposed statement is sufficiently general. For example, the Head Parameter becomes strengthened as a theoretical proposal if not just VPs present a given order in a language like English, but furthermore this order carries through to other relations between heads and complements of the sort seen in (9) and (10) – as is the case.<sup>16</sup> Indeed the best situation is one in which a parametric choice has consequences for apparently unrelated constructions in a language. For example, subject drop often correlates with post-verbal subjects and absence of overt pleonastic elements.<sup>17</sup> Likewise, it has been argued that, in languages in which complements follow heads, there is usually no overt morphological Case marking in complements and a rather fixed word-order, whereas the converse is true in languages in which complements precede heads.<sup>18</sup> Generalizations of this sort lend some plausibility to relevant parametric claims, and force theorists to look for unexpected, once again modular, connections between different parts of the theory (in this instance, agreement or Case on the one hand, word-order on the other).

A parametric theory of human language also makes predictions about what one should find across languages. The Head Parameter, for instance, predicts languages with heads underlyingly following or preceding complements, but not really the possibility of heads which 'do not care' about their position, or are somehow systematically superimposed onto their complements. Those are, by hypothesis, impossible languages, if the parameter is correct. At the same time, plausibility considerations make the theorist wary of hypothesized parameters that only serve to describe one language. For example, imagine a parameter stating the following:

(18) *The Crazy Parameter*

When a reflexive clitic takes on an impersonal use, it may or may not go with auxiliary *have*.

Interestingly, what (18) states has some real content. In languages with different auxiliaries for various constructions, reflexive clitics with or without an impersonal interpretation do not take auxiliary *have*, but rather auxiliary *be*. But when is the opposite setting of (18) true? To our knowledge, in exactly one instance: the Paduan Italian dialect studied by Kayne (1994):

- (19) Se ga/\*ze bala tuta la note.  
 REFL. has/is danced all the night  
 ‘There was dancing all night long.’

What sort of parameter is that, separating one language from all the rest, in the oddest way? In fact, to make things worse, as Kayne observes, auxiliary *be* can indeed be used even in these impersonal constructions if there is an added reflexive:

- (20) se se gera visti.  
 REFL. REFL. will.be seen  
 ‘One will see oneself.’

In situations of this sort, the theorist wants to go deeper – Kayne of course tries to – and takes facts of this sort as a surface indication of a non-obvious parameter that must be unearthed. We will see in chapter 3.2 that the MP actually takes a very strong position on what are the parameters of human language.

This meta-theoretical reasoning assumes a sort of *structural economy*, in such a way that the theorist is precluded from making certain theoretical statements if they violate the spirit of what is taken to be the structure of language. Of course, nobody really knows what that structure is like, but the minimalist gambles that it is somehow economic, without much room for odd statements about very particular parts of the theory, such as our Crazy Parameter. Again, this is not a necessary assumption, particularly when studying an aspect of the biological world.

That is why the minimalist is worried about (5), the Extended Projection Principle, which looks certainly as ugly and as ad hoc as the Crazy Parameter. In contrast, other principles look considerably more elegant. Take for instance the *Theta Criterion*:

- (21) *Theta Criterion (First Version)*  
 Every argument needs a theta role and every theta role must be assigned.

In other words there must be a match between the theta roles a head has and the types of arguments it takes. This seems a priori more pleasing and reasonable, perhaps as a correspondence between roles and arguments.<sup>19</sup> However, the ‘elegance game’ is dangerous. (21) is certainly rather clean; but is it a true claim about human language? As it turns out, this issue is highly debated in recent years, with Chomsky himself taking opposing views in different papers. We will examine the different alternatives in chapter 7.1, but for now

it should be understood that whereas the minimalist does not ultimately want principles that are not elegant, the mere elegance of a principle does not sanction its place in Universal Grammar (UG). As Chomsky likes to say, if only elegance were at stake, we'd hand the problem of human language to mathematicians.

Another principle we have implicitly assumed, and which is also fairly elegant, is the Case Filter:

- (22) *Case Filter (First Version)*  
Every NP (in a relevant class) needs Case.

