

THE RATIONALITY OF CHOMSKY'S LINGUISTICS AS INSTANTIATED  
BY THE DEVELOPMENT OF BINDING THEORY

by

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## Chapter 1

## INTRODUCTION

A notable feature of Chomsky's work on universal grammar (UG), is certainly the great many changes which this theory has undergone over the years.<sup>1)</sup> Binding theory, a fundamental component of all recent versions of UG proposed by Chomsky, provides an illustration of this point. Binding theory has been developed from two conditions proposed in the early seventies, namely the Specified Subject Condition and the Tensed S Condition.<sup>2)</sup> These conditions underwent numerous changes until, in 1978, they were reformulated as the so-called OB-binding theory. Since then, at least three different versions of binding theory have been proposed by Chomsky.

The greatest part of the present study consists of a detailed description of the changes which binding theory has undergone through the years, from the earliest formulations of the Specified Subject Condition and the Tensed S Condition, up to the most recent version of binding theory. Since binding theory constitutes such a fundamental component of UG, a detailed study of the developmental history of binding theory would be justified in its own right. Such a study could yield valuable insight for linguists - both those who accept Chomsky's assumptions about language and linguistic inquiry and those who reject these assumptions - into what has happened in Chomsky's linguistics over the past decade.<sup>3)</sup>

While a detailed study of the developmental history of a fundamental component of Chomsky's linguistic theory could be interesting in its own right, such a study would derive its real significance from the insight it could yield into the nature of rationality in Chomsky's linguistics. And the central aim of the present study is to acquire such insight into the nature of Chomsky's rationality. In § 7.2 below an account will be pre-

sented of what constitutes rationality in Chomsky's linguistics, an account which is based on the developmental history of binding theory. In essence, this account has the following components:

- i) A specification of what constitutes the goal of Chomsky's linguistic theory.
- ii) A specification of the principles of theory appraisal employed by Chomsky.
- iii) A specification of how the latter principles are used to attain the former goal.

It will subsequently be determined whether each of the changes Chomsky made to binding theory can be explained in terms of his goal and principles of theory appraisal. Before this notion of 'explanation' can be clarified, it is first necessary to consider what changing from one theory to another involves. Let the developmental history of binding theory be represented as a chronologically ordered series  $T_1, T_2, \dots, T_n$ , with  $T_1$  the first version of the Specified Subject Condition and the Tensed S Condition, and  $T_n$  the most recent version of binding theory. Alternatively,  $T_1$  can be interpreted as that version of UG which incorporates the first version of the relevant conditions,  $T_2$  the version of UG which incorporates the second version of the conditions, and so on, and where  $T_x$  and  $T_{x+1}$  differ only in that they incorporate different versions of binding theory. The term "theory change", as used in this study, denotes the replacement of any version  $T_x$  in a series such as the one defined above by a modified version  $T_{x+1}$ . Obviously, each case where  $T_x$  is replaced by  $T_{x+1}$  can be seen as an instance where  $T_x$  is chosen in preference to  $T_{x+1}$ . In the remaining part of this discussion the terms "theory change" and "theory choice" will be used interchangeably to refer to a transition from  $T_x$  to  $T_{x+1}$ , depending on the context of the discussion.

To explain/ . . .

To explain a transition from  $T_x$  to  $T_{x+1}$  -  $T_x \rightarrow T_{x+1}$  for short - that occurred during the developmental history of binding theory, is to show that, given Chomsky's goal and his principles of theory appraisal, replacing  $T_x$  by  $T_{x+1}$  was the best thing Chomsky could do. Or, to put it differently, given Chomsky's goal and principles of theory appraisal, and given the choice between  $T_x$  and  $T_{x+1}$ ,  $T_{x+1}$  was the better alternative. Insofar as such explanations can be provided for the various transitions that occurred during the developmental history of binding theory, these transitions can be said to instantiate the rationality of Chomsky's linguistics - or Chomsky's rationality, for short.

The precise content that will be assigned in this study to the notions 'rational' and 'rationality' will be outlined in § 2.2 below. However, in order to prevent any misunderstanding, it is necessary to note that the word "rational" is used here in the sense in which it is antonymous to "irrational", and not in the sense in which it is antonymous to "empiricist".<sup>4)</sup> It is well-known that Chomsky is a rationalist in the second sense of "rational" identified above. Chomsky's rationalism in this second sense will be dealt with below only insofar as it directly bears on his rationality, in the first sense of "rational" identified above.

An investigation into Chomsky's rationality is of interest within two different contexts: a narrower linguistic context, and a wider metascientific context. Within the narrower linguistic context, an explication of what constitutes rationality in Chomsky's linguistics should make it clearer what it means to work within this linguistic framework. The potential usefulness of the present study is enhanced by the fact that it will present analyses of a great variety of factors relevant to theory choice in Chomsky's linguistics. These factors will be incorporated in the account of Chomsky's rationality. In addition to the more conventional factors such as increased explanatory and predictive success, close attention will also be paid to the use of *ad hoc*

devices by Chomsky to protect his theories from potential negative evidence, his strategy of putting aside potential negative evidence which threatens his theories, and the important role which certain conceptual properties of linguistic theories play in the appraisal of these theories. An attempt will be made in § 7.2 to show that these apparently unconventional aspects of Chomsky's method do indeed fit in with his overall rationality.

That there is a need for such a clarification of Chomsky's rationality cannot be denied. It is well-known that there is a great deal of disagreement among linguists about the merit of almost every nontrivial hypothesis about the structure of language. Of particular interest is the disagreement that exists about components of UG which Chomsky values very highly. Binding theory and trace theory are two such components.<sup>5)</sup> There are indications that some disputes about the merit of such components of UG involve, amongst other things, disagreement about the appropriate criteria for theory appraisal. For instance, Chomsky and Lasnik's (1978:272) rejection of Postal and Pullum's (1978) criticism that one of the hypotheses of trace theory is *ad hoc* indicates a disagreement about the conditions under which *ad hoc*-ness seriously undermines a theory. Many controversies about the merits of components of UG in part spring from different views on the importance that should be attached to counterevidence. The analyses presented below will show that this is the case for the disagreement between Chomsky, on the one hand, and linguists such as Postal, Pullum, and Brame, on the other hand, about the merit of binding theory. An inquiry into Chomsky's rationality derives part of its interest from the possibility that it might help linguists to understand the issues involved in such recent disputes about the merit of linguistic theory.

There is a possible objection to the proposed inquiry into Chomsky's rationality. It could be argued that this study has only a very limited contribution to make since the nature of Chomsky's rationality is understood sufficiently well. The

proponent of such a view could present two considerations in support of this view. First, he could claim that in his recent works Chomsky devotes a great deal of space to explicate his views on scientific rationality and the best method of inquiry to be adopted by linguists.<sup>6)</sup> Second, he could claim that there are already many methodological studies of various aspects of Chomsky's linguistics.<sup>7)</sup> The correctness of these two considerations cannot be disputed. However, it certainly does not follow from this that the present study cannot contribute anything of significance to our understanding of Chomsky's rationality. Closer examination of the two considerations mentioned above rather reveals that they highlight the need for an inquiry such as the present one.

In the first place, it cannot simply be taken for granted that Chomsky's metatheoretical remarks on his research practice accurately characterize the properties of this practice. In the case of the natural sciences there is evidence of discrepancies between what scientists "preach" and what they "practise".<sup>8)</sup> Given such discrepancies in the natural sciences, it would be unjustified to assume that linguists' metatheoretical comments - including those by Chomsky - necessarily provide correct answers to questions about their actual research practice. Moreover, there is some evidence that Chomsky's metatheoretical comments do not always accurately reflect his practice. In this connection, Botha (1982a) argues persuasively that Chomsky's metatheoretical comments about the so-called Galilean style of inquiry do not provide much insight into the way Chomsky currently conducts linguistic inquiry. An account of Chomsky's rationality, based on his actual research practice, can hopefully provide an answer to the question of the accuracy of Chomsky's comments on this practice. The question of Chomsky's metascientific comments is taken up in § 7.5 below. One of the main conclusions of § 7.5 is that these comments are in various respects inaccurate. In the second place, while there are many methodological studies of Chomsky's linguistics, there are to my knowledge no studies which,

individually or jointly, provide a clear and systematic account of what constitutes rationality in Chomsky's linguistics.

The present study will analyse Chomsky's rationality from two complementary angles. First, Chomsky's rationality will be reconstructed in terms of his own goal for linguistic theory and his own beliefs about the factors which are relevant to theory appraisal. Second, Chomsky's rationality will be appraised with reference to the standards embodied in two recent models of scientific rationality, namely the models of Laudan and Newton-Smith.<sup>9)</sup> It will be argued in § 7.4 that Chomsky's rationality is consonant with the model developed by Newton-Smith. On the basis of the former reconstruction of Chomsky's rationality, it will moreover be argued that some recent accounts of theory appraisal within Chomsky's linguistics contain inaccurate claims about Chomsky's research practice. In particular, the accounts of Cook (1981) and Lightfoot (1982) will be shown to be inadequate.

Let us now consider the potential significance of the present inquiry into what constitutes rationality in Chomsky's linguistics within the wider metascientific context. The nature of scientific rationality is one of the most important issues in the philosophy of science, as is evidenced by the seemingly endless stream of publications that deal with it. Hacking (1983:1) singles out scientific rationality as one of two issues to have "obsessed" philosophers of science since the sixties.<sup>10)</sup> The present study is intended to make some contribution to the current debate on scientific rationality. First, this study should shed some light on the similarities and differences between rationality in Chomsky's linguistics and rationality in the natural sciences, as the latter is construed within the models of Laudan and of Newton-Smith. Such a comparison is of interest in view of Chomsky's claim that linguistics should adopt the method of the successful physical sciences.<sup>11)</sup> Second, this study of Chomsky's rationality will contribute to the appraisal of some of the contrasting claims made by Laudan and by Newton-Smith on scientific

rationality. In § 2.3 below some of these contrasting claims are explicated, and in § 7.4 these claims are appraised with reference to the account of Chomsky's rationality presented in § 7.2.

With regard to the aim of the present inquiry, there are two points that must still be clarified here: (i) why the focus is on *Chomsky's* linguistics, and (ii) why the developmental history of *binding theory* has been chosen as the source of data for the inquiry.

The nature of Chomsky's linguistics can be clarified with the aid of the following two distinctions: (i) the distinction between generative and non-generative linguistics, and (ii) the distinction between Chomskyan and non-Chomskyan linguistics. The fundamental assumption of generative linguistics is that scientific grammars of human languages must be generative systems, that is, they must be completely explicit.<sup>12)</sup> Any form of linguistics which do not adopt this assumption is non-generative. Within the domain of generative grammar a further distinction must be drawn between Chomskyan and non-Chomskyan linguistics. Chomskyan linguistics incorporates the assumption that the logical problem of language acquisition constitutes the fundamental problem of linguistics. The aim of linguistic theory is to explain how a person can acquire knowledge of his language. The object of inquiry is thus a mental capacity of humans, as is made clear by, for example, Chomsky (1980a:47ff).<sup>13)</sup> It follows that Chomskyan linguistics is mentalistic. Chomskyan linguistics must be distinguished from non-Chomskyan approaches, which are nonmentalistic and do not have a mental capacity as object of inquiry. An example of such a nonmentalistic approach is Katz's Platonism. In his recent book Katz (1981) argues that linguistics must aim at the description of a non-physical, abstract object "language".<sup>14)</sup>

Chomsky's linguistics is a form of generative linguistics and, more specifically, a form of Chomskyan linguistics. What dis-

tinguishes Chomsky's linguistics from the work of other linguists who adopt the Chomskyan generative approach to the study of language, is the set of assumptions which Chomsky holds at any particular stage about the structure of human language. His recent work, *Lectures on government and binding* (1981a), provides a good overview of the specific assumptions that Chomsky has recently held about the structure of language, or, strictly speaking, of grammar. Other Chomskyan linguists make different assumptions about the structure of language. For instance, Freidin and Koster, in contrast to Chomsky, claim that the grammars of natural languages do not contain transformational rules.<sup>15)</sup>

This study of rationality in linguistics deals exclusively with Chomsky's linguistics, in the narrow sense defined above. This focus is justified by the central role which Chomsky has played in developing Chomskyan linguistics, and, more generally, generative linguistics.

The choice of the developmental history of binding theory as the source of the data for this inquiry into the nature of rationality in Chomsky's linguistics can be motivated with reference to two considerations. Firstly, binding theory has had a fairly long and eventful history. The Specified Subject Condition and the Tensed S Condition - the predecessors of the current binding theory - have undergone numerous changes from the time of their introduction in the early seventies. As pointed out above, binding theory itself has undergone several changes. Consequently, it is reasonable to expect that the history of binding theory will provide a sufficiently rich corpus of data for pursuing the central aim of this study.

Secondly, both the original conditions and the current binding conditions are fundamental components of the versions of UG to which they belong. On the one hand, binding theory is fundamental in the sense that it interacts with various other components of Chomsky's theory, for example, with trace theory, with

Case theory, with the autonomy thesis, and with government theory.<sup>16)</sup> Consequently, it seems reasonable to expect that an analysis of the developmental history of binding theory will also yield some insight into the development of other components of Chomsky's theory. On the other hand, binding theory is fundamental in the sense that it is closely involved in fundamental conceptual developments in Chomsky's wider framework. The work in which the Specified Subject Condition and Tensed S Condition were originally proposed - "Conditions on transformations" (Chomsky 1973) - is widely recognized as one of the landmarks in Chomsky's linguistics, introducing the so-called "Conditions-framework".<sup>17)</sup> Chomsky (1982a:41) states that his own personal feeling is that (Chomsky 1973) "is the first work that I have done that may lead to the possibility of a conceptual revolution . . .". There are indications that the reformulation of the Specified Subject Condition and Tensed S Condition as binding conditions is also related to another development in Chomsky's framework. Referring to one of the reformulations of the binding conditions, Chomsky (1982a:75) suggests that this represents a "qualitative improvement".<sup>18)</sup>

As regards the organization of the rest of this study, it should be clear from the discussion above of the aims of this study that chapter 7 is the pivotal chapter. Chapter 7 presents an account of what constitutes rationality in Chomsky's linguistics (§ 7.2), an appraisal of Chomsky's rationality in terms of general norms such as absence of inconsistencies (§ 7.3), a comparison of Chomsky's rationality with the standards of scientific rationality contained in the models of Laudan and of Newton-Smith, respectively (§ 7.4), and an appraisal of the accuracy of Chomsky's metascientific comments on his research practice, as well as a brief appraisal of the accuracy of Lightfoot's and Cook's characterizations of Chomsky's research practice (§ 7.5).

Chapters 2 - 6 provide the background necessary for chapter 7: chapter 2 the philosophical background, and chapters 3 - 6 the

linguistic background. In chapter 2 the approach adopted here towards reconstructing Chomsky's rationality is set out (§ 2.2), and an account is presented of the models of scientific rationality proposed by Laudan and by Newton-Smith. In chapters 3 - 6 the developmental history of binding theory is outlined. Chapter 3 deals with the stage in which the Specified Subject Condition and the Tensed S Condition were interpreted as conditions that restrict the applicability of both syntactic transformations and rules of semantic interpretation. Chapter 4 deals with the stage in which the conditions were interpreted as restricting the application of rules of semantic interpretation only. Chapter 5 deals with the introduction of the OB-binding theory. Chapter 6 deals with the developmental history of the GB-binding theory. The reader who is not particularly interested in the finer details of Chomsky's binding theory may skip chapters 3 - 6, and only return to them when reading chapter 7.<sup>19)</sup>

The work is concluded with a brief summary of the main conclusions (chapter 8).

Footnotes/ . . .

Footnotes to chapter 1

1. For an explication of the notion 'universal grammar', cf. § 3.2.3 below.
2. In essence, binding theory stipulates in what domains the interpretation of an NP may be, or must (not) be, dependent upon another NP. Cf. chapters 3 - 6 below for a detailed exposition of the content of the various versions of binding theory, including the Specified Subject Condition and the Tensed S Condition. Note that the latter condition is also known as the "Propositional Island Condition".
3. Cf. the discussion of the notions 'Chomskyan linguistics' and 'Chomsky's linguistics' below for more detail on what these assumptions are.
4. Agassi (1981:25) provides the following useful clarification of the two distinct senses of the words "rational/rationalist":

"One meaning is that which is exhibited in the contrast between rationalism and irrationalism, namely rationalism as the view that man can and ought to use his reason or intellect to determine his beliefs, guide his actions, etc. The other meaning is that which is exhibited in the contrast within the rationalist school between rationalist and empiricist sub-schools, namely rationalism as the view that the grounds of reason are in the intellect itself rather than in the senses."

Chomsky's rationalism is in fact much stronger than the view explicated by Agassi. An essential part of Chomsky's rationalism is the assumption that a significant part of what Agassi calls "the grounds of reason" is in fact innate.

5. Cf. footnote 2 above for a brief informal statement of the central idea of binding theory. Trace theory, in essence, stipulates that all phrases moved by transformational rule leave behind a trace which marks the position from which the phrase is moved.

Since the introduction of the Specified Subject Condition and the Tensed S Condition in (Chomsky 1973), a number of works have appeared which are highly critical of these conditions. Cf., for example, Postal 1976, Bach and Horn 1976, Bach 1977, Brame 1977, 1979, Grosu 1978, Iwakuro 1980, Nanni and Stillings 1978, Pullum 1979a. In spite of these criticisms, Chomsky has retained the conditions. Recent developments, for instance, the replacement of the Specified Subject Condition and Tensed S Condition by the OB-binding theory - cf. chapter 5 below - have also been controversial. For example, while Chomsky (1981a:156) claims that the latter theory "has many desirable properties and considerable empirical support", Brame is highly critical of the OB-binding theory. Brame (1979:111) claims that the binding conditions, "very much like the earlier ones, serve to describe rather than explain the relevant range of data investigated". He (1979:114) also calls the conditions "*ad hoc*". Bresnan (1982b) also rejects the GB-binding theory.

Similar disagreement exists with respect to trace theory. While trace theory forms an integral component of all versions of UG proposed by Chomsky since the middle seventies up to the present, linguists such as Postal, Pullum, Barsley and Brame are highly critical of trace theory. Consider, for instance, the highly negative appraisals of trace theory by Pullum and Barsley (1980:96-7) - in (i) below - and Brame (1979:13) - in (ii) below.

- (i) "We believe that the current interest in elaborating TTC (= trace theory - M.S.) that has been evinced in some quarters is highly premature, if not completely misguided. Not enough has been set out explicitly to make it clear that there is any theory to be elaborated under the banner of TT, and what little has been made clear in the informal and disorganized work that has been published seems to rest almost entirely on two claims that we have argued are false . . .

This is a highly negative conclusion to come to, but we think it is an inevitable one."

(ii) / . . .

- (ii) "As time now runs out on trace theory, one sees ever more far-fetched devices proposed to accommodate counterexamples that genuinely follow from more realistic approaches. Just as generative semanticists were inspired to propose global rules and other prophylactic devices to immunize their theory against refutation, so also trace theorists have begun to follow suit by adopting theoretical constructs which are seldom made explicit."

Other works in which trace theory is criticized include (Postal and Pullum 1978), (Pullum and Postal 1979), (Pullum 1979b).

6. Cf., for example, Chomsky 1978a:9-10; 1978b:13-16; 1979a: 57, 73, 107-8, 177, 178f; 1980a:1-12, 24, 218. Some of Chomsky's close followers also pay much attention to these issues. Cf., for example, Koster 1978a:8f, 31, 38f, 59f; 1978b:566ff., 590; 1980:226; Hornstein and Lightfoot 1981b; Lightfoot 1982:55.
7. Cf., for example, Botha 1978, 1979, 1980, Ringen 1975, Sinclair 1977, 1978, Winston 1982, Cook 1981, to mention but a few examples.
8. Cf., for example, Sabra 1967 for examples of discrepancies between what scientists claimed to be doing and what they actually did in the history of optics. Cf. also the discussion in § 2.2 below, and the references cited there.
9. Note that in the rest of this work the term "model", rather than "theory", is used to refer to the different theories of scientific rationality and progress. The term "theory" is used exclusively to refer to the linguistic theories that are being investigated.
10. The other issue singled out by Hacking is scientific realism.

11. Cf., for example, Chomsky 1980a:9, 219.
12. Chomsky (1965:4) characterizes a generative grammar as follows.
 

"If the grammar is, furthermore, perfectly explicit - in other words, if it does not rely on the intelligence of the understanding reader but rather provides an explicit analysis of his contribution - we may (somewhat redundantly) call it a *generative grammar*."
13. These points about the fundamental problem of Chomsky's linguistics and its object of inquiry is dealt with more extensively in § 2.3 below.
14. Note, incidentally, that there was a time that Katz worked within the Chomskyan approach to linguistics. In particular, he assumed that a grammar is a theory of a human mental capacity. In the introduction of his book Katz briefly outlines why his views changed.
15. Cf. Chomsky 1981a:46 for a discussion of this point. More typical examples of cases where Chomskyan generative linguists have expounded views on the structure of language that differ from Chomsky's, can be found in *Levels of syntactic representation* (Koster and May (eds.) 1981), and *Theory of markedness in generative grammar* (Belletti, Brandi and Rizzi (eds.) 1981).
16. The discussion by Newmeyer (1980:Chapter 6) highlights the fundamental nature of the Specified Subject Condition and the Tensed S Condition during the middle seventies. Heny (1981b:10) claims that the binding conditions form "the heart" of the new framework that has been developed since the late seventies.
17. Cf., for example, Koster 1978a:551, and the remarks by Huybregts and Van Riemsdijk in the Preface to *On the*

*generative enterprise* (Chomsky 1982a).

18. The sense in which this reformulation represents a "qualitative improvement" in Chomsky's view will be examined in detail below.
19. (Sinclair 1982) contains an earlier version of the account of the developmental history of binding theory presented in chapters 3 - 6.

Chapter 2/ . . .

## Chapter 2

### PHILOSOPHICAL BACKGROUND

#### 2.1 Introduction

The expressions "scientific rationality" and "the rationality of science" are multiply ambiguous.<sup>1)</sup> This ambiguity carries over to the expression "the rationality of Chomsky's linguistics". The first task to be undertaken in chapter 2 is then to clarify the content of the notion 'rationality' with which the present study is concerned. This task is attempted in § 2.2 with the aid of Newton-Smith's distinction between minimal and maximal rationality.

It was stated in chapter 1 that one of the fundamental aims of the present study is to compare the rationality of Chomsky's linguistics (Chomsky's rationality, for short) with the accounts of scientific rationality provided by Laudan and by Newton-Smith. Since it cannot be assumed that linguists are familiar with the content of these two models of scientific rationality, an account of these models is presented in § 2.3.

One of the aims of the present study is to determine the accuracy of Chomsky's meta-comments on his work. In recent years Chomsky has devoted a great deal of attention to a certain style of inquiry which he calls "the Galilean style of inquiry". The question naturally arises to what extent Chomsky's work on binding theory has been conducted in this style. To serve as background to the analyses in chapters 3 - 6, a brief outline of the "Galilean style of inquiry", as seen by Chomsky, is presented § 2.4.

During the course of the discussions in §§ 2.2 - 2.4 it will also be made clear what method will be employed for the proposed reconstruction of what constitutes rationality in Chomsky's linguistics.

## 2.2 Minimal versus maximal rationality

Newton-Smith (1981:4) characterizes a model of rationality as comprising two components.<sup>2)</sup> First, it comprises a specification of what constitutes the goal of science (for example, the production of true explanatory theories, or the production of theories with maximal problem-solving effectiveness). Second, the model comprises a specification of a principle or set of principles for comparing rival theories against a given evidential background. These principles rate the extent to which theories actually achieve or are likely to achieve the goal in question.

How can a particular model of rationality be used to explain a change in a specific scientific theory (or, equivalently, a choice between two specific theories)? For reasons that will soon become clear, it will be useful to adopt the following answer provided by Newton-Smith (1981:271).

- (1) "To claim that a particular rational model can be used to explain a particular transition in the history of science is to claim that by and large the members of the community had as their goal the goal posited by the model, and that they made their judgments as to which theory was best by reference to the principles of comparison specified in the model. It is not enough to show merely that the transition fits the model in the sense that relative to the model the best theory triumphed. We have to show that the model encapsulates the goal and methodology of those concerned in the transition."

This account naturally applies not only to a community of scientists, but also to the decisions of an individual scientist, as Newton-Smith (1981:243) explains.

- (2) "A rational model specifies a goal for the scientific enterprise and a family of principles to be used in deciding between rival theories or research programmes. To use such a model to explain the action of a given scientist would be to show that he had the goal in question and that he believed in the principles, and that the action in question was the best thing for him to do given his goal and those beliefs."

The crucial point to note in connection with the remarks quoted in (1) and (2) is that, in Newton-Smith's view, a model of rationality can be used to explain specific theory choices only if it can be shown that the scientists involved actually had the goal and believed in the principles of theory comparison specified in the model. Newton-Smith further insists that a rational account need not include a normative appraisal of the goal or an evaluation of the truth or falsity, reasonableness or unreasonableness, of the beliefs of the scientists concerned. A rational account of the actions of a scientist that does not include a normative appraisal of the scientist's goal and beliefs is called a "minimal rational account" - "minirat account", for short - by Newton-Smith (1981:241). The conception of rationality which underlies Newton-Smith's account of minimal rationality is sometimes called "instrumental rationality".

As Newton-Smith points out, the vast majority of actions can be given a minirat account. What distinguishes a minirat account of the actions of a scientist *qua* scientist from a minirat account of actions in general, is that in the former case the goals and methods adopted must be "recognizably scientific", as Newton-Smith (1981:24) puts it. He (1981:271) explains that if a scientist's goal is to please the Vatican, or if he believes a specific theory to be the best because his mother told him so after asking her Ouija board, then he does not reach "the standards of scientific rationality". A minirat account of the actions of a scientist *qua* scientist is, according to Newton-Smith (1981:246), an account "in terms of internal factors, factors relating to a conception of the goal of science which is sufficiently close to ours to be legitimately seen as a conception of a goal for science, and factors relating to the relative merits of rival programmes which are sufficiently like the factors we take to be relevant for theory choice to be seen as scientific reasons for theory choice".<sup>3)</sup>

The notion 'recognizably scientific' clearly forms an important part of a general account of scientific rationality, in Newton-Smith's view of the issue. However, for the purposes of the

present study this notion, and any potential problems in precisely defining it, can be ignored. Insofar as the notion 'recognizably scientific' has a clear content, the principles of Chomsky's rationality formulated below are all scientific.

As explained above, a minirat account of the actions of a scientist does not involve any appraisal of the truth or falsity, reasonableness or unreasonableness, of the beliefs on which the actions are based. Newton-Smith (1981:254) assumes that, just as it is possible to provide minimal rational accounts of actions, so it is possible to provide minimal rational accounts of beliefs. In order to provide a minirat account of why someone, S, holds a particular belief, that *p*, it must be shown that within the context S's reasons for believing *p* justified a belief in *p* rather than disbelief or the suspension of judgment. If what S would offer as his reasons for believing that *p* does indeed within the overall web of S's beliefs provide reasons for believing that *p*, then S is "following the dictates of reason", according to Newton-Smith. A minirat account of S's belief that *p* neither involves an evaluation of the reasonableness of someone's here and now believing that *p* nor an evaluation of whether what was taken by S to justify the belief that *p* would here and now count as a reason to believe that *p*. Newton-Smith (1981:254) stresses that this notion of reason is not subjective, but contextualist. Whether something counts as a reason for something else depends on the overall web of beliefs of the individual concerned.

In discussions of the rationality of science, rationality is frequently defined in terms of some specific set of methodological rules. Moreover, it is usually assumed that these rules are universal. This view of rationality is called "idealism" by Feyerabend. He (1978:31-32) characterizes idealism as follows.

- (3) "According to *idealism* it is rational . . . to do certain things - *come what may*. It is rational . . . to avoid *ad hoc* hypotheses, . . . to remove inconsistencies, to support

progressive research programmes and so on. Rationality (justice, Divine Law) are universal, independent of mood, context, historical circumstances and give rise to equally universal rules and standards.

There is a version of idealism that seems to be somewhat more sophisticated but actually is not. Rationality . . . is no longer said to be universal, but there are universally valid statements asserting what is rational in what context and there are corresponding conditional rules."

It should be clear that the notion 'rationality' employed by Newton-Smith in his characterization of minimal rationality is completely different from the notion explicated in (3).<sup>4)</sup> In the idealist view, rationality must be determined on the basis of a normative appraisal of actions relative to a specified set of standards, or methodological rules. As explained above, a minimal rational account of the actions of a scientist need not include a normative appraisal of his goals or his beliefs.

However, Newton-Smith does allow for a normative appraisal of the goal and beliefs of a scientist in terms of a set of standards or methodological rules. A rational account of an action by some individual which includes a positive endorsement of the goal and beliefs of the individual concerned is called a "maximal rational account" - "maxirat account", for short - by Newton-Smith (1981: 258). According to Newton-Smith, a normative appraisal of the (minimal) rationality of a scientist or of a community of scientists against a general model of rationality is relevant when one is interested in the progress made in the domain in question. As he (1981:244) explains, "a rational model will encapsulate our current beliefs about the goal of science and the factors that ought to govern theory choice". If progress has been made in a specific domain, one would expect that the model of rationality in that domain closely resembles such a general model (given, of course, that the model is reasonably adequate).

Newton-Smith thus distinguishes between providing a (minimal) rational account of theory change/choice in some domain and a

normative appraisal of the minimal rationality in that domain. In Newton-Smith's view the rationality of the theory changes in a domain can be determined without reference to the goal and principles stipulated in a general model of scientific rationality. Newton-Smith makes the distinction between a rational account of theory change and a normative appraisal of rationality in order to overcome the problems that arise when current conceptions of rationality are used in the normative appraisal of the activities of past scientists. It is this distinction which distinguishes Newton-Smith's temperate rationalism from the strong rationalism of, for example, Laudan. For the strong rationalist, in contrast to the temperate rationalist, the rationality in a specific scientific domain cannot be determined without reference to the goal and principles of theory appraisal specified in a general model of rationality.<sup>5)</sup> Strong rationalism can in fact be equated with idealism, as defined in (3) above.

It must be emphasized that Newton-Smith's views on what is involved in providing a rational account of the theory choices made by a scientist are by no means unique to him. In the following comments on the rationality of actions, Hempel (1968:282, 283) defines rationality in terms of the objective of the agent and the information available to him at the time. The similarities between this view and Newton-Smith's account of minimal rationality in terms of the goal and beliefs of the scientist are obvious.

- (4) a. "Rationality in the sense here intended is obviously a relative concept. Whether a given action - or the decision to perform it - is rational will depend on the objectives that the action is meant to achieve and on the relevant empirical information available at the time of the decision. Broadly speaking, an action will qualify as rational if, on the basis of the given information, it offers optimal prospects of achieving its objectives."
- b. ". . . to judge the rationality of a decision, we have to consider, not what empirical facts . . . are actually relevant to the success or failure of the action decided upon but what information concerning such facts is avail-

able to the decision maker. Indeed, a decision may clearly qualify as rational even though it is based on incomplete or false empirical assumptions."

Moreover, Hempel (1965:471) explicitly dissociates a rational account of an action from a normative appraisal of the rationality in terms of some theoretical standard of rationality.

Like Newton-Smith, Finocchiaro links rationality with the "dictates of reason", rather than a specific theory of scientific rationality. Thus, in commenting on the epistemological practices engaged in by Galileo, Finocchiaro (1980:191) states that "the real test of their rationality or propriety is in their correspondence to basic and elementary forms of reasoning and argumentation, rather than to philosophically articulated theories of scientific rationality : . . ."

Although Laudan's views on what is involved in determining the rationality of a scientist differ widely from those of Newton-Smith, Laudan (1977:58-59) also acknowledges the relevance of a scientist's methodological beliefs for his scientific practice. He (1977:59) refers to several works in which "overwhelming evidence" is provided "that the methodological beliefs of scientists often do profoundly effect their research and their appraisals of scientific theories". This view of Laudan is taken up again in §§ 2.3.2.3 and 2.3.4.6 below.

Newton-Smith's views on what is involved in providing a minimal rational account of science are also similar to the anthropological approach to science favoured by Feyerabend (1975: 249ff, 1976:311). On the anthropological approach, according to Feyerabend (1976:311), "statements such as 'science proceeds by induction' are *factual* statements of the same kind as statements describing how a particular tribe builds houses, how the foundations are laid . . .". The essence of the anthropological approach is that the scientist's actual thoughts and beliefs must be reconstructed. This clearly corresponds to Newton-Smith's

view that a rational account of theory changes must make reference to the goal actually adopted by the scientist and the beliefs actually held by him. The most important point to note is that, for Feyerabend, this anthropological approach leads to the construction of accounts which he himself would call "rational". Thus, he (1978:159) states that in (Feyerabend 1975:chapter 12) he discussed "*a philosophy that makes sense of Galileo's procedure or, to use less neutral terms, makes it 'rational'*". (The italics are mine.)<sup>6)</sup>

The similarities and differences between Newton-Smith's and Feyerabend's views will be explored in greater detail in § 2.3 below. Feyerabend's anthropological approach was mentioned here only as evidence that Newton-Smith's conception of what is involved in providing a rational account of science is shared by philosophers of science who hold completely different views on other issues.

With the aid of Newton-Smith's distinction between minirat and maxirat accounts of scientists' actions and beliefs, the aims of the present inquiry into Chomsky's rationality can now be defined more precisely. The first aim is to reconstruct a model of what constitutes rationality in Chomsky's linguistics, a model which can provide minimal rational accounts of the various choices made by Chomsky during the developmental history of binding theory. Such a model of rationality is provided in § 7.2 below.

The second aim is to compare Chomsky's rationality with the standards of scientific rationality laid down in two recent models of scientific rationality, namely those of Laudan and of Newton-Smith. (The motivation for selecting these two models is presented in § 2.3.1 below.) Such a comparison will not only make possible a normative appraisal of Chomsky's rationality in terms of current conceptions about the goal of science and the factors which ought to guide theory choice. The comparison will also make it possible to identify potential shortcomings

in the/ . . .

in the relevant models of scientific rationality. A comparison of Chomsky's rationality with the standards contained in the models of Laudan and of Newton-Smith is given in § 7.4 below.

Newton-Smith's notion of a minirat account of beliefs makes it possible to say that a scientist from the past rationally held methodological beliefs which differ from the principles specified in the most adequate contemporary account of the goal of science and of the principles of theory appraisal. In the case of a contemporary scientist, one would expect him to hold the best available beliefs, that is, the beliefs encapsulated in the most adequate contemporary model of scientific rationality. Clearly, then, in the case of a contemporary scientist a minirat account of his methodological beliefs cannot be made without reference to an adequate contemporary model of scientific rationality. Consequently, no separate attempt will be made here to provide an extensive minirat account of Chomsky's methodological beliefs. However, in § 7.3 the reasonableness of Chomsky's methodological beliefs will be appraised in terms of certain general norms not specific to any particular model of scientific rationality, for example absence of inconsistencies and avoidance of obscurity in the notions employed in theory appraisal.<sup>7)</sup>

It should be emphasized that by adopting Newton-Smith's distinction between a rational account of the choices made by a scientist and a normative appraisal of this rationality in defining the aims of this study, one is not necessarily committed to Newton-Smith's temperate rationalism as opposed to strong rationalism. As should be clear from the discussion above, the temperate rationalism versus strong rationalism controversy is independent from any dispute about the goal which contemporary scientists have (or should have) or about the principles of theory appraisal which contemporary scientists employ (or should employ). While a study such as the present one can, at least in principle,

throw some light on the second controversy, it cannot throw any light on the first.

Reconstructing a model of Chomsky's rationality which can provide minirat accounts for Chomsky's theory choices is clearly the most fundamental task to be undertaken here. Not only is the reconstruction of such a model in itself one of the main aims of the present study. Such a reconstruction is also a prerequisite for achieving the second main aim, viz. a normative appraisal of Chomsky's rationality against the models of Laudan and of Newton-Smith. A few comments on the method to be employed here in the construction of the model of Chomsky's rationality are thus in order.

As explained in chapter 1 above, Chomsky's rationality will be reconstructed on the basis of a detailed analysis of the various choices made by him during the developmental history of binding theory. These choices are analyzed in chapters 3 - 6 below. While chapter 2 provides the necessary philosophical background for the present inquiry into Chomsky's rationality, chapters 3 - 6 thus provide the necessary linguistic background. For each choice made by Chomsky for a version  $T_{x+1}$  of binding theory over a version  $T_x$  - represented as  $T_x \rightarrow T_{x+1}$ , for short - it will be determined (i) what the respective contents of  $T_x$  and  $T_{x+1}$  are, (ii) what the similarities and differences between  $T_x$  and  $T_{x+1}$  are, and (iii) most important, the reasons for Chomsky's choice of  $T_{x+1}$  (or, to put it differently, the factors in terms of which Chomsky judged  $T_{x+1}$  to be better than  $T_x$ ).

The crucial question to be considered here is how it can be ensured that the model of Chomsky's rationality reconstructed on the basis of such an analysis of the developmental history of binding theory is in Newton-Smith's sense a model of Chomsky's rationality. Recall that Newton-Smith insists that a particular model of rationality may be used to explain scientific changes/choices only if it can be shown that the scientists involved

actually/ . . .

actually had the goal in question and believed in the principles specified in the model.

The recent literature on scientific rationality provides very little guidance on the method to be adopted in reconstructing the actual beliefs of scientists which bear on their rationality. Newton-Smith (1981) does not discuss the question of how one could, or should, show that a scientist has a specific goal and specific beliefs regarding theory appraisal. The aim of most recent case studies is not to determine the rationality of certain theory changes in the way outlined above - i.e., with reference to the actual goal and beliefs of the scientists involved - but rather to show that the changes were rational changes in terms of some general model of scientific rationality.<sup>8)</sup>

In the present study I will adopt what appears to be the best available strategy. In terms of this strategy, close attention must not only be paid to what a scientist actually does, i.e., what choices he actually makes - but also to his comments on these choices and on his method in general. As a consequence, three different types of evidence are in principle available for the various claims made in § 7.2 about Chomsky's rationality.

The first type of evidence is provided by the actual choices  $T_x \rightarrow T_{x+1}$  made by Chomsky. The model must predict, for each case in which Chomsky judged  $T_{x+1}$  to be better than  $T_x$ , that  $T_{x+1}$  is better than  $T_x$ .

The second type of evidence is provided by the actual reasons provided by Chomsky for the choice  $T_x \rightarrow T_{x+1}$ . The proposed principles of theory appraisal must account for the reasons provided by Chomsky for the various individual theory choices analyzed in chapters 3 - 6.

The third type of evidence is provided by Chomsky's metascientific

comments on his method in general. In addition to the technical works in which Chomsky develops his linguistic theory, there are many works in which Chomsky comments extensively on the goal of linguistic theory, the factors relevant to theory appraisal, the nature of progress and rationality, and so on. These works are potentially a rich source of data on Chomsky's beliefs about the goal of linguistic theory, and the principles of theory appraisal. For this reason extensive reference will be made below to these works.

It must be noted that it is not the case that evidence of all three types is provided for each individual hypothesis provided below. In particular, evidence of the third type is not in all cases available. There is no need to assume, for instance, that Chomsky believes in a certain principle of theory appraisal only if he has explicitly commented on this principle.

These remarks on the method to be employed here in the reconstruction of Chomsky's beliefs about the goal of science and the principles of theory appraisal raise numerous questions, many of which will simply have to be put aside.<sup>9)</sup> However, the use made of Chomsky's metascientific comments on his work deserves some amplification.

Reference was made in chapter 1 above to the notorious fact that what scientists do is often quite different from what they say they do. Agassi (1981:262-263) provides some indication of the complexities involved in the appraisal of the accuracy of scientists' comments on their method.

- (5) "It was Pierre Duhem who said, I think that scientists cannot be relied on regarding scientific method since they contradict each other. Alternatively, of course, they can all be relied upon and the conclusion should be pluralistic: there is no scientific method and each man of science is left to his own devices! In other words, though we need not believe what informants say about general matters, perhaps we can believe their own reports! Can we? The question is complicated. Some reports are made as sheer ritual, e.g. when a scientist claims to have gained inspiration from

Chairman Mao's little red book or from Stalin or Marx. Or when a scientist claims to have derived his theory from the facts: as if seeing a falling apple makes one a Newton. Some reports are distorted by a scientist's preconceived notions about scientific method, e.g. when he claims to have observed a fact by sheer accident, which, we know, is *a priori* an insufficient narrative because it omits to tell us why he noted the event and recorded it, etc."

These comments by Agassi indicate that great caution should be exercised in using a scientist's metascientific comments as a source of information on his methodological beliefs. However, it does not follow that a scientist's metascientific comments are of no use in a reconstruction of the scientist's methodological beliefs. In the analyses which are presented below I will assume that, unless there is evidence to the contrary, Chomsky's metascientific comments do reflect his methodological beliefs. This is clearly a reasonable assumption to adopt.

The use made here of a scientist's metascientific comments is by no means a unique feature of the present study. Reference was made above to various scholars who do acknowledge the relevance of scientists' methodological beliefs for their practice. A characteristic of the work done by those scholars is the close attention which they pay to the explicit comments by scientists on methodological issues. Sabra's (1967) study of the development of seventeenth-century theories of light is a case in point. According to Sabra (1967:11), his method was "to compare actual practice, in so far as it can be historically determined, with *the interpretations placed upon it by the practitioners themselves.*" (The italics are mine.)

The possibility of discrepancies between Chomsky's metascientific comments and what he actually does when working on linguistic theory is, of course, not ruled out. By comparing Chomsky's metascientific comments with the results of the analyses of the actual theory choices made by him, such discrepancies can be discovered. In the end it will be possible to appraise the accuracy of Chomsky's comments on his method. Such an appraisal is undertaken in

§ 7.5 below.

It should be emphasized that there can be no *guarantee* that the model presented in § 7.2 captures Chomsky's actual beliefs about the goal of science and the principles of theory appraisal. The claims embodied in this model must be regarded as *hypotheses* about Chomsky's beliefs, hypotheses which can be used to explain the various theory choices made by him.<sup>10)</sup> Like all hypotheses, these hypotheses are in principle open to criticism.

## 2.3 Laudan's and Newton-Smith's models of scientific rationality

### 2.3.1 Preliminary considerations

As was stated in chapter 1 above, one of the main aims of this study is to compare Chomsky's rationality with the standards contained in the models of scientific rationality proposed by Laudan and Newton-Smith. The following account of Laudan's and Newton-Smith's models is to serve as background for this comparison undertaken in § 7.4.

The account presented here of Laudan's and Newton-Smith's models has a second purpose. Both models identify a wide range of different factors which can play a role in rational theory choice. In the case of both models this range is far greater than the range of factors identified in the more familiar models of, for example, Popper and Lakatos. Also, there are a number of specific points on which the models of Laudan and of Newton-Smith make conflicting claims. Given these two factors - viz. the wide range of factors identified by the two models and the differences between them - it is possible to formulate a number of highly specific questions that could be asked about the developmental history of binding theory. Such a set of questions can play a very useful role in the present attempt to describe the developmental history of binding theory, and to reconstruct Chomsky's rationality. The questions could guide the proposed description and reconstruction,

in the/ . . .

in the sense that they identify the issues that must be attended to. In § 2.3.6 below a number of such guiding questions are formulated against the background of the exposition of Laudan's and Newton-Smith's views.

In addition to the models of Laudan and Newton-Smith, there are of course several other views on scientific rationality available which could be used in the reconstruction and appraisal of Chomsky's rationality. The best known alternatives are probably those of Popper, Kuhn, Lakatos, and Feyerabend.<sup>11)</sup> By selecting the views of Laudan and Newton-Smith for the purposes of the present study, I do not claim that an analysis of Chomsky's linguistics in terms of one or more of these other models would not yield any insight. Furthermore, it is not being claimed that, unlike the older models mentioned above, Laudan's and Newton-Smith's models have no serious shortcomings.<sup>12)</sup> As will be pointed out below, Laudan's model in particular has been criticized on various grounds. However, it will be argued that in spite of its obvious shortcomings, Laudan's model makes interesting novel claims which are worth investigating. I also do not assert that the claims contained in Laudan's and Newton-Smith's models are in all respects in conflict with the claims contained in the older models. As will be noted below, many of the insights contained in older models are retained in the two more recent models. This is true even for Feyerabend, whose highly controversial claims about science are at first sight completely irreconcilable with the views of, for example, Popper, Lakatos, Laudan and Newton-Smith, all of whom maintain that science is a rational affair. In the case of Feyerabend the links between his views and those of Laudan and Newton-Smith are less obvious than is the case with the other philosophers mentioned above. For this reason a brief account is presented in § 2.3.5 below of some of the similarities and differences between Feyerabend's views, on the one hand, and those of Laudan and Newton-Smith, on the other hand.

In spite of what is said above, there are nevertheless a number of considerations which point to the special interest of the views of Laudan and Newton-Smith for the purposes of the present study.

First, Laudan and Newton-Smith both have much richer theories of scientific rationality than, for example, those of Popper and Lakatos. In particular, Laudan's and Newton-Smith's models make provision for a great number of nonempirical considerations - i.e., considerations which do not bear on the ability of a theory to fit the facts in its domain - to play a role in theory appraisal. This greater richness constitutes one of the grounds on which Laudan's and Newton-Smith's models have been claimed to be more adequate than the older models. Even a very superficial look at recent developments in Chomsky's linguistics reveals that nonempirical factors play a significant role in this enterprise. There is then some reason to think that the richer models of Laudan and Newton-Smith may be more adequate for an analysis of Chomsky's linguistics than are the older models, with their emphasis on empirical factors.

Second, while they do have some features in common, Laudan's and Newton-Smith's models also differ in fundamental respects. If we were to compare Chomsky's rationality with these two different accounts of scientific rationality, it would be possible to focus on a number of topical issues related to scientific rationality. Even with respect to some of those aspects of Laudan's models which have been criticized, such a comparison will prove to be informative.

There is one more issue that must be dealt with before I can proceed with an exposition of Laudan's and Newton-Smith's models. It concerns the relevance of these models for the

analysis/ . . .

analysis of the small scale theory changes to be investigated in this study. To properly understand what this issue of relevance involves, it is useful to consider the characterization given by Laudan (1977:71) of two uses of the term "theory" within science.

First, the term "theory" is used to denote "a very specific set of related doctrines (commonly called 'hypotheses' or 'axioms' or 'principles') which can be utilized for making specific experimental predictions and for giving detailed explanations of natural phenomena". The examples of such specific theories mentioned by Laudan include Maxwell's theory of electromagnetism, Einstein's theory of the photoelectric effect, Marx's labour theory of value, and the Freudian theory of the Oedipal Complex.

Second, the term "theory" is also used to refer to "much more general, much less easily testable sets of doctrines or assumptions". For instance, one talks about "the atomic theory", or "the theory of evolution", or "the kinetic theory of gases". In each case the reference is not to a single theory, but to "a whole spectrum of individual theories". As Laudan (1977: 72) explains, "the term 'evolutionary theory' for instance, does not refer to any single theory, but to an entire family of doctrines, historically and conceptually related, all of which work from the assumption that organic species have common lines of descent". This second use of the term "theory" is more or less what Newton-Smith (1981:79) has in mind when he states that "in the more colloquial use of the term 'theories' (when, for instance, we talk of the wave theory of light or the atomic theory of matter) theories are taken to be constituted by an evolving system of assertions about some common subject matter . . .".

Philosophers/ . . .

Philosophers of science have used various terms to refer to these general theories: "paradigm" (Kuhn), "research programme" (Lakatos), "research tradition" (Laudan). Philosophers such as Kuhn, Lakatos, and Laudan not only use different terms to refer to the general theories distinguished above. They also disagree about the correct characterization of these general theories. However, for the present purposes the informal characterization of Laudan presented above will suffice.

When talking about theory change, it should be kept in mind that such changes can differ widely in scope. At one end of the spectrum, theory change can be on a very small scale, consisting in the modification or replacement of one of the hypotheses that form part of a specific theory. As pointed out above, such small-scale changes are the object of the present study. At the other end of the spectrum, theory change can be on a very large scale, consisting in the replacement of an entire general theory (paradigm/research programme/research tradition) by another.

Laudan and Newton-Smith, like Kuhn, Lakatos, and Feyerabend, are all ultimately interested in such large scale theory changes, and not in the small scale theory changes to be investigated in this study. Laudan (1977:72) agrees with Kuhn and Lakatos that "the more general theories, rather than the more specific ones, are the primary tool for understanding and appraising scientific progress". Newton-Smith uses the term "theory" in the general sense when he discusses theory appraisal. For instance, he uses the term "theory" to refer to Newtonian mechanics, and to Freud's theory of psychoanalysis, both of which are mentioned by Laudan (1977:78) as examples of general theories/research traditions.<sup>13)</sup> A look at Newton-Smith's list of "good-making features of theories" supports the view that he is primarily interested

in the appraisal of general theories. Considerations such as "track record" and "smoothness" clearly do not bear on the properties of some specific theory, but on the way a general theory developed through time.

Laudan (1977:72) claims that the "modes of appraisal and evaluation" appropriate to specific and general theories are radically different". A question then arises about the relevance of Laudan's and Newton-Smith's views for the analysis of the small scale changes in specific theories to be investigated here. However, closer inspection of the models of Laudan and Newton-Smith reveals that they also make claims about small-scale changes in specific theories.

Laudan's model incorporates an appraisal measure for specific theories. The central notion in this appraisal measure - problem-solving effectiveness - is also the central notion in the appraisal of general theories/research traditions. Moreover, his model for the appraisal of general theories presupposes detailed evaluations of the development of specific theories in terms of their problem-solving effectiveness.<sup>14)</sup>

The factors which Newton-Smith identifies as playing a role in large scale theory changes clearly affect small scale changes in specific theories. For instance, he (1981:228) claims that "the smoothness with which adjustments can be made in the face of failure is an important factor in theory evaluation". This implies that when scientists are considering how to change a specific theory, for example, in the face of counter-evidence, they will be influenced by the fact that the smoothness with which such changes can be made will eventually play a role in the appraisal of the associated general theory. That is, Newton-Smith's model implies that scientists will try to modify specific theories in such a way that the smoothness of the general theory is not adversely affected.