One has to characterize the 'relevant' class in (22) (which as we have already seen is far from clear), but the form of the principle will not really vary depending on that. Note that (22) becomes formally like (21) if, following Fukui and Speas (1986), we add a 'symmetry clause':

- (23) *Symmetry Clause for the Case Filter*  
Every Case feature must be assigned.

Interestingly, harmonizing the Case Filter with the Theta Criterion in terms of (23) derives a lot of facts concerning (5). To see that, recall cases where the violation of the EPP would not follow from the Theta Criterion; for instance, (24):

- (24)a. It seems that John is a liar.  
b. \*Seems that John is a liar.

There is no thematic reason why (24b) should be bad. However, not having a subject, the nominative Case feature associated with the matrix Tense is not assigned, and the sentence runs afoul of (23).

We may call the pair (22), (23) the *Case Criterion*. Does this finally allow us to get rid of (5)? One argument that it does not is provided by verbs like *believe*. A well-known and very interesting property that this verb has is that, apart from assigning a Case to its object, it can 'exceptionally' assign Case to the subject of its complement (see chapters 4 and 5):

- (25) [I believe [John/him to be a liar]].

By any semantic criterion, *John* is the subject of *to be a liar*. Yet the (accusative) Case *John* is receiving is associated with the verb *believe*. This can be demonstrated as follows.

One can show that the infinitival complement of the verb *believe* is similar to the infinitival complement of the verb *seem*:

- (26)a. I believe [John to be a liar]  
b. seems [John to be a liar]  
c. was believed [John to be a liar]

The main difference between these two is that *believes* has a Case feature which *seems* lacks, which presumably relates to the fact that *believe* can take a nominal complement while *seem* cannot. As a consequence, *John* must raise in (26b) (in the transformational sense to be discussed in later chapters) to yield the grammatical *John seems to be a liar*, while this is not the case in (26a). Note, however, that a passive version of (26a) – which by hypothesis is taken to involve the suppression of *believe*'s Case-assigning abilities (see chapter 4.7) – does involve *John*'s displacement to yield the grammatical *John was believed to be a liar* (cf. (26c)). In other words, the passivized *believe* without object Case-assignment abilities and the 'raising' verb *seems* are comparable. Confirmation of the analysis is provided by (27), where we add a 'control' verb like *try* to the discussion:

- (27)a. I tried [<sub>IP</sub>PRO to be a liar].  
 b. \*I tried [<sub>CP</sub> [<sub>IP</sub>John to be a liar]].  
 (cf. I tried the following: that John become a liar.)  
 c. \*was tried [<sub>CP</sub> [<sub>IP</sub>John to be a liar]].  
 (cf. \*John was tried to be a liar.)

The classic account of the paradigm (27) is that, for a 'control' verb, the clausal boundary prevents a Case relation between the matrix verb and the embedded subject. Intuitively, the complement of *try* involves 'more structure' than the complement of *believe* in an instance like (26a). Thus while the subject of *believe*'s complement can receive Case from this verb, the subject of *try*'s complement cannot, and must be instead the presumably Case-less, unpronounced category PRO (although see chapter 7.5 for a refinement). That is consistent with the fact that (27c) is ungrammatical, under the assumption that movement of the sort necessary to make (26b) or (26c) grammatical is not possible across 'too much structure'. As a conclusion, it is reasonable to take *believe* to be assigning (accusative) Case to *John* in (26a).

Now, suppose the complement of *believe* were one of those clauses whose subject is a merely pleonastic element:

- (28) ... [it to seem [that John is a liar]]

If *it* needs Case, it should be able to get it from *believe*. Thus, we can predict the ungrammaticality of (29), where *believe* cannot assign its Case feature, since *it* is missing:

- (29) \*[I believe [to seem [that John is a liar]]].