It is/ . . .

It is interesting to note that the main point made above also applies to Feyerabend, whose views are briefly outlined in § 2.3.5 below. The example of theory change to which Feyerabend devotes most of his attention is the Copernican revolution, which consists in the replacement of one general theory by another. The great emphasis which Feyerabend places on the issue of incommensurability also underlines his concern with large scale theory changes.<sup>15)</sup> At the same time, however, Feyerabend tries to analyze what can be called the micro-structure of such changes in detail. This is evidenced by his analysis of Galileo's attempt to promote the Copernican system.

The rest of § 2.3 is organized as follows. In §§ 2.3.2 and 2.3.3 brief overviews are presented of Laudan's and Newton-Smith's models of scientific rationality. The main emphasis is on the claims made by these models about the factors that play a role in theory appraisal at the level of specific theories. In § 2.3.4 the differences between Laudan's and Newton-Smith's models are outlined. In § 2.3.5 Laudan's and Newton-Smith's views are contrasted with those of Feyerabend. In § 2.3.6 a number of specific questions are formulated against the background of §§ 2.3.2 - 2.3.5 which could guide the attempted description of the developmental history of binding theory and the reconstruction of Chomsky's rationality.

### 2.3.2 Laudan's problem-solving model

#### 2.3.2.1 Truth versus problem-solving

One of the most controversial features of Laudan's model, as set out in his *Progress and its problems. Towards a theory of scientific growth* (1977), is the role which he assigns to truth in an account of the scientific enterprise.<sup>16)</sup> Laudan (1977:126) does not deny that scientific theories may be true, or that science may be moving nearer to truth. However, he denies that truth

should/ . . .

should play any role in an account of scientific progress and rationality. Specifically, Laudan (1977:125-6) claims that if truth is taken as the aim of science, then the scientific enterprise cannot be shown to be either rational or progressive. This negative conclusion on the role of truth in an account of scientific progress and rationality is based on two considerations.<sup>17)</sup> Firstly, no one has succeeded in demonstrating that the methods employed in science guarantee that it will reach truth. Secondly, all attempts to reconstruct science as moving closer to truth fail, since no one has been able to say what it would mean to be "closer to truth", or to offer criteria for assessing proximity to truth.

Stated in positive terms, Laudan claims that science can be shown to be rational and progressive if science is taken to be fundamentally a problem-solving activity. According to him (1977:66), the core assumptions of his theory are the following.

- (6) "(1) *the solved problem - empirical or conceptual - is the basic unit of scientific progress; and (2) the aim of science is to maximize the scope of solved empirical problems while minimizing the scope of anomalous and conceptual problems.*" (The italics are his.)

On the basis of (6), Laudan (1977:68) formulates his appraisal measure for specific scientific theories as follows.

- (7) "*the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalous and conceptual problems which the theory generates.*" (The italics are his.)

A specific theory change - for example, where a specific theory  $T_x$  is replaced by another theory  $T_{x+1}$  - is thus progressive if and only if the problem-solving effectiveness of  $T_{x+1}$  is greater than that of  $T_x$ , where problem-solving effectiveness is determined by application of the appraisal measure in (7). Rationality for Laudan (1977:125) consists in making progressive theory

choices, i.e., choices which lead to increases in problem-solving effectiveness. The replacement of  $T_x$  by  $T_{x+1}$  will thus be rational if and only if it is progressive in Laudan's sense.

### 2.3.2.2 Solving empirical problems

Laudan (1977:15) defines empirical problems as "*first order problems*: they are substantive questions about the objects which constitute the domain of any given science". A theory can be regarded as having solved an empirical problem if this theory functions in any schema of inference whose conclusion is an approximate statement of the problem.<sup>18)</sup> Laudan (1977:16-17, 22-26) distinguishes between "solving an empirical problem" and "explaining a fact", claiming that the former notion is the one appropriate to science.<sup>19)</sup>

Laudan's model has built into it the idea that not all empirical problems are equally significant. He (1977:32-40) lists a number of factors that affect the weight of empirical problems.<sup>20)</sup>

While he (1977:32) admits that the criteria he mentions "are not meant to exhaust the modes of rational weighting", he nevertheless presupposes that a calculus of problem weights is possible.

### 2.3.2.3 Solving conceptual problems

One of the prominent features of Laudan's model is the emphasis he places on the role which *conceptual* problems play in theory evaluation. Laudan (1977:48) defines conceptual problems as "higher order questions about the well-formedness of the conceptual structures (e.g., theories) which have been designed to answer the first order questions". Conceptual problems are thus problems exhibited by some theory or another. He (1977:48) points out that in fact "there is a continuous shading of problems intermediate between straight-forward empirical and conceptual problems". For heuristic reasons he concentrates on distant ends of the spectrum.

Two types of conceptual problems are distinguished.

- (i) *Internal* conceptual problems that arise from inconsistencies, conceptual ambiguities or circularity within the theory.
- (ii) *External* conceptual problems that arise when a theory T is in conflict with another theory or doctrine T' which proponents of T also believe to be rationally well-founded.<sup>21)</sup>

Laudan (1977:51-54) argues that it is not only a logical inconsistency or incompatibility that constitutes an external conceptual problem. Under certain conditions joint implausibility and even mere compatibility can also constitute external conceptual problems for the theories involved.<sup>22)</sup>

External conceptual problems can arise from a conflict between a theory and another scientific theory from a different domain, between a theory and the methodological theories of the relevant scientific community, and between a theory and the prevalent world view (Laudan 1977:55-64). In the case of a specific theory, the general theory or research tradition associated with it is identified as the major source of its conceptual problems (Laudan 1977:88). Laudan (1977:81) distinguishes two components of a research tradition. The *ontological* component consists of "a set of general assumptions about the entities and processes in a domain of study". The *methodological* component consists of a set of general assumptions "about the appropriate methods to be used for investigating the problems and constructing the theories in that domain".<sup>23)</sup>

Laudan is not very clear about the exact nature of the relation between a specific theory and its research tradition.<sup>24)</sup> Stated briefly, all the theories belonging to a certain research tradition share the ontology of that tradition, and can be evaluated using the methodological norms of the tradition. A research tradition can influence its constituent theories in various ways:

(i) a research tradition can justify many of the assertions which its theories make; (ii) a research tradition can rule out certain theories because they are incompatible with the ontology or methodology of the tradition; (iii) a research tradition strongly influences the recognition and weighting of empirical and conceptual problems for its theories; (iv) a research tradition can provide heuristic guidelines for the generation and modification of specific theories.

Laudan (1977:64-66) has built into his model the idea that conceptual problems, like empirical problems, can have different weights.<sup>25</sup> Moreover, he (1977:46) claims that, in general, a conceptual problem represents a more serious threat than an empirical anomaly.

Laudan's attempt to make provision for a wide range of conceptual factors in his appraisal measure for scientific theories is certainly one of the interesting features of his model. However, some reviewers have pointed out that Laudan's claims about the originality of this feature of his model are somewhat exaggerated. Laudan (1977:66) claims that "no major contemporary philosophy of science allows scope for the weighty role which conceptual problems have played in the history of science". Laudan continues that "even those philosophers who claim to take the actual evolution of science seriously (e.g., Lakatos, Kuhn, Feyerabend, and Hanson) have made no serious concessions to the nonempirical dimensions of scientific debate". These claims are convincingly rebutted by Feyerabend (1981:60-61), and McMullin (1979:625). The value of Laudan's contribution primarily lies in the wide scope, great depth, and the systematicity of his treatment of conceptual factors in theory appraisal.

#### 2.3.2.4 Unsolved problems and anomalies

Laudan (1977:18-22) argues that in appraising the relative merits of theories the class of unsolved problems is irrelevant.

According to him, the only reliable guide to the problems relevant to the appraisal of a particular theory is an examination of the problems which previous and competing theories in that domain have already solved. A theory's failure to solve a problem left unsolved by all other theories in that domain thus cannot count against it.

Laudan (1977:26-30) makes two important points in connection with the notion 'anomaly' that features in his appraisal measure. Firstly, an anomaly is not only generated by a conflict between a prediction of a theory and observations. Any empirical problem *p* that has been solved by a theory constitutes an anomaly for every competing or successive theory in that domain which fails to solve *p*, even if the latter theory is not inconsistent with the relevant observational results. Laudan's class of anomalies thus includes nonrefuting anomalies. Secondly, refuting anomalies can under certain conditions be rationally ignored in theory appraisal. In particular, such a refuting anomaly counts as an anomaly for the appraisal measure only if it is solved by at least one other theory in the domain. Refuting instances of a theory are thus irrelevant for an appraisal of this particular theory, unless some other theory in the domain provides a solution to the problem in question.

#### 2.3.2.5 Ad hocness

Laudan (1977:114-118) argues that a theoretical modification, including the introduction of an auxiliary hypothesis, is not in any pejorative sense *ad hoc* if the modification only manages to overcome an empirical problem which was a refuting instance for the earlier, unmodified theory. Rather, he claims that such *ad hoc* modifications are by definition progressive, in that they increase the problem-solving effectiveness of the theory. Laudan, in contrast to, for example, Popper, thus denies the need for the independent justification or independent testability of theoretical modifications.

According/ . . .

According to Laudan, there is only one context in which "*ad hoc*" may legitimately be used in a pejorative sense. This is where a theoretical modification leads to a reduction in the theory's overall problem-solving effectiveness by virtue of increasing the conceptual difficulties of the theory.<sup>26)</sup>

### 2.3.3 Newton-Smith's temperate rationalism

#### 2.3.3.1 Truth as the goal

In his book, *The rationality of science* (1981), Newton-Smith defends the assumption that the goal of science is truth. He qualifies this assumption in two respects. Firstly, the aim is not simply to discover any truth, but to discover explanatory truth.<sup>27)</sup> Secondly, since all current (and past) theories are strictly speaking false, science must be seen as aiming at theories that capture more and more truth about the world, i.e., as aiming at theories with an increasing degree of verisimilitude.<sup>28)</sup> Newton-Smith (1981:195ff.) attempts to provide a new analysis of verisimilitude, in order to overcome, for example, Laudan's objections against Popper's notion of verisimilitude.<sup>29)</sup> Of crucial importance is the link which Newton-Smith claims to have established between the verisimilitude of a theory and the predictive power, or more generally, the observational success of this theory. In particular, Newton-Smith argues that greater verisimilitude entails the likelihood of greater observational success.<sup>30)</sup> Newton-Smith (1981:197) claims that the latter premise "has a strong intuitive appeal". He states that "if a theory has latched on to more theoretical truth about the world one would expect it to give better predictions". He also tries to justify this premise, based on an analysis of the notion of verisimilitude.

By assuming that the goal of science is truth - or more specifically, increased verisimilitude - Newton-Smith retains Popper's view about the goal of science, and rejects Laudan's view. Their different views on the goal of science will be analyzed in § 2.3.4.2 below.

Newton-Smith's model for the appraisal of theories has two components: an ultimate test, and a set of "good-making features" that function as fallible indicators of likely long-term observational success. It was noted in § 2.3.2.1 above that Newton-Smith generally uses the term "theory" to refer to general theories rather than to specific theories. His model is in fact a model for the relative appraisal of general theories, which have, for example, a developmental history. In the following sections I briefly outline the two components of Newton-Smith's model, and try to determine what implications his model has for the appraisal of specific theories, and thus ultimately for the appraisal of small-scale changes in specific theories.

### 2.3.3.2 The ultimate test: observational success

For a theory to have explanatory power "it must latch on to something about the world", according to Newton-Smith (1981:223). The ultimate test as to whether one theory has more successfully latched on to a facet of the world than another is its relative observational success. Newton-Smith (1981:223-224) distinguishes two aspects of observational success. The most important aspect is the generation of novel predictions which are corroborated. The second aspect is success in accounting for known observations.

The reason why Newton-Smith regards the generation of corroborated novel predictions as the most important component of observational success, is that such novel predictions must serve as a guard against *ad hoc* theories. Thus, he (1981:224) states that ". . . given a finite set of known facts we could with ingenuity devise some theory (it might be very cumbersome and complex) from which we could derive those facts. Our primary guard against such *ad hoc* theories is the requirement that some corroborated novel predictions should be forthcoming".

Since Newton-Smith regards the distinction between the observa-

tional/ . . .

tional and the theoretical as a matter of degree, "observational success" for him includes theoretical success.<sup>31)</sup> He (1981:224) defines theoretical success as "a matter both of the generation of novel predictions which themselves are theoretical and of the explanation of accepted theories".

In the long run, then, observational success is the ultimate test of the superiority of one theory over another. However, Newton-Smith (1981:224) points out that this ultimate test cannot be employed by the working scientist faced with the choice between two rival theories. Newton-Smith illustrates this point by reference to relativistic mechanics and Newtonian mechanics. He claims that while relativistic mechanics has been established as observationally more successful than Newtonian mechanics since 1905, it was impossible to determine this in 1905. Consequently, Newton-Smith tries to find factors that can serve as fallible indicators of likely long-term observational success.

Clearly, the ultimate test of observational success applies to all levels of theories, including specific theories. In fact, whatever the exact nature of the relationship between a general theory and its associated specific theories, it seems clear that the observational success of a general theory is a function of the observational success of its associated specific theories.

### 2.3.3.3 The good-making features of theories

Since the ultimate test discussed above cannot be used by the working scientist faced with the choice between two rival theories, other factors are needed which can serve as fallible indicators of likely long-term observational success. These factors - or "good-making features of theories" - ought to guide the scientists before the final results are in. Note that Newton-Smith gives no indication of when this would be.

Newton-Smith/ . . .

Newton-Smith (1981:226-231) distinguishes the following good-making features of theories: (i) observational nesting, (ii) fertility, (iii) track record, (iv) inter-theory support, (v) smoothness, (vi) internal consistency, (vii) compatibility with well-grounded metaphysical beliefs. He (1981:224-226) makes a number of general remarks about these factors that guide theory choice.

Firstly, the grounds for including any particular factor are meta-inductive. That is, a particular factor is included on the ground that it is indicative of long-term observational success. One of the reasons Newton-Smith (1981:230-231) excludes simplicity from his set of good-making features is precisely that it is not clear that apparent simplicity has in the past been a good indicator of long-term observational success.

Secondly, the factors are inductively correlated with observational success. This means that even if they all point in the same direction, that may be the wrong direction.

Thirdly, the factors may point in different directions. In the case of divergence the relative importance of the differing factors cannot be weighed.

Fourthly, it will not always be clear whether a theory possesses a good-making feature to a higher degree than another theory.

Fifthly, the factors relevant to theory choice in science are not constitutive of a good theory. They are only fallible indicators of what is constitutive of a good theory, namely, verisimilitude.

Newton-Smith (1981:225-226) sums up the status of his good-making features by noting that "obviously these principles are not algorithms admitting of mechanical application and giving certain knowledge of the ultimate degree of success of a theory".

### 2.3.3.3.1 Observational nesting

A theory ought to preserve the observational successes of its predecessor. To the extent that a theory fails to replicate the observational successes of its predecessor, it is required to have "dramatic" observational successes in areas where the predecessor is not successful, according to Newton-Smith (1981:226). If the new theory not only preserves the observational successes of its predecessor/rival, but improves upon them by increasing the accuracy of corroborated predictions and/or by increasing the area in which corroborated predictions are made, then this obviously counts in favour of the theory. A theory observationally nests another theory if the former preserves the observational success of the latter and increases the accuracy of corroborated predictions. 32)

What is the relevance of the requirement of observational nesting for changes in specific theories? Newton-Smith's (1981:226) argument for the inclusion of observational nesting in his set of good-making features is as follows.

- (8) "Given that the goal of science is the discovery of explanatory theories of ever greater verisimilitude, and given that increasing observational success is our primary indicator of increasing verisimilitude, it will count against a theory if it is unable to replicate the observational successes of the theory currently in the field" {unless, as pointed out above, the former theory has dramatic new observational success in other areas - M.S.}

This argument obviously applies to general as well as specific theories. Given that observational success is the ultimate test of a theory's merit, scientists involved in (small scale) changes in specific theories will naturally also be concerned with preserving the observational successes of the earlier theories. That is, it will obviously count against a later version  $T_{x+1}$  of a specific theory  $T_x$  if  $T_{x+1}$  does not preserve the observational success of  $T_x$  - unless, of course,  $T_{x+1}$  has successes

in an/ . . .

in an area where  $T_x$  fails - which compensate for the lost successes.

In sum: Observational nesting is a good-making feature also of specific theories, and it is thus relevant for the appraisal of changes in specific theories.

#### 2.3.3.3.2 Fertility

By "fertility" Newton-Smith (1981:227) understands that a theory "should contain ideas to guide research". He mentions two possible sources of such ideas: (i) a metaphorical component, and (ii) a novel idea.<sup>33)</sup>

For changes in specific theories the requirement of fertility implies that scientists should attempt to increase the fertility of the theory, for instance by the introduction of an appropriate novel idea. Given the choice between a modification that increases the fertility of the theory and a modification that does not, the former must be chosen (all other things being equal).

Note that Newton-Smith's notion of fertility is extremely vague, and thus difficult to apply. Also, intuitively it seems clear that the smaller the scale of a change to a specific theory, the less likely it is that this change will affect the fertility of the theory. Thus, while in principle the requirement of fertility is relevant to specific scientific change, in practice its role will probably be very limited.

#### 2.3.3.3.3 Track record

In his discussion of this third good-making feature, Newton-Smith (1981:227) makes it clear that by "track record" he understands the track record from the point of view of observational success. A theory with a good track record has continual observational success. As explained in § 2.3.3.2 above, the

observational/ . . .

observational success of a general theory depends on the observational success of its associated specific theories. Consequently, the requirement of a good track record clearly entails that also at the level of specific theories there should be continual increases in observational success.<sup>34)</sup>

The importance of observational success in theory appraisal - including appraisal at the level of specific theories - has already been noted in the discussion of the ultimate test in § 2.3.3.2 and the discussion observational nesting in § 2.3.3.3.1 above. The brief discussion above of Newton-Smith's notion of a good track record underlines the importance of observational success in the appraisal of specific theories.

#### 2.3.3.3.4 Inter-theory support

Newton-Smith (1981:228) argues that "it counts in favour of a theory that it supports a successful extant theory". According to Newton-Smith, this support may take the form of providing an explanation of the laws of one theory by another.<sup>35)</sup> He also states that "it counts against a pair of theories if no matter how successful they are in their own domains they clash in the sense that they cannot be consistently worked together in domains of common application".<sup>36)</sup>

Newton-Smith links the requirement of inter-theory support to a metaphysical belief in a "unified physical world". According to him, "we consequently expect either to be able to unify diverse theories into a single all-encompassing theory or to have a family of mutually supporting theories". The great success which scientists have had in operating with this belief provides the ground for it.

The requirement of inter-theory support obviously applies at the level of general theories as well as at the level of specific theories. It is thus also relevant for the appraisal of specific theories.

Newton-Smith's notion of inter-theory support more or less corresponds to Laudan's notion that any tension between two scientific theories from different domains can generate conceptual problems for these theories.

#### 2.3.3.3.5 Smoothness

Newton-Smith (1981:228) claims that "the smoothness with which adjustments can be made in the face of failure is an important factor in theory evaluation". Smoothness has to do with the auxiliary hypotheses introduced to explain away the failures of a theory. The smoother a theory is, the more its failures can be covered by a single auxiliary hypothesis. If a theory is smooth in this sense, it means that there is something systematic about its failures. If, on the other hand, a theory is not smooth and requires a diverse range of different, unrelated auxiliary hypotheses to explain its failures, this suggests that "the theory is not headed in the right direction".<sup>37)</sup>

That the good-making feature of smoothness has implications for change at the level of specific theories is obvious. When attempting to modify a specific theory in order to accommodate failures, the scientist must try to show that these failures can be covered by a single auxiliary hypothesis. The introduction of a range of unrelated auxiliary hypotheses - for example, one for each failure - must be avoided. In essence, Newton-Smith's requirement of smoothness seems to rule out *ad hoc* modification, or the introduction of *ad hoc* auxiliary hypotheses, to cope with failures.<sup>38)</sup>

#### 2.3.3.3.6 Internal consistency

Newton-Smith (1981:229) argues that the grounds for including the consideration of internal consistency in his set of good-making features are *a priori*. Recall that Newton-Smith's concern is with increased verisimilitude. If a theory is incon-

sistent/ . . .

sistent, it will contain every sentence of the theory's language. No theory of verisimilitude would be acceptable that did not give the lowest degree of verisimilitude to a theory which contained each sentence of the theory's language and its negation. It follows that internal inconsistencies must be avoided.

Newton-Smith's argument for the inclusion of internal consistency in his list of good-making features applies without modification to general theories as well as specific theories. It follows that scientists involved in changes in specific theories will change these theories so as to eliminate internal inconsistencies, and they will not change the theories in such a way that internal inconsistencies are introduced.

Like Newton-Smith, Laudan also claims that internal inconsistencies adversely affect the merit of theories. According to him, internal inconsistencies generate conceptual problems for this theory.<sup>39)</sup>

#### 2.3.3.3.7 Compatibility with well-grounded metaphysical beliefs

Newton-Smith (1981:229) claims that "theory construction and theory choice are guided by certain very general metaphysical beliefs". Within the category of well-grounded metaphysical beliefs he (1981:230) includes not only topic-neutral principles that are applicable to all areas of science, but also some with specific content. An example of the former is the principle of the acausality of time, which precludes citing the mere time at which an event occurs as a causal factor in explaining why the event occurs. An example of the latter is the principle from physics which precludes postulating action at a distance. The requirement of compatibility with well-grounded metaphysical beliefs obviously applies at all levels of theories, including the level of specific theories. Consequently, this requirement is also relevant to the appraisal of specific theories.

Newton-Smith's requirement of compatibility with well-grounded

general metaphysical beliefs more or less corresponds to Laudan's notion that tension between a theory and the prevalent world-view can generate conceptual problems for the theory. However, Laudan's notion is much more general. He (1977:61) not only includes metaphysical beliefs, but also beliefs from "logic, ethics and theology". It is possible to regard Newton-Smith's metaphysical principles with specific content as corresponding to the ontological principles of a research tradition, as distinguished by Laudan. Thus, Laudan (1977:79) regards the principle prohibiting action at a distance - which Newton-Smith regards as a metaphysical principle with specific content - as a principle belonging to the ontological component of a research tradition.

#### 2.3.3.3.8 Simplicity

Newton-Smith (1981:230-231) argues that although many scientists and philosophers of science would include simplicity as a good-making feature of theories, the inclusion of this feature is in fact problematic. Firstly, Newton-Smith points out that no one has yet produced a criterion of relative simplicity that successfully measures the simplicity of the theory as opposed to the language within which the theory is expressed, or even the relative simplicity of different linguistic formulations of the same theory. Secondly, he claims that it is not clear that apparent simplicity is a good indicator of long-term observational success. However, Newton-Smith (1981:231) does present a pragmatic argument for the use of simplicity in contexts where the notion has "hard content": It is easier to calculate with simpler theories.

Newton-Smith's views on simplicity obviously apply to all levels of theories, including specific theories. At the level of specific theories, simplicity can thus be opted for in order to make calculations easier, but it must not be seen as an indicator of greater verisimilitude.

#### 2.3.3.4 The feedback mechanism

Newton-Smith (1981:269-270) claims that our rational model of science must be dynamic, and not static. That is, one must allow for changes in the method of science. He criticizes the models of Laudan, Popper, and Lakatos for being static.<sup>40)</sup>

The method of science can change through the operation of the feed-back mechanism, discussed by Newton-Smith (1981:231). Assumptions about which factors ought to guide us in theory choice ought to be assessed in the light of long-term success as measured by the ultimate test - observational success. Failure to make progress in increasing observational success might lead to a revision of the set of factors that function as fallible indicators of likely long-term observational success.

#### 2.3.3.5 The role of judgment

In his model Newton-Smith (1981:232-235) assigns an important role to non-rule-governed judgment in the scientific enterprise. In essence, his position is that scientific method cannot be exhaustively specified in some articulated system of rules, if for no reason other than that there are cases in which the rules conflict. As pointed out above, the good-making features may point in different directions. In such cases, the scientist has to exercise his judgment concerning the relative weight to be attached to the conflicting features. In addition, there is the fact that it will not always be clear whether one theory possesses a particular good-making feature to a higher degree than another. This fact also calls for non-rule governed judgment.

Newton-Smith (1981:270) points out that the inclusion of judgment in his model is one of the factors that distinguishes his model from Laudan's (and also Popper's and Lakatos'). As his (1981: 112ff.) discussion of Kuhn makes clear, by allowing for non-rule governed judgment Newton-Smith tries to accommodate, for

example/ . . .

example, Kuhn's observation that there is no algorithm which guides the decisions of scientists.

Newton-Smith (1981:235) emphasizes that reliance on judgment is a high-risk strategy. However, he adds that scientists are rarely in a situation in which they have "nothing to do" but to follow their intuitions.

### 2.3.4 Laudan versus Newton-Smith

#### 2.3.4.1 Preliminary considerations

Against the background of the brief overviews of Laudan's and Newton-Smith's models in §§ 2.3.2 and 2.3.3 above, it is now possible to consider some of the conflicting claims made by Laudan and Newton-Smith about theory appraisal in greater depth. The main aim of this section is to isolate those issues related to Laudan's and Newton-Smith's models which can, in principle, be most fruitfully investigated in the light of the developmental history of binding theory. Accordingly, the main emphasis is on the considerations that guide the choice between specific theories.

Before I can proceed with a discussion of the differences between Laudan's and Newton-Smith's models, it is necessary to clarify the notions 'empirical' and 'conceptual' employed in the following discussion. Recall Laudan's distinction between empirical and conceptual problems. He (1977:15) defines an *empirical* problem as "anything about the natural world that strikes us as odd, or otherwise in need of explanation". Empirical problems are "first order questions about the objects which constitute the domain of any given science". A *conceptual* problem, according to Laudan (1977:48), is "a problem exhibited by some theory or other". Conceptual problems are "higher order questions about the well-foundedness of the conceptual structures (e.g., theories) which have been devised to answer the first order questions". Laudan

(1977:48) also emphasizes that there is in fact a continuum of problems between "straightforward" empirical and "straight-forward" conceptual problems.

Laudan's empirical-conceptual distinction may be reconstructed as applying as follows to a specific theory,  $T_x$ , that belongs to a research tradition,  $RT_A$ .

- a. A consideration which plays a role in the evaluation of  $T_x$  is *empirical* in nature if the consideration bears on
  - (i) the success of  $T_x$  to solve the empirical problems in its domain,
  - (ii) the failure of  $T_x$  to solve the empirical problems in its domain,
  - (iii) any inconsistency between  $T_x$  and certain facts (that is,  $T_x$  faces counterexamples).
- b. A consideration which plays a role in the evaluation of  $T_x$  is *conceptual* in nature if the consideration bears on
  - (i) the relation between  $T_x$  and the ontological component of its research tradition,  $RT_A$ ,
  - (ii) the relation between  $T_x$  and the methodological component of  $RT_A$ ,
  - (iii) the relation between  $T_x$  and general metaphysical assumptions that do not form part of  $RT_A$ , religious beliefs, etc.,
  - (iv) the relation between  $T_x$  and a theory  $T_y$  from another research tradition,  $RT_B$ , which has a different domain of inquiry,

- (v) internal conceptual properties of  $T_x$ , for example, ambiguity, vagueness, inconsistency.

Let us put aside for the moment the differences which Laudan claims exist between solving empirical problems and explaining facts.<sup>41)</sup> That is, let us assume that Laudan's statements about solving empirical problems can be "translated" into statements about explaining facts. Laudan's notion 'empirical' is one commonly found in the literature. The same is true for his distinction between empirical and other considerations that play a role in theory evaluation.<sup>42)</sup> For instance, Laudan's distinction correlates exactly with Bunge's (1967b:347) distinction between empirical and non-empirical criteria of theory evaluation. Empirical criteria, for Bunge, bear on the agreement of a theory with observed facts. All other criteria are nonempirical for Bunge. Bunge's nonempirical criteria include, for example, internal consistency, linguistic exactness, external consistency, depth, testability, world-view compatibility. The similarity with Laudan's conceptual problems should be obvious.

Newton-Smith (1981:8a) also distinguishes considerations dealing with the explanation and prediction of facts - that is, *empirical* considerations in the sense defined above - from other considerations.

- (9) ". . . any model of science must leave room for the differential assessment of theories in terms of their power to avoid conceptual difficulties and *not just in terms of their power to predict novel facts and explain known facts*" {the italics are mine - M.S.}

Feyerabend, whose ideas are briefly discussed in § 2.3.5 below, adopts a similar distinction. His (1981) review of Laudan's book shows that he adopts the same empirical-conceptual distinction as Laudan. Consider in particular Feyerabend's (1981:60, fn. 3) discussion of the importance attached by various other philosophers of science to conceptual problems.

of/ . . .

Of special importance for the purposes of the present study is the fact that Chomsky adopts an empirical-conceptual distinction similar to the one set out above. Textual evidence in support of the claim that Chomsky adopts this distinction will be presented during the course of the analyses in chapters 3 - 6.

For the purposes of this study considerations which bear on the success of a theory in explaining facts and making correct predictions will then be distinguished from other considerations which play a role in theory appraisal. In accordance with the practice of Laudan and various other philosophers, the former considerations (but not the latter) will be regarded as *empirical* considerations. The term "empirical success" will be used to denote the success a theory has in explaining facts and making correct predictions (or, in Laudan's terminology, its success in solving empirical problems and avoiding anomalies). Empirical success is thus the same as Newton-Smith's (1981:223-224) observational success. Recall that Newton-Smith's notion 'observational success' includes theoretical success, that is, success in the generation of novel predictions, which themselves are theoretical, and of the explanation of accepted theories. Laudan must also regard theoretical success, in Newton-Smith's sense, as empirical success. Laudan (1977:15) acknowledges the theory-ladenness of empirical problems. For Laudan, theoretical success would then be success in solving an empirical problem with a high theoretical content.

Following Laudan, the term "conceptual consideration" will be used to refer to the various nonempirical considerations that play a role in theory appraisal. One of the questions to be considered below in connection with these conceptual considerations concerns the relation between the various conceptual considerations and the empirical success of a theory. It is interesting to note that Laudan and Newton-Smith have different views on this subject. Laudan does not try to establish any link between a theory's success in dealing with empirical problems and its

success/ . . .

success in dealing with conceptual problems. In particular, he does not claim that a theory's success in dealing with conceptual problems gives any indication of this theory's likely long-term success in dealing with empirical problems. For Newton-Smith, however, the use of a particular conceptual consideration in theory appraisal is justified only if it can be shown that this consideration does point to long-term observational success. This difference between Laudan's and Newton-Smith's views is taken up again in § 2.3.4.4 below.

#### 2.3.4.2 The role of truth in the scientific enterprise

One of the most important differences between Laudan's and Newton-Smith's models that emerges from the overviews presented above, concerns the role which truth must play in an account of the scientific enterprise. Newton-Smith claims that truth is the goal of science, and attempts to characterize the progressiveness of science in terms of its truth-directedness. In contrast, Laudan claims that if truth is taken as the goal of science, science cannot be shown to be either progressive or rational. Accordingly, he tries to characterize the progressiveness and rationality of science without reference to truth.<sup>43)</sup>

Laudan's claims about the possibility of constructing a truth-independent account of the scientific enterprise have been criticized on the grounds that his model has to appeal to considerations of truth on various points.<sup>44)</sup> Firstly, it has been argued that Laudan's notion of problem-solving effectiveness depends on considerations of truth.<sup>45)</sup> Secondly, it has been argued that Laudan's claim that a research tradition has an ontological component becomes intelligible only on the assumption that science is truth-directed.<sup>46)</sup> Thirdly, it has been argued that the problematic status of conceptual problems, so strongly emphasized by Laudan, can only be explained by reference to the truth-directedness of the scientific inquiry.<sup>47)</sup> Fourthly, it has been argued that problem-solving does not in itself constitute a

rational/ . . .

rational end for science, and that Laudan himself falls back on the idea that science is truth-directed when he (1977:225) argues that the scientific enterprise is justified in terms of our curiosity about the world and ourselves.<sup>48)</sup>

It seems that, in view of the criticisms mentioned above, one must conclude that Laudan has failed to establish his claim that a truth-independent account of the scientific enterprise is possible. Of course, it does not follow from Laudan's failure to establish a truth-independent account of the scientific enterprise that the truth-directed account of Newton-Smith is necessarily correct. However, to the extent that certain aspects of Chomsky's linguistics require an appeal to truth, this would support Newton-Smith's view over Laudan's. The question that must be considered is then whether an adequate account of Chomsky's rationality does require an appeal to the truth-directedness of this enterprise.

#### 2.3.4.3 The relative importance of empirical success

Laudan and Newton-Smith agree that conceptual considerations, in addition to empirical considerations, play a significant role in theory appraisal. Thus Laudan (1977:45) claims that the solving of conceptual problems "has been *at least as important* in the development of science as empirical problem solving". Newton-Smith (1981:89) declares that ". . . any model of science must leave room for the differential assessment of theories in terms of their power to avoid conceptual difficulties and not just in terms of their power to predict novel facts and explain known facts".

When one considers the appraisal of general theories/research traditions in the long run, an interesting difference emerges between Laudan's and Newton-Smith's models. Recall that for Newton-Smith observational success is the ultimate test of a theory's merit. This means that in the *final* evaluation of a theory (whenever that may be) empirical success is the only factor

that/ . . .

that determines its merit. In Laudan's model, conceptual considerations are in all instances, thus also the long-term evaluation of general theories, as important as empirical considerations. Since our concern in this study is with changes in specific theories that must be evaluated in the short-term, this particular difference between Laudan's and Newton-Smith's views will not be considered further.

As regards the short-term evaluation of specific theories, Laudan and Newton-Smith again agree that both empirical and conceptual considerations play a role. It is not quite clear whether there is any difference in the relative importance assigned to these two types of considerations by Laudan and Newton-Smith. What is clear, however, is that neither of them claims that in cases where empirical and conceptual considerations are in conflict, the empirical considerations must necessarily override the conceptual considerations. In Laudan's case, the weight of the relevant empirical and conceptual problems would have to be compared. In Newton-Smith's case, the scientist would have to use his (non-rule governed) judgment to decide the conflict.

In the analyses presented below it will be determined for each transition  $T_x \rightarrow T_{x+1}$  in the development of the binding theory what role empirical and conceptual considerations played in the transition. Of particular interest would be instances of conflict between empirical and conceptual considerations. The crucial question about such conflict - if instances do actually occur in the development of the binding theory - would be how Chomsky resolved this conflict.

#### 2.3.4.4 The status of the factors that play a role in theory choice

Newton-Smith (1981:225) states that the factors relevant to theory choice are not constitutive of a good theory. For him,

the goodness/ . . .

the goodness of a theory is constituted by its degree of verisimilitude. The factors are only fallible indicators of this goodness (via its link with observational success). For Laudan, in contrast, the factors relevant to theory choice - that is, effectiveness in solving empirical problems and avoiding anomalies and conceptual problems - are constitutive of a good theory. Problem-solving effectiveness is not indicative of some other property of a theory, which constitutes the goodness of the theory.

This difference between Laudan's and Newton-Smith's model bears in an interesting way on the interpretation of the role which conceptual considerations play in theory appraisal within the two models. For Newton-Smith, success in avoiding conceptual difficulties is relevant to theory choice only because such success is indicative of likely long-term observational success. If a particular conceptual consideration is not indicative of long-term observational success, it is not relevant to theory appraisal. Given the link which Newton-Smith claims to have established between observational success and verisimilitude, he can thus justify the role of conceptual considerations by referring to the truth-directedness of science. For Laudan, on the other hand, avoiding conceptual difficulties is in itself a goal of science. That is, success in avoiding conceptual problems is constitutive of a good theory. Laudan (1977:123) explicitly denies that problem-solving ability has any direct connection with truth. One of the main points of criticism levelled at Laudan's model is precisely that he cannot explain the importance of conceptual considerations in theory appraisal without referring to the truth-directedness of science, thus rendering his model incoherent.<sup>49)</sup>

Against this background the question arises whether Chomsky regards the various considerations that determine theory choice as indicative of truth (or something else), or whether he regards them as being in themselves constitutive of a good theory. In order to answer this question, the justification which Chomsky provides for the relevance of each consideration (in so far as

he does/ . . .

he does explicitly comment on the matter) will have to be analyzed.

#### 2.3.4.5 The importance of the general theory for the development of specific theories

Laudan's distinction between a specific theory and a general theory/research tradition is crucial for his account of the appraisal of specific theories. One of the influences of a research tradition on its associated specific theories, according to Laudan (1977:86-88), is that the research tradition strongly influences the range and weight of the empirical and conceptual problems with which its specific theories must deal.<sup>50)</sup> The generation of conceptual problems is particularly significant. Laudan (1977:88) claims that "the bulk of conceptual problems which any theory may face will arise because of tensions between that theory and the research tradition of which it is part".

While Newton-Smith (1981) recognizes the distinction between general and specific theories, this distinction is not built into his model for theory appraisal. Consequently, unlike Laudan, Newton-Smith does not single out conceptual difficulties generated by a general theory as a particularly important factor in the appraisal of specific theories. This does not mean that Newton-Smith fails to recognize the type of conceptual considerations which Laudan characterizes as arising from conflict between a research tradition and its associated specific theories. For instance, as argued in § 2.3.3.3.7 above, Newton-Smith's metaphysical principles with specific content correspond to the principles Laudan identifies as belonging to the ontological component of a research tradition.<sup>51)</sup> Just as conflict between a specific theory and the ontological component of its research tradition creates a conceptual difficulty, so does conflict between a specific theory and a metaphysical principle with specific content. However, unlike Laudan, Newton-Smith does not single out such conceptual difficulties as of particular importance in the appraisal of specific theories.

Given the background sketched above, it would be interesting to determine whether conceptual considerations related to Chomsky's general theory/research tradition play a particularly important role in the choice of each version  $T_{x+1}$  of the binding theory over the preceding version  $T_x$ .

If it should be found that the majority of the conceptual considerations that play a role in the appraisal of the various stages in the development of binding theory is related to Chomsky's research tradition, it would point to a shortcoming of Newton-Smith's model, a shortcoming that follows from the fact that he has not explicitly built the distinction between general and specific theories into his model for theory appraisal.

#### 2.3.4.6 The role of normative difficulties

Laudan (1977:55) claims that tension between a scientific theory and the methodological theories of the relevant scientific community can generate external conceptual problems for the theory.<sup>52)</sup> In fact, he (1977:58) claims that the norms of scientists (for example, norms about how science should be performed, about what counts as an adequate explanation, about the use of experimental control) "have been perhaps the single major source for most of the controversies in the history of science, and for the generation of many of the most acute conceptual problems with which scientists have had to cope". Research traditions consist in part of a set of methodological "do's and don'ts".<sup>53)</sup> It follows that the conceptual problems generated by a research tradition for its specific theories will include normative difficulties. Laudan's theory thus embodies the claim that normative difficulties play an important role in the appraisal of specific theories.

Newton-Smith's model makes no explicit provision for the elimination of such normative difficulties as a factor that influences theory appraisal. That is, Newton-Smith does not recognize the

avoidance/ . . .

avoidance of normative difficulties as a fallible indicator of long-term observational success.

The question naturally arises what role normative difficulties play in the development of binding theory.

If it should be found that such difficulties does indeed play an important role, then it would indicate a shortcoming in Newton-Smith's model.

#### 2.3.4.7 The role of non-rule governed judgment

Newton-Smith (1981:232-235) argues that non-rule-governed judgment plays a crucial role in the scientific enterprise. He (1981:225) explicitly denies that his goodmaking features are algorithms that can be mechanically applied. In certain cases the various features may not clearly point to the superiority of one theory over another. In such cases the scientists would have to exercise their judgment.<sup>54)</sup>

In contrast, Laudan does not provide a role for such judgment in theory evaluation. Instead, his model entails that there is a calculus of theory choice. Laudan (1977:127) argues that the "workability" of the problem-solving model is its greatest virtue. That is, Laudan claims that his model is workable as a calculus, one which would allow disputed cases of theory choice to be settled in terms of the rules of theory appraisal. Obviously, this can only be done if the counting and weighting of problems and problem solutions can be done on the basis of rules.

Laudan's claim about the workability of his model has been severely criticized. One of the main criticisms is that there are serious difficulties with the individuating, counting, and weighting of both empirical and conceptual problems.<sup>55)</sup> These criticisms not only establish that Laudan has as yet failed to justify his claim that his model is workable as a calculus, but they also raise serious doubts as to whether a calculus of problem-solving effectiveness is at all possible.<sup>56)</sup>

An important/ . . .

An important question to consider in the proposed reconstruction of Chomsky's rationality, is then whether non-rule governed judgment plays a role in theory appraisal within Chomsky's linguistics. If it should be found that such judgment does play a role, then this fact would provide support for Newton-Smith's claim that non-rule governed judgment forms an integral part of theory appraisal. At the same time, such a finding would further undermine Laudan's claim that there is a calculus for determining the relative success of theories.

#### 2.3.4.8 The importance of *ad hocness*

According to Laudan (1977:114ff) there is nothing wrong with modifying a theory - for example, through the introduction of an auxiliary hypothesis - in such a way that it overcomes just one empirical failure. The only condition for such a modification is that the modification must not give rise to conceptual problems that weigh more than the solved anomaly. Laudan thus rejects the idea that *ad hoc* theoretical modifications - that is, modifications that lack independent testability and/or independent justification - are objectionable.

Newton-Smith's position on the *ad hocness* of devices introduced to protect a theory from potential negative evidence is somewhat more complex. In his (1981:70-76) discussion of Popper's views on *ad hoc* hypotheses, Newton-Smith stresses that it is not possible to place a ban on auxiliary hypotheses that are without independent justification or independent testability. Newton-Smith (1981:73) argues that independent testability cannot be determined. As regards independent justification, he points out that there are many instances where scientists do introduce hypotheses which lack independent justification. Newton-Smith (1981:74) outlines the strategy that should be followed in distinguishing between "good moves" and "bad moves" in immunizing a theory as follows:

- (10) "We look at the positive evidence for the theory. We may have such good reasons for believing in the truth of a theory that those reasons provide a ground for thinking that the immunizing hypothesis is true. That is, the only viable means of distinguishing between good and bad moves in this context is by reference to a positive doctrine of evidence."

These remarks indicate that Newton-Smith has no objection to an auxiliary hypothesis that serves only to immunize a theory from potential counterevidence, and is thus without independent justification.

However, when one considers the implications which Newton-Smith's criterion of smoothness has for specific theories, then it is obvious that Newton-Smith must value modifications or auxiliary hypotheses which cover more than one failure higher than those which cover only a single failure. As is explained in § 2.3.3.3.5 above, since smoothness is a factor in the appraisal of general theories, scientists must attempt to cover as many failures as possible by a single modification or auxiliary hypothesis. Modifications or auxiliary hypotheses that serve to explain only one failure each thus adversely affect the smoothness of the theory. For Newton-Smith, then, a modification or auxiliary hypothesis which is independently justified in the sense that it covers more than one failure of the theory is more highly valued than a modification or auxiliary hypothesis which covers only one failure.

In sum, then: Neither Laudan nor Newton-Smith rules out the use of modifications or auxiliary hypotheses which are *ad hoc*, in the sense that they serve only to cover a single failure of a theory. However, Laudan and Newton-Smith differ in the claims which they make about the merit of such modifications and auxiliary hypotheses compared to modifications and auxiliary hypotheses which have independent justification. Laudan claims that modifications and auxiliary hypotheses with independent justification are not preferable to those without such justification. Newton-

Smith/ . . .

Smith claims that a modification or auxiliary hypothesis which has a measure of independent justification, in that it covers a number of failures, is preferable to a modification or auxiliary hypothesis which covers only one failure. This difference between Laudan and Newton-Smith can also be characterized in terms of the notion 'generality'. While Newton-Smith values modifications or auxiliary hypotheses which are general more highly than those which are not, Laudan does not differentiate between modifications and auxiliary hypotheses which are general and those which are not.

It is then not only important to ask whether a scientist actually makes use of *ad hoc* modifications and *ad hoc* auxiliary hypotheses, which each covers only one failure of the theory that they are protecting. Rather, the crucial question is whether, while using such *ad hoc* modifications and *ad hoc* auxiliary hypotheses, the scientist concerned nevertheless prefers modifications and auxiliary hypotheses which have some independent justification, at least in the sense that they cover more than one failure of the theory.

#### 2.3.4.9 Simplicity as a criterion in theory appraisal

Newton-Smith explicitly rules out the use of simplicity as a factor relevant to theory appraisal, except in so far as simplicity makes calculations easier.<sup>57)</sup> In contrast, nothing in Laudan's model rules out the use of simplicity in theory appraisal. The desirability of simplicity in a theory could follow either from a methodological or an ontological principle of a research tradition, or even from the prevailing world-view. Given this difference between Laudan's and Newton-Smith's models, the question naturally arises what role, if any, simplicity plays in theory appraisal in Chomsky's linguistics, and the development of binding theory, in particular.

2.3.4.10 Solving problems and explaining facts

Laudan (1977:15-17; 22-26) insists that "the solving of problems" is not the same as "the explaining of facts". He mentions various differences which he claims exist between solving problems and explaining facts. The main differences are as follows. First, a theory may solve a problem so long as it entails even an approximate statement of the problem, while an explaining theory must entail an exact statement of the fact to be explained. Second, in determining whether a theory solves a problem, the truth or falsity of the theory is irrelevant, while an explaining theory must be either true or highly probable. Third, what counts as a solution to a problem will not necessarily be regarded as such at all times, while an adequate explanation of any fact must be regarded as always having been such. There are also important differences between facts or states of affairs on the one hand, and empirical problems on the other. First, a problem need not describe a real state of affairs to be a problem. All that is required is that it be thought to be an actual state of affairs by someone. Second, many facts about the world do not pose empirical problems because they are unknown. Third, a known fact constitutes an empirical problem only if there is a premium on solving it. Fourth, problems recognized as such at one time can cease to be problems later, while facts cannot undergo this sort of transformation.

Given these differences, statements about problem-solving cannot be translated into statements about the explanation of facts. At this point there is then a potentially interesting contrast between Laudan's and Newton-Smith's models. For Newton-Smith, unlike Laudan, claims that theories must be seen as explaining facts. The notion of explaining facts forms a crucial part of Newton-Smith's notion 'observational success'.<sup>58)</sup> However, in spite of the potential interest of this contrast between the two models, it is highly unlikely that the present study will throw any light on the alleged differences between solving problems and

explaining/ . . .

explaining facts. Various reviewers have raised principled objections to the possibility of counting and weighing empirical problems.<sup>59)</sup> Also, objections have been raised at some of Laudan's claims about the differences between solving problems and explaining facts.<sup>60)</sup>

In view of the principled nature of the objections levelled at Laudan's distinction between solving empirical problems and explaining facts, the potential consequences of this distinction will be ignored in the analyses presented below. The "explain"-terminology will be used throughout.

#### 2.3.4.11 The possibility of changes in the criteria of theory evaluation

Newton-Smith (1981:221-223; 269-270) places great emphasis on the fact that scientific method evolves. While he (1981:269) does not believe that there has been an evolution in the goals of science, he claims that the principles of theory comparison have changed through time. Such changes take place under the regulation of the feedback mechanism of the ultimate test, observational success. Failure to make progress in improving observational success in the long run may lead to changes in the set of criteria used for appraising theories.

Newton-Smith (1981:270) contrasts his model on this point with those of Popper, Lakatos, and Laudan. He claims that their models of science are static, in that they do not allow for the evolution of method. As regards Laudan, at least, I believe that Newton-Smith is overstating his case.<sup>61)</sup> Laudan (1977:130) considers the problem "how we can, with the philosophers, continue to talk normatively about the rationality (and irrationality) of theory choices in the past, while at the same time avoid the grafting of anachronistic criteria of rationality onto these episodes?" He goes on to claim that his model "resolves part of that difficulty by exploiting the insights of our own time

about/ . . .

about the *general* nature of rationality, while making allowances for the fact that many of the *specific* parameters which constitute rationality are time- and culture-dependent". On the one hand, Laudan insists that for all times and for all cultures, rationality consists in accepting those research traditions which have the greatest problem-solving effectiveness. On the other hand, Laudan (1977:130-1) claims that "the model also insists that what is specifically rational in the past is partly a function of time and place and context. The kinds of things which count as empirical problems, the sorts of objections that are recognized as conceptual problems, the criteria of intelligibility, the standards for experimental control, the importance or weight assigned to problems, are all a function of the methodological-normative beliefs of a particular community of thinkers."

These remarks by Laudan clearly show that Laudan's model does allow for certain changes in the criteria of theory evaluation. Newton-Smith is thus wrong in claiming that Laudan's model has no "dynamical factor". It is not even clear that the two models differ with respect to the radicality of the change in method which they allow. Recall that Newton-Smith does not believe that the goal of science has evolved. For him, change in method must thus consist in a change in the set of good-making features. An example of a fairly radical change would be the exclusion of one of the good-making features from this set. For example, the criterion of compatibility with well-grounded metaphysical beliefs could be excluded. While Laudan stresses the overall importance of conceptual considerations in theory development, it is quite compatible with his model that scientists working within a certain research tradition do not take conceptual problems generated by tension with metaphysical beliefs into account. Consider in this connection Laudan's (1977:131) discussion of the modern view that science is independent of theology and metaphysics. He stresses that this view is of relatively recent origin. Earlier, however, it was rational to take such external conceptual problems into account.<sup>62)</sup>

It is/ . . .

It is then not at all clear that there is any real difference between Laudan's and Newton-Smith's views on the possibility of change in the criteria of theory appraisal. However, given that Laudan's and Newton-Smith's models differ from earlier models (such as those of Popper and Lakatos) in that they allow for changes in method, it is still of interest to ask whether the developmental history of binding theory provides any evidence that Chomsky's criteria of theory appraisal have changed since the early seventies. Of course, the history of binding theory covers a relatively short time-span, namely ten years. If this history were to provide no evidence of changes in Chomsky's method, one would certainly not be entitled to use this as evidence against Laudan's and Newton-Smith's claims about the possibility of evolution in scientific method. The main reason for considering possible changes in Chomsky's method is Chomsky's recent appeals to linguists to adopt a certain style of inquiry, the so-called "Galilean style". This point is taken up in § 2.4 below.

### 2.3.5 Feyerabend versus Laudan and Newton-Smith

Among those philosophers who study scientific method and scientific rationality, Feyerabend has a special status. Feyerabend argues that there is no such thing as a method of science in the sense of a system of exceptionless rules which infallibly guide scientists in making theory choices. Moreover, Feyerabend argues that the adoption of any particular set of exceptionless rules would have the effect of impeding scientific progress. According to Feyerabend science is not a rational affair as, for example, Popper and Lakatos claim it to be. Consequently, science ought not to have the special status which it does have in our society.

At first sight Feyerabend's views appear to be completely irreconcilable with those of Laudan and Newton-Smith, both of whom believe in the existence of a method of science and who try to articulate the rules which guide scientists in their theory

choices/ . . .

choices. The aim of the present section is to outline some of the issues on which Feyerabend is in agreement with Laudan and Newton-Smith. The question that must be answered is whether there are any differences between Feyerabend's views and those of Laudan and Newton-Smith which can fruitfully be examined within the context of the present study. The main emphasis will be on Newton-Smith's views. Naturally, what follows is not meant to be a complete overview of Feyerabend's views on science.<sup>64)</sup>

It was argued in § 2.2 above that Newton-Smith and Feyerabend are in agreement about what is involved in providing a rational account of the actions of an individual scientist. Specifically, to provide a minirat account of the actions of an individual scientist, in Newton-Smith's sense, is the same as providing an account of the actions of an individual scientist in terms of Feyerabend's anthropological approach. Textual evidence was presented in § 2.2 that Feyerabend does indeed regard the latter type of account as being rational.<sup>65)</sup>

If Feyerabend and Newton-Smith are in agreement with respect to the rationality of an individual scientist, then the question arises how their views on the rationality of science in general differ. To put it differently: How does Feyerabend's views on the possibility of a method of science compare with Newton-Smith's views? To answer this question, it is necessary to consider in broad outline Feyerabend's views on scientific method. The following account of Feyerabend's views is based on *Against method* (1975), and *Science in a free society* (1978).

Feyerabend (1975:23) argues against the existence of "firm, unchanging, and absolutely binding principles for conducting the business of science". Instead, he claims that "given any rule, however 'fundamental' or 'necessary' for science, there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite". Feyerabend (1978:127) explains the rationale behind his famous slogan "anything goes"

as follows/ . . .

as follows:

- (11) ". . . if you want advice that remains valid, no matter what, then the advice will have to be as empty and indefinite as 'anything goes'."

The following passage by Feyerabend (1978:32) provides such a clear account of his position on rules of method, that I quote it in full.

- (12) "The limitation of all rules and standards is recognized by *naïve anarchism*. A naïve anarchist says (a) that both absolute rules and context dependent rules have their limits and infers (b) that all rules and standards are worthless and should be given up. Most reviewers regard me as a naïve anarchist in this sense overlooking the many passages where I show how certain procedures *aided* scientists in their research. For in my studies of Galileo, of Brownian motion, of the Presocratics I not only try to show the *failures* of familiar standards, I also try to show what not so familiar procedures did actually *succeed*. I agree with (a) but I do not agree with (b). I argue that all rules have their limits and that there is no comprehensive 'rationality', I do not argue that we should proceed without rules and standards. I also argue for a contextual account but again the contextual rules are not to *replace* the absolute rules, they are to *supplement* them."66)

In a similar passage Feyerabend (1978:164) states that he does not want to eliminate rules or to show their worthlessness. His intuition is rather "to expand the inventory of rules . . ."

When one considers Newton-Smith's views on scientific method, it immediately becomes obvious that he does not hold the views criticized by Feyerabend. Specifically, Newton-Smith admits that method changes, and that there are no absolutely binding and exceptionless rules which guide scientists. Like Feyerabend, Newton-Smith tries to expand the inventory of rules. Consider in this connection Newton-Smith's attempt to include a variety of conceptual factors in his list of good-making features of theories.

As the/ . . .

As the discussion by Newton-Smith (1981:128ff) makes clear, the real difference between Feyerabend and Newton-Smith lies in their interpretation of the consequences of the above-mentioned facts about theory appraisal. Feyerabend assumes that the rationalist is committed to believing in unchanging, exceptionless algorithmic principles of comparison. He thus concludes that there is no system of rules which ought always to guide scientists in making theory choices, and that to adopt any particular set of rules would have the effect of impeding scientific progress. Newton-Smith assumes that the rationalist is not committed to the existence of unchanging, exceptionless algorithmic principles of comparison. Instead, the rationalist assumes that the rules for theory comparison are inductive rules which advise scientists as to which of a pair of rival empirical theories it is better to adopt in the face of available evidence. Unlike Feyerabend, then, Newton-Smith does not conclude from the facts about theory appraisal set out above that there is no scientific method.

Newton-Smith (1981:134) provides the following neat summary of the conflict between Feyerabend's position and his own rationalist position.

- (13) "Thus Feyerabend's easy defeat of a straw man (the rationalist who believes in infallible exceptionless rules) is construed by him as a victory over a real man (the rationalist who believes in general guiding fallible principles of comparison) who is in fact enlisted in the battle with the straw man!"

According to Newton-Smith (1981:129), the "believer in scientific method", or the rationalist, admits that the rules of theory appraisal have a high risk factor, and that they may on occasion point in the wrong direction. The crucial question is not whether a particular rule or set of rules has ever led us wrong. The crucial question is whether it led us wrong more often than not.<sup>67</sup> Also, the believer in method admits that the principles may on occasion point in different directions (Newton-Smith 1981:130).

Precisely/ . . .

Precisely because he does not believe that there are unchanging, exceptionless algorithmic rules of theory appraisal, Newton-Smith (1981:232ff.) makes provision for the role of non-rule governed judgment in theory appraisal. It is interesting to note that Finocchiaro (1980:200) argues that Feyerabend's phrase "anything goes" may be interpreted as expressing the fact that such judgment plays a role in science. Finocchiaro (1980:150) argues that judgment, which cannot be covered by generalizations, forms part of Galileo's work. He (1980:156) claims that Galileo's method is "judgmental: it is not a method in the sense in which some people conceive of method, namely as an infallible rule". Instead, "it offers no guarantee". In commenting on Feyerabend's views, Finocchiaro (1980:157) claims that ". . . Feyerabend's anarchism may be regarded as an extreme formulation of the . . . methodology of science which I am supporting here and which emphasizes a move away from method and in the direction of judgment".

What remains unclear is whether Feyerabend would admit that there is a method of science in Newton-Smith's sense, i.e., in the sense of there being general fallible rules which guide scientists in making their theory choices. And if so, what would these fallible rules be in the case of contemporary science, according to Feyerabend? In the absence of clear answers to these questions, it is not possible to determine precisely what differences there are between Feyerabend's views and Newton-Smith's views. It seems clear, however, that there are no clear differences between their views on scientific method which could fruitfully be investigated within the context of the present study.

Given the absence of interesting relevant differences between Newton-Smith's and Feyerabend's views, there is little point in considering in any great detail the differences between Laudan's and Feyerabend's views. There definitely are greater differences between Laudan's and Feyerabend's views than there are between

Newton-Smith's/ . . .

Newton-Smith's and Feyerabend's views. Laudan claims that problem-solving effectiveness is a universal and time-independent criterion for theories. He also assumes that the rules for theory choice are algorithmic, and he makes no provision for non-rule governed judgment. However, Laudan does allow for changes in the rules for theory appraisal, and makes provision for a very wide range of factors to play a role in theory appraisal, including, for example, the prevalent world-view, religious beliefs, and so on. Even in Laudan's and Feyerabend's case, then, the differences between their views which can be highlighted with the aid of a case study such as the present one may be very limited.

To conclude this brief overview of Feyerabend's claims about science, let us briefly consider his claims about the importance of rhetorical factors in the scientific enterprise. In his analysis of Galileo's work, Feyerabend isolates a number of rhetorical factors which featured in Galileo's work. These include deceptive tactics, utterances which are arguments in appearance only, propaganda, and psychological tricks.<sup>68)</sup>

Suppose Feyerabend is correct in claiming that such rhetorical factors play an important role in scientists' attempts to persuade others to adopt their theory choices. It is not quite clear what would follow from this fact. In his critical discussion of Feyerabend's views, Finocchiaro argues that it would be wrong to conclude that science is irrational. According to him (1980:191) rhetorical factors "are by themselves merely *alogical*, and they must be judged by their own criteria". Finocchiaro (1980:200) also claims that Feyerabend's "propagandistic-manipulative interpretation of scientific rationality may be taken as being itself a rhetorical exaggeration of the truth that rhetorical persuasion has an important role". The mere fact that rhetorical factors play a role in the scientific enterprise does not undermine the claim that science is a rational affair.

Newton-Smith also argues that even if it can be warranted that propaganda, etc., plays a role in science, this would not force

him to give up his rationalist position. He (1981:141) comments as follows on this issue, with specific reference to Feyerabend's analysis of Galileo's tower argument.

- (14) ". . . the rationalist will not be particularly interested in the claim, even if warranted, that Galileo succeeded only because of rhetoric, persuasion and propaganda. His claim is not that these never play a role but that a rational case can be reconstructed. He will argue that the rational case is to be construed through showing that this re-construal of the motion of the ball and the tower is justified in virtue of the fact that it is part and parcel of a general theory of motion superior to the pre-Copernican one."

As was pointed out above, both Laudan and Newton-Smith allow for a very wide variety of factors to play a legitimate role in theory choice. In this respect their models differ from the models of, for example, Popper and Lakatos. Against this background, it is very interesting to consider in more detail Feyerabend's claims about the importance of propaganda in determining the success of theories. He (1978:214) states that propaganda can be understood in one of two ways:

- (i) Propaganda can consist of "external" moves in favour of a theory which conflicts with "internal" standards.
- (ii) Propaganda can also consist of "misleading accounts which suppress difficulties in order to create a better press for some theory".

Feyerabend (1978:214) claims that he has shown "that Galileo used and had to use 'propaganda' in the sense of (1) [= (i) - M.S.] if we choose the usual 'internal' standards (up to and including Lakatos)". He claims that Galileo also made use of propaganda of the second type.

As regards the first type of propaganda, Feyerabend (1978:214) makes the following important remark on what counts as propaganda.

- (15) "Of course, if we choose different kinds of standards, for example if we permit standards to change, in an opportunistic manner from one case to the next, then the 'propaganda' turns into reason."

As was pointed out above, the range of internal factors identified by Laudan and Newton-Smith is much wider than the internal factors to which Feyerabend (1978:214) refers. Also, Newton-Smith (and to a lesser extent Laudan) allows for standards to vary from time to time, and from scientist to scientist. Much of what would count as propaganda in terms of, for example, Popper's and Lakatos' models would then not be propaganda relative to Laudan's and Newton-Smith's models. Given Laudan's and Newton-Smith's extended models of scientific rationality, Feyerabend's claims about the use of propaganda of the first type lose much of their force.

However, it would still be interesting to consider whether scientists make use of propaganda of the second type distinguished by Feyerabend, and of other rhetorical devices which are consciously and deliberately used with the intent to mislead. The expression "rhetorical trick" may be used to refer to such rhetorical devices.

One of the questions to be asked about Chomsky's work on binding theory will then be to what extent he makes use of such rhetorical tricks to persuade others to accept his theory choices. Note that no attempt will be made to provide a comprehensive account of the rhetorical aspect of Chomsky's work. Attention will only be paid to rhetorical tricks, in the sense outlined above.

### 2.3.6 Analysis of the various steps in the developmental history of binding theory

In §§ 2.3.2 - 2.3.5 a number of methodological issues have been isolated on which a case study such as the one proposed here can

in principle/ . . .

in principle throw some light. These issues are: (i) the role of truth in an account of the scientific enterprise; (ii) the relative importance of empirical and conceptual considerations in theory choice; (iii) the status of the considerations which play a role in theory choice; (iv) the role of conceptual considerations related to the associated general theory/research tradition in the appraisal of specific theories; (v) the role of normative difficulties in the development of specific theories; (vi) the role of non-rule governed judgment in theory choice; (vii) the importance of independent justification for modifications and auxiliary hypotheses introduced to protect a theory from potential negative evidence; (viii) the role of simplicity in theory choice; (ix) the possibility of change in the considerations which guide theory choice; (x) the role of rhetorical factors in the presentation of theory choices.

Against this background it is possible to formulate a number of specific questions that should be asked about each change  $T_x \rightarrow T_{x+1}$  which Chomsky made to binding theory. Only by paying attention to these questions when describing the developmental history of binding theory can one ensure that the resultant description will be "rich" enough to fulfil its intended role.

- (16) (a) What are the actual considerations on the basis of which  $T_{x+1}$  is chosen over  $T_x$ ?
- (b) For each consideration C that played a role in the choice of  $T_{x+1}$ : Is C an empirical or a conceptual consideration?
- (c) Does  $T_{x+1}$  have greater empirical success than  $T_x$ ?
- (d) Do the empirical and conceptual considerations relevant to the choice of  $T_{x+1}$  over  $T_x$  point in different directions? If so, how does Chomsky resolve the conflict?

(e) / . . .

- (e) Are there any aspects of the transition  $T_x \rightarrow T_{x+1}$  that can be explained only on the assumption that Chomsky's linguistics is truth-directed?
- (f) For each conceptual consideration C that played a role in the choice of  $T_{x+1}$ : Is C a conceptual difficulty generated by tension with a principle belonging to Chomsky's general theory/research tradition?
- (g) For each conceptual consideration C that played a role in the choice of  $T_{x+1}$ : Is C a conceptual difficulty generated by tension with the methodological norms of Chomsky's linguistics?
- (h) Does the change of  $T_x$  to  $T_{x+1}$  constitute an *ad hoc* (i.e., without independent justification) modification of  $T_x$  in the face of failure?
- (i) Do considerations of simplicity play any role in the choice of  $T_{x+1}$  over  $T_x$ ?
- (j) What role, if any, does non-rule governed judgment play in the choice of  $T_{x+1}$  over  $T_x$ ?
- (k) Has there been any change in the considerations on the basis of which  $T_{x+1}$  is chosen over  $T_x$ , compared to the considerations on the basis of which chronologically earlier choices were made?
- (l) Does Chomsky make use of rhetorical tricks to persuade others to accept the choice of  $T_{x+1}$  over  $T_x$ ?

#### 2.4 The "Galilean style of inquiry"

In several recent works - for example, (Chomsky 1978a:9-10; 1980a:24, 218) - Chomsky argues that linguists should adopt a

specific/ . . .

specific style of inquiry, the so-called "Galilean style of inquiry". The aim of § 2.4 is to determine what the main features of this style of inquiry are. Having identified the main features of this style of inquiry, it will then become possible to determine to what extent Chomsky's work on binding theory was conducted in this specific style.

Botha (1982a) contains an interesting analysis of various aspects of Chomsky's claims in this connection, including the main components of Chomsky's arguments for the adoption of this style of inquiry, Chomsky's attempt to give a metascientific characterization of the "Galilean style", the relation between Chomsky's conception of the "Galilean style" and the views held by philosophers and historians of science on Galileo's method(s) of inquiry, and so on. The following exposition of what constitutes the "Galilean style" of inquiry draws heavily on (Botha 1982a).

In his analysis of Chomsky's attempt at giving a metascientific characterization of the "Galilean style", Botha (1982a:5-6) identifies three mechanisms of this style of inquiry, as seen by Chomsky (1978a:9; 1980a:8, 218).<sup>69)</sup> The first mechanism is *abstraction*. Inquiry in the "Galilean style" entails the construction of abstract models. The second mechanism is *mathematization*. These abstract models are of a mathematical nature. The third mechanism is *epistemological tolerance*. The abstract, mathematical models are in some sense more real than the ordinary sensations of scientists. This third mechanism entails that scientists should adopt a tolerant attitude to empirical inadequacies exhibited by a theory - hence the name "epistemological tolerance". As Botha (1982a:12) explains, epistemological tolerance complements the use made of abstraction and idealization in defining the scope of a theory. If not all problematic data need to be explained by a linguistic theory, then not all linguistic data can constitute real negative evidence for this theory.

One of/ . . .

One of Botha's (1982a) main conclusions is that Chomsky's notion of the "Galilean style" is not itself adequate as a conceptual tool for gaining a better understanding of the way in which inquiry in generative grammar is currently conducted.<sup>70)</sup> Botha (1982a:9, 13) argues that both abstraction and epistemological tolerance have been characteristic of generative grammar, and Chomsky's work in particular, for many years. Neither of these can thus be the sole defining property of the "Galilean style" of inquiry as a *new* mode of linguistic inquiry. As regards mathematization, Botha (1982a:10) argues that mathematical concepts play no significant role in the construction of Chomskyan linguistic theories. Consequently, mathematization cannot be a defining property of the "Galilean style" in linguistics.

Botha (1982a:42) argues that it is possible to use the expression "the Galilean style" in a more liberal way with specific reference to Chomskyan linguistics. If the historical implications of the expression were "not taken too seriously", then it is possible to conceive of the mode of inquiry characterized in (17) as "the *lar* Galilean style of linguistic inquiry".

- (17) (a) To make progress in the scientific study of language (and mind), we should set, as the fundamental aim of inquiry, depth of understanding in restricted areas - and not gross coverage of data.
- (b) To get serious inquiry started, we should make radical abstractions and idealizations in defining the initial scope of the inquiry.
- (c) To capture the desired understanding or insight, we need unifying, principled theories deductively removed (perhaps far removed) from the primary problematic data.
- (d) To keep up the momentum of the inquiry, we should adopt
- an attitude/ . . .

an attitude of epistemological tolerance towards promising theories that are threatened by still unexplained or apparently negative data.

According to Botha (1982a:42), this mode of inquiry "undeniably represents one of the major tools of theoretical linguistics". He also argues that this mode of inquiry cannot be the sole methodological tool of theoretical linguistics. A mode of inquiry which allows for the establishment of empirical generalizations is also necessary.

Except where otherwise indicated, the expression "the Galilean style of inquiry" will be used below to refer to the style of inquiry characterized in (17), and called "the *lax* Galilean style of inquiry" by Botha (1982a). One of the questions to be asked about the developmental history of binding theory is to what extent Chomsky's work on this theory was conducted in this "lax Galilean style of inquiry", and to what extent Chomsky made use of other modes of inquiry.

Footnotes/ . . .

Footnotes to chapter 2

1. Cf. for example Bergström 1980 for an explication of six different interpretations of these terms which can be found in recent works. Some of these interpretations will be considered below.
2. Note that Newton-Smith uses the expression "a rational model" in place of "a model of rationality" used here.
3. Newton-Smith (1981:246) comments as follows on how cases should be handled where the goal of a scientist is not recognizably scientific in either his conception of science or in our conception of science:

"We can well imagine a scientist in an earlier era who seeks high office in the church being influenced by that goal to opt to work on the theory most pleasing to the church authorities (or a contemporary young scientist who seeks tenure selecting the programme advocated by the head of his department even though in his heart of hearts he believes it to be the scientifically inferior programme). In this case we can give a *minirat* account of his actions, but it will not be one that operates in terms of internal scientific factors. We do not explain his behaviour *qua* scientist, we explain it by reference to his non-scientific goals and beliefs."

4. This sense of rationality is the first identified by Bergström (1980:1-3). Minimal rationality, as set out above, corresponds to the third sense - called "subjective utility maximization" - identified by Bergström (1980:4-5).
5. Cf. § 2.3.3.6 below for more detail on Laudan's views on rationality.
6. Interestingly, Finocchiaro (1980:183) criticizes Feyerabend for not consistently practising the anthropological method which he preaches.

7. In this connection it is interesting to note that models of scientific rationality are frequently criticized for containing inconsistencies and obscurities. Consider in this connection the various critiques of proposed models of scientific rationality referred to in § 2.3 below.
8. Consider, for instance, the physics case studies by Clark, Frické, Musgrave, and Worrall in (Howson (ed.) 1976), the economics case studies by Blang, Coats, De Marchi, Leijonhufvud, in (Latsis (ed.) 1976), and various biology case studies, including for example (Michod 1981). A notable exception is Sabra's (1967) study of the history of theories of light. Sabra's approach is quite similar to the approach outlined below, in that he also paid close attention both to what the scientists in question actually did and their metascientific comments. Feyerabend's (1975) analyses of Galileo's work, being based on the anthropological approach, should also fall into this category. However, as pointed out by Finocchiaro, Feyerabend did not always apply this method. Cf. in this connection the reference in fn. 6 above.
9. For instance, questions arise concerning the consciousness of the beliefs in terms of which the actions of an agent can be explained. May reference be made to unconscious beliefs of the agent in a reconstruction of his rationality? Newton-Smith (1981:245) specifically refers to the *conscious* and beliefs of the scientist. Cf. for example Hempel 1965:478-486 for some discussion of the relevance of the conscious-unconscious distinction in determining rationality.
10. Consider in this connection the following remarks by Agassi (1981:322) on the explanation of historical events:

"We cannot explain historical events without making hypotheses concerning the aims, interests, and motives of those who have participated in them. And after we propose such explanatory hypotheses, we can try to argue rationally about their truth or falsity, and then improve on them."

11. Cf., for example Popper 1968; 1969; 1972; 1974 for Popper's views on scientific rationality, and other related issues.

Cf., for example, Lakatos 1970 for Lakatos' views of these issues.

Cf., for example, Kuhn 1967; 1970a; 1970b; 1974 for Kuhn's views on these issues.

Cf., for example, Feyerabend 1975; 1978 for Feyerabend's views on these issues.

Note that (Newton-Smith 1981) contains lucid expositions of the views of all the philosophers of science mentioned above.

12. The literature dealing critically with each of the four models referred to above is extensive, and no attempt will be made here to provide complete references. For criticisms of Popper's position, cf., for example, Grünbaum 1976a, b, c, d, Schilpp (ed.) 1974. For criticisms of Kuhn's position, cf., for example, Lakatos and Musgrave 1970, Gutting 1980b. For criticisms of Lakatos' position, cf., for example, Cohen e.a. 1976. For criticisms of Feyerabend's position cf., for example, the various reviews mentioned in (Feyerabend 1978). Newton-Smith's (1981:44-147) critical review of the views held by Popper, Lakatos, Kuhn, and Feyerabend also provides a useful account of the most serious shortcomings of these views.
13. Cf. Newton-Smith 1981:228 for the reference to Newtonian mechanics, and p. 224 for the reference to Freud's theory of psychoanalysis.
14. Cf. Laudan 1977:106-108 for some discussion of this point.
15. Cf., for example, Feyerabend 1975:chapter 17.

16. The overview which follows is based on this work. Cf. also Laudan 1981; 1982.

Many reviews of Laudan's views - many of them highly critical - have been published. Several of these will be referred to below.

17. Cf. Laudan 1977:125-127 for more detail.

18. Cf. Laudan 1977:23-25 for his notion 'empirical problem'. Laudan claims that empirical problems are easier to illustrate than to define. To ask, for instance, why heavy bodies fall toward the earth with regularity, is to pose an empirical problem.

19. Cf. § 2.3.4.10 below for more detail on the nature of the alleged differences between explaining facts and solving empirical problems.

20. According to Laudan, the cognitive weight/importance of an empirical problem is increased if

- (i) the problem is solved by a viable theory in the domain,
- (ii) the problem, which has proved anomalous for, or resisted solution by, certain theories in a domain, is solved by another theory,
- (iii) a new theory emerges which singles out the problem as archetypal,
- (iv) the problem can be shown to be more general than another.

The importance of an empirical problem within a domain is reduced if

(i) / . . .

- (i) the scientist's beliefs about what is the real state of affairs change, so that the presumed state of affairs which gave rise to the problem is no longer regarded as real,
  - (ii) the problem is expropriated by another domain,
  - (iii) a theory for which it was an archetype is abandoned.
21. Cf. Laudan 1977:49ff. for a detailed exposition of the nature of the two types of conceptual problems. As an example of an internal conceptual problem, Laudan (1977:50) refers to the alleged circularity of the kinetic-molecular theory. This theory explained the elasticity of gases by postulating elastic constituents (i.e., molecules). Critics of this theory pointed out that, because we understand no more about the causes of elasticity in solids than we do in fluids, the kinetic explanation is circular.

As an example of an external conceptual problem, Laudan (1977:51) refers to a problem faced by Ptolemy's astronomical theory. While this theory had great empirical virtues, it contained assumptions which were in conflict with an old astronomical assumption that the heavenly motions were "perfect" (i.e., that each planet moved in a perfect circle about the earth at constant speed). Laudan (1977:52) claims that "in spite of ingenious efforts to reconcile these differences by Ptolemy and others, most of the crucial conceptual problems remained, and were to plague the development of mathematical astronomy until the end of the seventeenth century . . .".

22. As an example of an external conceptual problem which arises from a relation weaker than logical incompatibility, Laudan (1977:52) refers to the problem which Newtonian physics created for seventeenth century mechanistic physiology,

which were/ . . .

which were based on the assumption that the various bodily processes were essentially caused by the mechanical processes of collision, filtration, and fluid flow. Laudan describes the way in which the problem arose as follows:

"Newtonian physics, while certainly allowing for the existence of collision phenomena, nonetheless shows that *most* physical processes depend upon more than the impacts between, and the motions of, particles. To the extent that 'mechanistic' (Cartesian inspired) theories of physiology postulate such processes as the *exclusive* determinant of organic change, they rest on a huge improbability. They are consistent with Newtonian physics (for that physics does not deny that there can be some material systems which are entirely mechanical); but it did seem highly implausible, given Newtonian physics, that a system as complex as a living organism could function with only a limited range of the processes exhibited in the inorganic realm."

23. The principle that particles can only interact by contact, and not by action at a distance, is an ontological principle of the research tradition of Cartesian physics (Laudan 1977:79). As an example of a methodological principle, Laudan (1977:80) refers to the inductivist principle - which allows for the espousal of only those theories which have been "inductively inferred from the data" - of a "strict Newtonian" research tradition.
24. Cf. Laudan 1977:78-95 for a discussion of the relation between a specific theory and its associated research tradition. During this discussion Laudan also provides historical examples to illustrate the different theoretical claims made by him.
25. Laudan distinguishes four factors which affect the importance of a conceptual problem.
  - (i) The greater the tension between two theories, the weightier the conceptual problem will be. That is, a problem arising from a logical inconsistency will

(all/ . . .

(all other things being equal) be weightier than a problem which arises from, for example, lack of mutual support.

- (ii) When a conceptual problem arises from a conflict between two theories,  $T_1$  and  $T_2$ , the seriousness of that problem for  $T_1$  depends on the acceptability of  $T_2$ . The greater the confidence about the acceptability of  $T_2$ , the weightier the problem, and vice versa.
- (iii) When two competing theories,  $T_1$  and  $T_2$ , exhibit the same conceptual problems, then these problems become relatively insignificant in the comparative appraisal of the two theories. However, when  $T_1$  generates conceptual problems which  $T_2$  does not, then these problems are highly significant in the appraisal of the relative merit of the two theories.
- (iv) The older a conceptual problem which threatens a theory, the more serious it is.

26. Laudan (1977:118) acknowledges that he is not the first to suggest a conceptual interpretation of *ad hocness*. He specifically refers to Lakatos, Zahar, and Schaffner in this regard. Laudan comments as follows on the difference between their conceptual interpretation of *ad hocness* and his own position:

"In all their discussion, however, conceptual *ad hocness* remains but one of many species of *ad hocness*, rather than the only legitimate sense. Still worse, none of these writers has indicated how conceptual *ad hocness* is to be assessed, nor even what it amounts to. Equally, all these writers leave us in the dark about how seriously, if at all, it should count against a theory if it is *ad hoc*. The seeming virtue of the present approach is that it separates spurious senses of *ad hoc* from legitimate ones, and it gives us machinery for assessing the degrees of cognitive threat posed by *ad hocness* to the theories which exhibit it."

27. Cf. Newton-Smith 1981:210-212 for this qualification.
28. Cf. Newton-Smith 1981:195 ff for this qualification.
29. Cf. Laudan 1977:125-126 for his objections against this notion. Cf. also Grünbaum 1976b for a well-known critique of Popper's theory of verisimilitude.
30. Cf. Newton-Smith 1981:198 for the technical details of his argument.
31. Cf. Newton-Smith 1981:22ff. for his views on the observational-theoretical distinction.
32. Cf. Newton-Smith 1981:206 for an explication of the notion 'observational nesting'.
33. Newton-Smith (1981:227) provides the following illustration of his views on the sources of fertility in a theory:

"This may come from a metaphorical component in the theory as in the early days of the ideal gas theory. Gases were thought to be like collections of small hard balls colliding in space. The metaphorical component suggests exploration of the similarities and dissimilarities with the phenomenon to which it has been likened. Fertility may also come from a novel idea as when, for example, Planck introduced the quantum of action in the course of explaining the distribution of radiation given off by a black body. This suggested the possibility of applying the idea of the quanta to other unexplained phenomena."

34. Newton-Smith (1981:227) refers to Freud's theory of psychoanalysis as a theory with fertility, but a poor track record.
35. Thus, according to Newton-Smith (1981:228), "it counted in favour of statistical mechanics that it was able to explain the predictively successful laws of thermodynamics".

36. To illustrate this point, Newton-Smith (1981:228) refers to the fact "most scientists would agree that if, as seems to be the case, there is no way of integrating Quantum Mechanics and General Relativity, one or other of those theories cannot be correct as they stand."
37. Newton-Smith (1981:228) refers to Newtonian mechanics as a smooth theory, since there is something systematic in its failures. For instance, this theory fails for high speeds. For this reason the theory is regarded "as being on to something, even though it will not do as it stands".
38. Cf. § 2.3.4.8 below for more detail on Newton-Smith's view on *ad hoc*-ness.
39. Cf. § 2.3.2.3 above for Laudan's view.
40. Cf. the discussion in § 2.3.4.11 below for more detail.
41. Cf. § 2.3.4.10 below for a brief discussion of the alleged differences, and for some of the criticisms raised against Laudan's notion of solving empirical problems. Note that even if Laudan's claim about the differences between solving empirical problems and explaining facts must be upheld, the main point - namely that Laudan's empirical-conceptual distinction is one commonly found in the literature - is not affected.
42. Cf., for example, Caws 1966:232 and Harré 1967, especially chapters 6 and 7, in which Harré distinguishes factual (= empirical) from nonfactual considerations. Note also that, on the whole, reviewers of Laudan's work also accept his empirical-conceptual distinction (even though they are highly critical of many of Laudan's claims in connection with the solving of empirical and conceptual problems). Cf. in this connection, for example, Gutting 1980a,

Feyerabend 1981, McMullin 1979, Nickles 1981, Sarkar 1981.

43. Laudan and Newton-Smith are in fact representatives of two general, conflicting views on the role which truth should play in theory appraisal. Newton-Smith represents realism, which crucially involves the assumption that theories should be appraised in terms of their truth or falsity, where truth is understood in terms of the correspondence view of truth. That is, the truth or falsity of a proposition depends on how the world is independently of ourselves. Laudan represents instrumentalism, which denies the appropriateness of appraising theories in terms of the categories of truth and falsehood.

Cf. Newton-Smith 1981:28-34, 187 for an exposition of the realism-instrumentalism controversy, during the course of which Newton-Smith tries to clarify his own position and that of Laudan within the context of the controversy. Note that Newton-Smith classifies Laudan as an epistemological instrumentalist, and not as a semantical instrumentalist on the grounds that Laudan agrees that theories have truth values.

44. Cf. Siegel 1983:109-110 for a summary of the main criticisms aimed at Laudan's claim that his model is truth-independent, as well as for additional references to critical discussions of Laudan's work.
45. Cf., for example, Newton-Smith 1981:186ff. The essence of Newton-Smith's criticism is that without reference to truth, no distinction can be made between spurious and nonspurious problems, and without such a distinction it is impossible to account for the scientific enterprise. Consider in this connection the following remarks by Newton-Smith (1981:190).

"Unless/ . . .

"Unless truth plays a regulative role, we can each select on the basis of our whims our own set of sentences which are statements of problems for us just because we so choose to regard them. We each then erect our own theories for solving these problems. Never mind how the world is, just solve your own problems! We should be faced with the unedifying spectacle of a plurality of freefloating sets of problems and their associated theories, where some of the theories would rate equally well on the theory assessment scale. It simply is just utterly implausible to suppose that progress could arise through a developing sequence of theories solving ever more spurious problems. This model makes nonsense of the entire scientific enterprise. For truth does play a regulative role in the sense that theories designed to solve a problem whose corresponding statement has been shown to be false (or likely to be false) are condemned for that very reason."

46. Cf., for example, Gutting 1980a:97; McMullin 1979:634. Gutting (1980a:97) comments as follows on the link between Laudan's emphasis on the ontological component of a research tradition and truth:

"Laudan also rightly insists that one essential component of almost all research traditions is an ontology; that is, a specification of the fundamental entities that populate a scientific domain and of the sorts of interactions that are possible among them. But once again this fact is hardly intelligible if we ignore the truth-directedness of science. If a theory is not directed toward truth, why should it be required to solve problems in terms of a particular view of the nature of reality? Purely formal accounts would surely be as satisfactory as any if we required only the solution of problems and had no pretension to describe reality."

47. Cf., for example, Gutting 1980a:96; Nickles 1981:102. Consider, for instance, Gutting's remarks on external conceptual problems which arise from conflict with nonscientific theories.

"Theological and natural scientific theories, for example, have such widely different domains and methodologies that inconsistencies in the limited areas where they occasionally overlap would surely be of the most minor significance were it not that inconsistency

entails the falsehood of at least one of the theories. And surely in the great historical instances of conflict between science and theology (e.g., the cases of Galileo and of Darwin), the violence of the controversies was due to the fact that those who saw an inconsistency in the conjunction of a theological and a scientific account regarded one or the other as false."

48. Cf., for example, Gutting 1980a:97-8; Leplin 1981:273; Nickles 1981:102.

Gutting (1980a:93) makes the relevant point about Laudan's justification for the scientific enterprise as follows:

"He [= Laudan - M.S.] immediately rejects any justification in terms of the truth science attains on the grounds that we have no reason to think any scientific theory is true or even probable. But oddly enough Laudan's own suggestion for a justification is that 'man's sense of curiosity about the world and himself is every bit as compelling as his need for clothing and food' (p. 225). Now surely man's 'curiosity about the world and himself' can refer only to a desire to know the *truth* about the world and himself and will not be satisfied by truth-independent solutions to intellectual problems. It seems that when an ultimate justification of the aim of science is required Laudan himself slips back into a truth-directed view."

49. Cf. the discussion in § 2.3.4.1 above, and the references cited there.
50. As regards empirical problems, Laudan (1977:87) claims that the rise of the Cartesian mechanistic research tradition in the seventeenth century radically transformed the accepted problem domain for optical theories.

"It did so by arguing, or rather by simply postulating, that problems of perception and vision - problems which had classically been regarded as legitimate empirical problems for any optical theory - should be relegated to psychology and to physiology, fields outside the domain of optics, so that such empirical problems could be safely ignored by the mechanistic optical theorist."

Laudan (1977:88) illustrates the influence of the research tradition on the conceptual problems of a theory with Huygens' general theory of motion.

"when . . . Huygens came to develop a general theory of motion, he found that the only empirically satisfactory theories were those which assumed vacua in nature. Unfortunately, Huygens was working squarely within the Cartesian research tradition, a tradition which identified space and matter and thus forbade empty spaces. As Leibniz and others pointed out to Huygens, his theories were running counter to the research tradition which they claimed to instantiate. This was an acute conceptual problem of the first magnitude, as Huygens himself sometimes acknowledged."

51. Cf. § 2.3.4.6 below for a discussion of the influence of the methodological component of a research tradition on its specific theories, and Newton-Smith's failure to build this into his model of theory appraisal.
52. Laudan (1977:59-60) provides the following example to illustrate how the methodological norms adhered to by scientists can generate external conceptual problems for their theories.

"By the 1720s, the dominant methodology accepted alike by scientists and philosophers was an *inductivist* one. Following the claims of Bacon, Locke, and Newton himself, researchers were convinced that the only legitimate theories were those which could be inductively inferred by simple generalization from observable data. Unfortunately, however, the direction of physical theory by the 1740s and 1750s scarcely seemed to square with this explicit inductivist methodology. Within electricity, heat theory, pneumatics, chemistry and physiology, Newtonian theories were emerging which postulated the existence of imperceptible particles and fluids - entities which could not conceivably be 'inductively inferred' from observed data. The incompatibility of these new theories with the explicit methodology of the Newtonian research tradition produced acute conceptual problems."

53. Cf. Laudan 1977:80 for this brief, informal characterization of the methodological component of a research tradition.

54. Cf. § 2.3.3.5 above for more detail on the role of non-rule government judgment in Newton-Smith's model.
55. Cf., for example, McMullin 1979:637ff.; Gutting 1980a:98ff.; Nickles 1981:104; Sarkar 1983:68, 70; Siegel 1983:104ff.; Newton-Smith 1981:192ff.
56. Cf. in particular the discussion by McMullin 1979:637ff. of the difficulties involved in individuating, counting, and weighting empirical and, especially, conceptual problems.
57. Cf. § 2.3.3.3.8 above for an exposition of Newton-Smith's views on simplicity in theory appraisal.
58. Cf. the discussion of Newton-Smith's notion 'observational success' in § 2.3.3.2 above.
59. Cf. the references cited in fn. 55 above. One of the criticisms levelled at Laudan's model is that the determination of the problem-solving effectiveness of a theory is objectionably internal. Both what counts as a problem for a theory and its weight are to a large extent determined by the theory's research tradition. In spite of what Laudan claims, it is then not possible to objectively appraise theories from competing research traditions on the basis of their problem-solving effectiveness.
60. Cf., for example, Newton-Smith 1981:186ff., and McMullin 1979:636.
61. Feyerabend (1981:63) argues that Popper, Lakatos, and Kuhn also made provision for paradigm-dependent standards, in addition to trans-temporal, trans-paradigmatic standards.
62. Note incidentally that, as regards the possibility of such changes, Feyerabend (1981:66ff.) criticizes Laudan's model, claiming/ . . .

claiming that the possibility of such changes allows the model to circumvent rules without violating them. In effect, then, Laudan's model can be "pushed into excluding trivialities only". Cf. also Gutting 1980a:99 for a similar criticism.

63. The reader will be struck by the fact that there is no reference in the discussion above to critical reviews of Newton-Smith's temperate rationalism. This is in sharp contrast with the extensive reference to critical reviews in the presentation of Laudan's views. The lack of references to critical reviews in Newton-Smith's case is merely a reflection of the fact that very few reviews of (Newton-Smith 1981) have as yet appeared. Only two fairly short reviews have come to my notice: (Adler 1983), and (Kourany 1983). While both reviewers (and Adler in particular) raise certain criticisms against Newton-Smith's work, both are, overall, fairly positive in their appraisal. Since none of the criticisms raised by Adler and Kourany have any direct bearing on those aspects of Newton-Smith's work that are relevant for the present study, these criticisms can be ignored. In fact, both Adler (1983:92) and Kourany (1983:475) appraise Newton-Smith's views on what constitutes a rational model of science - which is the aspect most relevant for the present study - positively.
64. Kuhn can also, at least on one reading of his work, be read as claiming that science is not a rational affair. Cf., for example, Newton-Smith's (1981:102-124) discussion of Kuhn's views. However, even on this interpretation Kuhn's views are less radical than Feyerabend. The following two quotations from (Newton-Smith 1981) capture the differences between their respective views.
- (i) "I have described Kuhn as a temperate non-rationalist, for unlike Feyerabend he sees the scientific community as agreed on certain good-making features of theories.

At times Kuhn gives a rationalistic-sounding perspective unlike that described in the above passage by stressing the role of internal factors in accounting for scientific change. For he argues that the build-up of anomalies in the case of a mature science is more important than the external factors in bringing about a paradigm shift. However, in spite of the fact that he talks at times of the possibility of there being good reasons to prefer one paradigm to another, he remains a non-rationalist. For, as we have seen, what are taken by the scientific community (according to Kuhn) to be good reasons for preferring one paradigm to another cannot be objectively justified. What makes the reasons 'good' is that they are generally accepted by the community, and if one wants to be a member of that community one will operate within the framework of this system of 'reasons'. Kuhn, in holding that there is a system of rules, differs dramatically from Feyerabend, the self-styled anarchistic non-rationalist, who denies that there is any agreement of this sort running through the historically evolving scientific community." (Newton-Smith 1981:122.)

- (ii) "He (= Feyerabend - M.S.) stands against the venerable tradition of searching for a system of rules which it is held ought to guide scientists in the business of theory choice. According to him no such system of rules can be found and to adopt any particular rules or methodology can only have the effect of impeding scientific progress: *'The only principle that does not inhibit progress: anything goes'*. By this he means that if one wants to have exceptionless rules that can be applied come what may, they will be so empty and indefinite that nothing is ruled out by them. Feyerabend is thus much more radical in his critique of rationalism than Kuhn. For Kuhn holds that there are rules held in common by all members of the scientific community. The application of the rules may be problematic and the rules cannot be given an objective justification. All the same there are rules (the five ways). For Feyerabend on the other hand no rules having any real content or force can be abstracted from scientific practice. Feyerabend is thus a paradigm case of what I called . . . a non-rationalist." (Newton-Smith 1981:126.)

In both (i) and (ii) the footnotes are omitted.

If, as will be argued below, there are significant similarities between the radical views held by Feyerabend and those

held by/ . . .

held by Newton-Smith, then there is little sense in exploring for the purposes of the present study, in detail the differences between the less radical views held by Kuhn and Newton-Smith's views.

It is not being claimed here that there are no differences between Kuhn's views and Newton-Smith's views, just as it is not being claimed that there are no significant differences between Feyerabend's and Newton-Smith's views. Rather, it is claimed that there are no differences which can fruitfully be investigated within the context of the present study.

65. In his discussion of Feyerabend's views, Finocchiaro (1980: 188) also argues that Feyerabend assumes that individual scientists are methodical, and that their method can be described.

66. Feyerabend (1978:32) continues:

"Moreover, I suggest a new *relation* between rules and practices. It is this relation and not any particular rule-content that characterizes the position I wish to defend."

Feyerabend (1978:165) clarifies his position on the relation between science and practice as follows.

*". . . I regard every action and every piece of research both as a potential instance of the application of rules and as a test case: we may permit a rule to guide our research, or the kinds of actions we are interested in, we may permit it to exclude some actions, to mould others and on the whole to preside like a tyrant over our activities, but we may also permit our research and our activities to suspend the rule or to regard it as inapplicable even though all the known conditions demand its application. In considering the latter possibility we assume that research has a dynamics of its own, that it can proceed in the absence of clearly formulated rules and that research so conducted is substantial enough to gain attention from the defenders of the status quo*

and orderly/ . . .

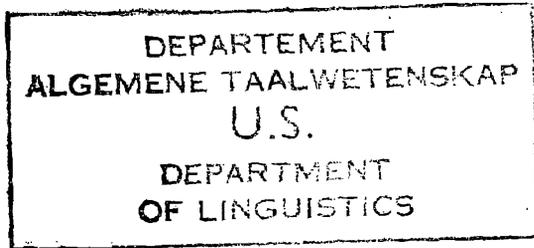
and orderly enough to serve as a source for new and as yet unknown procedures."

67. Cf. Newton-Smith 1981:134 on some practical and principled problems involved in obtaining the historical evidence required to support Feyerabend's position.
68. Cf. Finocchiaro 1980:190 for a complete list, plus page references to Feyerabend's work where he discusses the various devices.
69. As Botha (1982a:5) points out, Chomsky takes over his characterization of the Galilean Style from the physicist Weinberg, who (1976:28) presents the following definition of this style.

"We have all been working in what Husserl called the Galilean style; that is, we have all been making abstract mathematical models of the universe to which at least the physicists give a higher degree of reality than they accord to the ordinary world of sensation."

These remarks are quoted by Chomsky (1980a:8, 218), and paraphrased by Chomsky (1978a:9).

70. Botha's (1982a) other main conclusion is that the Chomsky-Weinberg characterization of the Galilean style lacks the necessary historical and philosophical basis.



## Chapter 3

## CONDITIONS WHICH RESTRICT THE APPLICABILITY OF SYNTACTIC TRANSFORMATIONS AND RULES OF SEMANTIC INTERPRETATION

3.1 General remarks

The primary work in which the Specified Subject Condition (SSC) and the Tensed-S Condition (TSC) are presented as conditions that restrict both syntactic transformations and rules of semantic interpretation is "Conditions on transformations" (Chomsky 1973). In § 3.2 below Chomsky's proposals regarding the two conditions in this work are discussed in detail. Other works by Chomsky in which the SSC and TSC are presented as conditions on syntactic transformations and rules of semantic interpretation include "Conditions on rules of grammar" (henceforth Chomsky 1976a), and "On the nature of language" (henceforth Chomsky 1976b). The relevance of these works for the developmental history of the SSC and TSC is discussed in § 3.3 below.

3.2 The introduction of the SSC and TSC in "Conditions on transformations"3.2.1 General remarks

§ 3.2 is organized as follows: In § 3.2.2 the content of the conditions, as presented in (Chomsky 1973), is outlined. The topics dealt with in the other subsections are: the incorporation of the conditions in linguistic theory and Chomsky's approach towards solving the problem of language acquisition (§ 3.2.3); the evidence presented by Chomsky for the status of the SSC and TSC as universal principles (§ 3.2.4); the effect of the introduction of the conditions on the formal power of transformational rules (§ 3.2.5); the naturalness of the SSC as a consideration that justifies the introduction of this condition (§ 3.2.6); Chomsky's reaction to various empirical difficulties noted at the time of the introduction of the conditions (§ 3.2.7).

3.2.2 Linguistic background

The formulations of the SSC and TSC adopted in (Chomsky 1973: 257) are as follows.

(1) *The SSC*

No rule can involve X, Y (X superior to Y) in the structure  
... X ... [<sub>α</sub> ... Z ... - WYV ...] ...

where Z is the subject of WYV and is not controlled by a category containing X.<sup>1)</sup>

(2) *The TSC*

No rule can involve X, Y (X superior to Y) in the structure  
... X ... [<sub>α</sub> ... Z ... - WYV ...] ...

where Y is not in COMP and α is a tensed S.

No definition of the notion 'involve' is provided in (Chomsky 1973). It is clear, however, that the notion must cover both syntactic movement rules and rules of semantic interpretation. Both these types of rules are claimed to be constrained by the SSC and TSC in (Chomsky 1973) - see the discussion in § 3.2.4 below.

Although the formulations (1) and (2) imply that X must be to the left of Y, Chomsky (1973:272) suggests that the conditions should be generalized, eliminating the left-right asymmetry.

The SSC, as formulated in (1), has two subcases: (i) where Z is not controlled at all, i.e., where Z is a lexical subject, and (ii) where Z is controlled by a category which does not contain X. The various components of the SSC are illustrated by the following sentences.<sup>2)</sup>

(3) a. The men each expected [<sub>S</sub>the soldier to shoot  
the other]

{25a}

b. \*The/ . . .

- b. \*The men expected the soldier to shoot each other {25b}
- (4) a. The men each saw [NP pictures of the other] {28a}  
 b. The men saw pictures of each other {28b}
- (5) a. The men each saw [NP John's pictures of the other] {29a}  
 b. \*The men saw John's pictures of each other {29b}
- (6) a. The candidates each expected [S PRO to defeat the other] {24a}  
 b. The candidates expected to defeat each other {24b}
- (7) a. We each persuaded Bill [COMP PRO to kill the other(s)] {113}  
 b. \*We persuaded Bill to kill each other {112}

Chomsky (1973:238) assumes that the (b)-sentences in (3)-(7) are all derived from the (a)-sentences by a rule of *each*-Movement, which moves *each* into the determiner position of *the other*.<sup>3)</sup> In each case X = *each*, and Y = *the other*.

In (3a) the lexical subject Z (= *the soldier*) intervenes between X and Y. Movement of X to Y to derive (3b) is thus prohibited by the SSC. In (3),  $\alpha$  is S. In (4) and (5),  $\alpha$  is NP. In (4a) there is no subject Z (subject being optional in NP), and *each*-Movement may apply to derive (4b). In (5a) a lexical subject *John's* intervenes between X and Y. The SSC thus prohibits the derivation of (5b).

In (6) and (7) there are no lexical subjects in the embedded clauses, but PRO-subjects controlled by some category. In (6a) the subject PRO is controlled by a category containing X, namely *the candidates each*. *each*-Movement can therefore apply to derive (6b). In (7a) PRO is controlled not by a category con-

taining/ . . .

taining *each*, but by *Bill*. The SSC thus prohibits the application of *each*-Movement to derive (7b).

The TSC stipulates that no rule can involve X and Y when Y is in a tensed sentence. This is illustrated by the following sentences. (The case where Y is in COMP will be discussed in § 3.2.7 below).

- (8) a. The candidates each expected the other(s)  
to win (21b)  
b. The candidates expected each other to win (22b)
- (9) a. The candidates each expected that the other(s)  
would win (21c)  
b. \*The candidates expected that each other would  
win (22c)

In (8) and (9) the (b)-sentence is derived by the rule of *each*-Movement. In (9) *each*-Movement moves X (= *each*) to the position Y (= *the other(s)*), which is in a tensed clause. Consequently, the derivation of (9b) is prohibited by the TSC. In (8) Y is in a nontensed clause, and so the TSC does not prohibit the derivation of (8b).