But now consider (30), involving a 'control' verb which cannot assign Case into its complement clause:

- (30) \*[I tried [[it to seem [that John is a liar]]]].  
 (cf. also [I believe [it to seem [that John is a liar]])

The ungrammaticality of the example can be explained if *it* is among the nominals that need Case – a plausible assumption. But let us finally take away the *it*; then the prediction is clear: if there is no EPP as in (5), the sentence should be good (there being no *it* and no Case feature involved, all the requirements of this clause are met). However, witness:

(31) \*[I tried [[to seem [that John is a liar]]]]

Now the elegant theory that has no EPP (and has ‘deduced’ its effects from Theta/Case criteria) has resulted in the inelegant conclusion of allowing (31). One could try to insist on salvaging the theory by blaming (31) on something else. For instance, one could stipulate that the subject of *try*’s complement needs to be the null category PRO. But is that statement more elegant than (5)?<sup>20</sup> Actually, one can show that even such a stipulation would not be general enough, and it still could not deduce all the effects of (5). Thus, consider (32a), to be compared to (26a):

(32)a. \*[My belief [John to be a liar]] was unfounded.  
 b. [My belief [that John is a liar]] was unfounded.

We can account for (32a) if we assume that, unlike corresponding verbal forms, nominals do not assign ‘exceptional’ Case – surely a reflex of their peculiar Case-assignment properties. In any case, there is nothing obviously wrong with the semantics of (32a), given that (32b) is actually perfect and seems to be semantically equivalent. With all this in mind consider (33):

(33)a. \*[my belief [[it to seem [that John is a liar]]]] was unfounded.  
 b. \*[my belief [[to seem [that John is a liar]]]] was unfounded.  
 c. [my belief [[that it seems [that John is a liar]]]] was unfounded.  
 d. \*[my belief [PRO to be a liar]]

(33a) is correctly predicted to be bad: as we saw for (30), *it* needs (and does not get) Case. So take away *it*, just as we did in (31). As we saw there, the result as in (33b) is bad. Blaming this on the semantics won’t do, given the perfect paraphrase in (33c); and we cannot blame anything on PRO this time, since there is no PRO around (in fact, PRO would be impossible in that context, as (33d) shows). So the ungrammaticality of (31) or (33b) is only explained by the EPP, in that nothing fills the intermediate subject position, as it apparently should.

## 1.7 The Empty Category Principle

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Programmatic desires aside, there are simple instances when it will not be easy to find the most elegant solution. This does not mean that we should lose sight of where we are heading, nor forget that *underspecified*, *symmetric*, *reduced*, and *economic* derivations may well carry us to equally pristine representations.

Surprisingly enough for a biological system, by and large this appears to be true. But the ‘by and large’ hedge is not a cop-out. Sometimes we may hit apparently still irreducible, unnatural properties like (5) – which will not be the only peculiar aspect of our theory. In any case, having a noble goal in mind we can at least ask to what extent certain stipulations are indeed irreducible. This is useful when we raise questions about equally disconcerting *prima facie* glitches: What is the role of morphology? Why does the system have transformations? Is systematic linguistic variation reduced to functional categories? In a perfectly elegant world (or for that matter a formal language, such as Predicate Calculus) there appear to be no morphology, transformations, or functional categories, and the entire ‘sound’ component would seem to be either irrelevant or even absent.<sup>21</sup> Actually, whether or not there ought to be *any* room for inelegance in the linguistic system has been the subject of much meta-theoretical debate. One immediately good aspect of MP has been its ‘therapeutic’ value, in denying us satisfaction in keeping (5) as a principle; if we have it, it is only because we have not found anything better which gets us the facts. But is there more to MP than that ultimately methodological concern? Aren’t those *underspecifications, symmetries, reductions*, and in general *economy* considerations the stuff of just about any scientific theorizing in the natural sciences? Surely no one wants his or her theory to be *overspecified* or *uneconomic* . . .

Here the field has offered two takes on MP that the reader ought to bear in mind. One is a ‘weak’ Minimalist Thesis (WMT), essentially good scientific practice. To put WMT in Hornstein’s (2001) words, fundamental issues haven’t really changed from the P&P model – only progress has been made. Classically, linguists of the generative tradition want to address the acquisition puzzle. The present picture (random fixation of open parametric options) does account, in principle, for standard learnability considerations. Once that is taken care of, other concerns in the natural sciences take over, those which focus (as need may arise) on the naturalness of given domains, elegance in the postulation of hypotheses, and so forth. However, Chomsky for one seems to have in mind a Strong Minimalist Thesis (SMT) which is harder to understand. To try and do this, let us concentrate on a real-life situation, a moment in the recent history of linguistics where Chomsky’s SMT forced the field in a direction different from that predicted by the more conservative WMT.