Chomsky (1973:236) points out that the TSC, formulated as in (2) above, subsumes the Insertion Prohibition, if the latter is in fact restricted to tensed clauses. The Insertion Prohibition was proposed by Chomsky (1965:146), and stipulated that morphological material cannot be inserted into sentences which have already been passed in the cycle.

### 3.2.3 The SSC and TSC and the fundamental empirical problem of linguistics

Chomsky (1973:232) explicitly relates the introduction of conditions such as the SSC and TSC to his attempt to solve what he

regards/ . . .

regards as the fundamental empirical problem of linguistics: How does a child acquire knowledge of his language?<sup>4)</sup> Since the early sixties Chomsky has singled out the solution of this problem as one of the main goals of linguistic theory. The discussions of explanatory adequacy<sup>5)</sup> in, for example, *Current issues in linguistic theory* (1964:28-29) and *Aspects of the theory of syntax* (1965:25-26), make it quite clear that linguistic theory must aim at providing an explanation for the acquisition of language by a child.<sup>6)</sup> Chomsky has stated and defended his position on the acquisition of knowledge of language in a number of works.<sup>7)</sup> If one puts aside certain irrelevant changes which this position has undergone through the years, the essence of Chomsky's position can be expressed as follows.<sup>8)</sup>

- (i) A human language is a rich and complex system.
- (ii) The data on the basis of which knowledge of this system can be acquired is impoverished.<sup>9)</sup>
- (iii) Given (i) and (ii), the acquisition of knowledge of a human language can only be explained on the assumption that human beings have, as part of their biological endowment, a set of restrictive principles "determining the general framework of each human language and perhaps much of its specific structure as well".<sup>10)</sup>

Since the middle of the seventies Chomsky has generally referred to the biological endowment that underlies language acquisition as "the initial state of the language faculty".<sup>11)</sup> Chomsky (1980a:233) explicates the role which the assumption of a rich, restrictive initial state of the language faculty must play in the explanation of the acquisition of knowledge of language as follows.<sup>12)</sup>

- (10) "The child's initial state, it seems, must lay down the general principles of language structure in fair detail, providing a rich and intricate schematism that determines

(1) the content of linguistic experience and (2) the specific language that develops under the boundary conditions given by this experience. If the initial restriction is sufficiently severe, it will be possible for the child to attain a system of great intricacy on the basis of limited data, data sufficient to rule out all possibilities but one or a few. Then he will know the language compatible with his limited experience, though there will be no relation of generalization, abstraction, induction, habit formation, or the like that relates the system attained at the final state to the data of experience. The relation between experience and knowledge will be quite abstract. The principles of language structure incorporated in the initial state express the relationship."

The linguist's characterization of the initial state of the language faculty is called "a universal grammar (UG)". (Note that the enterprise of characterizing the initial state of the language faculty is also called "universal grammar"). The various final states that are acquired are characterized by particular grammars.<sup>13)</sup> Against the background outlined above, it is obvious that a UG must be as restrictive as possible if it is to contribute to the solution of the problem of language acquisition. That is, a UG must delimit the class of grammars available to the language learner as narrowly as possible.<sup>14)</sup>

It is important to keep in mind that the crucial issue is not restricting the class of *possible* grammars. Rather, the crucial issue is restricting the class of *available* grammars. This point is explained as follows by Chomsky (1977c:125).

- (11) "Reduction of the class of available grammars is the major goal of linguistic theory. To account for the fact that language is acquired as it is, we must find ways to restrict the "space" of potential grammars to be searched by the language learner. Note that reduction of the class of grammars is not in itself an essential goal, nor is restriction of the class of generable languages; it is the class of 'available' grammars that is important. We might in principle achieve a very high degree of explanatory adequacy and a far-reaching psychological theory of language growth even with a theory that permitted a grammar for every recursively enumerable language. The reasons are those outlined in Chomsky (1965), chapter 1, section 9. What is important is the cardinality of the class of grammars that are compatible with reasonably limited data and that are sufficiently highly valued."

In a brief overview of the development of generative linguistics, Chomsky (1978a:13) point out that the emphasis was initially on descriptive adequacy.<sup>15)</sup> Descriptive adequacy, in contrast to explanatory adequacy, often requires elaborating the available theoretical mechanisms, and thus extending the class of available grammars.<sup>16)</sup> The concept of a transformational rule, for instance, was introduced precisely because such rules had great descriptive formal power, and could thus help in overcoming certain problems of descriptive adequacy faced by phrase-structure grammar.<sup>17)</sup> However, the initial concept of a transformational rule was too rich in formal power, and made far too large a class of grammars available. The basic goal of explanatory adequacy was therefore "left remote", as Chomsky (1978a:14) puts it.

By the early sixties the goal of explanatory adequacy became more prominent. More emphasis was accordingly placed on the development of a restrictive UG, that is, a UG that narrowly restricts the class of available grammars. Thus Chomsky (1965:46) declares that "the most crucial problem for linguistic theory seems to be to abstract statements and generalizations from particular descriptively adequate grammars and, wherever possible, to attribute them to the general theory of linguistic structure, thus enriching this theory and imposing more structure on the schema for grammatical description". In this way, according to Chomsky, linguistic theory may move towards explanatory adequacy (that is, towards an explanation of the acquisition of knowledge of language).<sup>18)</sup> In discussions of early conditions on transformations, these conditions are regarded as potential contributions to explanatory adequacy.<sup>19)</sup> Consider, for example, the discussions of the condition of recoverability of deletion in (Chomsky 1964:40-42) and (Chomsky 1965:144-145).<sup>20)</sup> In these discussions the condition of recoverability of deletion is explicitly presented as a restriction on the theory of transformations, a restriction that contributes to explanatory adequacy.

By the early seventies transformational rules still had too much

formal power/ . . .

formal power, especially if one examines the actual descriptive work of that period. Consider the characterization of the structural descriptions of transformational rules outlined by Chomsky (1976a:309). This characterization limits the structural descriptions of transformations to simple strings  $(\alpha_1, \dots, \alpha_n)$ , where  $\alpha$  can be a terminal string, a category, or a variable.<sup>21)</sup> Chomsky (1976a:310) claims that this still gives too much formal power to transformations. However, in the descriptive work of that period a much richer theory of transformations was presupposed. Chomsky (1976a:310) list eight proposed enrichments to the characterization of transformational structural descriptions referred to above. For example, the latter characterization excludes transformations formulated in terms of relational notions such as "subject". Postal (1976: 151, fn. 7) lists no less than sixteen works dating from the late sixties and early seventies in which such rules are proposed.<sup>22)</sup>

The introduction of the various conditions on transformations in (Chomsky 1973) must be seen against the background outlined above. Chomsky (1973:232-234) distinguishes between two complementary approaches towards solving the problem of language acquisition. A first approach attempts to formulate what Chomsky (1973:232) calls "conditions on form", that is, conditions on the systems that qualify as grammars. Conditions on form thus restrict the class of possible grammars. Among the conditions on form referred to by Chomsky (1973:233-234) is "the definition of a grammatical transformation as a structure-dependent mapping of phrase markers into phrase markers that is independent of the grammatical relations or meanings expressed in these grammatical relations". This entails that "transformations generally apply to phrase markers that meet some condition on analyzability with no regard to other associated properties". This characterization of transformations is in essence the same as that outlined in (Chomsky 1976a:308-309).

A second/ . . .

A second approach towards solving the problem of language acquisition attempts to formulate what Chomsky (1973:232) calls "conditions on function", that is, conditions on the way the rules of a grammar apply to generate structural descriptions. Such conditions on function limit the generative power of grammars of a given form. While conditions on function restrict the operation of the rules of a grammar, they do not affect the form of these rules. Thus, conditions on function do not directly restrict the class of formally possible rules (nor the class of formally possible grammars). The conditions on function mentioned by Chomsky (1973:234) include the condition of recoverability of deletion and the A-over-A condition.<sup>23)</sup> The SSC and the TSC (and the other conditions proposed in (Chomsky 1973)) are also conditions on function.<sup>24)</sup>

Although conditions on function do not directly restrict the class of possible rules (and grammars), they can indirectly contribute to this end.<sup>25)</sup> Recall that conditions on form directly restrict the class of possible rules and grammars. Restrictions on the form of rules can lead to misgeneration, since specific conditions on their application can no longer be built into the rules themselves. Suppose that general conditions on function (that is, condition on rule application) can compensate for the loss of formal power resulting from such restrictions on the form of rules. The conditions on the function of rules will then indirectly contribute towards restricting the class of formally possible rules by making it possible to uphold highly restrictive conditions on the form of rules, and hence the class of possible grammars.

In (Chomsky 1973), and also earlier works, no explicit mention is made of the link between conditions on form and conditions on function outlined above.<sup>26)</sup> However, this link is implicitly recognized by Chomsky (1973). Thus, in the argument involving the Passive transformation, Chomsky (1973:237) links the introduction of the TSC (a condition on function) with upholding

certain/ . . .

certain restrictions on the structural descriptions of transformations (conditions on form).<sup>27)</sup> In works that follow (Chomsky 1973), Chomsky strongly emphasizes the indirect contribution of conditions of function towards restricting the class of possible rules and grammars. Consider, for instance, the discussion of conditions on function in "Conditions on rules of grammar" (1976a:307-308), the "Introduction" to *Essays on form and interpretation* (1977b:20), and *Reflections on language* (1975a:111).

The great emphasis on the (indirect) contribution of conditions on form towards restricting the class of possible rules and grammars in works that follow (Chomsky 1973), reflects the crucial role which the goal of restricting the class of possible grammars has played in these works. While it is true that restricting the class of *available* grammars rather than restricting the class of *possible* grammars as such constitutes the major goal of linguistic theory, Chomsky's work since the middle seventies was quite explicitly aimed at achieving this by reducing the class of possible grammars. Consider, in this connection, the severe restrictions on the form of transformational rules and rules of construal proposed in (Chomsky 1976a:308-313) and (Chomsky 1977c:74-76). This work culminated in the formulation of a UG that permits only a finite number of possibilities.<sup>28)</sup> According to Chomsky (1981a:12), if it is indeed the case that UG permits only a finite number of possibilities, then the logical problem of language acquisition is in fact solved (or, at least, trivialized).<sup>29)</sup>

However, it is important to note that Chomsky's success in formulating a UG that permits only a finite number of possibilities crucially depends on two interrelated assumptions. Firstly, one must distinguish a core grammar for each language, within which all rules are subject to severe restrictions. Secondly, a language may have marked rules that fall outside the core grammar, and that do not obey the severe restrictions applicable to the

rules of core/ . . .

rules of core grammar. The important role which the notions of core grammar and markedness (and other related notions) have played in Chomsky's work during the past ten years will be examined in detail below. Here it is sufficient to note that, given the assumptions formulated above, it becomes possible to impose severe restrictions on the rules of core grammar, without any commitment to the claim that these restricted rules can generate all the sentences of a language. Thus, when it is claimed that UG permits only a finite number of possibilities, one must keep in mind that UG makes available a finite class of *core* grammars, where a core grammar is not a complete representation of the system of knowledge which a speaker of a language has "inside his head".<sup>30)</sup> It should be obvious that the finiteness of the class of core grammars provides a partial solution only for the logical problem of language acquisition. The problem of language acquisition outside the domain of core grammar would be left unsolved.

A question that arises at this point is why so much emphasis has been placed on conditions on function since the early seventies. The following remarks by Chomsky (1982a:74) shed some light on this question.

- (12) "What Ross's dissertation really did, and which was not done in *Current Issues* was to make it very clear that there was going to be a theory of conditions. *Current Issues* contained some proposals about some weird things, but Ross's work, I think, made it very clear that these were not just some weird things but that these were going to be the essence of the field, and that the main problem would then be to explain them." 31)

The theory of conditions referred to in (12) is in fact a theory of conditions on *function*. It seems clear then that Ross's work on the island conditions played a crucial role in directing the attention to conditions on function as a means of developing a UG that narrowly restricts the class of available grammars.<sup>32)</sup>

The conclusions that can be drawn about the incorporation of the SSC and TSC in UG on the basis of the discussion above, can be summarized as follows.

- (13) a. The incorporation of the SSC and TSC in UG is motivated by Chomsky's aim of developing a restrictive UG (that is, a UG that narrowly restricts the class of available grammars) by means of conditions on the function of rules.
- b. In the early works in which conditions on function are discussed, Chomsky emphasizes that such conditions contribute towards developing a restrictive UG by restricting the generative power of grammars of a given form. In works that follow (Chomsky 1973), the emphasis is on the indirect contribution of such conditions towards restricting the class of possible grammars. Although in (Chomsky 1973) the contribution of conditions on function towards restricting the class of possible grammars is implicitly recognized, in his explicit comments on such conditions Chomsky refers only to their contribution towards restricting the generative power of grammars of a given form.
- c. The emphasis on the contribution of conditions on function towards restricting the class of possible grammars in works that follow (Chomsky 1973), is a reflection of the fact that Chomsky's work since the middle seventies has been specifically aimed at reducing the class of possible grammars, work which culminated in the formulation of a UG which permits only a finite number of grammars.

#### 3.2.4 The SSC and TSC as principles of UG

Chomsky (1973) presents evidence that the following rules of English obey the SSC and TSC.<sup>33)</sup>

(i) *The Passive transformation*

Chomsky (1973:237) proposes that Passive has the structural description (X, NP, V, NP, Y), and that it rearranges the NPs. The examples in (14) illustrate that Passive obeys the TSC.

- (14) a. I believe the dog is hungry {17}  
b. \*The dog is believed is hungry (by me)

In (14a) the NP *the dog* is extracted from a tensed sentence in order to derive (14b). Consequently, (14b) is ruled out by the TSC.

(ii) *each-Movement/each-Insertion*

Chomsky (1973:238) follows Dougherty in adopting a rule which derives (15b) from (15a), by moving *each* into the determiner position of *the other(s)*.

- (15) a. The men each hated the other(s).  
b. The men hated each other.

Chomsky (1973:238, fn. 17) notes that if a rule of *each-Interpretation* were adopted instead of a rule of *each-Movement*, then the relevant conditions would apply to this interpretive rule.

The sentences in (16) and (17) illustrate that *each-Movement* obeys the TSC and SSC, respectively.

- (16) a. The candidates each expected [<sub>S</sub> that the  
other(s) would win] {21c}  
b. \*The candidates expected that each other  
would win {22c}

(17)/ . . .

- (17) a. The men each expected [<sub>S</sub>the soldier to shoot  
the other] (25a)
- b. \*The men expected the soldier to shoot each  
other (25b)

In (16a) Y (= *the other(s)*) is in a tensed clause. In (17a) a lexical subject (= *the soldier*) intervenes between X (= *each*) and Y (= *the other(s)*).

(iii) *it-Replacement*

Chomsky (1973:239) adopts a rule of *it-Replacement*, which derives sentences such as (18b) by moving the object of the embedded clause to the position of *it*.

- (18) a. It is easy to please John.  
b. John is easy to please.

The sentences in (19) are presented by Chomsky to illustrate that *it-Replacement* obeys the SSC.

- (19) a. It is a waste of time for us [<sub>S</sub>for them to  
teach us Latin] (32b)
- b. \*Latin is a waste of time for us for them  
to teach us (35b)

In (19a) the lexical subject *them* intervenes between X (= *it*) and Y (= *Latin*).

(iv) *Disjoint reference*

Chomsky (1973:241) adopts a rule of interpretation which, when applied to the structure NP-V-NP, seeks to interpret the NPs as nonintersecting in reference. Where this is impossible - for example in the case of first and second person pronouns - it assigns "strangeness". The sentences in (20) and (21) illustrate

that this/ . . .

that this rule obeys the SSC and TSC, respectively. (In the case of some of the examples discussed below, I indicate more structure than Chomsky does.)

(20) \*We expect [<sub>S</sub>them to visit me] {45a}

(21) \*We believe [<sub>S</sub>I may still win] {45d}

In (20) the application of the rule is blocked by the presence of the lexical subject *them*. In (21) the rule is blocked because Y (= I) is in a tensed clause.

(v) *The rule associating not and many*

Chomsky (1973:242) leaves open the question of whether the scope of negation in sentences such as (22) is determined by a syntactic transformation that extracts *not* from the NP object, or by an interpretive rule.

(22) a. I didn't see many of the pictures {46a}

b. I didn't see pictures of many of the children {46b}

Chomsky (1973:242) claims that, whatever the nature of the relevant rule, it obeys the SSC, as illustrated by (23).

(23) I didn't see [<sub>NP</sub>John's pictures of many of the children] {47}

In (23) the lexical subject *John* prevents the rule from associating *not* and *many*.

(vi) *The rule associating not and enough*

Chomsky (1973:242) tentatively adopts a rule which associates *enough* and *not* in sentences such as (24).

(24) / . . .

- (24) You didn't understand the proofs of enough of the theorems (for me to be justified in giving you an A) {48a}

The sentence in (25) illustrates that this rule obeys the SSC.

- (25) You didn't understand Euclid's proofs of enough of the theorems (for me to be justified in giving you an A) {48b}

The lexical subject (= *Euclid*) of the NP prevents the rule from associating *not* and *enough*. (25) thus receives no direct interpretation, according to Chomsky (1973:242).

(vii) *The respectively-Interpretation rule*

Chomsky (1973:261) briefly refers to the *respectively-Interpretation rule*, which associates *respective* with the matrix subject in sentences such as (26).

- (26) We will obey any request to kiss our respective wives {154a}

The sentence in (27) illustrates that this rule obeys the SSC.

- (27) \*We will okay any request to kiss our respective wives {154b}

The embedded sentence in (27) has a PRO subject, which is not controlled by X (= *we*).

(viii) *wh-Movement*

A large part of the discussion in (Chomsky 1973) is devoted to *wh-Movement*. The applicability of the SSC and TSC to this rule will be discussed in § 3.2.7 below.

The structure/ . . .

The structure of Chomsky's argumentation about the rules listed above can be illustrated by his remarks on the Passive transformation. He (1973:237) claims that the Passive transformation has the structural description (X, NP, V, NP, Y), and that it rearranges the NPs. Thus, the Passive transformation applies to (28a) to derive (28b).

- (28) a.  $[_S [_{NP} I] [_{VP} [_{V} believe] [_S [_{NP} the\ dog] [_{VP} to\ be\ hungry] ] ] ]$  {18}
- b. The dog is believed to be hungry (by me).

However, the Passive transformation does not apply to (29a) to derive (29b).

- (29) a. I believe  $[_S the\ dog\ is\ hungry]$  {17}
- b. \*The dog is believed is hungry (by me)

The fact that the Passive transformation does not apply to (29a) must be explained. Chomsky (1973:237) puts this point as follows.

- (30) "Notice that there is no problem in explaining why the Passive transformation, with its domain defined in terms of a structural condition on phrase markers in the conventional way, applies to (18) (= (28a) - M.S.); the problem rather, is to explain why it does not apply to (17) (= (29a) - M.S.)." 34

The TSC prohibits the extraction of an element from a tensed clause. If the Passive transformation were applied to (29a), it would have to move the NP *the dog* out of a tensed clause. The TSC, in conjunction with certain assumptions about the Passive transformation and the structure of the sentences involved, thus provides an explanation for the nonapplication of the Passive transformation to (29a). In the terminology of (Botha 1981: § 7.3.2), the TSC plays the role of a lawlike generalization in the grammatical explanation of the unacceptability of (29b). In the case of the other rules listed above, the argument is essen-

tially/ . . .

tially the same. In each case, the TSC and/or SSC functions as a lawlike generalization in a grammatical explanation of why certain English sentences are unacceptable (i.e., of why the relevant rule does not apply to generate these sentences).

The important question is how the evidence presented in (Chomsky 1973) that the SSC and TSC constrain the application of certain rules of *English* justifies the claim that these conditions are in fact *universal* conditions. The answer is that this is a case in which Chomsky can, in terms of his assumptions about the nature of language acquisition, legitimately claim universal status for a principle justified on the basis of data from a single language. Recall that Chomsky argues on the basis of the impoverished nature of the data available to the language learner - i.e., the "poverty of the stimulus" - that certain principles of language are innate. In particular, those principles that cannot reasonably be supposed to have been learned on the basis of the impoverished input must be assumed to be innate, and thus universal (given the assumption of uniformity across the species). Evidence that a principle P belongs to the grammar of a specific language L can thus be used as evidence for the universal status of P, provided that P is in fact an "unlearnable" principle. Chomsky (1975a:118) provides a clear statement of the argument.

- (31) "The discussion has been restricted to English, a serious limitation. Nevertheless, I have not hesitated to suggest that the principles that appear to have explanatory power for English are principles of universal grammar. On the assumption that the language faculty is a common human possession, the inference is plausible (though, obviously, nondemonstrative). The logic of the argument has already been outlined. On the assumption of uniformity of language capacity across the species, if a general principle is confirmed empirically for a given language and if, furthermore, there is reason to believe that it is not learned (and surely not taught), then it is proper to postulate that the principle belongs to universal grammar, as part of the system of 'pre-existent knowledge' that makes learning possible." 35)

The argument/ . . .

The argument from poverty of the stimulus outlined above is in fact an inference to the best explanation.<sup>36)</sup> As regards the SSC and TSC, no specific claim about their "unlearnability" is made in (Chomsky 1973). Nevertheless, these conditions are presented as universal conditions on the basis of the evidence that they constrain the rules of English. In this work it is in fact implicitly assumed that the two conditions are not learned, and that universal status may be claimed for them on the basis of the fact that they constrain the rules of English. In some of the other early works Chomsky is more explicit about the applicability of the argument of poverty of the stimulus to these conditions. The "principles" referred to by Chomsky (1975a:118) - see the remarks quoted in (31) above - include the SSC and TSC. In (Chomsky 1971:31, 32) the SSC and TSC are introduced as examples of "quite remarkable properties that appear to be inexplicable on the basis of experience alone", properties that must be assumed to be "part of the schematism applied by the mind in language learning".

Through the years Chomsky devoted a great deal of time to explicate his use of the argument from poverty of the stimulus, and also to defend his use of this argument from various criticisms.<sup>37)</sup> A detailed and comprehensive appraisal of the role which the argument from poverty of the stimulus plays in Chomsky's linguistics would constitute a complete study in its own right, and will therefore not be attempted here. However, there are certain aspects of this argument that are particularly relevant to the present inquiry. Below three such aspects are briefly considered. The first is the status of the argument from poverty of the stimulus as part of the methodological component of Chomsky's general theory, or research tradition. The second is the evidence presented by Chomsky for the unlearnability of principles such as the SSC and TSC. The third is the role which data from a variety of languages can play in the study of linguistic universals.

Consider/ . . .

Consider first the status of the argument from poverty of the stimulus as part of the methodological component of Chomsky's research tradition. Laudan (1977:81) provides the following "preliminary, working definition" of a research tradition.<sup>38)</sup>

- (32) ". . . a research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain."

Thus, as Laudan (1977:80) puts it, "*a research tradition is . . . a set of ontological and methodological 'do's' and 'dont's'*". Our main concern here is with the methodological component of a research tradition. Laudan (1977:79) clarifies the nature of this methodological component as follows.

- (33) "Very often, the research tradition will also specify certain modes of procedure which constitute the legitimate *methods of inquiry* open to a researcher within that tradition. These methodological principles will be wide-ranging in scope, addressing themselves to experimental techniques, modes of theoretical testing and evaluation, and the like. For instance, the methodological posture of the scientist in a strict Newtonian research tradition is inevitably inductivist, allowing for the espousal of only those theories which have been 'inductively inferred' from the data."

A specific theory belonging to a research tradition is an instantiation of the general ideas of a research tradition. What all the theories belonging to a research tradition have in common, according to Laudan (1981:151) "is that they share the ontology of the parent research tradition and can be tested and evaluated using its methodological norms". Specific theories belonging to the same research tradition can be mutually consistent - if they apply to different parts of the domain of study - or inconsistent - if they are rivals.

The research tradition associated with the specific theories proposed by Chomsky and his collaborators may be called the

"Chomskyan/ . . .

"Chomskyan research tradition".<sup>39)</sup> The specific theories belonging to this tradition include rival theories of the initial state of the language faculty (that is, rival versions of UG) and theories of the various final states (that is, grammars). Given the background presented above, it must be assumed that the methodological component of the Chomskyan research tradition contains a principle which stipulates that the argument from poverty of the stimulus may be used to justify hypotheses about linguistic universals. To put it differently: In terms of one of the methodological principles of the Chomskyan research tradition inquiry into linguistic universals may proceed on the basis of data from a single language, provided that the argument from poverty of the stimulus is used. The fact that linguists other than Chomsky who work within Chomskyan linguistics adopt this argument, provides evidence that the methodological principle permitting the use of the argument do not merely belong to the narrower domain of Chomsky's linguistics, but that it does in fact belong to Chomskyan linguistics. Linguists such as Lightfoot, Koster, and Freidin - who work within the domain of Chomskyan linguistics - all accept the use of the argument from poverty of the stimulus. When he is defending his use of the argument in question, Chomsky is in fact addressing scholars outside the Chomskyan research tradition.<sup>40)</sup>

The second point in connection with Chomsky's use of the argument from poverty of the stimulus to be considered here is the availability of evidence that the stimulus is indeed too impoverished for the putative universal principle to be learned. When one examines Chomsky's claims regarding the unlearnability of certain principles, one is struck by the lack of evidence in his work about the stimulus, or experience, available to the child learning a language. In addition to Chomsky's (1971) claims about the SSC and TSC quoted above, consider for example his (1975a:32) claim that it is "certainly absurd to argue that children are trained to use the structure-dependent rule". In a brief discussion of principles which govern the rules for

forming/ . . .

forming questions and reciprocal expressions, Chomsky (1980a: 42, 43) makes similar unsubstantiated claims about the evidence available to the language learner. Thus, he states that "it is difficult to imagine that people capable of these judgments have all had the relevant training or experience to block the obvious inductive generalization to the ill-formed example". Also, he claims that ". . . it can hardly be maintained that children learning English receive specific instruction about these matters or even that they are provided with relevant experience that informs them that they should not make the obvious inductive generalization . . ." The reason why Chomsky does not provide detailed evidence on the nature of the experience available to the language learner, is probably that he regards it as self-evident that the necessary experience is not available to the language learner. Thus, he (1980d:49) states that as regards the facts about question formation and reciprocal expressions referred to above, ". . . the environment is impoverished in that *it is surely false* that every person who knows these facts has been provided with specific data or training indicating that the facts are as they are . . ."

The question naturally arises whether the latter assumption is warranted. Some of the comments on (Chomsky 1980c) specifically concern the nature of the experience to the language learner - see (Cromer 1980) and (Rachlin 1980). Cromer's contribution is interesting in that he focuses on empirical research about language acquisition. Cromer's (1980:16) main claim is that "there is a great deal of empirical evidence to support Chomsky's claims" about the lack of experience available to the language learner, and the limited role of learning in language acquisition. Cromer cites various studies of child language and of cognitive growth in general which provide evidence to support Chomsky's claims. Perhaps not surprisingly, Chomsky (1980d:43) welcomes "Cromer's insistence on careful attention to data about language acquisition".

What is/ . . .

What is surprising, is that Chomsky fails to cite this evidence in support of his claims. One explanation is that, previous to (Cromer 1980), Chomsky was unaware of the existence of the evidence. While this may be true for some (and maybe even most) of the studies cited by Cromer, it is not true for all of them. As Cromer (1980:18) himself points out, Chomsky (1980c) does refer to some studies of adult aphasia which provides evidence for his views. However, as was argued above, the most probable explanation for Chomsky's failure to cite the relevant evidence is that he regards his claims as self-evidently true, and thus not in need of detailed supporting evidence. If this is indeed the correct interpretation, then the wisdom of Chomsky's strategy can be questioned. The fact is that many psychologists do not accept Chomsky's claims. As Cromer (1980:16) puts it, "Chomsky does himself a disservice by stating too much of his case in the form of assertions frequently not backed up with supporting evidence and therefore open to criticism". The danger is that Chomsky's position "will be ignored or dismissed for reasons not entirely relevant to the real issues involved."<sup>41)</sup>

As was pointed out above, a comprehensive analysis of Chomsky's use of the argument from poverty of the stimulus falls outside the scope of the present study. However, the discussion of Chomsky's failure to cite supporting evidence for his claims about the lack of appropriate experience available to the language learner highlights at least one issue that should be examined in such a comprehensive analysis. A prominent feature of Chomsky's argumentation for the use of the argument from poverty of stimulus in linguistics is his insistence that his use of the argument is in all relevant respects analogous to the use of this argument in, for example, biology. The question is then whether in biology too the argument from poverty of the stimulus is used in the absence of detailed evidence on the actual nature of the stimulus.

The third aspect of Chomsky's use of the argument from poverty

of the stimulus/ . . .

of the stimulus to be considered here concerns the role of data from a variety of languages in the study of linguistic universals. While insisting on the usefulness of studying linguistic universals through the analysis of single language, Chomsky has never denied the usefulness of data from a variety of languages for the study of universals.<sup>42)</sup> In his more recent works, in particular, Chomsky emphasizes the role which data from a variety of languages can play in the study of linguistic universals. Consider in this connection the remarks in (Chomsky 1981a:6; 1981b:71; 1982a:82). In the various sections below careful attention will be paid to the role which cross-linguistic data played in the developmental history of binding theory.

There are two further aspects of the evidence presented by Chomsky (1973) for the claim that the SSC and TSC constrain the rules of English which must be noted. Firstly, Chomsky presents evidence that a large number of the rules of English obey these conditions. That is, Chomsky presents evidence that the SSC and TSC play a significant role in the explanation of a large number of facts about the sentences of English. This aspect of the justification presented in (Chomsky 1973) illustrates the use of the criterion of evidential comprehensiveness in Chomsky's linguistics.<sup>43)</sup> In terms of this criterion, the larger the number of facts explained by a hypothesis, the greater the extent of the justification for this hypothesis.

Secondly, the data presented by Chomsky (1973) for the two conditions may be claimed to belong to two independent types. The rules claimed by Chomsky to be constrained by the SSC and TSC include both syntactic transformations and rules of semantic interpretation. Evidence that a certain condition applies to transformational rules is independent from evidence that the condition applies to rules of semantic interpretation.<sup>44)</sup> Since Chomsky (1973) is primarily concerned with conditions on the functioning of *transformational* rules, it may at first glance seem strange that he presents evidence that the relevant condi-

tions/ . . .

tions also apply to rules of semantic interpretation. However, given that a principle of evidential independence is used in Chomsky's linguistics, Chomsky's use of evidence that the TSC and SSC constrain rules of semantic interpretation can be explained.<sup>45)</sup> In terms of the principle of evidential independence, the explanatory power of a hypothesis depends not only on the number of facts explained by the hypothesis, but also on the variety of the mutually independent types of facts explained by it.

The criteria of evidential comprehensiveness and evidential independence instantiate two of the fundamental principles of Chomskyan generative grammar distinguished by Botha (1981:433). The first is the principle of *epistemological empiricism*, which stipulates that hypotheses "must be testable in principle and justified in fact". The second is the principle of *methodological generality*, which stipulates that hypotheses, and the theories within which they are integrated, "must be of maximal generality". The principles of epistemological empiricism and methodological generality, together with the criteria of evidential comprehensiveness and evidential independence, belong to the methodological component of the Chomskyan research tradition.

Both the principles of epistemological empiricism and methodological generality underline the importance of empirical success in the appraisal of Chomsky's linguistic theories. Although they bear on different aspects of the relation between a theory and the facts in its domain, both principles link the merit of a theory with its success in accounting for these facts. The role which the criteria of evidential comprehensiveness and evidential independence played in the justification of the SSC and TSC is thus a reflection of the importance attached to empirical success in theory appraisal.

The following conclusions can be drawn on the basis of the discussion above.

- (34) a. The success of the SSC and TSC in explaining certain facts, i.e., their empirical success, plays a very important role in their introduction. To put it differently, empirical success was an important consideration in the replacement of  $T_X$  by  $T_{X+1}$ , where  $T_X$  is UG prior to the introduction of the conditions, and  $T_{X+1}$  is UG after the introduction of the conditions.
- b. The fact that Chomsky presents evidence that the SSC and TSC explain a large number of facts, facts which moreover belong to two independent types, underlines the importance of empirical success as a consideration that justifies theory change within the Chomskyan research tradition.
- c. In his argumentation for the universal status of the SSC and TSC, Chomsky makes use of the argument from poverty of the stimulus.
- d. The adoption of the validity of the argument from poverty of the stimulus makes it possible to study linguistic universals on the basis of data from a single language. Nevertheless, both in principle and in practice Chomsky admits the relevance of data from a variety of languages for the study of linguistic universals in general, and the SSC and TSC in particular.
- e. The principle stipulating the validity of the argument from the poverty of the stimulus as a means of inquiring into linguistic universals belongs to the methodological component of the Chomskyan research tradition.
- f. The methodological component of the Chomskyan research tradition also includes the criterion of evidential comprehensiveness and the criterion of evidential independence, which instantiate the principle of episte-

mological empiricism and the principle of methodological generality respectively.

- g. The methodological principles mentioned above all underline the importance of empirical success in theory appraisal in Chomskyan linguistics.

### 3.2.5 The TSC and SSC and restrictions on the form of transformational rules

Although the potential contribution of conditions on function toward restricting the form of rules is recognized in (Chomsky 1973),<sup>46)</sup> this work contains very few specific claims about restrictions on the form of transformational rules made possible by the TSC and SSC. In this section I outline the various claims that are made, and consider their significance.

Chomsky commonly uses the phrase "to apply blindly" to describe the application of transformations to phrase markers as being independent of grammatical relations or meaning.<sup>47)</sup> In fact we are dealing here with a restriction on the *form* of transformational rules: The structural description of a transformational rule may not be formulated in terms of grammatical relations or meaning. In accordance with Chomsky's usage of the phrase "to apply blindly", I will use the term "the principle of blind application" to refer specifically to this restriction on the form of transformational rules. However, it is important to keep in mind that this particular restriction on the form of transformational rules is only a special case of a much more general restriction on the form of transformational rules. This more general restriction is the condition on structural descriptions discussed in § 3.2.3 above, which limits the structural descriptions of transformations to simple strings  $(\alpha_1, \dots, \alpha_n)$ , where  $\alpha$  can be a terminal string, a category, or a variable. This condition on the form of transformations automatically excludes the possibility of formulating transformational rules in terms of grammatical relations or meanings. In the discussion

that follows/ . . .

that follows I will refer to this general restriction on the form of transformational rules as "the simple string condition".<sup>48)</sup> Note that the simple string condition (which actually dates from 1961), is in fact assumed by Chomsky (1973).<sup>49)</sup> Thus, he (1973: 233) defines a transformation as "*a structure-dependent mapping of phrase markers into phrase markers that is independent of the grammatical relations or meanings expressed in these grammatical relations*", and he points out that "transformations generally apply to phrase markers that meet some condition on analyzability *with no regard to other associated properties*". (The italics are mine.)

The structural description proposed by Chomsky (1973:233, 237) for the Passive transformation obeys the principle of blind application (and also the simple string condition). However, if so formulated, the Passive transformation overgenerates. It not only generates acceptable sentences such as (28b), but also unacceptable sentences such as (29b). Adoption of the TSC overcomes this problem of overgeneration, as is explained in § 3.2.4 above. Chomsky (1973:237, fn. 15) briefly considers, and rejects, two alternative solutions to the problem of overgeneration by Passive. Both these solutions are primarily rejected because, unlike the solution in terms of the TSC, they are incompatible with the adoption of the simple string condition.

The first alternative is to add a rule-specific condition to the Passive transformation formulated with the structural description (X, NP, V, NP, Y). Such a rule-specific condition would presumably stipulate a condition on one (or more) of the factors of the rule. Such rule-specific conditions are in fact prohibited by the simple string condition. The solution in terms of a rule-specific condition would thus require relaxing this restriction on the form of transformations, with the result that more rules (and more grammars) become available. If a solution in terms of the TSC were adopted, the simple string condition could be upheld. Given the aim of reducing the class of available

grammars/ . . .

grammars, a solution in terms of the TSC is thus clearly more attractive than a solution in terms of a rule-specific condition.

Although Chomsky (1973:237, fn. 15) does not explicitly indicate on what grounds he rejects a solution in terms of a rule-specific condition, it is fairly obvious that the consideration of restricting the form of transformations is the crucial one. The discussions of rule-specific conditions in, for example, (Chomsky 1977b:19; 1978a:15) explicitly relate the elimination of rule-specific conditions in favour of general conditions such as the TSC with restricting the formal (descriptive/expressive) power of transformations. The link between conditions on the *form* of transformations and the *formal power* of transformations is obvious. The stricter the conditions on the form of transformations, the less formal power such rules would have (and vice versa).

The other solution to the problem of overgeneration by Passive mentioned by Chomsky is to define the Passive transformation in terms of relational notions such as 'subject' and 'object'. This solution entails giving up the principle of blind application (and thus also the simple string condition, of which the principle of blind application is a special case). Given the desirability of reducing the formal power of transformations, the solution in terms of the TSC is clearly more attractive.

The exact nature of the consideration of restricted formal power is discussed in detail below. At this point I will simply note that it is not a straightforward empirical consideration.

There are also straightforward empirical considerations that make this second alternative solution to the problem of overgeneration by Passive less attractive. Chomsky (1973:237, fn. 15) mentions a number of constructions in which NPs that are not direct objects are moved by the Passive transformation. On

Chomsky's/ . . .

Chomsky's analysis such constructions thus constitute counter-examples to a Passive transformation formulated in relational terms.<sup>50)</sup> It must be noted that even in the absence of such counterexamples to a reformulation of the Passive transformation in terms of relational notions, a solution in terms of the TSC would still be more attractive. The following remark by Chomsky (1973:255, fn. 34) shows clearly just how much weight he attaches to the consideration of restricting the form (and thus the formal power) of transformations.

(35) "In the absence of other considerations, the general point that the theory of transformations should not be extended to permit this option is compelling, if not decisive."

The extension of the theory of transformations referred to in (35) is not the same as that discussed immediately above, but the general principle is clearly applicable to all potential extensions of the theory of transformations. The implication for the proposed reformulation of the Passive transformation in relational terms is obvious. Even if the proposed reformulation faced no counterexamples, a solution in terms of the TSC to the problem of overgeneration by Passive would still be preferred. The reason is that the extension of the theory of transformations required by a reformulation of Passive in relational terms must not be permitted in the absence of "strong empirical motivation".

Chomsky (1973) makes a further specific claim about the form of transformations, in connection with an alternative to the SSC. Chomsky (1973:§§ 8 and 9) argues for the extension of the SSC to include the case where the subject of the embedded clause is controlled by a category containing X. One of Chomsky's arguments for such an extension of the SSC concerns the sentences presented in (36).

- (36) a. We *each* persuaded Bill [COMP PRO to kill *the* other(s)] (113)
- b. \*We persuaded Bill to kill each other (112)

(36b)/ . . .

(36b) is blocked by the SSC as formulated in (1) above. PRO is controlled by *Bill*, i.e., it is not controlled by the category containing X (= *each*). Chomsky (1973:255) considers an alternative to the extended SSC to cover the control case. The alternative is to restrict *each*-Movement to a single clause. One would then in fact be assuming that (37b) is derived from (37a), and (38b) from (38a).

- (37) a. We promised Bill [COMP PRO each to kill  
the other(s)] {116}  
b. We promised Bill to kill each other {114}
- (38) a. We wanted [COMP PRO each to kill the  
other(s)] {118}  
b. We wanted to kill each other {117}

Chomsky (1973:255) points out two empirical difficulties arising from the adoption of this alternative to the extended SSC. Firstly, if (38b) must be derived from (38a), then it becomes impossible to block the sentences in (39).

- (39) a. \*We each wanted to kill each other {119a}  
b. \*We would have both wanted to kill each  
other {119b}

Secondly, if *each*-Movement is restricted to a single clause, then it becomes impossible to derive the sentences in (40).

- (40) a. We like [S [NP pictures of each other]  
to be on sale] {107a}
- b. They expect [S [NP each other] to win]

The hypothesis that *each*-Movement is restricted to a single clause, together with the assumption that sentences like (36b) is derived from structures like (36a), thus make the wrong predictions about sentences like (39) and (40). These sentences in fact constitute potential counterexamples to the hypotheses in question. Chomsky's rejection of the hypothesis that *each*-Movement is restricted to a single clause is thus partly based on the existence of counterexamples to this hypothesis.

There is, however, another reason why Chomsky rejects this solution to the problem posed by sentences such as (36). According to Chomsky (1973:255), the extension of the theory of transformational rules to permit such rules to be restricted to a single clause, would be "highly undesirable". He goes on to claim that there are no "strong empirical reasons" to motivate such a change. It is in connection with the possibility of permitting transformational rules to be restricted to a single clause that Chomsky makes the remark quoted in (35) above. The point is that the restriction of *each*-Movement to a single clause must be ruled out, even if this restriction is not threatened by any counterexamples, because it requires an undesirable and unmotivated extension to the theory of transformations. Note that the condition on the form of transformations implicitly assumed is again the simple string condition which excludes the possibility of stipulating that two (or more) of the factors of an analyzed string are dominated by a single clause.

In sum/ . . .

In sum: The only specific claims made by Chomsky (1973) about restrictions on the form of transformations made possible by the adoption of the TSC and SSC concern the Passive transformation and *each*-Movement. In both cases the claim is that the adoption of the conditions makes it possible to uphold the simple string condition, while the alternatives require more formal power of UG. In this way the SSC and TSC contribute towards restricting the formal power of transformations (and, ultimately, towards restricting the class of available grammars).

The consideration of restricting the formal power of UG plays an important role in the justification of hypotheses that form part of UG - general-linguistic hypotheses for short.<sup>51)</sup> In general, the more a general-linguistic hypothesis contributes to restricting the formal power of UG, the greater the extent of the justification for this hypothesis. The fact that the SSC and TSC do contribute to restricting the formal power of transformations, is thus a consideration that increases the extent of the justification for these conditions. The consideration of restricting the formal power of (some component of) UG thus plays a role in the choice of  $T_{x+1}$  - i.e., a version of UG that incorporates the SSC and TSC - over  $T_x$  - i.e., a version of UG that differs from  $T_{x+1}$  in that it does not incorporate these conditions.

The consideration of restrictedness of formal power is in a dual sense empirical in nature. First, restrictedness of formal power has to do with the success of UG in explaining certain facts (or, in Laudan's terminology, to solve empirical problems).<sup>52)</sup> In particular, the more restricted the formal power of UG, the greater the success of UG in explaining the

acquisition/ . . .

acquisition of knowledge of grammar, given the impoverished nature of the data available to the language learner. Second, any restriction on the formal power of some component of UG is subject to empirical test. By restricting the formal power of UG, predictions are made about what constitutes a possible grammar. Such predictions can be tested on the basis of data from specific languages.

However, if one were to focus exclusively on the empirical nature of the consideration of restricted formal power, then the way this consideration is used in concrete cases would remain problematic. When one closely examines Chomsky's argumentation for a proposed restriction on the formal power of UG - see for example Chomsky's proposal regarding the simple string condition referred to above, and also the principle of minimal factorization discussed in § 3.3.2 below - it strikes one that in these cases no evidence is presented that a particular restriction on the formal power of UG makes it possible to explain a specific fact or set of facts about the acquisition of knowledge of a language previously left unexplained (or vice versa).

This feature of Chomsky's argumentation for restrictions on the formal power of UG can be highlighted by contrasting the argumentation with the argumentation presented for proposed conditions on the function of rules. For instance, in the case of the SSC and TSC Chomsky (1973) provides numerous arguments to the effect that the conditions make it possible to explain specific facts about the (un)acceptability of English sentences, and their interpretation. In the case of the simple string condition, or the principle of minimal factorization discussed in § 3.3.2 below, no analogous arguments are presented.

The relevant/ . . .

The relevant feature of Chomsky's use of the consideration of restricted formal power can be explained on the assumption that the consideration of restricted formal power has a conceptual aspect in addition to its empirical aspect. In terms of the empirical-conceptual distinction adopted in § 2.3.4.1 above, a consideration which plays a role in the appraisal of theories is conceptual if it bears on the relation between a specific theory and a principle of the general theory, or research tradition, associated with this specific theory. One of the most fundamental principles of Chomsky's research tradition is one which stipulates that, underlying language acquisition, there exists a set of rich and restrictive principles as part of the human biological endowment. Chomsky's argumentation for this assumption was outlined in § 3.2.3 above. A UG is an attempted characterization of this set of innate principles. A specific UG which makes a large number of options available to the language learner, and which is thus not restrictive, is in conflict with the general principle of Chomsky's research tradition referred to above - the innateness principle, for short. Such conflict, or tension, creates a conceptual problem within Chomsky's linguistics. By restricting the formal power of UG such tension can be reduced, or even eliminated. A UG which makes too many options available to the language learner is a theory with excessive formal power. Thus, any modification to a UG which leads to a reduction in its formal power would lessen the tension between this theory and the general innateness principle of Chomsky's research tradition. By the same token, any modification to a UG which leads to an increase in its formal power would increase the tension between this theory and the innateness principle. By recognizing this conceptual aspect of the consideration of restricted formal power, one can explain why Chomsky does not justify a restriction on the formal power of UG - including the simple string condition and the principle of minimal factorization - by citing specific facts about language, or language acquisition, which could be explained in terms of the restriction.

To avoid/ . . .

To avoid possible misunderstanding, it must be stressed that by distinguishing a conceptual aspect of the consideration of restricted formal power, it is not being claimed that the consideration is not also subject to empirical test. As pointed out above, any specific proposed restriction on the formal power of UG can be tested on the basis of data from a natural language. The innateness assumption - from which the desirability of restricted formal power follows - is also subject to empirical test. Obviously, this assumption is too general to be tested in the same way as, for example, the SSC. However, the general assumption can be evaluated by combining it with specific claims about the content of the innate principles which underlie language acquisition, and then testing the resulting specific theories in the usual way. To the extent that the specific theories "fit the facts", the general assumption is justified. To the extent that the resulting specific theories fail to fit the facts, this reflects negatively also on the general assumption. There is considerable textual evidence that Chomsky does in fact hold this view of the testing of general assumptions, such as the innateness assumption. For example, in his various discussions of the question whether there is a rich innate structure, as well as the closely related question of whether the principles underlying language acquisition are specific to language, Chomsky has always maintained that the issue must ultimately be resolved by the construction of specific theories.<sup>53)</sup> The success, or otherwise, of these specific theories in explaining the facts of language acquisition will then make it possible to determine the correctness of the conflicting general claims.<sup>54)</sup> The introductory remarks to *Rules and representations* (1980a:3) contain a particularly clear statement of Chomsky's position on the evaluation of such general claims as the one under discussion.

(41) "In these lectures, I would like to explore a number of issues relating to human cognitive capacities and the mental structures that serve as the vehicles for the exercise of these capacities. Plainly, this formulation of a problem

embodies/ . . .

embodies assumptions that are far from clear and highly controversial insofar as they are clear. I will try to make them clearer, and, I hope more plausible, as I proceed. In the end, the best way to clarify these assumptions and to evaluate them is to construct specific models guided by them in particular domains, then to ask how these models fare when interpreted as explanatory theories. If the leading ideas are appropriate, they will be sharpened and justified by the success of explanatory theories that develop them in a specific way." 55)

In sum, then, by distinguishing a conceptual aspect to the consideration of restricted formal power, one can gain greater insight into the nature of Chomsky's argumentation for specific proposed restrictions on the formal power of UG. The distinction of such a conceptual aspect in no way conflicts with the view that specific restrictions on formal power, and the innateness principle from which the desirability of restricted formal power follows, are subject to empirical test.

The main conclusions of § 3.2.5 can be summarized as follows.

- (42) a. Chomsky (1973) presents only limited justification for the claim that the SSC and TSC contribute towards restricting the form, and thus the formal power, of transformations. In particular, he argues in connection with the Passive transformation and *each*-Movement that the TSC and SSC make it possible to uphold the simple string condition (and thus also the principle of blind application).
- b. The consideration of restricted formal power has both an empirical aspect and a conceptual aspect.
- c. As regards the empirical aspect of the consideration, any proposed restriction on the formal power of UG is subject to empirical test. Also, restrictedness of formal power is an essential property of UG if this theory is to explain the acquisition of knowledge

of grammar/ . . .

of grammar, given the impoverished nature of the data available to the language learner.

- d. The consideration of restricted formal power has a conceptual aspect, insofar as a specific version of UG with excessive formal power is in conflict with the general assumption that there exists a set of rich and restrictive innate principles underlying language acquisition. Proposed restrictions on the formal power of UG can lessen the tension between a specific UG and the innateness principle.
- e. An adequate account of Chomsky's justification for the choice of a version  $T_{x+1}$  of UG over another version  $T_x$  in terms of the consideration of restricted formal power, requires reference to the conceptual aspect of this consideration.

3.2.6 The naturalness of the SSC

Chomsky (1973:270) claims that the SSC has "a certain naturalness". In particular, Chomsky observes that, in some cases, the SSC "has the effect of reducing ambiguity, or, to put it differently, of increasing the reliability of a reasonable perceptual strategy that seeks the nearest NP to a verb (or the head noun of a nominal phrase) as its subject". So, for example, the SSC implies that (43) must have the interpretation indicated in (44a), but not that indicated in (44b).

(43) The men expected [ the police to arrest each other ] {191}

(44) a. The men expected [ the police each to arrest the other(s) ] {192}

b. The men each expected [ the police to arrest the other(s) ] {193}

(43) cannot/ . . .

(43) cannot be derived from (44b), because of the presence of the specified subject *the police*.

If, contrary to the assumption made above, the deep structure position of *each* plays no role in the interpretation of a sentence, then the SSC will guarantee a correspondence between deep structure position and scope as determined by surface structure interpretation rules. The latter consequence is characterized by Chomsky (1973:270) as "rather natural". I will postpone an analysis of the role which this naturalness consideration plays in the justification of the SSC until later. The reason for this decision is that in some later works Chomsky is much more explicit on the relevance of considerations of naturalness in the evaluation of linguistic hypothesis, and the analysis of the consideration outlined above can be more insightfully presented against the background of these works.