In the 1980s, many linguists were concerned with the properties and distribution of empty categories, in particular traces of movement, in the sense to be fully examined in later chapters. Importantly, a trace that is not a complement to a lexical head (hence not ‘lexically governed’) was taken to require an antecedent nearby, in such a way that if ‘too much structure’ (which could be characterized as in chapters 3 and 4) separates the trace from the antecedent, an ungrammaticality ensues. Thus the contrast in (34):

- (34)a. [Who<sub>i</sub> did [you think [ [t<sub>i</sub> left]]]]  
 b. \*[Who<sub>i</sub> did [you think [that [t<sub>i</sub> left]]]]



‘TOO MUCH STRUCTURE’

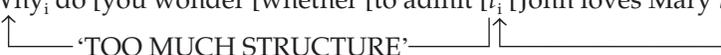
Subject and adjunct traces were shown to differ as follows:

- (35)a. Only subject traces are sensitive to deletable complementizers.  
 b. Adjunct traces require a completely barrier-free path between them and their antecedent.

(35a) can be illustrated by way of (36):

(36) [Why<sub>i</sub> do [you think [that [Mary left John t<sub>i</sub>]]]]

This is to be compared to the ungrammatical (34b); descriptively, the deletable complementizer *that* somehow disallows the argument–antecedent association in (34b), but obviously not the adjunct–antecedent relation in (36). In turn, compare the two instances in (37), where we take the (b) example to be considerably worse than the (a) example – which, although not perfect to some speakers, seems virtually fine to others.<sup>22</sup> Suppose for the sake of the analysis (it will become apparent why) that there is an intermediate trace left by the question word in the domain of the lowest complementizer,<sup>23</sup> which doesn't harbor any lexical entry:

- (37)a. [Who<sub>i</sub> do [you wonder [whether [to admit [t<sub>i</sub> [t<sub>i</sub> loves Mary]]]]]]  
  
 b. \*[Why<sub>i</sub> do [you wonder [whether [to admit [t<sub>i</sub> [John loves Mary t<sub>i</sub>]]]]]]  


Whereas the original argument trace in (37a) only needs to appropriately relate to the intermediate trace (and thus doesn't care about a subsequent 'barrier' higher up in the structure), the original adjunct trace in (37b) seems to be sensitive to the subsequent barrier. The issue is how to rationalize all of this.

Lasnik and Saito (1984) came up with a way of accounting for all these facts, which we will anachronistically present in the cleaner terms of Epstein (1991). Assume the machinery in (38):

- (38)a. Traces are marked + $\gamma$  if bound by an antecedent across no barriers (or if lexically governed).  
 b. Once a trace is marked + $\gamma$ , it cannot be marked – $\gamma$ .  
 c. Once a trace is indexed, it must be marked + or – $\gamma$ .  
 d. A – $\gamma$  trace can be deleted if no information is lost.  
 e. A – $\gamma$  trace left by the end of the derivation is ruled out.  
 f. An argument index must exist in D-structure.  
 g. Free indexation takes place at LF.

None of these claims is unreasonable. The first three are just an explicit statement of the fact that traces that are not lexically governed must locally relate to antecedents, or to put it differently, that *such a trace has an antecedent within some local domain*. Feature +/– $\gamma$ , and the conditions of its assignment, can be seen as a concrete notation to state this precisely. In its turn, (38d) is normal





kills the movement in (43d), even if it's done covertly. Finally, why can't we just delete the offending  $-\gamma$  trace in (43e), just as we did in (40)? The difference is that the intermediate trace in (40) was left *in the overt component*, whereas the intermediate trace in (43e) is left *in the covert component*. We don't have room any more to, as it were, come back and do the deletion we need in order to get rid of the trace in (43e); once the outer 'cycle' of the covert component is reached, as in (43d), we are done with the derivation (see chapter 7 for a more detailed discussion on cyclic considerations).