### 3.2.7 Chomsky's reaction to empirical difficulties threatening the SSC and TSC

At the time of their introduction, Chomsky (1973) noted certain empirical difficulties threatening the SSC and TSC. These empirical difficulties take two forms: counterexamples for the conditions, and phenomena left unexplained by the conditions. The exact nature of these empirical difficulties, as well as the specific steps taken by Chomsky to deal with these difficulties, are analyzed in detail in §§ 3.2.7.1 - 3.2.7.5 below. The cases discussed in these sections illustrate an important component of Chomsky's methodology, namely a tolerant attitude towards empirical difficulties, including counterexamples. This attitude - which, following Botha (1981:14), is called "epistemological tolerance" - is discussed in § 3.2.7.6.

#### 3.2.7.1 wh-Movement, Strict Cyclicity and the COMP-escape hatch

The formulation of the TSC presented in (2) above excludes from  
 this condition/ . . .

this condition a Y that is in COMP. The first formulation of the TSC considered in (Chomsky 1973:238) does not contain this clause.

- (45) "No rule can involve X, Y, in the structure {20}  
 ... X ... [  $\alpha$  ... Y ... ] ...  
 where  $\alpha$  is a tensed sentence."

Chomsky (1973:243) points out that *wh*-Movement in cases such as (46) violates the TSC, as formulated in (45), as well as the SSC.

- (46) a. COMP you told me [  $_S$  COMP Bill saw something ] {50}  
 b. What did you tell me that Bill saw {49}

Movement of the *wh*-phrase from the embedded sentence into the COMP position of the matrix clause violates both the SSC (because the embedded clause has a specified subject *Bill*), and the TSC as formulated in (45) (because the embedded clause is tensed). *wh*-Movement thus constitutes a potential counter-example to both the SSC and TSC.

In order to overcome the problem which *wh*-Movement poses for the SSC and TSC, Chomsky makes the following assumptions.

(i) The base rules include the following rules.

- (47) a.  $S \rightarrow \text{COMP } S'$   
 b.  $S' \rightarrow \text{NP AUX VP}$

(ii)  $S$ , but not  $S'$ , is the domain of cyclic rules.

(iii) An element in COMP can only be moved into another COMP. (This condition is known as the COMP-COMP condition, and is formulated as in (48) below.)

- (48) "No rule can involve X, Y in the structure {55b}  
 ... X ... [  $\alpha$  ... Z ... -WYV ... ] ...  
 where Y is in COMP and X is not in COMP"

(iv) / . . .

- (iv) *wh*-Movement moves a *wh*-phrase into COMP position.
- (v) *wh*-Movement applies cyclically, in accordance with the Strict Cycle Condition (49).

(49) "No rule can apply to a domain dominated by a cyclic {51} node A in such a way as to affect solely a proper subdomain of A dominated by a node B which is also a cyclic node."

- (vi) An element in COMP may be extracted from a tensed clause. The formulation (45) of the TSC is thus replaced with the formulation (2).

It follows from (i) - (v) that (46b) is not directly derived from (46a), but only via the intermediate stage (50), with *wh*-Movement on the lower cycle.

(50) COMP you told me [<sub>S</sub> [<sub>COMP</sub> what] Bill saw] {52}

The assumptions outlined above enable Chomsky to overcome the problem posed by *wh*-Movement in cases like (46b) for the SSC and TSC. Consider firstly the SSC. *wh*-Movement on the innermost cycle does not violate the SSC, since no element is moved out of a cyclic node. *wh*-Movement on the highest cycle - i.e., the movement of the *wh*-phrase from the COMP position of the embedded clause to the COMP position of the matrix clause - does not violate the SSC. The *wh*-phrase is not moved out of the embedded clause across the specified subject of this clause. In the case of the TSC, *wh*-Movement on the innermost cycle does not violate the TSC, since no element is moved out of a tensed clause. *wh*-Movement on the highest cycle does not violate the TSC, formulated as in (2), since the latter formulation allows for elements in COMP position to be moved out of a tensed clause.

In the terminology of Botha (1981:511.2.2), Chomsky's reaction

to the problem/ . . .

to the problem posed by *wh*-Movement for the SSC and TSC can be characterized as a combination of *protection* and *modification*.<sup>56)</sup> In the case of the SSC, the condition itself is retained without any modification. Certain auxiliary hypotheses are adopted to protect the SSC, including (i) S rewrites to COMP and S', (ii) S, but not S', is the domain of cyclic rules, (iii) an element in COMP can only be moved into another COMP, (iv) *wh*-Movement applies cyclically. These auxiliary hypotheses are also used by Chomsky to protect the TSC. However, the TSC itself is also modified, to exclude cases where Y is in COMP.

The first auxiliary hypothesis identified above which plays a role in Chomsky's protection of the SSC and TSC - viz. that the base rules include the rule  $S \rightarrow \text{COMP } S'$  - is, strictly speaking, not a hypothesis introduced by Chomsky. He actually takes over this hypothesis from Bresnan. She (1970, 1979, especially chapter 1), provides various arguments for it.<sup>57)</sup>

As regards the hypothesis that S, but not S', is the domain of cyclic rules, there is some independent justification for this hypothesis to be found in (Chomsky 1973) - independent, that is, from the SSC and TSC, although not independent from the conditions approach of which the SSC and TSC form part.<sup>58)</sup> If S and NP, but not S', were the cyclic nodes, then *wh*-Movement could extract a *wh*-phrase from a non-subject NP, without violating the Subjacency Condition.<sup>59)</sup> If, however, S' were also a cyclic node, *wh*-Movement in such cases would actually violate the latter condition. The example in (51) shows that sentences resulting from such an application of *wh*-Movement is acceptable.

- (51) a. who did you see a picture of {86a}  
b. COMP [<sub>S</sub> you saw [<sub>NP</sub> a picture of who]

It is then to prevent the Subjacency Condition from wrongly blocking the derivation of such sentences, that Chomsky (1973) assumes that S, but not S', is a cyclic node.

The COMP-COMP condition, plus the hypothesis that *wh*-Movement always moves a *wh*-phrase into COMP position (irrespective of whether this COMP is marked +WH), are also required by the Subjacency Condition. The Subjacency Condition rules out all unbounded movements (and unbounded deletions). The COMP-COMP condition, and the hypothesis that *wh*-Movement always moves a *wh*-phrase into COMP position, are needed to ensure the boundedness of the operation performed by *wh*-Movement. Given the desirability of the Subjacency Condition, there is thus some (indirect) independent justification for the auxiliary hypotheses under discussion.

Consider, finally, the auxiliary hypothesis that *wh*-Movement applies cyclically. Without giving any specific references, Chomsky (1973:243, fn. 22) dismisses all arguments that *wh*-Movement cannot be a cyclic rule as irrelevant. He claims that none of the arguments in the literature apply to the formulation given in (Chomsky 1973). In their review of (Chomsky 1973) - "Remarks on 'Conditions on transformations'" - Bach and Horn (1976:289) mention a few works in which arguments against the cyclicity of *wh*-Movement is presented: (Bach 1971), (Postal 1971, 1972).<sup>60</sup> Bach and Horn (1976) challenge Chomsky's claim that the arguments in the literature against the cyclicity of *wh*-Movement do not apply to his formulation. They discuss one example to support their claim, an example that crucially depends on the assumption that *wh*-Movement in the embedded clause is obligatory. As Chomsky (1977c:128, fn. 19) points out, he (1973:§13) actually assumes that the rule is *optional*. Note, incidentally, that Chomsky (1977c:128, fn. 19) also rejects Bach and Horn's claim that the "possibility of optional *Wh* Movement destroys the only remaining positive argument for successive-cyclic application". Instead, Chomsky claims that the optionality of *wh*-Movement is irrelevant to the arguments for successive cyclicity.

Chomsky (1973) not only rejects as irrelevant all arguments

against/ . . .

against the cyclicity of *wh*-Movement. He (1973:244) also provides some justification for the hypothesis that *wh*-Movement applies cyclically, justification that is independent from the problem posed by *wh*-Movement for the SSC and TSC. Chomsky claims that this hypothesis can explain why a *wh*-phrase cannot be moved out of an indirect question. Consider, for example, the unacceptable sentence (52a), with the underlying structure (52b).<sup>61)</sup>

- (52) a. \*What did he wonder where John put {57}
- b. COMP he wondered [<sub>S</sub> COMP John put what where] {58}

Given the Strict Cycle Condition, the hypothesis that *wh*-Movement applies cyclically permits no rule application to give (52a). Suppose, for example, that *what* is first moved into the COMP position of the matrix clause. Then *wh*-Movement cannot return to the lower cycle to move *there* to the embedded COMP position. The fact that *wh*-phrases cannot be extracted from indirect questions is in an obvious sense independent from the fact that *wh*-phrases can escape from an embedded clause that is tensed or contains a specified subject. These facts concern two different constructions - indirect questions versus embedded clauses that are not indirect questions. Moreover, the properties of the two constructions to be explained differ. On the one hand, it must be explained why *wh*-phrases cannot be moved out of indirect questions. On the other hand, it must be explained why *wh*-phrases can be moved out of embedded clauses in violation of the SSC and TSC. There is also some indirect independent justification for cyclicity deriving from the Subjacency Condition, since the latter condition presupposes cyclicity.<sup>62)</sup>

In sum, then, Chomsky (1973) uses the same auxiliary hypotheses to overcome potential counterexamples to the SSC and the TSC. Moreover, all the hypotheses used by Chomsky (1973) to overcome the problem which *wh*-Movement poses for the SSC and the TSC are claimed by him to have some justification which is independent from the two conditions.

Let us/ . . .

Let us now consider the modification to the TSC. In terms of this modification the domain of the TSC is restricted to exclude all movement of elements that are in COMP to another clause. This modification is not effected in an arbitrary way. The modification indicates that the exceptions to the TSC belong to a well-defined class, namely extractions of elements from COMP. Such extractions actually have another special property. Any element extracted from COMP can only be moved to another COMP.

The main points of the discussion above of Chomsky's reaction to the problem which *wh*-Movement poses for the SSC and TSC can be summarized as follows.

- (53) a. *wh*-Movement violates both the SSC and TSC, that is, it is a potential counterexample to these conditions.
- b. In order to overcome the problem which *wh*-Movement poses for these conditions, Chomsky adopts a number of auxiliary hypotheses to protect the conditions, and also proposes a modification of the TSC.
- c. The same auxiliary hypotheses which Chomsky (1973) uses to overcome the problem which *wh*-Movement poses for the SSC are also used to overcome the problem which *wh*-Movement poses for the TSC.
- d. All the auxiliary hypotheses used by Chomsky to protect the SSC and TSC from the problem posed by *wh*-Movement are independently justified within the context of (Chomsky 1973).
- e. The modification of the TSC is systematic, in that it excludes a well-defined class of operations from the condition.
- f. There is a close interrelationship between the SSC and

the TSC/ . . .

the TSC, on the one hand, and the Subjacency Condition, on the other hand. This is emphasized by the fact that several of the assumptions needed to make *wh*-Movement consistent with the SSC and TSC (for example, that *S*, but not *S'*, is a cyclic node, that *wh*-Movement applies cyclically) are also presupposed by the Subjacency Condition.

3.2.7.2 The SSC and traces

Chomsky (1973:§10) considers certain aspects of the rules of English that cannot be explained on the basis of the SSC, as formulated in (1) above. The examples discussed in Chomsky's § 10 are those in which *X*, in the structure ... *X* ... [<sub>α</sub> ... *Z* ... - *WYV* ... ] ..., is not a possible controller. Two subcases are distinguished: *X* = *it*, as in the case of *it*-Replacement, and *X* = COMP, as in the case of *wh*-Movement. During his discussion of these cases, Chomsky takes the important step of introducing the notion 'trace' to deal with certain empirical inadequacies of his conditions.

Consider, firstly, the case of *it*-Replacement. The SSC makes the wrong predictions about the applicability of *it*-Replacement in cases like (54) and (55).

- (54) a. It is pleasant for the rich [<sub>S</sub> COMP PRO to do  
the hard work] {164a}
- b. The hard work is pleasant for the rich to do {164b}
- (55) a. It is tough for me [<sub>S</sub> COMP PRO to stop  
[COMP PRO looking at Harriet] ] {166b}
- b. Harriet is tough for me to stop looking at {167b}

In (54a) *X* = *it*, *Y* = *the hard work*, and *Z* (= PRO) is controlled by *the rich*. The SSC wrongly predicts that *it*-Replacement cannot apply to extract *the hard work* from the embedded clause,

because *Z*/ . . .

because Z is not controlled by a category containing X. The acceptability of (54a) shows that, contrary to the prediction of the SSC, *it*-Replacement can perform the relevant operation. Exactly the same is true for (55), with X = *it*, Y = *Harriet*, and Z (= PRO) is controlled by *me*. *it*-Replacement thus constitutes a potential counterexample to the SSC.

Chomsky (1973:262f) considers two possible ways to deal with the problematic *it*-Replacement data. A first possibility is to supplement the subcase (56b) of the SSC with the provision presented in (57).

- (56) a. Z is not controlled at all (160)  
 b. Z is controlled by a category not containing X
- (57) "where the minimal major category containing X (i.e., MMC (X)) is a possible controller."<sup>63</sup> (161)

If (57) were added to subcase (56b) of the SSC, then this condition would predict that *it*-Replacement can apply to derive (54a) and (55a). In both these cases the minimal major category containing X is not a possible controller. Characterized in general terms, this option involves modifying the SSC by adding the clause in (57) to one of the subcases of the SSC, namely (56b). In terms of this modification, the domain of the SSC is restricted, by the exclusion of all cases where X is not a possible controller of Z.

The second possibility considered by Chomsky is to adopt a rule of PRO-Replacement, which moves the NP *the hard work* in (54a) to the position of PRO on the internal cycle. The structure in (58) will then be derived.

- (58) It is pleasant for the rich [<sub>S</sub> COMP the hard work  
 to do] (170)

*it*-Replacement/ . . .

*it*-Replacement can then extract the NP *the hard work* from the embedded clause, because the structure in (58) is not of the form to which the SSC applies. A similar analysis can be made in the case of (55). Characterized in general terms, this second option involves retaining the SSC in an unmodified form, and adopting an auxiliary hypothesis to protect the SSC.

Chomsky (1973:264) chooses this second possibility to deal with the relevant counterexamples to the SSC. The five considerations on which he bases his choice are the following.

- (i) If the provision (57) were added to subcase (56b) of the SSC, then the SSC would wrongly predict that *wh*-Movement can derive (59a, b).

- (59) a. \*Who did John make a fortune by cheating {163a}  
 b. \*Where did John make a fortune while living {163b}

In (59a, b) X is COMP, which is not a possible controller. Under the proposed revision of the SSC, *wh*-Movement might then apply to extract *who* from the embedded clause. The unacceptability of (59a, b) indicates that, in fact, *wh*-Movement cannot perform the relevant operation. Chomsky thus rejects the modification of the SSC on the basis of empirical criticism that can be brought against it. Note that the relevant criticism does not apply to the hypothesis that English contains a rule of PRO-Replacement. The latter hypothesis correctly predicts that *wh*-Movement cannot apply to (59a, b) since the subject PRO of the embedded clause is controlled by *John*.

- (ii) If the provision (57) were added to subcase (56b) of the SSC, then the application of *it*-Replacement to extract *Harriet* in (55a) and in (60a) would violate the Subjacency Condition.

- (60) a. It is tough for me [<sub>S</sub> COMP PRO to stop Bill  
from [<sub>S</sub> COMP PRO looking at Harriet] ] {168}
- b. Harriet is tough for me to stop Bill from  
looking at {169}

However, if a rule of PRO-Replacement were adopted, *it*-Replacement would not violate the Subjacency Condition. From his remarks (1973:264), it is obvious that Chomsky chooses the rule of PRO-Replacement, rather than modifying the SSC, because the first possibility allows him to "preserve" the Subjacency Condition.<sup>64)</sup> The Subjacency Condition explains a large number of facts about the application of the rules of English. Giving up this condition would leave these facts unexplained. This second consideration on which Chomsky bases his choice of the PRO-Replacement rule is thus also empirical, since it bears on the ability of UG to explain certain facts (specifically, those that follow from the Subjacency Condition).

- (iii) Chomsky claims that a rule of PRO-Replacement contributes very little to the overall complexity of the grammar of English. Adding this rule permits at very little cost, a generalization of an obligatory rule already required in the grammar, namely the rule that derives (61) from (62).

- (61) a. John is likely to leave {172a}
- b. John seems to be a nice fellow {172b}
- (62) a. It is likely [<sub>S</sub> COMP John to leave] {171a}
- b. It seems [<sub>S</sub> COMP John to be a nice fellow] {171b}

The rule in question is, of course, *it*-Replacement. This third consideration is partly empirical and partly conceptual. It is empirical in that the evidence for *it*-Replacement also supports PRO-Replacement, given that PRO-Replacement can be merged with *it*-Replacement. It is conceptual in that it bears on a concep-

tual property/ . . .

tual property of the grammar of English, namely its overall complexity.<sup>65)</sup>

- (iv) If there were a rule of PRO-Replacement, with the added proviso that it is obligatory under certain circumstances, then some of the restrictions on *it*-Replacement could be explained in terms of the ordering of Passive and PRO-Replacement.<sup>66)</sup> If postulating a rule of PRO-Replacement can indeed explain the restrictions on *it*-Replacement, then there is additional empirical support for PRO-Replacement.
- (v) If there were a rule of PRO-Replacement, then certain observations by Bresnan about stress contours could be explained.<sup>67)</sup> These facts provide additional empirical support for PRO-Replacement.

Chomsky's choice of the possibility of adding a rule of PRO-Replacement to the grammar of English over the possibility of modifying the SSC by adding the provision (57) is thus based on empirical considerations, with only a very limited role played by a conceptual consideration. The discussion above also makes it clear that there is some independent justification for PRO-Replacement.

Chomsky (1973:265ff) points out that the adoption of a rule of PRO-Replacement does not solve all problems. Consider the sentences in (63b), (64b), and (65b).

- (63) a. It is easy for the others [COMP PRO to please  
each of the men] (174a)  
b. \*The men are easy for each other to please (173a)
- (64) a. It seems to each of the men [COMP John to like  
the others] (174b)  
b. \*John seems to the men to like each other (173b)

- (65) a. It is fun for each of the kids [COMP PRO to  
give toys to the others] (174c)
- b. Toys are fun for the kids to give each  
other (?) (173c)

According to Chomsky, (64b) is the worst, and (65b) is better than (63b). Even if English contained a rule of PRO-Replacement, none of the conditions discussed in (Chomsky 1973) could explain the unacceptability of (63b) and (64b). The failure of the conditions - including the SSC and TSC - to explain the unacceptability of these sentences, constitutes an empirical problem for the conditions.

To overcome this problem, Chomsky (1973:266) first considers ordering *each*-Movement before *it*-Replacement. The unacceptability of (63b) and (64b) could then be explained. In order to derive (64b), for example, *each*-Movement would have to apply while *John* is still in the subject position of the embedded clause. Such an application is ruled out by the SSC. However, the hypothesis that *each*-Movement applies before *it*-Replacement leads to a problem in the case of (65b). Application of PRO-Replacement on the innermost cycle of (65a) gives (66).

- (66) It is fun for each of the kids [COMP toys to give  
the others] (176)

If it is assumed that *each*-Movement applies before *it*-Replacement, then *each*-Movement must apply to (66) at this point if (65b) is to be derived. However, *each*-Movement can only apply if the position of PRO in (65b), now occupied by the complex structure [toys, PRO]<sup>68</sup>, is still controlled by the phrase *each of the kids* of the matrix clause. If this position is no longer controlled by *each of the kids*, the SSC will block the application of *each*-Movement. In order to derive (65b), control must thus be regarded as an enduring property of the paired positions in cases like (66), where PRO-Replacement has created a complex

structure/ . . .

structure consisting of PRO plus lexical item in the embedded subject position. Also, the SSC would have to be reformulated so that a position is not considered to be lexically specified if it is controlled. Chomsky (1973:266) remarks that "these consequences, while not intolerable, nevertheless do not seem to me particularly desirable". In view of these undesirable consequences, Chomsky drops the assumption that *each*-Movement precedes *it*-Replacement.

(65b) in fact constitutes a counterexample to an analysis of (63b) and (64b) that incorporates the assumption that *each*-Movement precedes *it*-Replacement. This counterexample could be avoided if two additional steps were taken: (i) It must be assumed that control is an enduring property of the paired positions in cases like (66), and (ii) the SSC must be reformulated so that a position is not to be considered lexically specified if it is controlled. These two steps are rejected as "undesirable" by Chomsky. He does not provide any reasons as to why he regards these steps as undesirable. An obvious consequence of the second step would be that the SSC can no longer block (63b). Recall that the alternative to the SSC - formulated to include the control case - in such cases is to restrict *each*-Movement to a single clause. The reasons why Chomsky rejects the latter move is outlined in § 3.2.5 above.

If the assumption that *each*-Movement precedes *it*-Replacement is dropped, then the derivation of (65b) becomes unproblematic. In order to account for (64b), Chomsky assumes that when the NP *John* replaces *it* in (64b), it leaves behind a "trace" which it controls. Given the presence of this controlled trace in the subject position of the embedded clause, the SSC will block the application of *each*-Movement, thus explaining the unacceptability of (64b). However, as Chomsky (1973:267) points out, the trace-approach will not work in the case of (63b). The unacceptability of the latter sentence thus remains unexplained.

The hypothesis/ . . .

The hypothesis that *it*-Replacement leaves behind a controlled trace thus enables the SSC to overcome an empirical difficulty. Specifically, the adoption of this hypothesis enables the SSC to explain the unacceptability of (64b). Note, however, that the unacceptability of (63b) remains unexplained.

Let us now turn to *wh*-Movement. Consider the application of *wh*-Movement and *each*-Movement in (67).

- (67) a. COMP they each expected [COMP who to kill the others] {182}  
 b. Who they each expected to kill the others {187}  
 c. \*Who did they expect to kill each other {188}

Cyclic application of *wh*-Movement in (67a) gives (67b). *each*-Movement can then apply to give (67c). However, (67c) does not have the interpretation of (67b). To explain this fact, Chomsky first considers ordering *each*-Movement before *wh*-Movement. The SSC would then block the application of *each*-Movement, because the subject position of the embedded clause contains the specified subject *who* at the stage where *each*-Movement must apply. The derivation of (67c) from (67b) would then be blocked. However, the assumption that *each*-Movement precedes *wh*-Movement does not suffice in all cases. Consider the derivation in (68).

- (68) a. COMP Bill wanted [COMP they each to expect [COMP who to kill the others]] {189}  
 b. COMP Bill wanted [COMP they each to expect [who to kill the others]]  
 c. COMP Bill wanted [COMP they to expect who to kill each other]  
 d. COMP Bill wanted [who they to expect to kill each other]  
 e. \*Who did Bill want them to expect to kill each other

On the/ . . .

On the innermost cycle *wh*-Movement applies to (68a) to give (68b). On the next cycle - assuming that *each*-Movement precedes *wh*-Movement - (68c) is first derived by applying *each*-Movement, and then (68d) is derived by *wh*-Movement. On the last cycle, *wh*-Movement (together with the obligatory rules of Auxiliary Inversion and Case Assignment) derives (68e). The assumption that *each*-Movement precedes *wh*-Movement therefore does not suffice to rule out the derivation of the unacceptable (68e). In fact, (68e) constitutes a potential counterexample to the proposed analysis of (67), an analysis that incorporates the assumption that *each*-Movement precedes *wh*-Movement.

In view of the empirical criticisms that can be raised against the assumption that *each*-Movement precedes *wh*-Movement, Chomsky rejects this assumption. Instead, he assumes that *wh*-Movement - like *it*-Replacement - leaves behind a trace. In the case of *wh*-Movement, this trace is controlled by the moved *wh*-phrase. In (68b) *who* will thus control its trace in the subject position of the lowest embedded clause. Because of the presence of this controlled subject, the SSC will prohibit *each*-Movement from moving *each* into the embedded clause, thus blocking the derivation of (68e). In the same manner the trace of *who* in (67b) will prevent the application of *each*-Movement to give (67c). The SSC can then explain the nonapplication of *each*-Movement in (67c) and (68c), if it is assumed that *wh*-Movement leaves a controlled trace behind.

Recall that Chomsky's (1973) aim is to restrict the formal power of transformations. In order to achieve this aim Chomsky must show that constraints on the applicability of transformations can be explained in terms of general - i.e., universal - conditions on rules. Cases in which these general conditions fail to explain the constraints on rule application are then clearly problematical. In fact, such cases can be regarded as potential counterexamples to the system of conditions proposed in (Chomsky 1973) - for short, the "Conditions"-framework. Cases such as

(64b) and/ . . .

(64b) and (68e), in which the conditions fail to explain the non-applicability of *each*-Movement, thus constitute potential counter-examples to the "Conditions"-framework. When viewed against this background, it becomes obvious that the hypothesis that *it*-Replacement and *wh*-Movement leave behind controlled traces are very important within the context of (Chomsky 1973).

One striking feature of Chomsky's (1973) presentation of the notion that transformational rules leave behind controlled traces, is the extent to which crucial issues are left unclear. To mention but a few examples: Chomsky does not specify exactly what class of movement rules leave behind traces. While he (1973: 269, fn. 4) does suggest "that every rule that moves an item from an obligatory category (in the sense of Emonds (1970))<sup>69</sup> leaves a trace", it is by no means clear whether these are the only rules that leave traces. Also very little information is provided about the nature of traces, and the ways in which they interact with conditions other than the SSC. No information is provided as to how traces can be associated with the correct moved phrase, a problem that obviously arises in structures where there is more than one trace. Trace theory, as presented in (Chomsky 1973), is thus in crucial respects obscure.

Despite this obscurity in its content, Chomsky (1973) tries to show that there is some independent justification for the notion that (certain) movement rules leave traces. He (1973:269, fn. 49) claims that this notion makes it possible to explain the obligatory character of NP-Preposing in Passive in sentences versus its optional character in noun phrases. Of crucial importance is the fact that this explanation incorporates the assumption that in simple N-V-N sentences the subject position is filled by a full NP in the underlying structure.<sup>70</sup> It is thus possible to avoid the assumption made by Emonds, namely that the subject position of such sentences is obligatorily empty in the underlying structure. The latter assumption is claimed by Chomsky to be problematical, although he provides no reasons for his judgment.

The second fragment of independent justification which Chomsky (1973) presents for the notion that movement rules leave traces, specifically bears on *wh*-Movement. He (1973:282) argues that trace theory makes it possible to adopt "a fairly simple rule of interpretation for *wh*-Questions", namely the rule in (69).

- (69) "The phrase [ <sub>$\alpha$</sub>  [*wh*, NP] + WH]... PRO ...] is interpreted with PRO a variable bound by the node [*wh*, NP] and ... the semantic interpretation determined by the derivation of  $\alpha$ <sup>65</sup>."

Chomsky (1973:fn 65) explains that PRO is the trace of *wh*-Movement. The fact that traces facilitate semantic interpretation provides some independent justification for the notion that movement rules leave traces, in Chomsky's view.

When considering the question of independent justification for traces presented in (Chomsky 1973), one must keep in mind that since the early seventies there has been an ongoing attempt by Chomsky (and his students) to find independent justification for traces.<sup>71)</sup> Drawing on work done by, for example, Wasow and Fiengo, Chomsky (1975a) presents several arguments which are claimed to be independent for trace theory: (i) trace theory facilitates semantic interpretation (pp. 93-96); (ii) trace theory provides a solution to the "crossover" problem originally noted by Postal (1971) (pp. 99-100); (iii) trace theory provides an explanation of where downgrading rules are possible (pp. 106-110). The argument that traces enable the SSC to apply to a wider class of cases also features quite prominently in works that follow (Chomsky 1973) - see, for example, Chomsky (1975a: 102-103); Chomsky (1976a:320f.). Let us briefly look at the argument in the latter work.

- |      |  |       |
|------|--|-------|
| (70) | the men like each other                      | {14a} |
| (71) | the men want [John to like each other]       | {15a} |
| (72) | the men seem to John [t to like each other]  | {19a} |
| (73) | John seems to the men [t to like each other] | {20a} |

(70) is/ . . .

(70) is analogous to (72), and (71) is analogous to (73). The rule of Reciprocal Interpretation applies in (70).<sup>72)</sup> Similarly, it applies in (72), as if there is no specified subject in the embedded sentence, *t* being the trace of *the men*. The SSC blocks the application of the Reciprocal rule in (71), because of the presence of the specified subject *John* in the embedded sentence. Similarly, the SSC blocks the application of the Reciprocal Rule in (73), with *t*, the trace of *John*, acting as the specified subject. Chomsky (1976a:321) concludes on the basis of such examples that "the trace theory thus permits otherwise valid conditions to apply, again overcoming cases of misapplication of rules: overgeneration in the case of the reciprocal rule . . .".

The main points of the discussion above can be summarized as follows.

- (74) a. *it*-Replacement in sentences like (54), (55) constitutes a potential counterexample to the SSC. In order to make *it*-Replacement in such cases consistent with the SSC, Chomsky adopts an auxiliary hypothesis, namely that English has a rule of PRO-Replacement. Independent justification of a mainly empirical nature is presented for this hypothesis.
- b. The conditions face a further empirical difficulty, in that they fail to explain certain constraints on the interaction between *it*-Replacement and *each*-Movement, and between *wh*-Movement and *each*-Movement. In order to overcome this difficulty, Chomsky adopts another auxiliary hypothesis, namely that *it*-Replacement and *wh*-Movement leave behind controlled traces which act as specified subjects.
- c. Chomsky (1973) clearly regards it as important that there should be independent justification for trace theory, and in (Chomsky 1973) a limited amount of in-

dependent/ . . .

dependent justification is in fact presented for trace theory. When considering the question of independent justification for trace theory, one must also take into account the fact that since the early seventies there was a determined effort by Chomsky and his associates to find a wide range of independent justification for this theory.

- d. Within the context of (Chomsky 1973), a conceptual criticism can be raised against trace theory, on the basis of its obscurity of content.
- e. Even with the adoption of trace theory, all empirical problems are not overcome - see (63b) above. The problem raised by (63b) is noted by Chomsky, and further ignored.

### 3.2.7.3 The SSC and the feature [+ definite]

The sentences (75) - (77) below illustrate a three-way gradation of acceptability with respect to *wh*-Movement from NPs.

- (75) a. COMP you saw [NP pictures of who] {30a}  
b. Who did you see pictures of {30b}
- (76) a. COMP you saw [NP the pictures of who]  
b. ?Who did you see the picture of
- (77) a. COMP you saw [NP John's pictures of who] {31a}  
b. \*Who did you see John's pictures of {31b}

According to Chomsky (1973:239, fn. 19), (75b) is completely acceptable, (76b) less acceptable than (75b) but more acceptable than (77b), and (77b) completely unacceptable. The SSC, formulated as in (1) above, cannot explain why (76b) is less acceptable than (75b), but more acceptable than (77b). The failure

of the SSC/ . . .

of the SSC to explain this gradation of acceptability could constitute grounds for empirical criticism of this condition, and in fact the whole "Condition"-framework. As is explained in § 3.2.7.2 above, cases like these, in which the conditions proposed in (Chomsky 1973) fail to explain the constraints on rule application, can be regarded as constituting potential counter-examples to the "Conditions"-framework.

Chomsky claims that the incorporation of the feature [+ definite] in the SSC could overcome the problem posed by sentences like (75) - (77). If the SSC were to include the feature [+ definite], then (77b) would constitute a double violation of the SSC (given that lexical subjects are [+ definite]), (76b) would constitute a single violation, and (75b) no violation at all. The distinction between a double violation, a single violation, and no violation of the SSC could then account for the differences in acceptability exhibited by (75) - (77).<sup>73)</sup>

Chomsky (1973) does not provide a definition of the notion 'definite' used in the proposed modification of the SSC. In his article, "Toward an explanation of certain peculiarities of the existential construction in English", Milsark (1977:5) observes that the status of the notion in linguistic theory is "anything but clear". He points out that while there is a set of distributional criteria for recognizing so-called "definite noun phrases", there is no successful characterization of the notion 'definite'. Given this state of affairs with respect to the notion 'definite', it could be argued that Chomsky's proposed modification of the SSC in terms of the notion 'definite' introduces an obscure element into the condition. Such an obscurity of content could constitute grounds for a conceptual criticism of the proposed modification.

An interesting aspect of the proposed modification of the SSC is that it is not taken up in the works that follow (Chomsky 1973), for example, (Chomsky 1976a) and (Chomsky 1977c). Chomsky's

claim/ . . .

claim that the problem posed by sentences like (75) - (77) can be overcome by incorporating the notion 'definite' in the SSC must in fact not be seen as a firm proposal that the SSC must be modified in the relevant manner. Rather, this claim must be seen as a tentative suggestion that the problem could possibly be overcome if the SSC were modified by the incorporation of the feature [+ definite].

The main conclusions of the discussion above can be summarized as follows:

- (78) a. In order to overcome an empirical difficulty (in the form of unexplained phenomena) Chomsky tentatively proposes that the SSC should incorporate the notion 'definite'.
- b. The proposed modification of the SSC can be criticized on the grounds of obscurity, i.e., on conceptual grounds.
- c. Chomsky's claim that the empirical difficulty in question could be overcome by the incorporation of the notion 'definite' in the SSC has the status of a tentative suggestion, rather than a firm proposal.

3.2.7.4 The SSC and the notion 'agency'

Chomsky (1973:261) points out that *each*-Movement in cases like (79) and (81) violates the SSC.

(79) Why are John and Mary letting the honey drip  
on each other's feet {155}

(80) \*Why are John and Mary letting Bill drip honey on  
each other's feet {156}

(81) / . . .

- (81) Why are they letting the baby fall on each  
other's laps (157)
- (82) \*Why are they letting Bill drop the baby on each  
other's laps (158)

The nonapplicability of *each*-Movement in (80) and (82) follows from the SSC. Both these sentences contain a specified subject - *Bill* - which blocks *each*-Movement. In (79) and (81) there are also specified subjects: *the honey* and *the baby*, respectively. However, the SSC does not block *each*-Movement in these cases. *each*-Movement in such cases thus constitutes a potential counter-example to the SSC.

Chomsky (1973:261) proposes a modification to the SSC that would overcome the problem posed by *each*-Movement in cases like (79), (81). In terms of Chomsky's proposal, the notion 'specified agent' must replace the notion of a formal subject in the SSC. If the SSC were formulated in terms of the semantic notion 'specified agent', then the application of *each*-Movement in (79) and (81) would no longer constitute a violation of the SSC. *the honey* in (79) and *the baby* in (81) are not agents, even though they are specified subjects.

Chomsky (1973:257, fn. 37) hints that the reformulation of the SSC in terms of the notion 'agency' would also make it possible to explain the difference in acceptability between (83) and (84).

- (83) The men wanted to tell stories about killing each other.  
(84) The men wanted to hear stories about killing each other.

According to Chomsky, (83) - with *the men* the understood subject of *kill* and a relation of semantic agency between *the men* and *stories* - is more "natural" than (84). In (84) *the men* is not in a relation of agency to *stories*. Presumably the modified SSC could explain the difference in acceptability between (83)

and (84)/ . . .

and (84) as follows: In (83), but not in (84), the phrase containing *each* in the underlying structure - *the men each* - is also the agent of the NP [*stories about killing each other*]. The agent of the latter NP in (84) thus qualifies as a specified agent. *each*-Movement is thus blocked by the modified SSC in (84), but not in (83).

Chomsky's proposal on reformulating the SSC in terms of the notion 'semantic agent' is very tentative, and also very vague. Not only does Chomsky fail to provide a definition of the notion 'semantic agent', but he also does not make any explicit claim as to precisely how the SSC should be modified. He does not even consider the question of how an SSC formulated in terms of a notion 'semantic agent' could account for the constraints on extraction from NPs with specified subjects. As in the case of the proposed modification discussed in § 3.2.7.3, the modification under discussion is not taken up in works that follow (Chomsky 1973). Again, Chomsky is tentatively suggesting a possible solution to an empirical problem, rather than making any firm proposal.

The main conclusions of the discussion above are briefly summarized in (85).

- (85) a. In order to overcome an empirical difficulty (in the form of potential counterexamples) Chomsky tentatively proposes that the SSC should be reformulated in terms of the notion 'semantic agency'.
- b. The proposed modification can be criticized on the grounds of obscurity and vagueness, i.e., on conceptual grounds.
- c. Chomsky's claim that the empirical problem in question could be overcome by reformulating the SSC in terms of the notion 'semantic agency' has the status of a tentative suggestion, rather than a firm proposal.

### 3.2.7.5 The unsolved problem of Coreference Assignment

Chomsky (1973:238, fn. 16) notes that Coreference Assignment does not obey the TSC. Coreference Assignment is the rule that relates the NP *John* and the pronoun *he* in sentences like (86), with *John* and *he* interpreted as coreferential.

(86) John said [that he would leave]

In (86) the pronoun *he* is within a tensed clause. Coreference Assignment is thus a potential counterexample to the TSC. Although Chomsky (1973) does not mention it, Coreference Assignment also violates the SSC, as in the following example.

(87) John thought that Bill liked him

In (87) Coreference Assignment associates *John* and *him* across the specified subject *Bill*. Coreference Assignment is thus also a potential counterexample for the SSC.

Chomsky points out that Coreference Assignment applies in other structures, for example, coordinate structures, in which various other types of rules are blocked. For instance, the application of Coreference Assignment in (88) conflicts with the Coordinate Structure Constraint.<sup>74)</sup>

(88) John said that he and Bill would leave

Coreference Assignment is thus problematical with respect to conditions other than the TSC (and SSC) as well. Chomsky (1973) does not take any specific steps to solve the problem which Coreference Assignment poses for his conditions.

The main conclusions of this section can then be summarized as follows.

(89) / . . .

- (89) a. Coreference Assignment constitutes a potential counterexample to the SSC and TSC.
- b. Chomsky offers no solution to the problem which Coreference Assignment poses to the conditions in question, that is, he puts the problem aside.

### 3.2.7.6 Chomsky's attitude of epistemological tolerance

Three main points emerge from the discussion above (particularly the conclusions presented in (53), (74), (78), (85), and (89).

- (i) Even at the time of their introduction, Chomsky recognized that the SSC and TSC (and also the greater "Conditions"-framework of which they form part) exhibit numerous empirical inadequacies, including potential counterexamples.
- (ii) On the one hand, Chomsky's reaction to these empirical inadequacies is characterized by a willingness "to make the conditions work", rather than to abandon them in the face of these empirical inadequacies. In order to make the conditions work, he makes use of auxiliary hypotheses and modifications to the conditions themselves, modifications which leave the core content of the conditions intact.
- (iii) On the other hand, Chomsky is willing to set aside those empirical difficulties for which he has no solution at present - cf. (63b), § 3.2.7.5 - or for which he can only tentatively suggest the direction of a possible solution - cf. §§ 3.2.7.3, 3.2.7.4.

The last-mentioned aspect of Chomsky's handling of the empirical inadequacies noted at the time of the introduction of the SSC and TSC illustrates the tolerant attitude to empirical inadequacies so explicitly advocated by him in his recent works. So, for

instance/ . . .

instance, Chomsky (1980a:9-10) advocates that linguists should exhibit a "readiness to tolerate unexplained phenomena or even as yet unexplained counterevidence to theoretical constructs that have achieved a certain degree of explanatory depth in some limited domain". Other works in which Chomsky advocates this tolerant attitude towards empirical difficulties - particularly potential counterexamples - include (Chomsky 1978a:10), (Chomsky 1978b:14), (Chomsky 1979a:188). In these works, Chomsky depicts a tolerant attitude toward empirical difficulties as a feature of the so-called "Galilean style of inquiry", a style of inquiry which he argues ought to be adopted by linguists.<sup>75)</sup>

A comprehensive and detailed analysis of Chomsky's attitude of epistemological tolerance is presented in § 7.2.3.6 below. However, in order to avoid possible confusion, there is one point in connection with this attitude that must briefly be made here. Attempts by Chomsky to overcome some of the empirical inadequacies of the SSC and TSC - see point (ii) above - do not conflict with the attitude of epistemological tolerance adopted by Chomsky. The adoption of such an attitude does not entail that negative evidence - either in the form of potential counterevidence or in the form of unexplained facts - threatening a theory becomes irrelevant for the appraisal of the theory. Consider, for example, the discussion in (Chomsky 1979a:188), where it is made quite clear that, while it is reasonable to put aside counterexamples to a theory "with some degree of explanatory force", ultimately all potential counterexamples must be explained. All other things being equal, the elimination of potential counterexamples threatening a theory constitutes a step forward in Chomsky's linguistics. The adoption of an attitude of epistemological tolerance rather means that where a theory which has some explanatory success faces potential counterexamples that cannot at the time be explained, these counterexamples must be set aside in the hope that it might later on become possible to explain them. This aspect of Chomsky's epistemological tolerance is analyzed in more detail in § 7.2.3.6 below.

### 3.3 The SSC and TSC as presented in "Conditions on rules of grammar"

#### 3.3.1 General remarks

In § 3.3 the focus is on "Conditions on rules of grammar" (Chomsky 1976a), the second major work in which the SSC and TSC are presented as conditions that restrict the applicability of both transformational rules and rules of semantic interpretation. The topics dealt with below are: further restrictions on the formal power of transformational rules (§ 3.3.2), some changes in the formulation of the SSC (§ 3.3.3), the definition of the notion 'involve' (§ 3.3.4), and the role of the idealization of sentence grammar in Chomsky's handling of potential counter-examples to the SSC and TSC (§ 3.3.5).

#### 3.3.2 Further restrictions on the formal power of transformational rules

As regards the relation between conditions such as the SSC and TSC and restrictions on the formal power of transformational rules, (Chomsky 1976a) differs in two respects from (Chomsky 1973). Firstly, Chomsky (1976a) places much greater emphasis on the contribution which conditions on function - such as the SSC and TSC - can make to restricting the formal power of transformational rules. As is pointed out in §§ 3.2.3 and 3.2.5 above, Chomsky (1973) recognizes the relationship between conditions such as the SSC and TSC and restrictions on the formal power of transformations. In the case of the Passive transformation, for instance, he explicitly argues that the introduction of the TSC makes it possible to uphold the principle of blind application (a special case of the simple string condition). However, in characterizing the contribution of conditions on function to the fundamental empirical problem of linguistics, Chomsky (1973:234) focusses on the way such conditions limit the generative power of grammars of a given form. In contrast, Chomsky (1976a)

places/ . . .

places great emphasis on the fact that it is in fact conditions on function which make it possible to impose significant restrictions on the formal power of transformational rules. For instance, Chomsky (1976a:174-178) argues that conditions such as the SSC and TSC can overcome the problems of overgeneration that arise under a restricted theory of transformations. Indirectly, then, such conditions do contribute to restricting the formal power of transformations.

Secondly, Chomsky (1976a) proposes that even stronger restrictions than those embodied in the simple string condition should be imposed on the formal power of transformations. Specifically, he (1976a:312) proposes that the latter restriction on the formal power of transformations must be strengthened by a condition of *minimal factorization*. This condition of minimal factorization rules out a structural description with two successive categorial terms, unless one of them is satisfied by a factor changed by the rule. For example, the condition of minimal factorization (but not the simple string condition) rules out the structural description (90) for Passive, since only NP is changed by the rule.

(90) (*vbl*, NP, AUX, V, NP, #, *vbl*) (3)

Instead, Passive must now be formulated as (91a), or equivalently, as (91b) (given Emonds' structure-preserving hypothesis).

(91) a. (*vbl*, NP, *vbl*, NP, *vbl*)  
 b. Move NP

The adoption of the condition of minimal factorization obviously leads to a drastic reduction in the formal power of transformational rules. However, just as obviously, a grammar limited to rules such as (91) will overgenerate massively. Chomsky then proceeds to show that this problem can, to a significant extent, be overcome by general (= universal) conditions on rules in-

cluding/ . . .

cluding, specifically, the SSC and TSC. Consider, for example, the following examples presented in (Chomsky 1976a).

- (92) a. John<sub>i</sub> is believed [t<sub>i</sub> to be incompetent] (10b)  
b. \*John<sub>i</sub> is believed [t<sub>i</sub> is incompetent] (10a)  
c. \*John<sub>i</sub> seems [Bill to like t<sub>i</sub>] (10e)

The rule (91) generates all the sentences of (92), with *t* the trace of *John*. Only (92a) is acceptable. The rule (91) thus overgenerates drastically. The unacceptable sentences (92b) and (92c) are ruled out by the TSC and SSC, respectively. In (92b) *Y* (= *t<sub>i</sub>*) is within a tensed clause. In (92c) there is a specified subject, *Bill*, in the embedded clause.

Note, incidentally, that while Chomsky admits that *universal* conditions on rules constitute "the best case", he (1976a:315) claims that it is not only universal conditions that make it possible to reduce formal power. Language-particular, or even rule-particular, conditions may also lead to a reduction in expressive power, if these conditions are regarded as parameters that must be fixed. It follows, for example, that if a rule *X* from a language *Y* does not obey the SSC and/or TSC, one need not abandon the conditions, and thus give up the reduction in formal power made possible by them. The contribution of non-universal conditions to the reduction of formal power is taken up in § 4.3 below.

The role which the consideration of restricted formal power plays in the justification of general-linguistic hypotheses, as well as the nature of this consideration, are analysed in detail in § 3.2.5 above. The main points of the present section can be summarized as follows.

- (93) a. In (Chomsky 1976a) the consideration of restricted formal power features much more prominently in the justification of the SSC and TSC than it did in (Chomsky 1973).

- b. Chomsky (1976a) argues that the simple string condition adopted in (Chomsky 1973) should, and in fact can, be strengthened by the condition of minimal factorization. The SSC and TSC are instrumental in making this restriction on the formal power of transformational rules possible.

### 3.3.3 Some changes in the formulation of the SSC

Chomsky (1976a:316) formulates the SSC as follows:

(94) "Consider a structure of the form:

(11) ... X ... [  $\alpha$  ... Y ... ] ... X ...

Then no rule can involve X and Y in (11) . . . where  $\alpha$  contains a subject distinct from Y and not controlled by X . . . ."

The formulation (94) differs from the formulation adopted in (Chomsky 1973), and presented as (1) above, in two respects. First, in (1) it is stipulated that the specified subject must be to the left of Y within  $\alpha$ , i.e., the specified subject must intervene between X and Y. In (94) above this stipulation is omitted. Reference is now only made to a subject distinct from Y. The formulation of (94) above is obviously more general than that of (1). For example, while (1) allows the association of X and Y in (95), (94) prohibits it (under the assumption that  $\bar{S}$  and NP are the cyclic nodes).

(95) ... X ... [  $\bar{S}$  [ COMP Y ] [ S ... Z ... ] ]

It is quite unclear what considerations (if any) have led to this change in the formulation of the SSC. Note that in the formulations presented in (Chomsky 1975a:101, 150) and (Chomsky 1976b: 52) - two works dating from the period between (Chomsky 1973) and (Chomsky 1976a) - the SSC is formulated as in (1), with the specified subject to the left of Y. Moreover, Chomsky (1976a)

does not/ . . .

does not consider the empirical consequences of this change in the formulation of the SSC. The issue is taken up again in "On *wh* Movement" (Chomsky 1977c) - see § 4.4.5 below for discussion.

Second, in the formulation (1) it is stipulated that Y is "not controlled by a category containing X", while in (94) above it is stipulated that Y is "not controlled by X". The stipulation in (1) concerning a category containing X was needed in (Chomsky 1973) for the purpose of accommodating *each*-Movement. Consider the following sentences.

- (96) a. We each persuaded Bill [COMP PRO to kill the  
other(s)] {113}
- b. \*We persuaded Bill to kill each other {112}
- (97) a. We each promised Bill [COMP PRO to kill the  
other(s)] {115}
- b. We promised Bill to kill each other {114}

*each*-Movement derives sentences such as (96b) and (97b) from structures such as (96a) and (97a), respectively. In both cases X = *each*, Y = *the other(s)*. In (96a) PRO is controlled by *Bill*, i.e., not by a category containing X. The derivation of (96b) thus violates the SSC. In (97a) PRO is not controlled by X. It is, however, controlled by a category containing X, namely *we each*. (97b) can thus be derived. In works that follow (Chomsky 1973) - including (Chomsky 1976a) - it is assumed that an interpretive rule associates *we* and *each other* in sentences such as (96b) and (97b). In (97b) X (= *we*) is then the controller of PRO. There is thus no longer any need to distinguish between X and a category containing X in the SSC.

In sum, then:

- (98) a. The version  $T_{X+1}$  of the SSC adopted in (Chomsky 1976a) differs in two respects from the previous version  $T_X$ .

b./ . . .

- b. The first difference - the absence in the version  $T_{X+1}$  of the stipulation that the specified subject intervenes between X and Y - appears to be quite arbitrary within the context of (Chomsky 1976a).
  
- c. The second difference - the absence in the version  $T_{X+1}$  of the stipulation category *containing X* - is simply the result of a change in Chomsky's views on the nature of the rule which associates *we* and *each other*.

The second difference requires no further comment. The first difference is taken up again in § 4.4.5 below.

#### 3.3.4 The notion 'involve'

As pointed out in § 3.2.2 above, Chomsky (1973) does not provide a definition of the notion 'involve' which features in the formulation of the SSC and TSC. Such a definition is provided by Chomsky (1976a:316).

- (99) "In the case of a transformational rule, we may understand 'X is involved in the rule' to mean that X is changed by the rule or is a constant context for some change . . . Thus the terms involved in the rule are the factors that are not arbitrary strings, in accordance with the SD. In an interpretive rule, we may say that X and Y are involved if the rule establishes a relation of anaphora or control relating X and Y."

By providing this definition of the notion 'involve', Chomsky (1976a) overcomes an obvious shortcoming of the presentation in (Chomsky 1973). Without a precise definition of 'involve', it is simply not possible to determine what rules, or subclasses of rules, are supposed to be subject to the conditions.

An interesting feature of the definition of 'involve' presented by Chomsky (1976a), is that it enables him to overcome some

potential/ . . .

potential counterexamples to the SSC and TSC. In the case of interpretive rules, the definition presented in (99) above restricts the class of rules subject to the SSC and TSC to rules of anaphora and control. In (Chomsky 1973) it was assumed, at least implicitly, that a much wider class of interpretive rules fall under the conditions. For instance, the rule associating *not* and *many* in sentences such as (100) is claimed by Chomsky (1973:242) to be constrained by the SSC.<sup>76)</sup>

(100) I didn't see [ <sub>NP</sub> John's pictures of many of  
the children ] (47)

However, this rule is not a rule of anaphora or control, and it thus falls outside the scope of the SSC, as presented in (Chomsky 1976a). Chomsky (1976a) does not examine the consequences of restricting the conditions, in the case of interpretive rules, to rules of anaphora and control. However, in (Chomsky 1977c) it becomes obvious that this step enables him to deal with some potential counterexamples to the conditions including the rule associating *not* and *many*. These cases are discussed in § 4.4.3 below.

In the case of transformational rules, Chomsky (1976a:316, fn. 22) distinguishes two subcases: (i) X is changed by the rule, and (ii) X is a constant context for some change. Chomsky refers to work by Fiengo and Lasnik (1976) for an example that falls under the second case. The rule in question is Q-float. While Chomsky (1976a) does not provide any detail on the matter, (Chomsky 1977c:77f.) contains a fairly detailed discussion of the problem posed by Q-float. This discussion makes it quite clear that the case where X is a constant context for some change is incorporated in the definition of 'involve' in order to overcome a potential counterexample to the SSC, and in fact to the "Conditions"-framework as a whole. Although, strictly speaking, (Chomsky 1977c) belongs to the second stage of the development of the SSC and TSC, its discussion of Q-float will be considered

in this/ . . .

in this section. There are two reasons for this. - First, the problem in question is first raised in (Chomsky 1976a), and second, the issue is unaffected by the transition from the first to the second stage.

Fiengo and Lasnik (1976:188) formulate Q-float with the structural description

$$X, Q, NP, \left\{ \begin{array}{l} AP \\ NP \\ VP \end{array} \right\}, Y.$$

Q can then be moved to the position between the third and fourth factors. As Chomsky (1977c:78) points out, Q-float will then generate the acceptable sentences in (101), but not the unacceptable sentence (102).

- (101) a. I gave the men all presents {17a-c}  
 b. I persuaded the men all to leave  
 c. I painted the houses all reddish-yellow

- (102) \*I saw the men all {18}

Q-float, as formulated by Fiengo and Lasnik, will also generate (103).

- (103) \*I promised the men all to leave {19}

The unacceptability of (103), in contrast to the acceptability of (101b), represents a potential counterexample to Fiengo and Lasnik's formulation of Q-float. Fiengo and Lasnik (1976:189f.) argue that the unacceptability of sentences such as (103) can be explained on the basis of a modified version of the SSC. They (1976:189) assume that the complements in cases such as (101b) and (103) are VPs. This assumption conflicts with Chomsky's assumption that the embedded clause in such cases has the form  $[_S \text{ COMP } [_S \text{ PRO to VP}]]$ . Chomsky (1977c:78) argues that the unacceptability of (103) can be accounted for by the

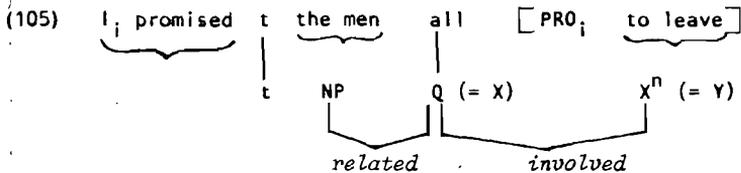
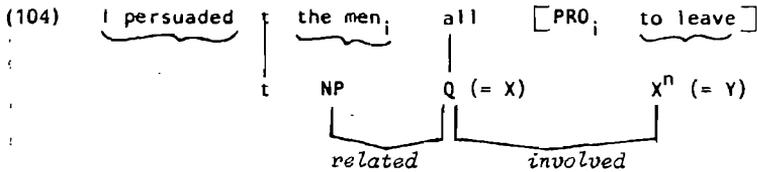
SSC even if the latter assumption about the underlying structure of the complements is made.

The principal elements of Chomsky's proposed solution to the problem raised by the acceptability of (101b), as opposed to the unacceptability of (103), are the following.

- (i) An assumption of the known control properties of *promise* and *persuade*.
- (ii) An extension of the notion 'involvement' to cover adjacent constant terms, one of which is either an antecedent or anaphor and the other a constant category of the  $\bar{X}$ -system. From this it follows that *all* and *to leave* in (101b) and (103) are involved in Q-float.
- (iii) An assumption that PRO is a nonterminal node.
- (iv) A modification of the notion 'specified subject', so that no rule can apply in the structure  
 $\dots X \dots [\alpha \dots Z \dots - WYV \dots]$  ... if X and Y are involved in the rule and  $\alpha$  contains a subject not containing Y and not controlled by the category containing X or its trace. In terms of this modification the control of the subject of  $\alpha$  by the trace of X will also permit the application of a rule involving X and Y in the relevant structure.

Given these assumptions, Chomsky is able to explain why (101b) is acceptable. Consider the following schematic representations of his analyses of (101b) and (103), respectively.<sup>77)</sup>

(104)/ . . .



In (104) PRO is controlled by  $t$  *the men*, that is, by the category containing the trace of X. Since PRO in (104) is not specified in the appropriate sense, the SSC does not block the rule relating the NP *the men* and Q *all*. Consequently, (101b) is acceptable. In (105) PRO is controlled by  $I$ . PRO is thus not controlled by X (= Q), or by its trace. Therefore, PRO is a specified subject. The SSC thus prevents Q-float from associating *the men* and *all* in (105).

The proposed modification of the SSC and the extension of the notion 'involve', have the status of tentative suggestions in (Chomsky 1977c). From the following remarks by Chomsky (1977c: 78) it is clear that he himself is by no means convinced of the correctness of the proposed modifications.

(106) "The case is interesting in that the constant terms 'involved' are Q and VP, although the application of the rule related NP and Q. Judgments are unfortunately somewhat variable in the relevant cases and there are other possible analyses, but perhaps we can take this example at least as an illustration of the logic of the problem, and perhaps an actual illustration of the operative principles, though I am rather sceptical."

The aim of Chomsky's discussion of the problematic Q-float data is to show that these data can be handled without complicating

the rule/ . . .

the rule itself. Given the status of Q-float in the controversy between Postal on the one hand, and Fiengo and Lasnik on the other hand, it is quite important for Chomsky to be able to show this. Postal (1976) argues that the theory of transformations must be enriched to allow rules such as Q-float to refer to grammatical functions, including subject. That is, Postal argues that the principle of blind application - and consequently also the simple string condition - must be rejected. In particular, Postal (1976:161ff.) argues that if Q-float is formulated without reference to the notion 'subject', the rule faces numerous potential counterexamples. According to him, these examples are automatically accounted for if a formulation referring to the notion 'subject' is adopted.

As should be quite clear from the discussion above - see especially §§ 3.2.5, 3.3.2 - allowing transformational rules to refer to grammatical functions represents an undesirable enrichment of transformational theory from Chomsky's point of view. Such a step would lead to an increase in the formal power of transformational rules. Fiengo and Lasnik (1976), who share Chomsky's views about the enrichment of transformational theory, reject Postal's argument. They (1976:188) argue that there is a "reasonably adequate analysis of Q-Floating", consistent with a more restrictive theory of transformations that prohibits reference to notions such as 'subject'. Chomsky cannot simply adopt Fiengo and Lasnik's solution to the problem posed by Q-float, since he assumes that the complement of verbs like *promise* is not VP. Chomsky is therefore compelled to show that within the framework of his own assumptions, the problem posed by Q-float can be handled without allowing reference to 'subject'.

Postal (1976:Appendix) actually admits that at least some of the potential counterexamples to a formulation of Q-float that does not refer to 'subject' can be handled by the SSC. However, on the basis of numerous potential counterexamples to the SSC, Postal argues that the SSC is neither a universal condition,

nor a/ . . .

nor a condition particular to English. Postal claims that, consequently, the SSC is not available as a means of ensuring that a formulation of Q-float that does not refer to 'subject' attains descriptive adequacy.

Fiengo and Lasnik, in turn, reject Postal's argument about the status of the SSC. In particular, they claim that some of the rules mentioned by Postal are counterexamples to almost all known conditions on rule applicability, and that some do not belong to sentence grammar.<sup>78)</sup> Because the SSC "correctly constrains the application of a wide variety of syntactic and semantic rules" (p. 190), Fiengo and Lasnik are unwilling to reject the SSC on the basis of the counterexamples cited by Postal. In their response to Postal's criticisms of the SSC, Fiengo and Lasnik exhibit the attitude of epistemological tolerance advocated by Chomsky. Chomsky's own response to Postal's criticisms of the SSC is also characterized by epistemological tolerance. Like Fiengo and Lasnik, he claims that some of the rules cited by Postal as constituting potential counterexamples to the SSC do not belong to sentence grammar, and thus fall outside the scope of the SSC. These rules are discussed in § 3.3.5 below. For the rest, Chomsky (1976a) simply ignores the negative evidence for the SSC presented by Postal.

The main points of the discussion above can be summarized as follows.

- (107) a. Chomsky (1976a) provides a definition of the notion "involve", a notion that was not explicitly defined in (Chomsky 1973).
- b. In the case of rules of semantic interpretation, the definition presented in (Chomsky 1976a) specifies a narrower scope for the SSC and TSC than is implicitly assumed in (Chomsky 1973). This narrower scope enables Chomsky to handle some potential counterexamples to the conditions.

- c. In the case of transformational rules, the stipulation that X may be a constant context for some change is required only to enable the SSC to apply to Q-float.
- d. The importance of Q-float derives from the fact that Postal (1976) argues that Q-float cannot be formulated without reference to 'subject'. If Postal were right, Q-float would constitute a potential counterexample to Chomsky's claim that the formal power of transformational rules can be restricted to exclude the option of referring to relational notions.
- e. Chomsky's attempt to accommodate Q-float under the SSC again illustrates his willingness to make his conditions work through the introduction of auxiliary hypotheses and modifications to the conditions themselves. This willingness is also exhibited by Fiengo and Lasnik (1976).
- f. Chomsky's reaction to the potential counterevidence for the SSC presented by Postal partly exemplifies the attitude of epistemological tolerance advocated by him. This is also true for Fiengo and Lasnik's reaction to Postal's claims.

### 3.3.5 The idealization of sentence grammar

One of the questions which arises from Chomsky comments on the so-called Galilean style of inquiry, is what role abstraction and idealization play in Chomsky's handling of negative evidence threatening the SSC and TSC (or the later versions of binding theory). In this section the role which the idealization of sentence grammar plays in Chomsky's (1976a) handling of negative evidence threatening the conditions is analyzed.

Chomsky (1975a:105) distinguishes between sentence grammar and

non-sentence/ . . .

non-sentence grammar. The SSC and TSC form part of sentence grammar. Consequently, rules that fall outside sentence grammar need not obey these conditions, and so cannot constitute counterexamples to them.

In the early works dealing with the SSC and TSC, Chomsky invokes the idealization of sentence grammar in three cases where rules apparently violate the SSC and TSC. A first case concerns so-called "Picture Noun Reflexivization". Postal (1976:172) lists Picture Noun Reflexivization in sentences such as (108) as a potential counterexample to the SSC.

(108) Mike will not believe that this is a picture of himself.

The lexically specified subject *this* intervenes between *Mike* and *himself*. The association of *Mike* and *himself* in (108) by the rule of Picture Noun Reflexivization thus violates the SSC. Chomsky (1976a:316, fn. 23) notes that Picture Noun Reflexivization resists analysis under any general theory known to him. Fiengo and Lasnik (1976:190) observe that Picture Noun Reflexivization violates a number of proposed conditions on rules, including the TSC, the Complex NP Constraint, the Coordinate Structure Constraint, and the Sentential Subject Constraint.<sup>79)</sup> Chomsky (1976a:316, fn. 23) tentatively suggests, following Helke (1971), that reflexivization in English consists of two parts: a process of bound anaphora subject to the conditions of sentence grammar, and another "more general" process that falls outside sentence grammar. The fact that Picture Noun Reflexivization resists analysis and violates a number of proposed conditions on rules, supports the hypothesis that this reflexivization process falls outside sentence grammar, according to Chomsky. If Picture Noun Reflexivization were outside sentence grammar, then it could no longer constitute a potential counterexample to the SSC or TSC (or indeed to any other condition of sentence grammar).

A second/ . . .

A second case in which Chomsky (1976a) invokes the idealization of sentence grammar to deal with an empirical inadequacy of the SSC, concerns the rule that assigns an interpretation to *the others*. Chomsky (1976a:321f.) compares the following two sets of sentences:

- (109) a. the men like each other (21)  
 b. the men want [John to like each other]  
 c. the men seems to John [t to like each other]  
 d. John seems to the men [t to like each other]
- (110) a. each of the men likes the other(s) (22)  
 b. each of the men wants [John to like the other(s)]  
 c. each of the men seems to John [t to like the other(s)]  
 d. John seems to each of the men [t to like the other(s)]

The pair (*each of the men, the other(s)*) is similar in meaning to the pair (*the men, each other*). The sentences in (109) thus correspond to the sentences in (110). However, while all the sentences in (110) are acceptable, only the (a) and (c) sentences of (109) are acceptable. The unacceptability of (109b) and (109d) can be explained on the basis of the SSC. The Reciprocal Rule, which relates *the men* and *each other*, is blocked in these sentences because of the presence of a specified subject in the embedded clause: *John* in (109b) and the trace of *John* in (109d). The acceptability of (110b) and (110d) indicates that the rule associating *each of the men* and *the other(s)* in (110) is not blocked by the SSC. This constitutes a potential counterexample to the claim that the SSC is a universal condition on rules. As Chomsky (1976a:322) points out, it seems as if the difference between the Reciprocal Rule and the rule assigning an interpretation to *the other(s)* forces one to formulate the SSC as a rule-particular principle. 80)

Chomsky avoids this undesirable reformulation by arguing, on the basis of the sentences presented in (111) - (112), that there is a principled difference between the two cases.

- (111) a. Some of the men left today. The others will  
leave later. {23a}
- b. \*Some of the men left today. Each other will  
leave later. {23a'}
- (112) a. Some of the articles are incomprehensible, but  
we each expected John to understand the others {23b}
- b. \*Some of the articles are incomprehensible, but  
we expected John to understand each other {23b'}

(111b) and (112b) are unacceptable. The unacceptability of (111b) shows that the Reciprocal Rule is a rule of sentence grammar. Being a rule of sentence grammar, it is blocked by the SSC in (112b), because of the presence of the specified subject *John*. The acceptability of (111a) indicates that the rule relating *the other(s)* to a suitable NP is not a rule of sentence grammar. Consequently, it is not subject to the conditions of sentence grammar. In cases such as (112a) the SSC thus does not block the rule. By arguing that the rule which assigns an interpretation to *the other(s)* is not a rule of sentence grammar, Chomsky avoids formulating the SSC as a rule-specific condition.

A third case in which Chomsky (1976a) uses the idealization of sentence grammar to overcome a problem for his conditions, concerns the rule of Coreference Assignment. Chomsky (1973:238, fn. 16) has observed that this rule violates the TSC. In (Chomsky 1973) no steps are taken to overcome the problem posed by Coreference Assignment - see the discussion in § 3.2.4.5 above. Chomsky (1976a:323) returns to Coreference Assignment, claiming that his observation that Coreference Assignment presents a problem for his theory "was simply an error". He now claims, following Lasnik (1976), that the rule of anaphora

which/ . . .

which (optionally) associates *he/him* and *John* in (113) is not a rule of sentence grammar.

- (113) a. John thought that he would win. {25}  
 b. John thought that Bill liked him.

Note that in (113b) the rule violates not only the TSC, but also the SSC. Because the rule of anaphora applying in sentences such as (113) is not a rule of sentence grammar, it is not subject to conditions such as the SSC and TSC. Consequently this rule cannot constitute a potential counterexample to Chomsky's theory.

In sum: Chomsky claims that Picture Noun Reflexivization, the rule assigning an interpretation to *the other(s)*, and Coreference Assignment cannot constitute actual counterexamples to the SSC and TSC, since they fall outside the scope of these conditions. While the SSC and TSC are conditions belonging to sentence grammar, the rules in question do not belong to sentence grammar. Note that in the case of each of these rules there is some independent justification that the rule does not belong to sentence grammar. In the case of the rule assigning an interpretation to *the other(s)*, Chomsky presents some evidence that this rule (in contrast with the Reciprocal Rule) applies across sentence boundaries. Although he presents no such evidence in the case of Picture Noun Reflexivization and Coreference Assignment, Chomsky does refer to the work of others who have argued that the rules fall outside sentence grammar: Helke (1971) for arguments concerning reflexivization, and Lasnik (1976) for arguments concerning Coreference Assignment.

Chomsky's claim that certain rules fall outside the scope of sentence grammar gives rise to a question about the nature of such rules. To be more specific: Questions arise about the nature of discourse grammar, and the nature of the interaction between sentence grammar and discourse grammar.<sup>81)</sup> In this connection it

is interesting/ . . .

is interesting to note that at least some of the work recently being done on discourse phenomena is intended to complement Chomsky's theory of sentence grammar. A case in point is Williams' (1977) work on the relation between sentence grammar and discourse grammar.<sup>82)</sup>

As pointed out in § 3.2.7.6 above, Chomsky's tolerant attitude to potential counterexamples does not entail that all counterexamples must be completely ignored. Instead, potential counterexamples must be set aside in the hope that it will become possible to explain them at some later stage. Thus, Chomsky (1979a: 188) states that "the willingness to put aside the counterexamples to a theory with some degree of explanatory force, a theory that provides a degree of insight, *and to take them up again at a higher level of understanding*, is quite simply the path of rationality" (the italics are mine). Provided that one accepts Chomsky's idealization of sentence grammar, and his claim that Coreference Assignment does not belong to sentence grammar, then the case of Coreference Assignment provides some confirmation for the fruitfulness of such an approach. Chomsky (1973) noted that Coreference Assignment constituted a potential counterexample to his conditions. At that stage Chomsky simply put this potential counterexample to his conditions aside. However, in (Chomsky 1976a), with the introduction of the idealization of sentence grammar, an explanation is provided for this case. In the words of Chomsky (1979a:188), the case of Coreference Assignment was taken up again "at a higher level of understanding".

The main points of § 3.3.5 can be summarized as follows.

- (114) a. Chomsky uses the idealization of sentence grammar to explain three potential counterexamples to the SSC and TSC.
- b. For each of the rules in question Chomsky either provides some independent justification that the

rule/ . . .

rule does not belong to sentence grammar, or he refers to works by others in which such evidence is provided.

- c. There is some evidence that the use of the idealization of sentence grammar within Chomskyan linguistics may lead to insight into the principles of discourse grammar, and into the interaction between sentence grammar and discourse grammar.
- d. The case of Coreference Assignment provides some justification for the policy of putting potential counterexamples aside in the hope that they may be explained at some further stage in the development of linguistic theory.

Footnotes/ . . .

Footnotes to chapter 3

1. Chomsky (1973:246) defines the notion 'superior' as follows:  
A category A is superior to a category B in the phrase marker if every major category dominating A dominates B as well, but not conversely. Chomsky takes N, V, A, and the categories that dominate them, to be the major categories.
2. I adopt the following convention for the use of brackets around numbers: Numbers in this study are always in round brackets. Numbers in curly brackets represent numbers in the work under discussion.
3. Chomsky (1973:230, fn. 17) leaves open the possibility that the rule relating NP - *each other* in sentences such as (3)-(7) is an interpretive rule, rather than a syntactic transformation.
4. Chomsky (1973:232) defines a language as a set of structural descriptions of sentences. A grammar is a system of rules that generates this language. In terms of these definitions, knowledge of a language is equivalent to knowledge of a grammar. Recently - cf., for example, Chomsky 1980a:90ff - Chomsky explicitly draws a distinction between knowledge of grammar and knowledge of language. Knowledge of grammar now constitutes only a subcomponent of knowledge of language. Knowledge of language is thus no longer equivalent to knowledge of grammar. Cf. also Chomsky 1981a:4 for the derivative status of the concept 'language'. The basic ideas outlined in § 3.2.3 are not affected by this change.
5. According to Chomsky (1965:25) a linguistic theory meets the condition of explanatory adequacy to the extent that it "succeeds in selecting a descriptively adequate grammar on the basis of primary linguistic data". He (1965:25-26) continues that ". . . to this extent, it offers an explanation for the intuition of the native speaker {i.e., the *linguistic*

intuition/ . . .

intuition of the native speaker - M.S.) on the basis of an empirical hypothesis concerning the innate predisposition of the child to develop a certain kind of theory to deal with the evidence presented to him". A grammar is descriptively adequate "to the extent that it correctly describes the intrinsic competence of the idealized native speaker". Cf. also "Explanatory models in linguistics" (1962:549-550) for an early statement on the importance of explanatory adequacy for linguistic theory.

Note that in *Language and responsibility* (1979a:111) Chomsky suggests that the goal of explaining language acquisition dates from his earliest work: "A third goal appeared clearly only later, at the end of the fifties (before that it was implicit): It had to do with considering the general principles of language as the properties of a biologically given system that underlies the acquisition of language".

6. It is important to keep in mind that Chomsky is concerned with the so-called "logical problem of language acquisition". Chomsky (1972b:125) formulates this problem as follows:

"The fundamental problem of linguistic theory, as I see it at least, is to account for the choice of a particular grammar, given the data available to the language-learner."

Hornstein and Lightfoot (1981b:7) provide the following characterization of this logical problem of language acquisition.

". . . it seems clear that a child must have access to something independent of experience in order for language acquisition even to get started. The question is: exactly what? This is what we call 'the logical problem of acquisition' . . ."

The logical problem of language acquisition must be distinguished from the psychological problem of language acquisition/ . . .

- tion, which is the problem of real-time acquisition. Cf., for example, Chomsky 1981b:35 for an indication of the issues that fall under the problem of real-time acquisition. Cf. also Kean 1981:196-197 for a more detailed discussion of these issues.
7. For such discussion cf., for example, Chomsky 1965:Chapter 1; 1972a, especially the second lecture; 1971:25-46; 1973:232; 1975a:Chapter 1; 1977a:2f, 18f, 62f, 164; 1978a:7f; 1980a:42f, 134f, 232f.
  8. One obvious change has to do with the way the process of language acquisition is described. Initially Chomsky talked of "learning" a language, and described the task of the language learner as that of devising a hypothesis consistent with the available data. Cf., for example, Chomsky 1965:36. Since the middle of the seventies, however, Chomsky has characterized the acquisition of knowledge of language as the *growth* of a mental organ. Cf. Chomsky 1980a:Chapter 1 for the most detailed account of this view. On the possible significance of the "learn" versus the "growth" metaphor for language acquisition, cf., for example, Chomsky 1980a:134-136. See also the change in Chomsky's concept 'language' referred to in footnote 4 above.
  9. The data is impoverished in the sense that certain properties of the acquired system cannot be found in it. Chomsky (1980d:42) distinguishes *poverty* of the stimulus from *degeneracy* of the stimulus, and stresses the more fundamental nature of the first concept for his argument.
  10. Cf. Chomsky 1980a:232 for this formulation.
  11. In works dating from the sixties the term "language acquisition device/system" is commonly used to refer to this biological endowment. Cf., for example, Chomsky 1964:26, 29; 1965:54.

12. Note that this work is in fact the text of a lecture delivered in 1976. Cf. Chomsky 1980a:217 for details.
13. Cf., for example, Chomsky 1978a:7-8 for an explication of the terms "final state of the language faculty" versus "initial state of the language faculty".
14. Cf. also Chomsky 1965:§1.9 and Chomsky 1982a:27 in this connection.
15. The condition of descriptive adequacy requires that UG make available a descriptively adequate grammar for each natural language. Cf. Chomsky 1965:24. Cf. footnote 5 above for the notion 'a descriptively adequate grammar'.
16. Cf., for example, Chomsky and Lasnik 1977:427 for a similar characterization of the nature of the conflict between explanatory and descriptive adequacy.
17. Cf., for example, the discussion of transformational rules by Chomsky in *Syntactic Structures*: Chapter 5 (1957).
18. Cf. also the remarks by Chomsky 1965:35 on the need to reduce the class of attainable grammars.
19. In commenting on these conditions, Chomsky (1972b:124-127) also claims that their introduction must be seen against this background. Cf. also the discussion by Newmeyer (1980: 175) of the introduction of these early conditions on transformations.
20. Informally, the condition of recoverability of deletion stipulates that elements may be deleted only if they are in some sense "recoverable", for example, if the deleted element is a designated representative of a category (for example, *it*, *some*, *one*, a dummy element), or if the struc-  
tural/ . . .

tural description of the transformation states that the deleted element is identical to another element of the transformed string.

21. The characterization presented by Chomsky (1976a) is in fact an informal version of the following formal characterization of the structural descriptions of transformations by Chomsky (1961:19).

"We can formulate such a notion of 'grammatical transformation' in the following way. Suppose that  $Q$  is a P-marker of the terminal string  $t$  and that  $t$  can be subdivided into successive segments  $t_1, \dots, t_n$ , in such a way that each  $t_i$  is traceable, in  $Q$ , to a node labelled  $A_i$ . We say, in such a case, that

$t$  is analyzable as  $(t_1, \dots, t_n; A_1, \dots, A_n)$   
with respect to  $Q$ .

In the simplest case, a transformation  $T$  will be specified in part by a sequence of symbols  $(A_1, \dots, A_n)$  that defines its domain by the following rule:

a string  $t$  with P-marker  $Q$  is in the domain of  $T$  if  $t$  is analyzable as  $(t_1, \dots, t_n; A_1, \dots, A_n)$  with respect to  $Q$ .

In this case, we will call  $(t_1, \dots, t_n)$  a *proper analysis* of  $t$  with respect to  $Q, T$ , and we will call  $(A_1, \dots, A_n)$  the structure index of  $T$ ."

Cf. also Fiengo and Lasnik 1976:182-184 for an explication of the formal definition presented above.

22. Newmeyer (1980:175-6) observes that the need for restrictions on the formal power of transformational rules became particularly acute around 1970, as a result of Peters and Ritchie's work on the weak generative capacity of transformational grammars. According to Newmeyer, Peters and Ritchie showed that transformational grammar, as formulated then, made only one weak claim about human language, namely that its sentences could be generated by some set of rules. However, it is not clear that Peters and Ritchie's work did

play/ . . .

play such a motivating role in Chomsky's attempt to restrict the class of available grammars through imposing restrictions on transformations. Chomsky (1977b:19, fn. 16) points out that the crucial issue is that of restricting the class of accessible grammars, and not the recursiveness of generable languages, which is the issue on which Peters and Ritchie's work primarily bears. Cf. also Chomsky 1982a: 101, where Chomsky explicitly states that in Peters and Ritchie's work on restricting generative capacity, "the vast richness of the transformational apparatus didn't play much of a role".

23. For an informal characterization of the condition of recoverability of deletion, cf. footnote 20 above. The A-over-A condition stipulates that if a transformation applies to a structure of the form  $[\alpha \dots [A \dots] \dots]$ , where  $\alpha$  is a cyclic node, the transformation must apply to the maximal phrase of the type A.
24. In fact, Chomsky (1973:234, fn. 7) also distinguishes a third approach towards solving the problem of language acquisition, namely to refine the evaluation measure. This approach is rejected by him, since it seems to him that "only limited progress" is likely on this approach. Cf. Chomsky 1965:37-47 for a discussion of the role of an evaluation measure in linguistic theory.
25. Cf., for example, Chomsky 1976a:307-308 for an explication of the way conditions on function can indirectly contribute towards restricting the class of possible rules (and grammars).
26. Cf., for example, Chomsky 1972b:126, where conditions on function are discussed, but no connection is made between such conditions and reducing the class of possible rules.

27. Cf. § 3.2.4 below for a more detailed exposition of this argumentation.
28. Cf. Chomsky 1981a:11; 1982a:112 for the finiteness of the number of possibilities permitted by current versions of UG.
29. Cf. Chomsky 1982a:112 for further discussion of the consequences which the finiteness of the number of core grammars have for the problem of language acquisition.
30. Chomsky (1981a:8) puts this as follows: "Viewed against the reality of what a particular person may have inside his head, core grammar is an idealization". Cf. also the references cited in footnote 4 above for further discussion of this, and related, matters.
31. Ross's dissertation referred to in (12) is his 1967 doctoral dissertation *Constraints on variables in syntax*. "Current Issues" is Chomsky's *Current issues in linguistic theory*, listed as (Chomsky 1964) in the references.
32. Cf. also Newmeyer 1980:179 on the importance of Ross's work on the island conditions.
33. In some of the examples discussed below I indicate more structure than Chomsky does. This is done in order to make certain points clearer. Since the "additional" structure indicated by me does not in any respect conflict with the structure assumed by Chomsky, I do not comment on it in individual cases.
34. Footnote 15 is omitted from the remarks quoted in (30).
35. For more detailed discussion of the relevant argument by Chomsky cf., for example, Chomsky 1975a:Chapter 1, and

Chomsky 1980a: Chapters 1 and 6.

36. Cf. Chomsky 1980a:36, 68-69 for the latter point.
37. *Among the earlier works, cf. for example Chomsky 1971: Chapter 1; Chomsky 1975a:Chapter 1. A recent, and very extensive, attempt can be found in Chapters 1 and 2 of (Chomsky 1980a), and also in Chomsky's Author's Response to the Open Peer Commentary on (Chomsky 1980c) in The Behavioral and Brain Sciences (Vol. 3:1980). Consider in particular the commentaries of Dennett, Hudson, Rachlin, Schank, and Stich, and Chomsky's response to them.*
38. Cf. also § 2.3.4.6 above for the methodological component of Laudan's research tradition.
39. Roughly speaking, the term "Chomskyan research tradition" and the term "Chomskyan generative grammar" used by Botha (1981) refer to the same entity.
40. In particular, Chomsky's target is those who adopt an empiricist viewpoint on language acquisition, that is, those who claim that knowledge of language is in some way inductively inferred from the data on the basis of certain general principles of learning. Such empiricists reject one of the most central ontological principles of the Chomskyan research tradition, namely the existence of a rich, and restrictive, set of innate principles as part of the human biological endowment that underlies language acquisition. This particular ontological principle is closely related to the methodological principle stipulating the validity of the argument from the poverty of the stimulus. Cf. § 3.2.5 below for some discussion.
41. It should be emphasized that Cromer's claim is not that there is evidence to support all Chomsky's claims about

innateness/ . . .

innateness. He (1980:18) states that there is more than one way to view innateness and growth. He refers to Catlin's characterization of two basic approaches to innate structures.

"In one, the preformationist view attributed to Chomsky, the various innate properties are in some sense fully formed at the beginning of development. Environmental factors play little or no role in the formation of universal grammar. Thus Chomsky takes universal grammar as a given property that influences the acquisition of particular languages. By contrast, Lenneburg's view is characterized by Catlin as 'epigenetic'; environmental influences are seen as playing a role in development as certain innate aspects unfold and interact with the environment."

Cramer (1980:18) claims that "at present there is no empirical way to judge between these two ways of viewing possible innate factors in language", a point with which Chomsky (1980c:43) agrees.

42. In this connection, cf., for example, Chomsky 1977a:65; 1980a:44; 1981a:6.
43. Botha (1981:289) formulates the principle of evidential comprehensiveness as follows:

"The larger the number of positive instances of a hypothesis, the greater the extent of the factual justification for the hypothesis."

The positive instances of a hypothesis include not only data explained by the hypothesis (as in the case under discussion) but also data which indicate the correctness of the predictions made by the hypothesis.

(Botha 1981) is a systematic and comprehensive account of the general nature and individual aspects of linguistic inquiry as it is practised within the framework of generative grammar. Frequent reference will be made to this

work for clarification of aspects of linguistic inquiry, as well as for background information on basic philosophical concepts and principles. Linguists who require such background information may find it easier to consult (Botha 1981), in which the relevant philosophical information is specifically packaged for linguists, than to consult the original philosophical works. In any case, (Botha 1981) contains extensive references to the relevant philosophical literature.

44. Cf. Botha 1981:311-312 {64} for a general principle of evidential independence in Chomskyan linguistics. Data concerning the applicability of syntactic transformations are independent from data concerning the applicability of rules of semantic interpretation in terms of {64a}, i.e., such data are about different types of linguistic units.

45. Botha (1981:289) formulates the principle of evidential independence as follows:

"The larger the variety of mutually independent types of data to which the positive instances of a hypothesis belong, the greater the extent of the factual justification for the hypothesis."

46. Cf. the discussion in § 3.2.3 for this point.

47. Thus Chomsky (1972b:197) says that "the transformation applies blindly to any phrase-marker of the proper form, caring nothing about meanings or grammatical relations". Referring to the Passive transformation, Chomsky (1973: 233) states that ". . . the semantic and grammatical relation of the main verb to the following noun phrase varies in these examples . . ., but these relations are of no concern to the transformation, which applies blindly in all cases . . .".

48. Note that Newmeyer (1980:185f) uses the term "the principle of blind application" to refer to the condition on the form of transformations that I call the "simple string condition".
49. Cf. the reference in footnote 21 above for the introduction of the simple string condition.
50. Botha (1981:365) defines counterexamples for a hypothesis as "data which show that certain projections that can be made on the basis of the hypothesis are incorrect".
51. Cf. also Botha (1981:340) for a methodological perspective on the role which this consideration plays in the justification of general-linguistic hypotheses.
52. Cf. § 2.3.2.2 above for Laudan's views on the solving of empirical problems.
53. Cf. Chomsky 1980a:40f. for some remarks on the relationship between the innateness claim and the modularity claim. While the two issues can be distinguished, opinions on them tend to "cluster". Those who assume modularity usually also assume rich innate structure, and those who assume limited innate structure usually deny modularity.
54. For further remarks by Chomsky on the issue, cf. Chomsky 1965:53; 1971:26-27, 1972a:64, 86-87, 92-93, 170, 184-185; 1975a:126.
55. The issue of the appraisal of the general principles or assumptions which guide theory choice in Chomsky's linguistics will be considered in greater detail in § 7 below.
56. Protection, as a means of reaction to criticism, involves  
the formulation/ . . .

the formulation of auxiliary hypotheses to protect the criticized hypothesis or theory from the criticism. The criticized hypothesis or theory is retained without any internal changes. In the case of criticism based on counterexamples, the auxiliary hypotheses make the criticized hypothesis or theory consistent with the data that were initially counterexamples. Cf., for example, Botha 1981: 414 for a characterization of protection as a means of reaction to criticism.

Modification, as a means of reaction to criticism, involves the reformulation of a criticized hypothesis or theory in such a way that its defects are eliminated, while the non-problematic core is retained. Cf., for example, Botha 1981:417 for a characterization of modification as a means of reaction to criticism.

57. (Bresnan 1972) is listed as (Bresnan 1979) in the references below. Note that Bresnan uses  $\bar{S}$  for Chomsky's  $S$ , and  $S$  for Chomsky's  $S'$ . In works that follow (Chomsky 1973), Chomsky takes over Bresnan's convention. That is, the base rules in question are  $\bar{S} + COMP S$  and  $S + NP AUX VP$ .
58. Cf. Botha 1981:311-312 {64} for a general principle of evidential independence.
59. Cf. Chomsky 1977c:112 for evidence that such constructions did indeed provide the justification in (Chomsky 1973) to take  $S$ , but not  $S'$ , as the cyclic node.

The Subjacency Condition restrict transformational rules to apply only within the domain of one cyclic node, or the domain of two adjacent (i.e., successive) cyclic nodes. Cf. Chomsky 1973:247.

60. Bach and Horn also mention a fourth work in which such

arguments/ . . .

arguments are presented, an article, "On interrogative word movement in English" by Bach. Since this article only appeared in 1975, it was obviously not available at the time Chomsky wrote "Conditions on transformations".

61. For additional examples that illustrate the point under discussion, cf., for example, Chomsky 1973:246 (75), (76). In his discussion of the strict Cycle Condition, Freidin (1978:521) also identifies such cases as providing the empirical justification for this condition.
62. In his overview of the development of the cycle, Newmeyer (1980:201) makes the interesting point that one would expect the cyclic principle not to have strong support in Chomsky's interpretivist model. The reason for this is that many of the arguments involve rules whose existence is denied in that model (for example, Raising-to-object). As he observes, the opposite is in fact the case. All Chomsky's conditions, and in particular the Subjacency Condition, presuppose cyclic application.

It is interesting to note that Freidin (1978) argues that the empirical effects of the Strict Cycle Condition follow from independently motivated principles, given trace theory. Cf. § 5.3 for more detail. According to Freidin, there is then no need for the notion of a cycle. Pullum (1979a:131-132) also claims that, given trace theory, the cycle is redundant in Chomsky's framework.

63. Chomsky (1973:263) points out that provision (57)/(161) cannot be assigned to case (56a)/(160a) of the SSC - i.e., where Z is not controlled at all. If it were added, the SSC could no longer block the derivation of (ii) below from (i), with Z = *it*.

(i)/ . . .

(i) It is pleasant for the rich [<sub>S</sub> COMP poor  
immigrants to do the hard work] {165a}

(ii) \*The hard work is pleasant for the rich for  
poor immigrants to do {165b}

64. It is interesting to note that Chomsky does not mention the possibility of regarding *it*-Replacement as a marked rule, a possibility that would enable him to "preserve" the Subjacency Condition even if *it*-Replacement violated it. Cf. § 4.3 below for a discussion of the role which the notion of markedness play in Chomsky's early works dealing with the SSC and TSC.

65. An explication of the content of Chomsky's notion 'complexity' - and the related notion 'simplicity' - will be undertaken in later sections.

66. Cf. Chomsky 1973:265 for details of these restrictions.

67. Cf. Chomsky 1973:265 for details of these observations.

68. Cf. Chomsky 1973:264, footnote 43 for a brief discussion of the operation performed by PRO-Replacement.

69. According to Emonds (1970:29), obligatory nodes must be present in deep structure, and they must be non-empty at some point in a transformational derivation. Optional nodes need not be present in deep structure.

70. Chomsky's explanation goes more or less like this: The trace left by a movement rule is either PRO or \*. If the latter, the sentence will be blocked as ungrammatical unless the position with \* is filled by some subsequent rule. Assume that in simple N-V-N sentences the subject position is filled by a full NP in the underlying structure. If

this subject/ . . .

this subject NP is moved, a trace \* remains in the subject position. NP-Preposing must then apply, otherwise the sentence will be blocked as ungrammatical. Since NPs have no obligatory subject position, there is no such need for NP-Preposing to apply.

71. Apart from (Chomsky 1975a), works in which independent justification is presented for trace theory include *Anaphoric relations in English* (Wasow 1972), *Semantic conditions on surface structure* (Fiengo 1974), "On trace theory" (Fiengo 1977), "Trace theory and twice-moved NPs" (Lightfoot 1976). It must be noted that trace theory has also been severely criticized. Cf. Newmeyer 1980:235 for a brief overview of works in which trace theory is criticized.
72. According to Chomsky (1976a:319), the rule of Reciprocal Interpretation assigns an appropriate sense to sentences of the form NP ... *each other*.
73. Note that the distinction between a single violation and a double violation of conditions on rules is also used in other cases by Chomsky. Cf. for example his (1981a:158-159) discussion of *wh*-Movement.
74. This constraint - proposed by Ross (1967) - prohibits the movement of material out of coordinate structures.
75. Cf. § 2.4 above for an explication of the content of this style, and for some of the questions which arise from Chomsky's comments on the style.
76. Cf. § 3.2.4 above for more detail. Note that, as in the case of many of the other rules discussed by him, Chomsky (1973) leaves open the question of whether the rule must be formulated as a movement transformation or a rule of semantic interpretation.

77. In (104) and (105)  $X^n$  stands for the categories NP, VP, AP, and  $t$  is the trace of the quantifier.
78. Cf. § 3.5 below for more detail on the role which the notion "sentence grammar" plays in the protection of the SSC and TSC from potential counterevidence.
79. The Complex NP Constraint, the Coordinate Structure Constraint and the Sentential Subject Constraint were all proposed by Ross (1967). The Coordinate Structure Constraint stipulates that no conjunct in a coordinate structure, or any element in a conjunct, may be moved from this coordinate structure. The Complex NP Constraint stipulates that no element may be extracted from a sentence dominated by a noun phrase with a lexical head. The Sentential Subject Constraint prohibits the extraction of any element from the sentential subject of a sentence.
80. Chomsky (1967a:322) claims that this would not be an "un-tolerable" consequence. Given the option of regarding rule-specific conditions as parameters to be fixed for rules during language-learning, it might still be possible to restrict the formal power of transformations. See in this connection the discussion in Chomsky 1967a:315.
81. It is not quite clear whether knowledge of discourse falls within the domain of pragmatic competence identified by Chomsky (1980a:59, 224-225). According to Chomsky, "pragmatic competence may include what Paul Grice has called a 'logic of conversation'. We might say that pragmatic competence places language in the institutional setting of its use, relating intentions and purposes to the linguistic means at hand".
82. Williams (1977:102) defines 'rules of discourse grammar' as "rules whose relevant terms, such as deletion site and

antecedent/ . . .

antecedent, are not in general contained within a single sentence". Such rules "specify the relationship of a sentence to its linguistic context - that is, its relationship to other sentences in a discourse". As regards the relation between sentence grammar and discourse grammar, Williams' main claim is that the rules of discourse grammar follow all rules of sentence grammar, including those that derive logical form.

## Chapter 4

## CONDITIONS WHICH RESTRICT THE APPLICABILITY OF RULES OF SEMANTIC INTERPRETATION ONLY

4.1 General remarks

The proposal that the SSC and TSC restrict the applicability of interpretive rules only (rather than interpretive *and* transformational rules), is first made in (Chomsky 1976a). In "On *wh*-Movement" (henceforth (Chomsky 1977c)) the proposed reinterpretation of the two conditions is actually adopted. This reinterpretation of the SSC and TSC is analyzed in § 4.2. In §§ 4.3 - 4.7 various other aspects of the conditions dealt with in (Chomsky 1977c)<sup>1)</sup> are considered.

From (Chomsky 1977c) onwards, Chomsky uses the term "Propositional Island Condition"/PIC to refer to the TSC. His example is followed in the discussion below.

4.2 The reinterpretation of the SSC and TSC as conditions that restrict interpretive rules only

Chomsky (1976a:314) distinguishes two general approaches to the problem of overgeneration which results from the radical reduction in the expressive power of transformations proposed by him.<sup>2)</sup>

- (1) "There are two general approaches to the problem of overgeneration in such cases as these: we may try to impose (I) conditions on the application of rules or (II) conditions on the output of rules, i.e., on surface structures. The latter will generally be related to rules of semantic interpretation that determine LF, under the assumptions of EST. As we will see, (I) and (II) may fall together."

The two approaches distinguished above can be illustrated with the aid of the sentences in (2). Both sentences are derived by the rule "Move NP". In both cases *t* is the trace of *John*.

(2) / . . . .

- (2) a. John seems [ t to like Bill ] (10d)  
 b. \*John seems [ Bill to like t ] (10c)

(2b) is blocked by the SSC. Chomsky (1976a:319) points out that this can be interpreted in two ways. Assume that (2b) is derived by NP-Movement from the underlying structure "X seems [Bill to like John]", with X some kind of place-holder for NP. On one interpretation, the SSC prevents the NP-Movement rule from preposing *John* in "X seems [Bill to like John]" to derive (2b). On this interpretation, the ill-formed structure (2b) - which could only result if the SSC were ignored - is not generated at all. This interpretation represents the first general approach distinguished in (2), and is the interpretation adopted in the works discussed in § 3 above.

On the second interpretation, the SSC is regarded as a condition on surface structure interpretation, but not on the applicability of transformations. The NP-Movement rule applies freely, giving both (2a) and (2b). The SSC must then filter out the ill-formed structure in some way. This can be done if the relation between an NP and its trace is regarded as a special case of bound anaphora. The SSC will then block the rule of bound anaphora in just those cases where movement would lead to an antecedent-anaphor relation which violates the SSC. The SSC will thus filter out (2b), but not (2a). This interpretation represents the second approach distinguished in (1) above.

The question of the reinterpretation of the SSC and PIC is taken up again in (Chomsky 1977c). He (1977c:74) formulates the SSC and PIC as follows.

- (3) "The conditions (4) and (5) (PIC and SSC) refer to structures of the form (11), where  $\alpha$  is a cyclic node:

(11) ... X ... [  $\alpha$  ... Y ... ] ... X ...

As in the case of subjacency, I will take  $\bar{S}$  and NP to be the cyclic nodes, delaying the discussion of other choices

until/ . . .

until later. PIC (the 'tensed-S condition' of the references cited) asserts that no rule can 'involve' X and Y where  $\alpha$  is a finite clause (tensed-S). SSC asserts that no rule can 'involve' X and Y where  $\alpha$  contains a specified subject, i.e., a subject not containing Y and not controlled by X (I modify an earlier formulation here; I assume that Y contains Y). If  $\alpha$  contains a subject, then only the subject is accessible to rule, if the subject is specified in the defined sense."

Chomsky (1977c:75) provides the following explication of the notion 'involve'.

- (4) "We now say that a transformational rule *involves* X and Y when it moves a phrase from position X to position Y and a rule of construal *involves* X and Y when it assigns Y the feature  $[\pm \text{anaphoric to } i]$ , where X has the index  $i$  (or conversely, in both cases)."

In the case of transformational rules, the notion 'involve' is now restricted to movement transformations. In the case of interpretive rules, the notion 'involve' is now restricted to rules of construal. Suppose that the relation between a moved phrase and its trace is regarded as one of bound anaphora. It then becomes possible to provide a principled explanation for the fact that certain rules are not permissible. If the relation between a moved phrase and its trace is that of bound anaphora, it follows that any movement rule which would lead to a violation of one of the conditions on bound anaphora will be excluded. For instance, any downgrading rule which would lead to a violation of the requirement that an antecedent is superior to its anaphor, will be excluded.<sup>3)</sup>

Chomsky (1977c:76) notes that if the relation between a moved phrase and its trace is regarded as that of bound anaphora, it also becomes possible to unify the two cases of involvement defined in (4) above. The notion 'involved in' as defined for rules of construal can be extended to movement rules by permitting the latter to apply freely, and applying the conditions to the moved phrase (the antecedent) and its trace (the anaphor). The

SSC and PIC are then, in effect, interpreted as applying to transformational rules as filters. The result of applying a transformational rule may or may not yield an appropriate case of bound anaphora.

No clear choice between the two possible interpretations of the SSC and PIC is made in (Chomsky 1976a). Chomsky (1976a:320) states that "in principle, the two interpretations of SSC have distinct empirical consequences, but the issue is complex and it is not easy to sort out consequences". Chomsky (1976a:fn. 32) briefly refers to a case discussed by Fiengo and Lasnik (1973), which suggests that the SSC must also constrain transformational rules. He does not, however, discuss the matter. Note that although in the remarks quoted above Chomsky only mentions the possible reinterpretation of the SSC, he clearly has in mind the reinterpretation of the PIC/TSC as well. Thus, in his (1976a:317) reference to the possible reinterpretation, he includes the latter condition.

Chomsky (1977c) unambiguously opts for the interpretation of the SSC and PIC as applying to rules of construal only, that is, as conditions on well-formed surface structures. As in (Chomsky 1976a), no evidence is presented that the empirical consequences of this interpretation of the conditions are better than the consequences of the alternative interpretation. The question then arises why, in the apparent absence of any clear empirical evidence supporting the reinterpretation, Chomsky would want to reinterpret the SSC and PIC as conditions on surface structure. Chomsky offers two considerations which, in his view, provide some support for this reinterpretation. Firstly, such a reinterpretation allows a partial unification of conditions on transformational rules and conditions on rules of construal. Secondly, this reinterpretation makes it possible to uphold a stronger condition of autonomy of syntax than would otherwise be the case. Both these considerations are conceptual, in terms of the empirical-conceptual distinction adopted in § 2.3.4.1.

By/ . . .

By reinterpreting the SSC and PIC as conditions on surface structure, Chomsky in effect manages to partially collapse conditions on transformational rules and rules of semantic interpretation.<sup>4</sup> Sentences such as (5) are then ruled out at the same level of representation, and for the same reason.

- (5) a. \*Bill seems [John to like t] (t = trace of *Bill*)  
 b. \*Bill expected [Mary to like himself]  
 c. \*Bill expected [Mary to find his way home]<sup>5)</sup>

In several of his recent works, Chomsky stressed the importance of unifiedness as a desirable metascientific property or linguistic theory. Consider, for instance, (Chomsky 1978a:16, 24; 1978b:15; 1980b:1; 1981a:338-339; 1981b:48, 50, 60). It is not easy to determine exactly what Chomsky understands under unifiedness as a metascientific property of linguistic theories. However, consideration of the change under discussion, as well as some changes that will be analyzed below, indicates that a characterization of this metascientific notion must at least cover the following case. A theory  $T_{x+1}$  is more unified than a theory  $T_x$  if a principle that must be stipulated in  $T_x$  can in  $T_{x+1}$  be derived from (an) independently required principle(s). This formulation presupposes that the notion of 'deductive depth' - that is, the distance between theory and primary data - is an essential component of Chomsky's notion of theoretical unifiedness. The remarks by Chomsky (1978a:16; 1981b:48, 50), in which he explicitly links unifiedness of a theory with its degree of deductive depth, provide some support for this analysis.