Lasnik and Saito had not just completely unconstrained rule applications, but in fact they also had unconstrained rule types: theirs was not really 'Move  $\alpha$ ', but, as they called it, 'Affect  $\alpha$ ', which included deletion, insertion – indeed whatever was necessary to accord with the principles. Is that a simpler, more elegant theory? On the methodological grounds that WMT cares about, it certainly is. The classical P&P theory, in addition to (38) in some version, had a further axiom demanding certain defining properties of transformations, such as their being limited to movement or their involving upward paths, and so forth. Call those defining properties D. The Lasnik and Saito take on the model did not have to assume D, hence had one less axiom or set of axioms; that, on anybody's count, should be a simpler theory. Of course, the results were less constrained, in fact so much so that they allowed the kinds of derivational processes that we have just seen in (43) and the like. That was the whole point of that system, and as we saw it had interesting empirical consequences.

Chomsky's early work on MP (e.g. Chomsky 1991) flat-out rejected a grammar that allows the movements back-and-forth that we have just considered. Clearly, he could not have done that on the basis of WMT alone, since that thesis would favor a theory with fewer stipulations, and at the time the ban on lowering was just that. And he certainly did not give up the account on simple empirical grounds. At first his move may seem perplexing: he proposed a new axiom that allowed movement only as 'a last resort' – that is, when motivated on independent grounds (we will discuss this thoroughly in chapter 5). Evidently, that disallows the 'idle' movements above, whose sole purpose was to achieve a grammatical representation. The system at the time allowed transformations if nothing prevented them, while Chomsky's new design allowed them only if something forced their application. On what grounds is adding this axiom *economical*?

## 1.8 A mind plan?

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At stake for Chomsky's SMT is, in large part, the very nature of language. The common-sense view is that language exists because it serves to express elaborate thoughts and feelings, to communicate. This is the naive version of a supposedly much more scientific claim: language has evolved in the human species because language-speaking creatures are biologically more adaptive than their ancestors (see Pinker 1994 for a popular presentation of this view).

Interestingly, Chomsky has always claimed that this view of language makes little sense, surprising even some of the most ardent defenders of his ideas.

Chomsky's position is admittedly provocative. Here is a person who has persuasively argued for the innate properties of human language, and who is yet insistent upon rejecting the standard account we have for innate ideas: instinct. Presumably, instinct is a consequence of the way in which species evolved, itself a consequence of natural selection and related mechanisms. So how can you both claim that language is innate and yet that it is not instinctive? If it is not instinctive, then what is it?

That question is not peculiar to linguistic studies. Current biological research, stemming from the work of D'Arcy Thompson at the turn of the twentieth century, notes that a variety of features of body plans and the morphology of organs, as well as intricate behaviors and systematic social interactions, are not the direct results of natural selection. Nobody denies natural selection as a way for species to become extinct (upon systematically failing to survive some environment), but quite a few people wonder whether natural selection alone is responsible for the intricate features of species of all sorts at various levels of complexity. A group of researchers is starting to search for laws of *complex systems* in nature, at various scales (from the 'merely' physico-chemical to the more articulated biological or sociological levels). Some of these research trends are based on such varied sub-fields as fluid or population dynamics, and their details should not concern us here (see Uriagereka 1998, Hinzen forthcoming). Chomsky's point is this: why could language not be seen in this light as well, together with other studies of organ morphology or systemic behavior?

A cursory look at some of the agreed-upon properties of complex systems should interest any linguist. Observe, for example, what Goldenfeld and Kadanoff – two physicists writing in the *Science* report on 'Simple Lessons from Complexity' (1999:87) – have to say about the equations that describe fluid dynamics, which are derived from three basic ideas:

(44) *Locality* – A particle is influenced only by other particles in its immediate neighborhood.

*Conservation* – Some things are never lost, only moved around (e.g. particles and momentum).

*Symmetry* – A fluid is rotationally invariant.

How many words do we have to substitute in the general statements to the right of the dash for these ideas to be linguistically relevant? How metaphorical would those substitutions be? Suppose we consider the operations that describe derivational dynamics in transformational syntax. The following are not meaningless claims; indeed, we will defend them in this book:

(45) *Locality* – A **category** is **indexed** only by other **categories** in its immediate **domain**.

*Conservation* – Some things are never lost, only moved around (e.g. **lexical information**).

*Symmetry* – An **X'-schema** is **categorially** invariant.

That we should be using terms like ‘category’, or ‘X’-schema’, is hardly worthy of notice, since obviously in our case we’re dealing with those ontologies, not particles or fluids. What seems well worth examining is that the only other significant differences in these statements relate to abstractly similar technical notions.