Consider now the reinterpretation of the SSC and PIC. Let  $T_x$  be the version of UG which incorporates the SSC and PIC as conditions that restrict both syntactic transformations and rules of semantic interpretation, and  $T_{x+1}$  the version which incorporates the reinterpreted conditions. In  $T_x$  it must be stipulated - in the definition of 'involve' - that the SSC and PIC constrain transformational rules, in addition to rules of semantic inter-

pretation/ . . .

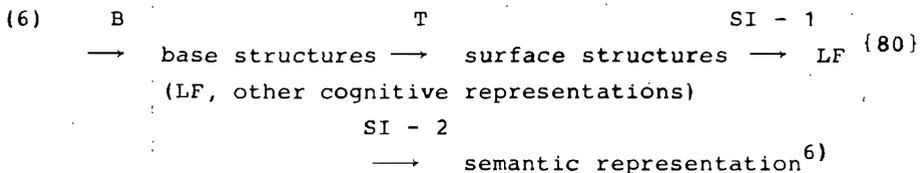
pretation. In  $T_{x+1}$  no stipulation regarding the effect of the SSC and PIC on transformational rules is required. Instead, given trace theory, the effect of the conditions on transformational rules follows from independently required constraints on anaphora. In terms of the norm formulated above,  $T_{x+1}$  is then more unified than  $T_x$ . The crucial difference between  $T_x$  and  $T_{x+1}$  can also be formulated as follows: In  $T_{x+1}$ , but not in  $T_x$ , there is an answer to the question of why the SSC and PIC affect both transformational rules and rules of semantic interpretation. Note that the change under discussion also involves the elimination of a clause in the definition of 'involve'. In  $T_x$  this definition must have two clauses: one defining 'involve' in the case of transformational rules, and one defining 'involve' in the case of rules of construal. In  $T_{x+1}$  only the latter clause is required. The link between Chomsky's metascientific notion 'unifiedness', the notion 'deductive depth', and this type of simplification is considered in more detail in § 7.2.2.2 below.

Let us now briefly consider why the consideration of greater theoretical unifiedness must be regarded as being conceptual, in terms of the empirical-conceptual distinction adopted in § 2.3.4.1 above. From his (1981a:14-15, 338-339) discussion of unifiedness as a metascientific property of linguistic theories, it is clear that for Chomsky the desirability of theoretical unifiedness follows from a tentative assumption made by him about the nature of the world at which linguistic theory is directed. Specifically, he tentatively adopts the assumption that neural structure in the domain of the language faculty constitutes a simple and unified system. An implicit assumption with which Chomsky operates is that unifiedness as a metascientific property of a linguistic theory directly reflects unifiedness in the linguistic reality. To the extent that a specific theory of the language faculty fails to exhibit unifiedness, such a theory is in tension with the relevant assumption about the nature of the world that must be described by the theory. Any change in the linguistic theory which would increase its unifiedness would reduce this tension.

This/ . . .

This brief exposition of the notion 'theoretical unifiedness' employed by Chomsky lends plausibility to the claim that the consideration of increased unifiedness of linguistic theories is indeed conceptual. In § 6.3 below additional textual evidence from Chomsky's recent works which supports this view will be presented. Admittedly, the discussion above gives rise to many questions regarding Chomsky's views on the desirability of unifiedness as a metatheoretical property of linguistic theories. A more detailed, and critical, discussion of Chomsky's views on this issue must be postponed until chapter 7.

While the consideration of increased theoretical unifiedness is conceptual in nature, the specific unification under discussion also has an empirical aspect. According to Chomsky (1976a:345), the fact that some conditions on transformations - specifically the SSC and PIC - can be assimilated to conditions on rules of semantic interpretation, provides some indirect support for the Extended Standard Theory/EST, in general, and trace theory, in particular. Chomsky (1976a:336) proposes the following general structure for the EST.



Chomsky (1976a:345) notes that, given that surface structure determines LF, one would expect principles of semantic interpretation (at least those of SI - 1) to be related closely to conditions on surface structure. The fact that this expectation is fulfilled in the case of the SSC and PIC thus provides some indirect support for the EST, in general, and trace theory, in particular.

Let us now consider the second consideration used by Chomsky to

justify/ . . .

justify the reinterpretation of the SSC and PIC, namely that such a reinterpretation makes it possible to uphold a stronger version of the thesis of the autonomy of syntax. Under the reinterpretation the semantic conditions that enter into the SSC - i.e., the notion of 'control' in the definition of 'specified subject' - no longer affects the applicability of transformational rules. Instead, they affect the applicability of certain rules of semantic interpretation only. Chomsky (1975b:92) defines the absolute autonomy thesis as follows:

- (7) "the absolute autonomy thesis implies that the formal conditions on 'possible grammars' and a formal property of 'optimality' are so narrow and restrictive that a formal grammar can in principle be selected (and its structure generated) on the basis of a preliminary analysis of data in terms of formal primitives excluding the core notions of semantics, and that the systematic connections between formal grammar and semantics are determined on the basis of this independently selected system and the analysis of data in terms of the full range of semantic primitives."

In fact, Chomsky (1975b) argues for a weaker version of the autonomy thesis, the so-called *parameterized* autonomy thesis. That is, even though the theory of linguistic form may have significant internal structure, it will be constructed with semantic parameters. The actual choice of formal grammar will then be determined by fixing these parameters. As Chomsky (1975b:92) puts it, "the significant question with regard to the autonomy thesis may not be a question of 'yes' or 'no', but rather of 'more' or 'less', or more correctly, 'where' and 'how much'".

The parameterized autonomy thesis is in principle compatible with semantic considerations - control properties in the case of the SSC - affecting the applicability of syntactic rules. Also, the autonomy thesis (absolute or parameterized) is a hypothesis about specific grammars, and not about universal grammar (UG). Chomsky (1975b:96) explicitly states that where a property of universal grammar is involved, "the matter is irrelevant to the absolute autonomy thesis". This obviously also holds for the

parameterized/ . . .

parameterized autonomy thesis. In his discussion of the semantic component of the SSC, Lightfoot (1976:570) also makes these points - that is, that the semantic content of the SSC is in any event compatible with the parameterized autonomy thesis, and that the autonomy thesis bears on specific grammars, rather than UG. Nevertheless, Lightfoot (1976:570) says that "we could construct a plausibility argument for some version of the autonomy thesis, if we could show that syntactic rules are subject only to the syntactic aspect of the SSC and that only semantic rules are subject to the notion of control". Chomsky achieves the same result as Lightfoot - viz. the possibility of a stronger version of the autonomy thesis, which presumably rules out semantic conditions on syntactic rules - by changing his theory so that only semantic rules are constrained by the SSC.

Like the first consideration discussed above, this second consideration in terms of which Chomsky (1977c) justifies the reinterpretation of the SSC and PIC is conceptual in nature. It bears on the relation between a specific theory - UG - and a general assumption of Chomsky's linguistics - the autonomy thesis. By changing UG so that the semantic condition in the SSC no longer restricts the application of syntactic transformations, the joint plausibility of this UG and the autonomy thesis is increased.

Note that the consideration of a stronger version of the autonomy thesis applies only to the SSC. The PIC, as formulated above, has no semantic content. Nevertheless, Chomsky's proposal is that the PIC must also be restricted to semantic rules. Presumably Chomsky's extension of the proposed reinterpretation to the PIC is based on two considerations. On the one hand, this increases the unity of the theory. On the other hand, the consideration concerning the desirability of collapsing conditions on syntactic transformations and rules of semantic interpretation holds for both the SSC and the PIC.

In his/ . . .

In his recent works Chomsky uses the term "syntax" in a wide sense, to refer to the computational component of the language faculty. In this use, syntax includes not only the traditional syntactic rules - base rules and transformational rules - but also the rules that map S-structures onto phonological representations and logical forms. Since the rule of control also belongs to syntax under this sense, it might be argued that the consideration of strengthening the autonomy thesis of syntax is quite irrelevant to the proposed reinterpretation of the SSC and PIC. However, one must distinguish between two autonomy theses.<sup>7)</sup> The first thesis - which I will call *the thesis of external autonomy* - asserts the existence of syntax, in the general sense, as an autonomous subsystem of mind. The second thesis - *the thesis of internal autonomy* - asserts the autonomy of the various subsystems of syntax (in the general sense) with respect to one another. This second autonomy thesis is the classical autonomy thesis discussed in, for example, (Chomsky 1975b). It is obviously the thesis of internal autonomy that bears on the proposed reformulation of the SSC and PIC. In particular, the issue involved is the autonomy of the subcomponent deriving S-structure with respect to the subcomponent that maps S-structures onto logical forms.

The main points made above are summarized in (10).

- (10) a. Given that the relation between a moved phrase and its trace is regarded as that of bound anaphora, it becomes possible to reinterpret the SSC and PIC as conditions that restrict rules of semantic interpretation only, specifically, rules of construal.
- b. The first consideration used by Chomsky to justify the choice of  $T_{x+1}$  - the version of UG incorporating the reinterpreted conditions - over the earlier version  $T_x$  is that the reinterpretation of the conditions leads to a unification in the theory. Specifically,

the reinterpretation/ . . .

the reinterpretation allows a partial unification of syntactic movement transformations and rules of construal.

- c. The consideration of increased theoretical unifiedness of linguistic theory is conceptual, in that it bears on the relation between a specific linguistic theory and a general assumption made by Chomsky about the nature of the world that must be described by this theory.
- d. The second consideration used by Chomsky to justify the choice of  $T_{x+1}$  over  $T_x$  is that the reinterpretation of the conditions makes it possible to adopt a stronger version of the thesis of autonomy of syntax.
- e. The consideration of strengthening the thesis of autonomy of syntax is conceptual in nature, in that it bears on the relation between a specific linguistic theory and a general assumption made by Chomsky about the autonomy of the various subcomponents of the language faculty with respect to one another.
- f. While Chomsky (1977c) does not provide empirical justification for the proposed reinterpretation of the SSC and PIC, the fact that this reinterpretation is possible provides some indirect evidence for the EST, in general, and trace theory, in particular.

#### 4.3 The SSC and PIC as part of core grammar

Two important points briefly mentioned in (Chomsky 1973) are taken up again in (Chomsky 1977c), where they are worked out in more detail. The first is that of the relative interpretation of conditions on rules. The second is the presence of language specific parameters in general conditions on rules.<sup>8)</sup>

Under/ . . .

Under the relative interpretation of conditions, a condition does not impose an absolute restriction on rules of a certain type. Rather, rules are taken to obey the condition unless otherwise specified. Such a specification would lead to the rules being marked, in contrast to the unmarked rules that obey the condition. Thus, as Chomsky (1977c:76) puts it, ". . . the conditions become an integral part of an evaluation measure, rather than imposing absolute restrictions".

Chomsky (1977c:77) illustrates these general points about the relative interpretation of conditions on rules with reference to a French rule that must handle "the peripheral *Tous*-Movement phenomena" of Kayne (1975:63-64). Kayne argues for a general rule *L-Tous* that moves quantifiers to the left. Generally, this rule observes the SSC and PIC (and Subjacency). However, there are cases which apparently involve a violation of the PIC.

- (11) a. il faut toutes [qu'elles s'en aillent] {14a}  
 b. il faut tous [qu'on se tire] {14b}

In (11), the quantifier *tous* is in each case construed with a pronoun that is within a tensed S. For reasons noted by Kayne, the *L-Tous* rule cannot be modified so as to derive (11). Chomsky proposes that the sentences of (11) are derived by a second rule with the structural description (12).

- (12) (*vbl*, *V\**, *Q*, *que*,  $\alpha$ , *PRO*, *vbl*) {15}

In (12) *Q* is construed with *PRO*. *V\** represents a certain class of verbs, including *falloir*, *vouloir*, *Q* is a quantifier, and  $\alpha$  is either null or a "sufficiently short" NP. In (12) the antecedent - *Q* - is not adjacent to the anaphor - *PRO* (or trace, if the rule in question is a movement rule).<sup>9)</sup> Two terms that are not variables intervene between *Q* and *PRO*, namely *que* and  $\alpha$ . Chomsky (1977c:76) says the following about rules with structural descriptions such as (12), in which the antecedent and the anaphor are not adjacent.

- (13) "Let us say that the antecedent and the anaphor are *involved* in the rule if they are adjacent; otherwise not. Specification of constant terms intervening between antecedent and anaphor will then make the conditions inapplicable, at a cost, in accordance with the logic of markedness."

The derivation of (11), accordingly, does not involve a violation of the PIC. The rule (12) which is responsible for such cases is a marked rule, its marked status being the result of its complexity, according to Chomsky. This then illustrates how, under the relative interpretation of conditions on rules, the rules of a specific language can differ with respect to a general - i.e., universal - condition on rules.

Chomsky (1977c:75) also rejects the view that conditions on rules must be invariant. Instead, he suggests that the conditions may vary "within fixed limits". As regards the PIC, Chomsky refers to observations by Kim that the rules of anaphora in Korean meet a condition similar to the PIC, but with a somewhat different condition on  $\alpha$ .<sup>10)</sup> In Korean there is no formal distinction between tensed and tenseless clauses. There is, however, a category of embedded clauses that are not islands, as is the case with the infinitival clauses of English and the Romance languages. These "non-islands" in Korean are the complements of a certain class of "assertive" verbs. Chomsky notes that these verbs are very close in meaning to the verbs that in English take infinitival complements. Chomsky then suggests that a variant of the PIC can be formulated for Korean, with a different condition on  $\alpha$ . A more abstract formulation of the PIC can then be provided, with the English and Korean versions of the conditions as special cases. Notice, however, that Chomsky does not make any specific proposals concerning the value of  $\alpha$  in Korean, or the more abstract formulation of the PIC. In fact, he (1977c:75) says that "in the absence of more extensive work on rule systems in other languages, I am reluctant to suggest anything further". The possibility that  $\alpha$  in the PIC/TSC might have different values for different languages, was, of course,

already/ . . .

already mentioned by Chomsky (1973:238, fn. 16), when he suggested that  $\alpha$  in the TSC might be a language-specific parameter.

In the case of the SSC, the application of this condition in a particular language depends on the characterization of the notion 'subject' in this language. While languages such as English and French seem to require a formal definition of 'subject', some case languages may require a characterization in terms of such notions as 'ergative', 'absolutive', or 'non-oblique'. Following a suggestion by Hale, that there are certain conditions on what can be taken as subject in the syntactically unmarked situation, Chomsky proposes that a language might characterize the notion 'subject' differently, "but at a cost in the grammar, in accordance with the logic of markedness".

Chomsky (1977c) also discusses the possibility of parametric variation in the class of cyclic nodes in the SSC and PIC. Specifically, Chomsky (1977c:111f.) considers the effect that it would have on the SSC and PIC (and Subjacency) if S, in addition to NP and  $\bar{S}$ , were to be regarded as a cyclic node. The PIC would only require a slight reformulation in order to ensure that movement from within a tensed S to the COMP position of the immediately dominating  $\bar{S}$  is not blocked. As far as the effect on the SSC is concerned, Chomsky (1977c:111) suggests that it would be in order to take S as a cyclic node in those languages in which there are many rules to which only subjects are accessible. If S is a cyclic node, then in a structure of the form  $[ \dots X \dots [ {}_S \dots Y \dots ] \dots X \dots ]$  X and Y cannot be related by a rule, if S contains a subject not containing Y and not controlled by X. That is, under this formulation of the SSC only subjects are accessible to movement rules involving an element outside of S. For instance, *wh*-Movement will be able to move an NP (= Y) to the COMP position (= X) only if Y is the subject of S. Chomsky claims that "it is well known that in many languages only subjects are accessible to many rules". The cyclic category in the SSC thus constitutes another parameter in

terms of/ . . .

terms of which a universal condition can vary across languages.

Chomsky (1977c:75-6) sums up his position on the status of conditions such as the SSC and PIC by saying that he "would prefer to think of the conditions cited as instances of condition-schemata, part of the core grammar of English, pending further relevant work on rule systems that may provide evidence bearing on their viability and the more general formulation of the relevant schemata". The core grammar of English, according to Chomsky (1977c:72-73), includes two transformational rules ("Move NP", "Move *wh*-phrase"), three interpretive rules (the Reciprocal Rule, the rule of Bound Anaphora, the rule of Disjoint Reference) and three conditions on rules (the Strict Cycle condition, with the Subjacency Condition as part of the definition of the cycle, the PIC, and the SSC). Chomsky's (1977c) views on the issue of the relative interpretation of conditions on rules, and the issue of language-specific parameters in conditions, constitute the essence of the theory of core grammar, which forms such an integral part of current Chomskyan linguistic theory. (Chomsky 1977c) contains very few explicit remarks on the nature of core grammar. Consider, however, the following remarks from a publication that dates from the same year, namely (Chomsky and Lasnik 1977:430).

- (14) "We will assume that UG is not an 'undifferentiated' system, but rather incorporates something analogous to a 'theory of markedness'. Specifically, there is a theory of core grammar with highly restricted options, limited expressive power, and a few parameters. Systems that fall within core grammar constitute 'the unmarked case'; we may think of them as optimal in terms of the evaluation metric. An actual language is determined by fixing the parameters of core grammar and then adding rules or rule conditions, using much richer resources, . . . ."

Rules which belong to the core grammar of a language are unmarked. Rules which belong to the non-core (or periphery) are marked. The rule of peripheral *L-Tous* Movement discussed

above/ . . .

above is an example of such a marked rule. The value of  $\alpha$  in the PIC is an example of a parameter that must be fixed for each language.

In works which follow (Chomsky 1977c) and (Chomsky and Lasnik 1977), the notions 'core grammar' and 'markedness' play an increasingly important role in Chomsky's work. In some of these later works Chomsky also elaborates on the content of these notions. A comprehensive account of the role which the related notions 'core grammar' and 'markedness' play in Chomsky's linguistics must be postponed until these works have been analyzed.<sup>11)</sup> However, there are two aspects of the notion 'core grammar' that require clarification at this point: (i) core grammar and Chomsky's handling of the conflict between descriptive adequacy and explanatory adequacy, and (ii) core grammar as an idealization, analogous to the idealization of sentence grammar.

The development of the theory of core grammar should be seen as an attempt by Chomsky to overcome a well-known dilemma for linguistics,<sup>12)</sup> namely, that of developing a UG which is sufficiently rich and highly structured to allow the selection of descriptively adequate grammars, and which at the same time is sufficiently open to allow for the variety of languages.<sup>13)</sup> Given the notion 'core grammar', a highly restrictive theory of UG can be proposed. This theory of UG will define only a limited number of core grammars. The possibility of parametric variation, and the possibility of adding marked rules to the core, enable such a restrictive UG to account (at least in principle) for the variety of languages. The SSC and PIC play an important role in the development of the theory of core grammar, in that they (and the conditions that will replace them) form an integral part of the core grammars defined by UG.

The sense in which core grammar constitutes an idealization is outlined by, for example, Chomsky (1981a:7-8). Core grammar departs in two respects from "what a particular person may have

inside his head". First, core grammar abstracts away from the effects of the heterogeneous character of actual experience in real speech communities. Second, core grammar abstracts away from the "periphery of borrowings, historical residues, inventions, and so on" incorporated in each actual language. Consequently, it is not to be expected that the systems called "languages" conform precisely, or even closely, to the core grammars defined by UG.<sup>14)</sup> In several recent works Chomsky emphasizes that knowledge of grammar constitutes only part of knowledge of language. Knowledge of language also incorporates what Chomsky (1981a:55) calls a "conceptual system" - comprising knowledge of object reference, relations such as 'agent', 'goal', 'instrument' - and pragmatic competence.<sup>15)</sup> In fact, Chomsky (1981a:90; 1982a:107-108) suggests that the concept 'language' may actually be an uninteresting and useless concept, and that 'grammar' is the fundamental notion.<sup>16)</sup>

Chomsky's views on the nature of the relation between grammar and language set out above differs from his views in, for example, (Chomsky 1973). In the latter work, knowledge of grammar is seen as equivalent to knowledge of language. One consequence of the change in Chomsky's views on the nature of the relation between grammar and language, is that the domain of facts to be accounted for by a theory of grammar is now smaller than before. Given that a UG is a theory of core grammar, many facts previously considered relevant to the formulation of a UG are now irrelevant. How such a restriction in the domain of UG fits in with Chomsky's views on the aim of linguistic inquiry will be considered in § 7.2.3.5 below. At this point it is only necessary to point out that idealizations, like the idealization of core grammar, are seen by Chomsky as a tool in making progress towards depth of understanding.<sup>17)</sup>

During the brief discussion of the 'Galilean style' in § 2.4 above, it was noted that Chomsky's use of abstractions and idealizations in defining the scope of a theory is complemented

by a/ . . .

by a tolerant attitude to apparently negative evidence. This is obviously also true for the idealization of core grammar. The adoption of the latter idealization leads to a considerable complication of the relation between a UG and linguistic data, thus warranting a tolerant attitude to apparently negative evidence.<sup>18)</sup> In order to determine whether a specific datum is relevant to a UG, it must be determined whether the datum bears on an unmarked aspect of language, or on a marked aspect. If the former, then the datum falls within the scope of a UG. If the latter, the datum falls outside the scope of a UG. Only in the former case can the datum constitute negative evidence for UG. Specific instances in which Chomsky makes use of the idealization of core grammar to protect his theory from potential counterevidence will be considered below.<sup>19)</sup>

The main points of the discussion above can be summarized as follows.

- (15) a. Chomsky's (1977c) views on the issues of the relative interpretation of rules and of language particular parameters in conditions on rules constitute the essence of his theory of core grammar.
- b. Given that UG is a theory of core grammar, the development of the notion 'core grammar' forms part of the attempt to reconcile the conflict between descriptive adequacy and explanatory adequacy.
- c. The adoption of the notion 'core grammar' constitutes another basic idealization in Chomsky's linguistics, analogous to, for example, the idealization of the ideal speaker-hearer, the homogeneous speech community, instantaneous language acquisition, sentence grammar.
- d. Like the other idealizations adopted in Chomsky's linguistics, the idealization of core grammar is

complemented by a tolerant attitude to potential negative evidence.

- e. The adoption of the notion 'core grammar' has contributed to a change in Chomsky's views on the relation between 'language' and 'grammar'. One effect of this change is that the notion 'language' is no longer regarded as fundamental, or even useful.

#### 4.4 Chomsky's handling of potential counterexamples to the SSC and PIC

##### 4.4.1 General remarks

Several potential counterexamples to the SSC and PIC are discussed in (Chomsky 1977c). In § 4.4 the nature of each such counterexample is briefly outlined, and Chomsky's handling of it analyzed. One of the problematic cases considered by Chomsky (1977c) is Quantifier Movement/Quantifier Construal. In order to accommodate this rule, Chomsky (1977c:78) considers extending the notion of 'involvement' "to relate also adjacent constant terms, one of which is either antecedent or anaphor and the other a constant category of the X-bar system". The details of this case were discussed in § 3.3.4 above, and will not be repeated here.

##### 4.4.2 The idealization of sentence grammar again

A detailed discussion of the idealization of sentence grammar, and particularly of its role in Chomsky's handling of potential counterevidence to the SSC and PIC, is presented in § 3.3.5 above. Chomsky (1977c:81) also makes use of this idealization to accommodate a potential counterexample to the conditions. The rule in question is VP-deletion. In sentences such as the following VP-deletion applies, in apparent violation of the SSC and PIC.

(16) that John didn't hit a home run is not surprising,  
but that Bill knows that John didn't - is a  
real shock.

(26c)

Following Sag and Hankamer (1976), Chomsky points out that VP-deletion "can apply across speakers in discourses". Consequently, VP-deletion is not a rule of sentence grammar, and not subject to the principles of sentence grammar. Given the idealization of sentence grammar, VP-deletion thus falls outside the scope of conditions such as the SSC and PIC.

The role which the idealization of sentence grammar plays in Chomsky's handling of VP-deletion is identical to the role played by this idealization in his handling of the rules discussed in § 3.3.5 above. All the general points made in that section about this idealization carry over without modification to the present case. In sum, then:

- (17) a. Chomsky (1977c) uses the idealization of sentence grammar to explain a potential counterexample to the SSC and PIC, namely VP-Deletion.
- b. His handling of the problem posed by VP-deletion illustrates his willingness to make his conditions work in the face of empirical problems.

#### 4.4.3 Restricting the conditions to rules of construal

It has already been pointed out - see § 4.2 above - that in terms of the definition (4) the SSC and PIC constrain a subclass of interpretive rules only: the rules of construal. This contrasts with the position adopted in (Chomsky 1973), where it was implicitly assumed that all interpretive rules are constrained by the conditions. This restriction in the definition of 'involve' enables Chomsky to deal with what would otherwise have constituted counterexamples for the SSC and PIC.

For instance/ . . .

For instance, Chomsky (1973:242) previously claimed that the interpretive rule associating *not* . . . *many* in sentences such as (18) below, giving the meaning "few", was subject to the SSC.

(18) we didn't see pictures of many of the children {210a} of  
(Chomsky  
1977c)

The SSC would thus block the association of *not* and *many* in (19), because of the presence of the specified subject *John*.

(19) \*we didn't see John's pictures of many of the children (\* on the relevant interpretation) {210b} of  
(Chomsky  
1977c)

Chomsky (1977c:116) provides the following example in which the rule associating *not* and *many* violates both the SSC and PIC.

(20) we didn't believe that Bill had seen pictures of many of the children {211}

Chomsky (1977c:116) claims that there is no reason to suppose that the rule associating *not* and *many* is a rule of construal. Consequently, its application in cases such as (20) does not present any problem to the SSC and PIC. Chomsky (1977c:116) suggests that the unacceptability of (19) follows from quite a different principle: *not* and *many* cannot be associated when *many* is within a "specific" NP, where the NP [*John's pictures of many of the children*] is specific. In (Chomsky 1977c) no independent justification is provided for this principle.

Chomsky (1977c:80) briefly mentions other interpretive rules which violate the SSC and PIC, but which are not rules of construal. One is a relativization process that does not involve

movement/ . . .

movement, but only interpretation of a base generated pronoun in the relative clause. Consider in this connection the following Hebrew sentences.

(21) a. ze ha - iš [še (oto) ra'iti etmol] (23i)  
(this-is the-man [that (him) I-saw yesterday])

b. ra'iti et ha-iš [še natata li et ha-sefer  
(I saw the-man [that you gave me the-book

[še hu katav oto] ] (23ii)  
[that he wrote it ] ]

The rule which associates *ha-sefer* and *oto* in (21b) apparently violates both the SSC and PIC. However, since the relevant rule is in Chomsky's view not a rule of construal it does not represent a real problem for these conditions. No evidence is provided for Chomsky's belief that the Hebrew rule is not a rule of construal.

Chomsky points out that in the "rather artificial" English *such that* construction the SSC and PIC are also apparently violated. Again the relevant rule does not represent a real problem for the condition, since it is not in Chomsky's view a rule of construal. The same is true for the rule involved in left-dislocation in structures such as (22).

(22) as far as John is concerned, I will never believe the  
claims that have been made about him (24)

In (22) *John* and *him* are coreferential, apparently in violation of the SSC and PIC. However, Chomsky (1977c:81) argues that the relevant rule is not a rule of construal, and thus not subject to the conditions in question.

The history of the notion 'involve' is quite interesting. In

the earlier/ . . .

the earlier works, specifically (Chomsky 1973), the notion was used without an explicit definition. However, implicitly it was assumed that a large variety of interpretive rules (maybe even all such rules) fall under the concept 'involve', and are thus subject to the SSC and TSC/PIC. Later it turned out that several interpretive rules, including some explicitly mentioned in (Chomsky 1973) as being subject to the conditions, in fact violate the conditions. All these rules constitute potential counterexamples to the SSC and TSC/PIC. Chomsky (1977c) provides an explicit definition of 'involve' that covers rules of construal only. The domain of the SSC and TSC is consequently restricted to exclude all rules of interpretation that are not rules of construal. Many rules previously considered to be relevant to the conditions now become irrelevant, including a number of rules which are potential counterexamples. According to Chomsky (1977c:74), these features of the development of the notion 'involve' and the delimitation of the domain of the SSC and TSC/PIC, are the results of a deliberate strategy followed by him. (The italics are mine.)

- (23) "The term 'involved in' was left deliberately vague in the exploratory studies cited above, as was the category of rules to which the conditions are relevant. *We may sharpen the formulation somewhat to include the desired cases and exclude unwanted ones.*"

In § 7.2.3.5 below this strategy of Chomsky with respect to the notion 'involve' is analyzed within the context of his views on the aim of linguistic inquiry.

The main conclusions of this section can be summarized as follows.

- (24) a. Chomsky's (1977c) handling of certain rules of semantic interpretation that violate the SSC and PIC again illustrates his willingness to make the conditions work, rather than to abandon them in the face of negative evidence.

b./ . . .

- b. The notion 'involve' in the SSC and TSC/PIC was initially left without any explicit definition, a fact which adversely affected the testability of the conditions.
- c. The explicit definition of 'involve' adopted by Chomsky (1977c) narrows the domain of the conditions to rules of construal, whereas it has been previously assumed that the conditions apply to all rules of semantic interpretation.
- d. By restricting the domain of the conditions to rules of construal, Chomsky (1977c) can protect the SSC and PIC from potential counterexamples.

#### 4.4.4 A modification to the PIC

Chomsky (1977c:75) adopts a certain modification to the PIC proposed by Vergnaud. In terms of this modification, a stipulation is to be added to the PIC, stating that  $\alpha$  is the cyclic node which immediately dominates the category of Y. This stipulation is needed to overcome a problem posed by sentences such as (25) below:

- (25) the men expected [ $\bar{S}$  that [ $S$  [ $NP$  pictures of each other] would be on sale] ] (8)

The Reciprocal Rule, which associates *the men* and *each other* in (25), violates the PIC as formulated in (3). *each other* (= Y) is in a tensed S. Nevertheless (25) is acceptable. Suppose now that the stipulation mentioned above is incorporated in the PIC. In (25) the cyclic node which immediately dominates Y is NP. Consequently, the PIC will no longer prohibit the application of the Reciprocal Rule in (25).

In sum, then:

(26) / . . .

- (26) a. Chomsky (1977c) complicates the PIC by adding a special clause in order to overcome a potential counterexample to this condition.
- b. This modification once more demonstrates Chomsky's willingness to make his conditions work, rather than to abandon them in the face of potential negative evidence.

#### 4.4.5 The case of *wh*-Movement

Chomsky's willingness to accommodate potential counterexamples threatening his conditions is also clearly illustrated by his (1977c) handling of the problem posed by *wh*-Movement. The problem is that while the rules and conditions as formulated in (Chomsky 1977c) allow *wh*-Movement within a clause, they do not allow extraction of a *wh*-phrase from a clause. That is, they block COMP-COMP movement.

- (27) a. who did Mary meet t {40}  
 b. Mary met who
- (28) a. who did you tell Mary that she should meet t {41}  
 b. you told Mary [<sub>S</sub> who that she should meet t]

The derivation of (27a) from (27b) does not violate the SSC or the PIC. The *wh*-phrase is not moved out of  $\alpha$ . However, in the derivation of (28a) from its immediately underlying form (28b) both the SSC and the PIC are violated: the SSC because the embedded  $\bar{S}$  (=  $\alpha$ ) contains a specified subject *she*, and the PIC because the embedded  $\bar{S}$  (=  $\alpha$ ) is tensed. COMP-to-COMP *wh*-Movement thus constitutes a potential counterexample to the SSC and PIC.

As far as the SSC is concerned, the predictions made in (Chomsky 1977c) about *wh*-Movement differ from the predictions made in (Chomsky 1973). In (Chomsky 1973) it is stipulated that the

specified/ . . .

specified subject intervenes between X and Y.<sup>20)</sup> In (28b) Y (= *who*) is to the left of the specified subject. The SSC, as formulated in 1973, would then not block the derivation of (28a). In (Chomsky 1977c) the SSC simply stipulates that  $\alpha$  (=  $\bar{S}$ , NP) contains a specified subject.<sup>21)</sup> Consequently, the derivation of (28b) violates the SSC as formulated in (Chomsky 1977c). As far as the TSC/PIC is concerned, both the 1973- and 1977- formulations would have the effect of blocking (28a).<sup>22)</sup>

Chomsky (1977c:85) points out two differences between clause internal *wh*-Movement and the extraction of a *wh*-phrase from a clause. Firstly, there are many languages (for example, Russian, German) which allow movement of a *wh*-phrase within a clause, but not extraction of a *wh*-phrase from a clause. Secondly, while clause-internal *wh*-Movement in English is unconstrained, the extraction of a *wh*-phrase from a clause is lexically governed. Referring to the "bridge" character of certain matrix verbs that permit the escape of the *wh*-phrase from the embedded  $\bar{S}$ , Chomsky states that it is unclear just what property of matrix verbs allows them to function as "bridges".

Having formulated *wh*-Movement as "Move *wh*-phrase into COMP",<sup>23)</sup> Chomsky considers two possible solutions to the problem of extracting a *wh*-phrase from a clause. The first solution involves a language-specific COMP-COMP Movement rule.

(29) "move *wh*-phrase from COMP to a higher COMP over  
a bridge" {44}

Chomsky suggests that the structural description of this rule must be approximately as in (30).

(30) "(COMP, X, *wh*-phrase, *vbl*), where X contains a VP  
with certain special properties" {45}

If the structural description of the COMP-COMP Movement rule

were to/ . . .

were to incorporate a reference to "bridge" properties, as indicated in (30), then the rule would not satisfy the format proposed for transformational rules by Chomsky (1977c:74-75). According to the relative interpretation of conditions on rules, it could then be argued that the SSC and PIC are inapplicable to (29), the cost of this solution being the adoption of a complex rule. Extraction of a *wh*-phrase from a clause in a language such as English would then be the result of the application of this complex rule. On the relative interpretation of conditions on rules no violation of the SSC and PIC would be involved.

The second possible solution to the problem of extracting a *wh*-phrase from a clause dispenses with a language-specific COMP-COMP Movement rule. The "bridge" conditions could be interpreted as conditions on rules of interpretation. To prevent the SSC and PIC from blocking COMP-COMP movement, the language-specific proviso (31) would have to be incorporated in the SSC and PIC.

(31) "where Y is not in COMP" (46)

Where Y is in COMP, the SSC and PIC would no longer be applicable. Consequently, the conditions would no longer block the extraction of a *wh*-phrase (= Y) from the COMP-position of an embedded clause. The adoption of the proviso in (31) in order to permit COMP-COMP movement was first proposed in (Chomsky 1973:144). Chomsky (1977c:85) states that it is unclear which of the two approaches to the problem of extracting a *wh*-phrase from a clause is preferable. He nevertheless adopts the second approach - i.e., the adoption of the language-specific proviso (31) to the SSC and PIC - "without much reason".

Chomsky (1977c:99) mentions a potential problem raised for his analysis by COMP-COMP Movement in infinitival relatives such as (32).

(32) / . . .

- (32) a. I found a book for you to insist that Bill  
 should read t (106c)
- b. I found a book for you to insist that Bill  
 tell Mary that Tom should read t (106d)

Chomsky claims that, although he is not sure about the judgments, these sentences seem to him to be less acceptable than the comparable examples with *wh*-Movement in finite clauses. If this judgment is correct, then COMP-COMP Movement is less readily available in the case of infinitival relatives. Chomsky (1977c: 99, fn. 38) briefly mentions a number of solutions to this problem. He does not, however, make a choice from among the available solutions. He clearly does not regard the problem as important, noting that "all that seems to be involved is a language-specific proviso and the precise formulation of a general principle for a domain of facts that are rather marginal".

What the two alternative approaches to the extraction of a *wh*-phrase from a clause have in common, is their reliance on the relative interpretation of conditions on rules, and the associated "logic of markedness". In the case of both approaches, COMP-COMP Movement violates the SSC and PIC at a cost. In the first case the cost is the addition of a complex rule to the grammar of English. In the second case the cost is the addition of a language-specific proviso to the grammar of English. In both cases the grammar of English would be more highly marked than, for example, the grammar of Russian, which does not allow COMP-COMP Movement of *wh*-phrases.

Chomsky's (1977c) handling of COMP-COMP Movement clearly illustrates how the notion of the relative interpretation of conditions on rules may be used in the handling of potential counterexamples to conditions on rules. Under this interpretation, Chomsky can retain the SSC and PIC as principles of UG, and at the same time he can adopt a COMP-COMP Movement rule for English. The status of COMP-COMP Movement also brings into focus the

question/ . . .

question of the justification of markedness claims. Although Chomsky (1977c) does not *explicitly* claim that COMP-COMP Movement is a marked process, this idea is clearly implicit in his account of this process. Koster (1978b:62-63) explicitly claims that such movement is marked, and he bases his claim on the considerations used by Chomsky (1977c) to distinguish between clause-internal *wh*-Movement and extraction of a *wh*-phrase from a clause. These are (i) the fact that many languages which permit clause internal *wh*-Movement do not permit extraction of a *wh*-phrase from a clause and (ii) the fact that extraction of a *wh*-phrase from a clause is lexically governed. Chomsky (1980b:15) accepts Koster's interpretation of Chomsky's (1977c) account of clause external *wh*-Movement.<sup>24)</sup>

Markedness claims have the status of hypotheses. Unlike linguistic intuitions they do not represent 'basic sensations' or 'primary linguistic data'.<sup>25)</sup> Given their hypothetical status, markedness claims must therefore be justified. When one considers the justification provided by Chomsky (and Koster) for their claim about the markedness of clause external *wh*-Movement, it strikes one that this claim is not only hypothetical, but also highly speculative. One of the two considerations used to justify the markedness claim is the absence of clause external *wh*-Movement in Russian and German. No mention is made of descriptively adequate analyses of these languages which support the claim that they do not have clause external *wh*-Movement. The question of how common or uncommon clause external *wh*-Movement is in natural languages is not at all answered. In fact, it is not even raised. The same is true for the crucial question of the relevance of such cross-linguistic data for the justification of markedness claims.

Chomsky's (and Koster's) attempted justification of the claim that *wh*-Movement is marked raises other questions. Is it, for instance, the case that evidence derived from a variety of languages is necessary for the validation of markedness claims? Has external linguistic evidence - i.e., evidence derived from

sources/ . . .

sources such as non-idealized language acquisition, speech production and perception, language pathology, linguistic change, linguistic variation, pidginization and creolization - a role to play in the validation of markedness claims?<sup>26)</sup> The formulation of answers to such questions will be postponed until more markedness claims made by Chomsky have been analyzed.<sup>27)</sup>

The main points of § 4.4.5 can be summarized as follows.

- (33) a. Chomsky's (1977c) reaction to the problem which clause external *wh*-Movement poses for the SSC and PIC again illustrates his willingness to take special steps in order to make his theory work in the face of threatening counterevidence.
- b. Chomsky's reaction to the problem posed by clause external *wh*-Movement illustrates the role which the notion of the relative interpretation of conditions on rules can play in overcoming potential counterevidence threatening these conditions.
- c. Chomsky's reaction to the problem posed by clause external *wh*-Movement also instantiates the extension of the evidential base of syntactic theory to include markedness judgments.
- d. Chomsky's claim about the markedness of clause external *wh*-Movement is not only hypothetical, but also speculative.
- e. Chomsky's attempted justification of his claim about the markedness of clause external *wh*-Movement raises certain questions, for example, about the role of cross-linguistic evidence and external linguistic evidence in the validation of markedness claims.

#### 4.5 Explaining the island conditions

Chomsky (1977c:89) discusses an implication of his hypothesis that configurations derived by rules with the properties of (49) - (34) below - always result from the application of *wh*-Movement.

- (34) "a. it leaves a gap  
b. where there is a bridge, there is an apparent violation of subjacency, PIC, and SSC  
c. it observes CNPC  
d. it observes *wh*-island constraints."

If Chomsky's hypothesis is correct, then there is an explanation available for the island conditions, including the Complex Noun Phrase Constraint (CNPC) and the *wh*-island Constraint. Chomsky (1977c:89) explicates this point as follows:

- (35) ". . . we have some evidence that the island constraints of (50 iii, iv) (= the latter should read (49 c, d) - M.S.) can be explained in terms of general and quite reasonable 'computational' properties of formal grammar (i.e., subjacency, a property of cyclic rules that states, in effect, that transformational rules have a restricted domain of potential application; SSC, which states that only the most 'prominent' phrase in an embedded structure is accessible to rules relating it to phrases outside; PIC, which stipulates that clauses are islands, subject to the language specific 'escape hatch' (46)). If this conclusion can be sustained, it will be a significant result, since such conditions as CNPC and the independent *wh*-island constraint seem very curious and difficult to explain on other grounds."

The discussion in (Chomsky 1978a:16ff) about the relation between the island conditions and the Subjacency principle sheds some light on the content of these claims. Chomsky (1978a:16) argues that the island constraints fail to meet two conditions which principles of UG must meet in order to qualify as "deep unifying principles". Firstly, they are not natural as principles of

mental/ . . .

mental computation. Secondly, they constitute a descriptive catalogue. They are not "genuinely explanatory" in that they unify a variety of generalizations and ground them in a system that has a certain degree of deductive structure. The main point made by Chomsky (1977c:89) is then that the SSC, the PIC, and the Subjacency Condition are "natural". That is, they do not have the first-mentioned shortcoming of the island constraints.

It is not easy to determine the exact content of the consideration of naturalness on which Chomsky bases his claim for the superiority of the Subjacency Condition, the SSC and the PIC over the island conditions. His (1978a:17-18) remarks seem to suggest that this consideration concerns the relationship between linguistic theory - as a theory of mental representations and mental computations - and other theories of mental computations.<sup>28)</sup> Thus Chomsky states that the Subjacency Condition is "a natural principle . . . , that is, it makes sense to suppose that mental computation is restricted by principles that limit the range over which such calculation applies". Recall also Chomsky's (1973) claim that the SSC is a natural principle, in that it facilitates a certain perceptual strategy.<sup>29)</sup> If it is indeed the case that the consideration of greater naturalness concerns the relation between linguistic theory and other theories of mental computation, then this consideration is conceptual in nature, in terms of the empirical-conceptual distinction made in § 2.3.4.1 above. In the terminology of Laudan, a linguistic theory with principles that are unnatural as principles of mental computation faces an external conceptual problem created by conflict between the linguistic theory and other theories of mental computation - the conflict in this case apparently taking the form of joint implausibility. Newton-Smith would say that such a theory lacks inter-theory support.

This account of Chomsky's notion 'naturalness' raises a crucial question, namely, what are the other theories of mental compu-

tation in/ . . .

tation in terms of which the plausibility of the principles of linguistic theory can be determined? Chomsky (1977c; 1978a) provides no answer to this question. In the absence of an answer to this question, Chomsky's claim about the naturalness of the conditions is without any content. The implications of this will be considered in chapter 7 below.

Chomsky (1977c:89) also claims that the SSC, the PIC, and the Subjacency Condition are "general". Chomsky's claim about the greater generality of the newer conditions must be seen against the background of the discussion in § 3.2.4, where it was explained that Chomsky adheres to a principle of methodological generality. This principle stipulates that hypotheses, and the theories within which they are integrated, must be of maximal generality. The question of whether the SSC and PIC meet the second requirement which Chomsky (1978a:16) places on principles of UG will be discussed in chapters 6 and 7. Note, however, that in Chomsky's opinion the Subjacency Condition does meet this requirement. Consider in this connection the discussion in (Chomsky 1978a:16ff.), where it is argued that the Subjacency Condition qualifies as a "genuine unifying" principle, since Ross' island constraints can be deduced from it.<sup>30)</sup>

The main points of this discussion can be summarized as follows.

- (36) a. Chomsky (1977c) claims that the SSC, the PIC, and the Subjacency Condition can replace Ross' island constraints.
- b. Chomsky claims that the newer conditions are natural as principles of mental computation, while the island constraints are not natural.
- c. The consideration of greater naturalness is a conceptual one. It bears on the relation between a linguistic theory and other theories of mental computation.

- d. Chomsky's claim about the naturalness of the SSC, the PIC, and the Subjacency Condition appears to be devoid of content.
- e. Chomsky claims that the SSC, the PIC, and the Subjacency Condition are general principles.
- f. Chomsky claims that the Subjacency Condition meets the second requirement which principles of UG must meet, that is, it unifies "a variety of generalizations and ground them in a system that has a certain degree of deductive structure".

Footnotes/ . . .

Footnotes to chapter 4

1. Note that some aspects of the conditions dealt with in (Chomsky 1976a) have already been considered in § 3.3 above.
2. Cf. the discussion of the examples 3 (92) above for an illustration of this problem.
3. Cf. Chomsky 1975a:109 for more detailed discussion of these points. Note that Koster (1978b:31) regards the possibility of explaining the non-permissibility of certain rules as providing very strong support for trace theory.
4. Note that only a *partial* collapsing of conditions on transformational rules and rules of semantic interpretation can be effected. The Subjacency Condition does not restrict interpretive rules. Cf. Chomsky 1977c:73, 80 for some discussion.
5. Cf. Chomsky 1977c:(13) for the examples of (5).
6. In (6) *B* stands for "base rules", *T* stands for "transformational rules", *SI* stands for "rules of semantic interpretation", and *LF* stands for "logical form".
7. Cf. Chomsky 1982a:114-117 for more detail on the two autonomy theses.
8. Cf. Chomsky 1973:235 for a few remarks on the relative versus the absolute interpretation of conditions on rules, and Chomsky 1973:238, footnote 16 for a brief remark (in connection with the TSC) about the possibility of parametric variation.
9. Chomsky (1977c:76) specifies that two terms in the structural/ . . .

tural description of a transformation are adjacent only if each is constant, and any term that intervenes between them is a variable.

10. Cf. the formulation (3) above for  $\alpha$  in the TSC.
11. Such an attempt will in fact only be made in chapter 7.
12. Cf. also the discussion in § 3.2.3 above - and the references cited there - on the conflict between descriptive and explanatory adequacy.
13. Cf. Chomsky 1981a:3, 7f for a recent discussion of the nature of core grammar, and of its role in overcoming the conflict under discussion.
14. Cf. also Chomsky 1981b:38-9 for discussion of the point that core grammars do not generate what are called "languages" in normal colloquial use.
- 15.\* Cf. for example, also Chomsky 1980a:90 for some discussion of the various components of knowledge of language.
16. Cf. for example, Chomsky 1982a:107-8 for a discussion of the idea that 'language' may be a useless concept. Chomsky also argues - cf. for example, Chomsky 1980a:86, 1982a:107-8 - that 'language' is more abstract than 'grammar'. Chomsky (1982a:107) elaborates as follows on this last point:

". . . it seems obvious, when you think about it, that the notion language is a much more abstract notion than the notion of grammar. The reason is that grammars have to have a real existence, that is, there is something in your brain that corresponds to the grammar. That's got to be true. But there is nothing in the real world corresponding to language. In fact it could very well turn out that there is no intelligible notion of language. Even if there is, the

notion/ . . .

notion will raise new problems and more difficult ones because it is at a higher level of abstraction from actual mechanisms."

17. Cf. in this connection the brief outline of the Galilean style of inquiry in § 2.4, where the use of idealizations is linked to the pursuit of depth of understanding.
18. This is one of the main points of Koster's (1978b:566-7) comparison of the introduction of the idealization of sentence grammar and the introduction of the idealization of core grammar.
19. Cf. in particular §§ 4.4.5 and 6.5 below.
20. Cf. the formulation in 2 (1) above.
21. Cf. the formulation in (3) above.
22. The formulation presented in (Chomsky 1976a) is similar to that of (Chomsky 1977c) in the relevant respect. See in this connection the discussion in § 3.3.3 above. Cf. also Chomsky 1977c:fn. 17 for an explication of the different implications of the 1973 and 1977 formulations of the SSC for COMP-COMP Movement.
23. Cf. Chomsky 1977c:85 {43} for this formulation.
24. Note that in terms of the formulations of the SSC and PIC adopted in (Chomsky 1980b), COMP-COMP Movement is not blocked. Accordingly, Chomsky (1980b:15) appeals to the subjacency Condition to express the marked character of such movement.
25. The products of linguistic performance, i.e., utterances and the intuitive judgments of speaker-hearers about the linguistic properties of these utterances, constitute the  
  
primary/ . . .



## Chapter 5

## THE OB BINDING THEORY

5.1 General remarks

In the second phase of their development the SSC and PIC were interpreted as filters, or wellformedness conditions, on the output of transformational rules. They were, however, still interpreted as conditions on *rules*. In particular, they restricted the application of rules of construal. In "On binding" - written in 1978 and first published in 1980 - Chomsky reformulates the SSC and PIC so that they no longer restrict the application of any rule. Instead, they form part of a binding theory which sets limits on the domain within which an anaphor may or must find an antecedent. The binding theory presented in "On binding" - henceforth (Chomsky 1980b) - is known as the OB theory. The details of the adoption of the OB theory forms the subject matter of chapter 5.

5.2 The SSC and PIC reformulated as the Opacity Condition and the Nominative Island Condition

The binding theory adopted in (Chomsky 1980b) comprises the Opacity Condition (1), and the Nominative Island Condition (2).<sup>1)</sup> The Opacity Condition replaces the SSC, and the Nominative Island Condition (henceforth NIC) replaces the PIC.

(1) *Opacity Condition* {27}

If  $\alpha$  is in the domain of the subject of  $\beta$ ,  
 $\beta$  minimal, then  $\alpha$  cannot be free in  $\beta$ .

(2) *Nominative Island Condition* {103}

A nominative anaphor cannot be free in  $\bar{S}$ .

In (1)/ . . . .

In (1)  $\alpha$  is an anaphor. In English at least PRO, reflexives, reciprocals and pronouns in idioms such as *John lost his way* are anaphors.<sup>2)</sup> Lexical NPs are not anaphors. Chomsky (1980b: 15) suggests that languages may vary as to what elements count as anaphors for the binding conditions (1) and (2). He proposes that  $[\alpha \ e]$  (i.e., PRO and trace) fall under the binding conditions universally, while "more 'lexicalized' items" do so less freely. In this connection Chomsky mentions the equivalents of "reflexive" in Japanese and Korean, which apparently do not fall under the binding conditions, and in fact may be governed by conditions that fall outside sentence grammar. The fact that the notion 'anaphor' in the binding conditions (1) and (2) may vary from language to language again illustrates the possibility of parametric variation in conditions belonging to core grammar.