Of course, to discuss seriously whether these analogies are more than fanciful, we need to at least share the syntactic knowledge presented in this book (and surely more). But two things are certain. First of all, within complexity approaches, one does not need to justify such notions as those in (45) – or the ‘last resort’ character of transformations, or in general conditions on design economy – on any methodological consideration. This is what these systems are all about. The hunch is that under those circumstances life plans emerge, whether it be in bodies or in minds, in part because those conditions create pockets of equilibrium within the dynamics of the physico-chemical channel which foster form, whatever that is.

The second certain thing is that Chomsky believes this, and is not apologetic about his view. We finish this introduction with a recent quote in this respect, from an in-depth interview with Adriana Belletti and Luigi Rizzi (Chomsky 2002). After discussing how natural selection works within a structured range of options given by physical law and historical contingency, and how a tradition in modern biology exploits that idea, Chomsky conjectures:

It is possible that what happened is what Richard Lewontin and others have speculated: the brain was exploding for a million years; it was getting way bigger than among other surviving primates, and at some stage (for all we know about 100,000 years ago) some slight change may have taken place and the brain was reorganized to incorporate a language faculty. Maybe. That would be like angel-fish stripes, polyhedral shells, etc. The emergence seems to be fairly sudden, in evolutionary terms, in an organism with a very large brain, which was developed for whatever reason, and conceivably through some reconstruction of the brain that brought into play physical processes that led to something that works close to optimally, like a virus shell. If the minimalist thesis actually gains some significant credibility, that would be not an unreasonable conclusion; of course you have to establish the thesis.

## Notes

- 1 This was done, in large part, by way of intermediate theoretical devices such as *filters* (see Chomsky and Lasnik 1977).
- 2 Chomsky notes the redundancy already in *Aspects of the Theory of Syntax* (1965:46), but he does not discuss the point further.
- 3 Lasnik and Kupin offer a mechanism of constructing a reduced phrase-marker from scratch without appealing to any PS rules. Their strategy was to claim that any set of mono-strings (strings containing at most one non-terminal element such as VP or NP) is a (reduced) phrase-marker, and then to impose representational conditions on its completeness, consistency, etc.
- 4 Evidence for this position, as opposed to treating ‘s as a suffix on *Rome*, comes from the behavior of *whose* (= *who’s*), as in *whose book did you buy?* If ‘s is treated as

a D-head, we can readily explain why the following is bad: *\*whose did you see [t book]*. Moving *whose* only would target a non-constituent (Specifier + head to the exclusion of the complement). On the other hand, if 's were simply a suffix, this ungrammaticality would be unexplained.

- 5 To be fair, in so-called ergative languages such as Djirbal, it has been noted that patient expressions appear in subject position; these have been analyzed, however, in transformational terms, of the sort to be discussed in ch. 3 and subsequent chapters. For an overview of 'ergativity' within generative grammar, see Ura (2001).
- 6 Such an approach provides a natural way of encoding thematic hierarchies of the type Jackendoff and others posited in the early 1980s, by capitalizing on syntactic hierarchies arising from sheer constituency.
- 7 Another reason why thematic information cannot be reduced to subcategorization is that, as we have seen above, the latter doesn't tell us whether a predicate has a thematic subject or not while its thematic information does. See ch. 7.1 for more discussion on thematic roles.
- 8 By 'outer' we simply mean that those components have properties that are more directly manifested in the outside world (sounds, signs, etc.).
- 9 We understand 'level' in the classical sense (Chomsky 1955): as consisting of elementary units (primes), an operation of concatenation by which strings of primes can be constructed, and various relations defined on primes, strings of primes, and sets and sequences of these strings, together with some criterion for unification of the various formal objects into a coherent whole. The Multiple Spell-out hypothesis, to be entertained in ch. 2.6, rejects the idea of LF and PF levels by not forming a fully connected string of primes.
- 10 The issue revolves around the proper characterization of 'covert' movement processes, which we discuss in chs. 6 and 7.
- 11 This is assuming, as we did in section 1.2, that the syntactic encoding of propositions is as S. It is actually not clear why that is (see Carstairs-McCarthy 2000), or whether the child does possess, in some form, the same capacity the linguist should have, regarding these sorts of higher order deductions (which would allow him to deduce semantic information from syntactic analysis).
- 12 Light verb constructions in Japanese (i) (Grimshaw and Mester 1988; Saito and Hoshi 2000) may provide another instance of the formal property of Case under discussion.

(i) John-ga Mary-ni [toti-no zyooto]-o si-ta.  
 John-NOM Mary-DAT land-GEN giving-ACC do-PAST  
 'John gave a land to Mary.'

In (i), the verbal noun *zyooto* 'giving' receives accusative Case from the semantically defective 'light verb' *su* (or incorporates into *su* in LF) even though the nominal receiving Case is not an argument of the Case-marking element.

- 13 Given the impossibility of simultaneously pronouncing two different speech items, the Head Parameter must be set rigidly one way or the other.
- 14 Chomsky tried to finesse the first of these problems by saying that 'inherent' Case is assigned to one of the complements, the direct object. Unfortunately, Chomsky has used the word 'inherent Case' in too many different senses, and resorting to the notion 'inherent' Case in this particular instance is arguably not very principled. There is a fairly principled use of inherent Case which is introduced in *Knowledge of Language* (KoL) (1986b), when Chomsky discusses *of*-insertion. In particular, Chomsky argues that the *of* we see attached to *Rome* in *the destruction of Rome* is a form of inherent Case, which makes *Rome* satisfy its Case requirements.

From this perspective, we do not say that N and A do not assign Case, but rather that they assign *inherent* Case – whereas V and P assign *structural* Case. One might conceivably argue that the *to* attached to *Mary* in *John gave a book to Mary* is an inherent Case marker, but it is harder to see how this line of reasoning extends to *a book* in *John gave Mary a book*.

- 15 Note that if *of* is taken to be a form of inherent Case, as Chomsky suggested in KoL (see the previous note), we would have to say that, at least in English, clauses resist inherent Case assignment, hence are impossible as complements of *of*, as in (15b).
- 16 From this perspective, German is a problem, since it exhibits mixed-headedness characteristics. Kayne's (1994) Antisymmetry hypothesis (which we discuss in ch. 2) offers a solution by assuming that all languages are head-initial, apparent head-final structures being derived from (massive) movement operations.
- 17 The correlation is not always observed. Chinese, for one, lacks post-verbal subjects despite being *pro*-drop, while Portuguese shows overt expletives alongside null subjects.
- 18 Again, the generalization does not always hold. Icelandic is consistently SVO (Subject–Verb–Object) but shows rich Case marking, while Dutch is head-final in the same contexts as German is (see note 16 above), but does not display any overt Case marking (except for pronouns).
- 19 Although note that, stated as in (21), the criterion does not entail that arguments and roles must be in one-to-one correspondence. What would give us that correspondence would be some constraint to the effect that arguments cannot move to a theta position, or a theory saying that the level of D-structure has to satisfy the theta requirement in (21). See ch. 7.1 on this.
- 20 Much depends on the treatment of PRO, a matter we address in chs. 4 and 7.
- 21 Indeed philosophers of language explicitly set out to 'liberate' language, in the formal sense they were interested in exploring in the late nineteenth and early twentieth century, from the 'inadequacies' of morphology, stylistic expression (that is, movement and the like), or actual linguistic variation. A 'formal language' is supposed to have none of these glitches, and is thus in a sense a mathematical form, pure and eternal, without any of the properties that most interest the working linguist. On these and related matters, see Hinzen (forthcoming).
- 22 Linguists of our orientation are interested in relative native judgments, which should be taken as small experiments about a speaker's knowledge of his or her language. Indeed, from this perspective, there is no such thing as 'total ungrammaticality', but simply a degree of perceived deviance which the theorist seeks to account for in some interesting way.
- 23 When Lasnik and Saito made their analysis (1984), it was assumed that Wh-words move literally through complementizers. Later on the theory evolved to the point where movement was assumed to be through the *specifier* of a complementizer, as indirectly alluded to in section 1.2 above. Later on (1992), Lasnik and Saito adapted their analysis to the new more principled formulation. See ch. 7 for further elaboration on these matters.