$\alpha$  is in the domain of  $\beta$  if  $\beta$  c-commands  $\alpha$ .  $\beta$  c-commands  $\alpha$  if  $\beta$  does not contain  $\alpha$  (and therefore  $\beta \neq \alpha$ ) and  $\alpha$  is dominated by the first branching category dominating  $\beta$ .

$\alpha$  is bound in  $\beta$  if there is a category c-commanding it and co-indexed with it in  $\beta$ . Otherwise,  $\alpha$  is free in  $\beta$ .  $NP_{arb}$  (i.e., PRO with arbitrary reference) is thus always free. The Opacity Condition and the NIC are called "binding" conditions because they stipulate the domain in which an anaphor must find an antecedent, i.e., the domain in which it must be bound. In the terminology of (Chomsky 1980b:11), the domains of subject and Tense are "opaque" in the sense that anaphors that appear in these domains must be bound in the  $\bar{S}$  or NP that immediately dominates subject or Tense.

Chomsky (1980b:10) assumes that the basic expansions of  $\bar{S}$  and S are (3), so that Tense c-commands both the subject and the predicate of S. He also assumes that NP is the subject of  $\bar{S}$  in (3) and of NP' in (4).

(3)  $[\bar{S} \text{ COMP } [_{\bar{S}} \text{ NP Tense VP}]]$  (17a)

(4)  $[_{\text{NP}} \text{ NP } \bar{N}]$  (17b)

Nominative Case is assigned to the subject of a tensed clause, under the theory of Case Assignment proposed by (Chomsky 1980b:25).

The Opacity Condition and the NIC can be illustrated with the following examples.<sup>3)</sup>

- (5) a. \*The men<sub>i</sub> expected  $[\bar{S}_1$  the soldier to shoot each other<sub>i</sub>]
- b. \*The candidates<sub>i</sub> expected  $[\bar{S}_1$  that each other<sub>i</sub> would win]
- c. The candidates<sub>i</sub> expected  $[\bar{S}_1$  each other<sub>i</sub> to win]

In (5a)  $\alpha$  (= *each other*) is in the domain of the subject of  $\beta$  (=  $\bar{S}_1$ ) namely *the soldier*. *each other* thus cannot be free in  $\bar{S}_1$ , according to the Opacity Condition. It is, however, free in  $\bar{S}_1$ , being coindexed with an NP (= *the men*) in the matrix  $\bar{S}$ . Consequently, (5a) is ill-formed. In (5b)  $\alpha$  (= *each other*) is a nominative anaphor, since it appears in the subject position of a tensed clause. According to the NIC, *each other* thus cannot be free in  $\bar{S}_1$ . It is, however, free in  $\bar{S}_1$ , being coindexed with an NP (= *the candidates*) in the matrix  $\bar{S}$ . Consequently, (5b) is ill-formed. In (5c)  $\alpha$  (= *each other*) is neither in the domain of the subject of  $\bar{S}_1$ , nor a nominative anaphor. Consequently, *each other* can be free in the embedded  $\bar{S}_1$ , and coindexed with an NP (= *the candidates*) in the matrix  $\bar{S}$ .

The qualification in the Opacity Condition, that  $\beta$  is minimal, is required for cases such as (6), with PRO<sub>arb</sub>.<sup>4)</sup>

- (6) a.  $[\beta_2$  it is unclear  $[\beta_1$  what PRO to do]]
- b.  $[\beta_2$  their uncertainty as to  $[\beta_1$  what PRO to do]]

In/ . . .

In  $\beta_1$ , PRO can be free, since it is not in the domain of the subject, nor a nominative anaphor. The minimality qualification prevents (1) from taking  $\beta$  to be  $\beta_2$ . Consequently, PRO can be indexed *arb* in (6a) and (6b), even though it is in the domain of the subject of  $\beta_2$  (*it* in (6a) and *their* in (6b)).

Chomsky (1980b) in fact considers various reformulations of the binding conditions which he rejects in favour of (1) and (2). For ease of later reference two other formulations considered by (Chomsky 1980b) are presented in (7) and (8).

(7) If  $\alpha$  is an anaphor in the domain of the tense or the subject of  $\beta$ ,  $\beta$  minimal, then  $\alpha$  cannot be free in  $\beta$ ,  $\beta = \text{NP}$  or  $\bar{S}$ . {19}

(8) A nominative anaphor in  $S$  cannot be free in  $\bar{S}$  containing  $S$ . {26}

The condition (7) represents the first reformulation of the SSC and PIC considered in (Chomsky 1980b). (7) incorporates both the SSC and the PIC. (8) is the first formulation of the NIC presented in (Chomsky 1980b). (8) differs from (2) in that the former, but not the latter, contains a reference to  $S$ .

### 5.3 The justification for the reformulation of the SSC and PIC as the Opacity Condition and Nominative Island Condition

Chomsky (1980b:12) discusses three differences between the binding conditions and earlier formulations of the SSC and PIC. In this discussion Chomsky explicitly refers to the reformulation (7) of the SSC and PIC. However, the differences distinguished by Chomsky exist between earlier formulations of the SSC and PIC and any of the reformulations in (Chomsky 1980b). Consequently, any advantage which follows from these differences is, at the same time, an advantage of the Opacity Condition (1) and the NIC, relative to earlier formulations of the SSC and PIC.

The first/ . . .

The first difference mentioned by Chomsky is that the binding conditions are no longer conditions on some collection of rules in the grammar, as was the case with previous formulations of the SSC and PIC. Instead, the reformulated versions of the SSC and PIC adopted in (Chomsky 1980b) are conditions on some level of representation. Specifically, Chomsky assumes that the binding conditions are conditions on logical form (henceforth LF), or on some late stage of interpretation within the rules giving LF. Chomsky does not mention an advantage that follows directly from this difference. One obvious advantage is that it is no longer necessary to stipulate what subclass of syntactic and/or semantic rules are subject to these conditions. That is, it is no longer necessary to define a notion 'X and Y involved in a rule'.

The second difference mentioned by Chomsky is that the binding conditions are conditions on anaphors, while earlier formulations of the SSC and PIC placed constraints on variables relating two positions involved in some rule. The significance of this difference, according to Chomsky (1980b:12), is that "it allows us to incorporate without specific mention the case of Arbitrary (uncontrolled) Reference . . .". Arbitrary Reference has essentially the same properties as bound anaphora. Consider for example the sentences in (9).

- (9) a. it is unclear  $[\bar{s}$  who  $t$  to visit PRO] {22b}
- b. it is unclear  $[\bar{s}$  who PRO visited  $t$ ]
- c. it is unclear  $[\bar{s}$  who PRO to visit  $t$ ] {22a}

In each case  $t$  is the trace of *who*. (9a), (9b) and (9c) correspond to (5a), (5b), (5c), respectively. In the ungrammatical (9a) PRO is in the domain of the subject  $t$ . In the ungrammatical (9b) PRO is a nominative anaphor. In the grammatical (9c) PRO is neither in the domain of a subject ( $\beta$  minimal), nor nominative. The binding conditions thus make exactly the right predictions about Arbitrary Reference.

Given the Opacity Condition, PRO cannot be free, and thus arbitrary in reference, in (9a). Given the NIC, PRO cannot be free, and thus arbitrary in reference, in (9b). Neither condition prevents PRO from being free, and thus arbitrary in reference, in (9c). Since there is no question of two positions being involved in these cases, earlier formulations of the SSC and PIC could not cover Arbitrary Reference. This particular advantage of the reformulated versions of the conditions -  $T_{x+1}$  - over the earlier versions -  $T_x$  - is clearly empirical in nature. While  $T_x$  failed to make any predictions about Arbitrary Reference,  $T_{x+1}$  makes the correct predictions.<sup>5)</sup>

The third difference mentioned by Chomsky is the absence of the notion 'specified subject' from the reformulated versions of the SSC, i.e., the Opacity Condition (1) and the second part of (7). The absence of the notion 'specified subject' means that the reformulated versions of the SSC can overcome an empirical problem faced by earlier formulations, a problem pointed out by Lasnik. Consider the sentence in (10).

(10) which men did Tom think that Bill believed t saw  
each other

{23}

$t$  is the trace of *which men*. The SSC would prevent the Reciprocal Rule from associating *which men* and *each other*, since the specified subjects *Tom* and *Bill* intervene. Given the reformulation of the SSC as a binding condition, *each other* can be co-indexed with the trace  $t$ , so that it is not free in any opaque context. (Chomsky (1980b:13) notes that it was in any event improper to relate the quantifier phrase *which men* to the reciprocal *each other*, since the latter requires a "referring expression" as its antecedent.) This advantage of the reformulated version of the conditions is also empirical in nature:

$T_{x+1}$  - which incorporates the reformulated versions of the conditions - makes the correct prediction about (10), while  $T_x$  - which incorporates the SSC and PIC as conditions on rules - fails to make the correct prediction about (10).

Chomsky (1980b:14) mentions a further advantage of the binding conditions over earlier versions of the SSC and PIC. Given the binding conditions, the COMP position of a tensed clause need no longer be stipulated as an "escape hatch" for movement. In the case of earlier formulations of the SSC and PIC the escape hatch status of COMP had to be stipulated - see for example, the discussions in §§ 3.2.7.1 and 4.4.5. To see how the status of COMP as an "escape hatch" follows from the binding conditions, consider the structure (11).

(11) who do they think  $\left[ \bar{S} \left[ \text{COMP } t^1 \right] \left[ \text{Bill will see } t^2 \right] \right]$  (31)

$t^2$  is not free in  $\bar{S}$ , since it is coindexed with  $t^1$ .  $t^1$  is free in  $\bar{S}$ , since it is not c-commanded by  $t^2$ . However, since  $t^1$  is not nominative or in the domain of a subject, it can be free in  $\bar{S}$ . Thus, the binding conditions do not block (11). Under the binding conditions an element can thus escape from an opaque domain via COMP.

It was explained in § 4.2 above that a theory  $T_{x+1}$  has greater deductive depth than another version  $T_x$  if a principle which must be stipulated in  $T_x$  follows from independently motivated principles in  $T_{x+1}$ . Let  $T_{x+1}$  be the binding conditions, and  $T_x$  the earlier formulations of the SSC and TSC. While the escape hatch status of COMP must be stipulated for  $T_x$ , it follows without stipulation from  $T_{x+1}$ .  $T_{x+1}$  thus has a greater degree of deductive depth than  $T_x$ . To put it in another way:  $T_{x+1}$  can explain the escape hatch status of COMP, while  $T_x$  fails to do so.

There is a second respect in which the OB binding conditions have greater deductive depth than the earlier formulations of the SSC and PIC. Chomsky (1980b:12) briefly refers to (Freidin 1978), where it is argued that the principle of strict cyclic application of rules follows deductively from the binding conditions and "reasonable" conditions on argument position in LF. Thus, as Chomsky (1980b:12) explains, there is no need to stipulate

the principle/ . . .

the principle of strict cyclic application of rules.

It was argued in § 4.2 above that the deductive depth of a theory is linked to the unifiedness of the theory, where theoretical unifiedness is a conceptual property of theories. A comprehensive account of the role and status of the consideration of greater deductive depth will only be attempted in § 7.2 below. Note, however, that Chomsky (1981a) provides confirmation that deductive depth must be regarded as a conceptual factor in theory appraisal. In his discussion of problems in the OB-theory, Chomsky (1981a:157ff.) calls problems raised by lack of deductive depth of the type discussed above "conceptual".

Let us now consider why Chomsky adopts a binding theory consisting of the Opacity Condition and the NIC, rather than condition (7), which incorporates both the SSC and PIC. The reasons for this choice are discussed by Chomsky (1980b:13-14). The first consideration that motivates the choice of the Opacity Condition and the NIC concerns a redundancy exhibited by (7), and in fact by all earlier formulations of the SSC and TSC/PIC. Consider the sentence in (12).

(12) they told me [<sub>S</sub> what I gave each other] {24}

The sentence in (12) is blocked by both the SSC and PIC - in their earlier formulations as conditions on rules, as well as by the reformulation (7). In the terminology of the latter, the anaphor *each other* is in the domain of Tense and in the domain of the subject *I*. The redundancy illustrated in connection with (12) can be eliminated if the PIC is restricted to the subject of a tensed clause, as is the case with the NIC. The NIC does not rule out sentences such as (12), because the anaphor *each other* is not in subject position. However, (12) is ruled out by the Opacity Condition, because *each other* is in the domain of the subject.

Chomsky regards the fact that the redundancy under discussion is avoided by the binding theory consisting of the Opacity Condition and the NIC as an advantage of this theory. He (1980b:13) characterizes the avoidance of the redundancy as an advantage "at the metatheoretic level", where such an advantage contrasts with an empirical advantage. In terms of the empirical-conceptual distinction of § 2.3.4.1, the elimination of a redundancy is a conceptual consideration. The fact that Chomsky characterizes the elimination of redundancy as a metatheoretic advantage, provides support for the view that Chomsky adopts an empirical-conceptual distinction similar to the one adopted in § 2.3.4.1. As will be shown in § 6.3 below, Chomsky (1981a) even calls considerations such as the elimination of redundancy "conceptual". "Metatheoretic" can then be regarded as a synonym for "conceptual".

The exact nature of the consideration under discussion will be considered in § 7.2 below. Briefly, Chomsky seems to link the desirability of eliminating redundancy in linguistic theory with a lack of redundancies in the language faculty. Consider in this connection Chomsky's (1981a:14-15) reference to the possibility of redundancies in the language faculty. The avoidance of redundancy in linguistic theory is thus an external conceptual consideration. The existence of a redundancy in a linguistic theory creates a tension between this theory and a general assumption made by Chomsky about the language faculty to be described by linguistic theory.<sup>6)</sup> Note that Chomsky's (1981a:14-15; 338-339) remarks also indicate that his assumption about the absence of redundancies in the language faculty forms part of a more general assumption, namely that the language faculty is simple.

The second consideration which motivates the choice of the Opacity Condition and the NIC over the combined condition (7) concerns a difference "at the empirical level", in Chomsky's (1980b:13) own words. Consider the sentence in (13).

(13) / . . .

- (13) they expected [<sub>S</sub> that pictures of each other (each other's pictures) would be on sale] {28}

The NIC makes the correct prediction about (13). The reciprocal phrase *each other* in (13) is not nominative, and thus not subject to the NIC. The PIC, in its pre-1980 formulations and in the reformulated version (7), makes the wrong prediction about (13). It was in order to overcome this problem that Chomsky (1977c:75) adopted the assumption that the PIC is constrained by Subjacency.<sup>7)</sup> The NIC makes the correct prediction without recourse to the subjacency stipulation. Chomsky (1980b:14) claims that "now we have a much simpler explanation" for the grammaticality of (13).

Let  $T_x$  be the version of UG which incorporates either the formulation (7) or any previous formulation of the PIC. Let  $T_{x+1}$  be the version of UG which incorporates the NIC. Both  $T_x$  and  $T_{x+1}$  can make the correct prediction about (13). The difference is that  $T_x$  can do so only if an extra stipulation, to the effect that the PIC/NIC is constrained by subjacency, is added to  $T_x$ , while  $T_{x+1}$  requires no such additional stipulation. As Chomsky himself points out,  $T_{x+1}$  is to be preferred because it is simpler than  $T_x$ . Ultimately, then, the consideration on which the choice of  $T_{x+1}$  is based is conceptual in nature. In this instance the greater simplicity of  $T_{x+1}$  is not the result of the elimination of a redundancy, but of the elimination of a special stipulation. It will be argued in § 7.2 below that the desirability of avoiding such special stipulations is also linked to a general assumption made by Chomsky about the simplicity of the language faculty.

A last point to be discussed in this section concerns the choice of the formulation (2) of the NIC over the formulation (8). Chomsky (1980b:fn. 19) explains that the reference to S in (8) is required to deal with the trace in COMP of a *wh*-moved subject, under the assumption that this trace is also nominative. Consider the structure in (14).

(14) who did they think  $\left[ \bar{S} \left[ \text{COMP } t^1 \right] \left[ S t^2 \text{ would win} \right] \right]$

Assuming that both  $t^1$  and  $t^2$  are nominative, (8) allows this structure.  $t^2$  is bound in  $\bar{S}$ , while  $t^1$ , though free in  $\bar{S}$ , is not free in  $S$  contained in  $\bar{S}$ . In a discussion of Case theory, Chomsky (1980b:36) makes the assumption that the trace in COMP of a *wh*-moved subject is not nominative. It then becomes possible to eliminate the reference to  $S$  in the NIC, and to adopt the formulation (2).

Chomsky (1980b:13) claims that the reference to  $S$  in the formulation (8) of the NIC constitutes a disadvantage at the meta-theoretic level. Neither condition (7), nor earlier formulations of the PIC, contained a reference to  $S$ . Chomsky (1980b:13) says that the reference to  $S$  in (8) represents "an undesirable complication", and he (1980b:36) also refers to it as an "inelegance of formulation". The advantage of the formulation (2) of the NIC is then that it avoids this "undesirable complication" and "inelegance of formulation". In terms of the present framework this consideration which justifies the choice of the formulation (2) is conceptual in nature.

The main points of the discussion above can be summarized as follows. Let  $T_{x+1}$  be any version of the binding conditions presented in (Chomsky 1980b), and  $T_x$  any earlier version of the SSC and PIC formulated as conditions on rules. The choice of  $T_{x+1}$  over  $T_x$  is justified in terms of the following considerations:

- (15) a.  $T_{x+1}$ , but not  $T_x$ , can explain the properties of Arbitrary Reference.  $T_{x+1}$  thus has greater empirical success than  $T_x$ .
- b.  $T_{x+1}$ , but not  $T_x$ , can make the correct predictions about sentences like (10).  $T_{x+1}$  thus has greater empirical success than  $T_x$ .

c. The/ . . .

- c. The escape hatch status of COMP follows deductively from  $T_{x+1}$ , but must be stipulated in a version of UG which contains  $T_x$ .  $T_{x+1}$  thus has greater deductive depth than  $T_x$ , where deductive depth is a conceptual factor in theory appraisal.
- d. The strict cyclic application of rules follows deductively from  $T_{x+1}$ , but must be stipulated in a version of UG which contains  $T_x$ .  $T_{x+1}$  thus has greater deductive depth than  $T_x$ .

Let  $T_{x+1}$  be the version of the binding conditions consisting of the Opacity Condition (1) and the NIC (2). Let  $T_x$  be any earlier version of the SSC and PIC, or the version of the OB binding theory consisting of the Opacity Condition (7). The choice of  $T_{x+1}$  over  $T_x$  is justified in terms of the following considerations.

- (16) a.  $T_{x+1}$  avoids a redundancy exhibited by  $T_x$ .  $T_{x+1}$  thus has a conceptual advantage over  $T_x$ .
- b.  $T_{x+1}$  can provide a simpler explanation than  $T_x$  for the grammaticality of sentences such as (13), in that  $T_{x+1}$  does not require a stipulation to the effect that the NIC is constrained by Subjacency.  $T_{x+1}$  thus has a conceptual advantage over  $T_x$ .

Let  $T_{x+1}$  be the version (2) of the NIC, and  $T_x$  the version (8) of the NIC. The choice of  $T_{x+1}$  over  $T_x$  is justified in terms of the following consideration.

- (17)  $T_{x+1}$  is simpler and more elegant than  $T_x$ , in that  $T_{x+1}$  avoids a special reference to S.  $T_{x+1}$  thus has a conceptual advantage over  $T_x$ .

#### 5.4 The role of the conditions in an optimal theory of construal

In the works discussed in chapters 3 - 4 much attention is given to the effect of the SSC and TSC/PIC on the form of transformational rules. It is argued by Chomsky in these works that the incorporation of these conditions in UG permits a significant simplification of transformational rules. This simplification, in turn, leads to a desirable reduction in the expressive power of transformations. Right from the beginning, the SSC and PIC were interpreted as also restricting rules of semantic interpretation, specifically the rules of construal.<sup>8)</sup> In (Chomsky 1976a) and (Chomsky 1977c) it is suggested that the SSC and PIC also permit a simplification of the rules of construal. Referring to these rules, Chomsky (1976a:319) claims that "... the SSC functions so as to permit a very simple formulation of rules". In (Chomsky 1977c:76) he proposes that the structural descriptions of rules of construal must conform to the same narrow format stipulated for transformational rules. The effect of the SSC/Opacity Condition and PIC/NIC on the formulation of rules of construal is considered in more detail by Chomsky (1980b:6-10).

Chomsky first examines the case of control, and he (1980b:8) explores the possibility of using "the simplest possible rule", namely *Coindex*. This rule must be interpreted as meaning that an arbitrary PRO in an embedded structure is coindexed with some NP in a higher clause or is assigned the index *arb* if there is no lexical NP in a higher clause. Chomsky argues that this approach will work for both the familiar cases of control, i.e., for indirect questions such as those in (18), and for sentences such as those in (19).

- (18) a. ... [ <sub>$\alpha$</sub>  who [ <sub>$\beta$</sub>  NP visited t] ]  
 (It is unclear who Bill visited) (13a)

b./ . . .

- b. ... [<sub>α</sub> who [<sub>β</sub> t visited NP] ]  
*(It is unclear who visited Bill)* {13b}
- c. ... [<sub>α</sub> who [<sub>β</sub> NP<sub>1</sub> to visit NP<sub>2</sub>] ]  
*(It is unclear who to visit)* {13c}
- (19) a. John promised (persuaded) Bill [<sub>α</sub> that NP<sub>1</sub>  
would (should) visit NP<sub>2</sub>] {14a}
- b. John promised (persuaded) Bill [<sub>α</sub> to visit  
NP] {14b}
- c. John tried [<sub>α</sub> to visit NP] {14c}
- d. it is time [<sub>α</sub> to visit NP] {14d}

Chomsky assumes that (19b)-(19d) have the embedded structure (20).

- (20) [<sub>S</sub> COMP [<sub>S</sub> NP<sub>1</sub> to visit NP<sub>2</sub>] ] {15}

In (18a) and (18b) NP ≠ PRO, that is, control is impossible. In (18c) NP<sub>2</sub> ≠ PRO. In (19a) neither NP<sub>1</sub> nor NP<sub>2</sub> can be PRO. In (19b)-(19d) NP<sub>2</sub> cannot be PRO. However, NP<sub>1</sub> in (18c) and (19b)-(19d) can be PRO. That is, only the subject of an infinite verb is open to control. This property of control follows automatically from the SSC/Opacity Condition and PIC/NIC.<sup>9)</sup> Control can thus be assigned by the simple rule *Coindex*.

Chomsky (1980b:9) claims that the SSC/Opacity Condition and PIC/NIC also make it possible to adopt the simplest possible formulation of the rule that assigns an antecedent to *each other*.

- (21) *Each other* is a reciprocal phrase. {16}

Chomsky claims that the grammar of English can be reduced to (21) "for the core cases of reciprocals". Conventions belonging

to UG will ensure that the coindexing of *each other* and some NP effected by (21) is correct. In particular, the SSC/Opaicity Condition and PIC/NIC will ensure that only *each other* in the subject position of embedded infinitives can be coindexed with an NP in a higher clause. Note that Chomsky does not explain on what grounds the "core cases" of reciprocals can be distinguished from the non-core cases.

It is further claimed by Chomsky (1980b:9) that other cases of bound anaphora, including reflexives, can be dealt with in the same way as the control and reciprocal cases. Recall that traces are also regarded as bound anaphors. In addition, Chomsky claims that "essentially the same analysis carries over to disjoint reference". (However, in order to incorporate disjoint reference in his general approach, Chomsky must adopt a fairly complex indexing theory. See the discussion in § 5.5 below.) He (1980b:9-10) comments as follows on the advantages of the approach outlined above.

(22) "In this way, we considerably reduce the complexity of the required rules, approaching the potential limits. And we also have a highly unified theory, with a few abstract principles governing a wide range of phenomena."

Two points must be noted here. Firstly, the simplification of the rules of construal effected by the SSC/Opaicity Condition and PIC/NIC derives its significance from the fact that it can lead to a reduction in the formal power of such rules. This link between a simplification of rules of construal and a reduction in the formal power of such rules is emphasized in Chomsky's (1980b:1-2) introductory comments. The role which the consideration of restricted formal power of rules of construal plays in the justification of the SSC/Opaicity Condition and PIC/NIC is identical to the role which the consideration of restricted formal power of transformational rules plays in the justification of these conditions. The only difference is that while transformations were right from the beginning the focus of attempts

to restrict/ . . .

to restrict the formal power of linguistic theory, rules of semantic interpretation did not always feature so prominently in these attempts. However, Chomsky has always emphasized that there are various other points at which linguistic theory can be restricted, including interpretive rules. Thus, Chomsky (1977b:18) states that within the EST the class of accessible grammars can be constrained "by conditions on the base, the transformational component, the system of interpretive rules, the shallow structures that are produced by transformational derivations, and the system LF". The reason why the emphasis initially was on reducing the formal power of transformational rules, rather than interpretive rules, is that for a long time Chomskyan linguists were mainly working on transformational rules.

The role of the consideration of restricted formal power in justifying general-linguistic hypotheses is analyzed in detail in § 3.2.5 above. The main conclusions of the latter section - particularly 3.(42b-d) - carry over without modification to the role which the consideration of restricted formal power of rules of construal plays in justifying the SSC/Opacity Condition and PIC/NIC.

The second point to be noted about Chomsky's remarks quoted in (22) above is that, in his view, the theory of which the SSC/Opacity Condition and PIC/NIC form part has the property of being "highly unified". The SSC/Opacity Condition and PIC/NIC qualify as "abstract principles covering a wide range of phenomena". In essence, Chomsky is claiming that the relevant conditions are very general. It was explained in § 4.2.4 above that both the quantity of data explained by a theory (i.e., evidential comprehensiveness) and the variety of data explained by a theory (i.e., evidential independence) determine its generality. It seems then that the SSC/Opacity Condition and PIC/NIC partly meet at least one of the conditions which Chomsky (1978a:16) imposes on the explanatory principles of linguistic

theory/ . . .

theory: Such principles must "unify a variety of generalizations and ground them in a system that has a certain degree of deductive structure". Chomsky's (1980b:10) remarks indicate that in his view the conditions meet the first part of this condition. As regards the second part of Chomsky's condition - namely, that the principles must ground the generalizations "in a system that has a certain degree of deductive structure" - his reference to the abstractness of the conditions suggests that in his opinion they also meet this second requirement. Recall also that the OB binding conditions have greater deductive depth than earlier formulations of the SSC and PIC, in that the escape hatch status of COMP and the principle of strict cyclic application of rules can be deduced from the OB binding conditions. In § 6.3.3 below the question of the extent to which the OB binding conditions do qualify as a system with "a certain degree of deductive structure" will be considered in greater detail.

The main conclusions of this section can be summarized as follows.

- (23) a. The SSC/Opacity Condition and PIC/NIC are partly justified because they make it possible to restrict the formal power of rules of construal significantly.
- b. The SSC/Opacity Condition and PIC/NIC are partly justified because they unify a wide range of phenomena.

### 5.5 The binding conditions and Disjoint Reference

Chomsky (1977c:72) formulates the rule of Disjoint Reference as follows:

- (24) Assign to a pronoun the feature  $[-\text{anaphoric to } i]$  in a structure containing  $NP_i$ .

The rule/ . . .



The indices of NPs are nonnegative integers. The index 1 is reserved for arbitrary reference. Some NPs receive indices via the movement rules, and others via the rules of construal. The latter indexing applies from "top to bottom" in the structure. An NP is assigned an index only when all NPs that c-command or dominate it have been indexed. The only NPs not assigned indices by the movement rules or rules of construal are the nonanaphors: lexical NPs and pronouns (apart from the bound idioms, as in *John lost his way*).

The rule of Disjoint Reference assigns indices to the non-anaphors. Each nonanaphor is assigned a complex index  $\langle r, A \rangle$ , where  $r$  is the referential index and  $A$  the anaphoric index. The complex index is assigned as follows:

- (i) Indexing proceeds from top to bottom, until a nonanaphor  $\alpha$  is reached.
- (ii) If  $\alpha$  has already been assigned an index  $i$  by a movement rule, then  $i$  is its referential index. If it has no index, it is assigned a new referential index  $i \geq 2$ .
- (iii) The referential indices of all NPs that c-command  $\alpha$  are assigned to  $\alpha$  as its anaphoric index. If there is no c-commanding NP, then the anaphoric index of  $\alpha$  is empty.

The anaphoric index  $a_1, \dots, a_n$  of  $\alpha$  means that  $\alpha$  is disjoint in reference from each NP with referential index  $a_i$ . The binding conditions are taken as deleting certain indices from the anaphoric index of a pronoun, thus in effect blocking disjoint reference in some cases and permitting reference to be free. The binding conditions hold for pronouns, but not for lexical NPs.

In order to unify anaphors and pronouns for the purposes of the

binding/ . . .

binding conditions the notion 'designated index of  $\alpha$ ' is introduced. In the case of an anaphor its referential index is its designated index. In the case of a pronoun its anaphoric index is its designated index. The notion "free" defined by Chomsky (1980b:10) is now generalized as follows.

- (26) "Suppose that  $\alpha$  has the designated index  $j$  and  $i$  is an integer such that  $i = j$  or  $i \in j$ . Then  $\alpha$  is free ( $i$ ) in  $\beta$  if there is no  $\gamma$  in  $\beta$  with the index  $i$  that c-commands  $\alpha$ ." {111}

The index  $i$  is necessarily referential. The case  $i = j$  is the case of an anaphor, and the case  $i \in j$  is the case of a pronoun. The binding conditions are then reformulated as rules that modify the designated index.

- (27) Suppose that  $\alpha$  has the designated index  $j$  and is free ( $i$ ) in  $\beta$  ( $\beta = \text{NP}$  or  $\bar{S}$ )  
 where (a)  $\alpha$  is nominative  
 or (b)  $\alpha$  is in the domain of the subject of  $\beta$ ,  $\beta$  minimal.  
 Then  $j \rightarrow 0$  if  $j$  is an integer, and  $j \rightarrow (j - \{i\})$   
 if  $j$  is a set. {112}

Case (a) of (27) is the NIC, and (b) the Opacity Condition.  $\text{NP}_0$  is not permitted in LF, where  $o$  is the referential index.  $\text{NP}_o$  is an inadmissible free variable, an anaphor that is not properly bound. The effect of (27) on the rule of Disjoint Reference is illustrated by the structure in (28).

- (28)  $\text{John}_2$  told  $\text{Bill}_{(3, \{2\})}$  [ $\bar{S}$   $\text{PRO}_3$  to visit him] {113}

The full anaphoric index of *John* is omitted in (28). *John* and *Bill* have been indexed by the assignment rule for nonanaphors; *PRO* by the rule of Control. *him*, as a nonanaphor, is assigned the index  $(4, \{2, 3\})$ . *him* is free (2) in  $\bar{S}$  but not free (3) in  $\bar{S}$ , and is in the domain of the subject of  $\bar{S}$ . *him* thus undergoes

rule (27)/ . . .

rule (27), which removes 2 from its anaphoric index, leaving *him* with the index (4, 3). *him* in (28) is thus understood as disjoint in reference from *PRO* and *Bill*, but not necessarily disjoint in reference from *John*.

The main points of the discussion above can be summarized as follows.

- (29) a. In order to maintain his claim that the binding conditions make possible optimally simple formulations of the rules of construal, Chomsky (1980b) must show that Disjoint Reference - which involve nonanaphors (pronouns) - can be incorporated under the binding conditions - which apply to anaphors.
- b. Chomsky (1980b) is able to incorporate Disjoint Reference under the binding conditions by adopting a fairly complex indexing theory, which includes anaphoric indices.

#### 5.6 A potential empirical problem for the Opacity Condition solved by structure-building rules

Chomsky (1980b:16) argues that the notion 'subject' which figures in the Opacity Condition is a *syntactic*, and not a *semantic*, notion. In sentences such as (30), the phrases *the books* and *John* are not subjects of *given* and *appear* "in any semantically significant sense of the notion 'subject'". Nevertheless, they invoke Opacity, blocking the Reciprocal Rule and the rule of Disjoint Reference.

- (30) a. They expect the books to be given to each other  
(to them) {36a}
- b. They expected John to appear to each other (to  
them) to be qualified for the job {36b}

In (30a) / . . .

In (30a) and (30b) *each other* cannot be coindexed with *they*, and *they* and *them* can be coreferential.

In sentences such as (31) the rules of Reciprocal Interpretation and Disjoint Reference are blocked in the domain of the trace of the *wh*-phrase, indicating that overt subjects need not appear to invoke Opacity.

- (31) a. what books did they expect t to be given to  
each other (to them) {37a}
- b. who did they expect t to appear to each other  
(to them) to be qualified for the job {37b}

In (31a) and (31b) *they* and *each other* cannot be coindexed, and *they* and *them* can be coreferential. The Opacity Condition is thus analogous to the Specified Subject Condition, in that traces count as subjects in both cases. Chomsky (1980b:16) concludes that it is "the abstract syntactic subject that invokes Opacity, where 'syntactic subject' is a formal, configurational notion in English". The semantic relation between the subject that invokes Opacity and the elements in its domain is irrelevant.

Chomsky (1980b:17) also discusses sentences which lack a syntactic subject at surface structure, but which nevertheless appear to be subject to the Opacity Condition.

- (32) a. They regard me as very much like each other  
(them) {38a}
- b. I impress them as very much like each other  
(them) {38b}

Chomsky (1980b:17) observes that "there seems to be no syntactic motivation for assigning anything beyond the obvious surface structure to such sentences". Nevertheless, in (32a, b) the rules of Reciprocal Interpretation and Disjoint Reference are

blocked/ . . .

blocked, as if these sentences contained a subject which invokes Opacity. In (32a, b) *they* and *each other* cannot be coindexed, and *they* and *them* can be coreferential, exactly as in (30) and (31). If subject and object are inverted, as in (33), then neither rule is blocked.

- (33) a. I regard them as very much like each other (them)
- b. They impress me as very much like each other (them)

In (33) *they/them* and *each other* can be coindexed, and *they/them* and *them* cannot be coreferential.

The interpretation possibilities for *each other* and *them* in (32) are thus similar to those of (30) and (31). However, while the Opacity Condition can account for the interpretation possibilities in (30) and (31), it fails to account for the interpretations of (32). This failure on the part of the Opacity Condition constitutes a potential empirical problem for it.

One possible solution to the problem posed by sentences such as (32) is to develop a semantic analogue to the Opacity Condition. This is rejected by Chomsky as "a dubious move", since it has already been shown that the Opacity Condition relates to the syntax, not the semantics, of LF. Given the similarity between the properties of (32) and those of sentences (such as (30) and (31)) that fall under the syntactic notion of Opacity, Chomsky states that "it seems natural to extend the Opacity Condition directly" to (32). In order to achieve such an extension, it must be assumed that sentences such as (32) are represented as (34) at the level of LF, where the binding conditions apply.

- (34) a. they regard me as [<sub>S</sub> PRO be very much like  
          each other (them)] (39a)
- b. I impress them as [<sub>S</sub> PRO be very much like  
          each other (them)] (39b)

The verbs/ . . .

The verbs *regard* and *impress* have essentially the control properties of *persuade* and *promise*, respectively. *regard* assigns object control, and *impress* assigns subject control. In (34a) PRO is thus coindexed with *me*, and in (34b) with *I*. Thus PRO, coindexed with *me/I*, invokes Opacity in (32). *each other* must be bound in the embedded  $\bar{S}$ , but because it needs a plural antecedent it cannot be coindexed with PRO. And because *them* is free in the domain of a subject (PRO), it can be coreferential with the plural pronoun in the matrix clause. In (33), with subject and object inverted, the situation is reversed.

In order to extend the Opacity Condition to sentences such as (32), Chomsky must provide for a new class of rules among the rules that generate representations in LF: structure-building rules that assign LF-representations such as (34) to sentences such as (32). The fact that the existence of such structure-building rules enables the Opacity Condition to apply directly to (32) provides "positive, though indirect, evidence for such rules", according to Chomsky (1980b:18). Chomsky defends his structure-building rules from the potential charge that they exhibit all the weaknesses of earlier transformational rules involved in lexical decomposition. His main argument is that, since there are built-in restrictions on both the input of these rules (i.e. S-structure) and their output (i.e. LF-representations), it is unlikely that his structure-building rules of interpretation "go beyond narrow limits". This, according to him, is in contrast to the arbitrary and varied nature of earlier lexical decomposition rules.<sup>10)</sup>

The claim that there exist structure-building rules that assign the LF-representations (34) to sentences such as (32) clearly has the status of an auxiliary hypothesis, introduced by Chomsky to protect the Opacity Condition from the criticism that it fails to account for sentences such as (32). The only function of structure-building rules is to extend the Opacity

Condition to (32). The relevant auxiliary hypothesis is thus without independent justification. Moreover, in the form that it is presented by Chomsky (1980b) the auxiliary hypothesis is not independently testable either. The content of the relevant auxiliary hypothesis is in crucial respects left obscure. No detail is provided about the properties of structure-building rules in general, or the particular rules required for the derivations of (34).

It was argued in § 3.2.5 above that, apart from its obvious empirical aspects, the consideration of restricted formal power also has a conceptual aspect. One of Chomsky's main criticisms of the earlier lexical decomposition rules was that the inclusion of such rules in linguistic theory lead to an undesirable extension of the formal power of linguistic. If Chomsky's structure-building rules were subject to the same criticism, then he would have solved an empirical problem at the cost of creating a conceptual problem. The essence of Chomsky's (1980b:18) remarks is that his structure-building rules will not lead to an undesirable extension of the formal power of UG, given the strong constraints on their input and output. In effect, then, Chomsky is trying to argue that the introduction of structure-building rules do not create a conceptual problem for his theory.

The main points of this section can be summarized as follows.

- (35) a. The Opacity Condition fails to account for the fact that Reciprocal Interpretation and Disjoint Reference apply in sentences such as (32) exactly as if these sentences contained a syntactic surface subject, even though these sentences do not have such a subject.
- b. In order to overcome this problem facing the Opacity Condition Chomsky claims that there are structure-building rules which assign representations in logical form containing subjects to the sentences in (32).

- c. Chomsky introduces structure-building rules solely to protect the Opacity Condition from criticism that it fails to account for the application of Reciprocal Interpretation and Disjoint Reference in sentences such as (32). That is, he presents no independent justification for the existence of such rules.
- d. Chomsky claims that, unlike earlier decomposition rules, his structure-building rules do not lead to an undesirable extension of the formal power of linguistic theory. That is, he is not solving an empirical problem at the cost of creating a conceptual problem.

### 5.7 The elimination of the \* $[\overline{\text{NP to VP}}]$ filter

One of the important topics of discussion in (Chomsky 1980b) is the elimination of the \* $[\overline{\text{NP to VP}}]$  filter. In view of various problems with this filter, Chomsky (1980b:20ff.) attempts to develop an alternative to it. The Opacity Condition/SSC and NIC/PIC are relevant to this development, since they figure in the motivation provided for doing away with the filter.

The \* $[\overline{\text{NP to VP}}]$  filter, proposed by Chomsky and Lasnik (1977: 458) and presented as (36) below, explains the obligatory nature of control in structures such as (37).

(36) \* $[\overline{\alpha \text{ NP to VP}}]$ , unless  $\alpha$  is adjacent to and in the domain of Verb or *for* ( $[-\overline{\text{N}}]$ ).

(37) ...  $[\overline{\text{S}} \text{ wh-phras e } [\overline{\text{S}} \text{ NP to VP}]]$  ...

By convention, NP in filters is taken to be "lexical", i.e., containing lexical material or trace. In the immediate domain of a *wh*-phrase, as in (37), the \* $[\overline{\text{NP to VP}}]$  filter thus requires NP = PRO. In this way the filter explains the obligatory

character/ . . .

character of control in such structures, a property of control not explained by the binding conditions.

Chomsky (1980b:19-20) discusses a number of "metatheoretic" and "technical" problems raised by the \* $[NP \text{ to } VP]$  filter. One of the metatheoretic problems concerns a certain redundancy in the filter and the binding conditions. As Chomsky (1980b:19) puts it, "it {i.e., the \* $[NP \text{ to } VP]$  filter - M.S.} in effect recapitulates the basic content of the PIC and SSC (NIC and Opacity), in that it explicitly stipulates a property of subjects of infinitives".

The alternative to the \* $[NP \text{ to } VP]$  filter proposed by Chomsky comprises a rule of obligatory deletion in COMP up to recoverability,<sup>11)</sup> and a Case theory. The latter consists of the general principles (38) and the filter (39).

- (38) a. NP is oblique when governed by P and certain marked verbs;
- b. NP is objective when governed by V;
- c. NP is nominative when governed by Tense. (68)

- (39) \*N, where N has no Case. <sup>12)</sup> (70)

The Case theory can account for the obligatory character of control in structures such as (37). No Case will be assigned to the subject NP position in such structures. Consequently, given the filter (39), no lexical NP can appear in this position, i.e., PRO must appear.

Chomsky (1980b:27) claims that the Case theory manages to avoid the redundancy in the \* $[NP \text{ to } VP]$  filter. He argues that "the principle (68) [= (38) - M.S.] and (70) [= (39) - M.S.] do single out the subject of an infinitive, but indirectly, without the explicit redundancy of the \* $[NP\text{-to-}VP]$  filter, and on principled grounds, if (68) and (70) prove to be of some generality".

Notice that the redundancy in the \* $\overline{[NP\ to\ VP]}$  filter and the binding conditions differs from the redundancy exhibited by the SSC and PIC, and the reformulated version (7). In the latter case the problem is that the theory contains two mechanisms whose functions overlap. In the case of the filter, the problem is not that of two mechanisms having the same function. The filter and the binding conditions have distinct functions in the theory. The filter stipulates that only *PRO* can appear in the subject position of certain infinitives, while the binding conditions stipulate that the only position in which *PRO* and other anaphors can appear in embedded clauses is in the subject position of infinitives. The problem is rather that the filter and the binding conditions concern the same entity, an entity explicitly referred to by the filter, namely the subject position of infinitives.

The consideration of eliminating the redundancy in the \* $\overline{[NP\ to\ VP]}$  filter and binding theory is a conceptual consideration. The presence of this redundancy in UG leads to a conflict between UG and Chomsky's general assumption that the language faculty is a simple system, without redundancies.<sup>13)</sup> By eliminating the \* $\overline{[NP\ to\ VP]}$  filter, the redundancy is removed, and the conflict resolved.

The main points of this section are briefly summarized in (40).

- (40) a. The redundancy in the \* $\overline{[NP\ to\ VP]}$  filter and the binding conditions creates an external conceptual problem for UG, given Chomsky's assumption that the language faculty is a simple system without redundancies.
- b. The removal of the \* $\overline{[NP\ to\ VP]}$  filter from the theory eliminates the redundancy, and consequently also the conflict between UG and the relevant general assumption.

Footnotes to chapter 5

1. Chomsky (1980b:10) mentions another condition that restricts the binding of anaphors. This is the Command Condition, which stipulates that an antecedent must c-command its anaphor. The c-command requirement on anaphors is built into the definition of the notion 'bound' adopted by Chomsky. Cf. the discussion immediately below. Consequently, no separate Command Condition is required in Chomsky's theory. That Chomsky sees the binding theory as consisting only of the NIC and the Opacity Condition, is clear from his summary (1980b:38), where only these two conditions are mentioned as binding conditions.
2. Cf. in this connection Chomsky 1980b:10, 15, 39. Pronouns are partly like lexical NPs, and partly like anaphors. Cf. in this connection the discussion in the Appendix to (Chomsky 1980b), and in § 5.5 below. Except where otherwise noted, the definitions presented below are from (Chomsky 1980b:10).
3. The sentences in (5) were discussed above, where they were numbered 3.(3b), 3.(9b), and 3.(8b) respectively.
4. Cf. Chomsky 1980b:fn. 15 for a discussion of these cases.
5. Koster (1981:187) also points out that while the SSC and PIC can block only (i) below when formulated as conditions on rules relating two positions, they can block both (i) and (ii) when formulated as conditions on representations.
  - (i) \**They* said [<sub>S</sub> that *themselves* were happy]
  - (ii) \* [<sub>S</sub> *Themselves* were happy]

Chomsky (1980b) does not discuss this point, and it is unclear whether he would regard it as very significant. From

his discussion/ . . .

his discussion (1980b:9) of the language-specific rule for *each other*, it appears as if the need for an anaphor to have some antecedent follows from the relevant rule itself. The discussion by Chomsky (1981b:62) - where an interpretive principle for the anaphor *each other* is also mentioned - provides some support for this view.

6. Cf. also Koster's 1978b:38, 42 remarks on the role of the elimination of redundancies in linguistics. Koster usually enthusiastically and explicitly adopts Chomsky's methodology, and then tries to consciously apply it in his own work.
7. Cf. the discussion in § 4.4.4 above for details.
8. Cf. in this connection the definition of 'involve' presented in (Chomsky 1976a:316, fn. 22), and discussed in § 3.3.4 above.
9. What does not follow from the SSC and PIC is that NP *must* be PRO in these cases, i.e., that control is obligatory in an infinitive. See Chomsky 1980b:18ff. for a possible explanation of this property.
10. Cf. in this connection the discussion of lexical decomposition rules in (Chomsky 1972b).
11. Cf. Chomsky 1980b:21 for details on the application of the recoverability condition in this case.
12. Chomsky (1980b:25) makes the following comments on the notion 'government':

"The notion 'government' will no doubt be related to grammatical relations. In a configurational language such as English, we can specify it in terms of c-command, perhaps as follows:

- (69)  $\alpha$  is *governed* by  $\beta$  if  $\alpha$  is *c-commanded* by  $\beta$  and no major category or major category boundary appears between  $\alpha$  and  $\beta$ . 29)"

In footnote 29 Chomsky (i) explains that (69) builds in the "adjacency and c-command condition of the \* $[\text{NP to VP}]$  filter", (ii) explains that structures such as  $\beta \text{ } [\text{ } \gamma \alpha$  and  $\beta \text{ } \gamma \text{ } \alpha$  are excluded, where  $\gamma$  is a major category, (iii) points out that the notion 'government' must be defined at a level of abstraction that excludes from consideration parenthetical elements, interpolated adverbs, etc.

13. Cf. § 5.3 above for more discussion of the link between the relevant principle of Chomsky's linguistics and the elimination of redundancies.

Chapter 6/ . . .

## Chapter 6

### THE GB BINDING THEORY

#### 6.1 General remarks

Recall that "On binding" - referred to as (Chomsky 1980b) above - was written in 1978. From 1979 onwards, Chomsky argued for the replacement of the binding theory presented in (Chomsky 1980b) by a new binding theory, called a "government binding (GB) theory". The relevant works by Chomsky are the following.

- (i) "Markedness and core grammar" (henceforth (Chomsky 1981d)).

This paper was presented at the GLOW-conference in 1979. It contains a fairly detailed account of the reasons for developing an alternative to the binding theory of (Chomsky 1980b), as well as a brief outline of a possible alternative.

- (ii) "Principles and parameters in syntactic theory" (henceforth (Chomsky 1981b)).

This paper, which dates from the same year - 1979 - as "Markedness and core grammar", contains a brief and informal exposition of the relevant ideas.

- (iii) The Pisa lectures (henceforth (Chomsky 1979b)).

These lectures contain a detailed account of the proposed alternative binding theory, and of an important new principle of UG, the "empty category principle"/ECP.

- (iv) Lectures on government and binding (henceforth (Chomsky 1981a)).

In this work, the ideas contained in the works mentioned

above are brought together, and some proposals are made as to how these ideas can be modified. The work also provides a fairly detailed account of other principles of UG with which the binding theory interacts.

The aim of chapter 6 is to provide an account of the development of the new government-binding theory, insofar as it can be reconstructed from these works. The feature that distinguishes this fourth stage in the development of the SSC/Opacity Condition and the PIC/NIC, is the fact that these conditions are no longer stipulated as part of UG, but are made to follow from other more general principles.

In the discussions that follow the term "OB theory/framework" is used to refer to the overall theory of UG assumed in (Chomsky 1980b). The term "OB binding theory" refers to the binding theory which is incorporated in the OB theory. Other components of the OB theory are identified in the same manner, for example "OB Case theory". The term "GB binding theory" is used to refer to the binding theory which replaces the OB binding theory, and which forms part of the larger GB theory/framework. The use of the term "GB binding theory" must not be taken to imply that only one version of a government-binding theory is presented in the literature. In fact, there are several versions of such a binding theory. In the discussion that follows it will always be indicated explicitly which version of the GB binding theory it is that is being discussed.

The organization of chapter 6 is as follows. § 6.2 contains a detailed account of the GB binding theory presented in (Chomsky 1981a:183-209). As Chomsky's exposition of this version of the GB binding theory is much more detailed than that of any other version, it constitutes a useful starting point for the proposed analysis of the GB binding theory. In § 6.3 I consider the degree of success attained by this version of the GB binding theory in overcoming the conceptual and empirical problems which

triggered/ . . .

triggered the search for an alternative to the OB binding theory. Differences between this version of the GB binding theory and two earlier versions are analyzed in § 6.4. In § 6.5 the 1979 and the 1981 interpretations of the empirical differences between the OB and GB binding theories are compared. In § 6.6 some further modifications to the GB binding theory are discussed. In § 6.7 some remaining problems for the GB binding theory are briefly outlined. In § 6.8 the role of structure-building rules within the GB-framework is discussed.

## 6.2 The GB binding theory as an alternative to the OB binding theory

### 6.2.1 General remarks

As in the case of the earlier versions of the SSC and PIC, the OB binding theory and the GB binding theory must be seen within the context of the overall theory of UG of which they form part. According to Chomsky (1980b:3) the OB theory (of core grammar) has the following structure.

- (1) 1. Base rules
2. Transformational rules
  - 3a. Deletion rules
  - 3b. Construal rules
  - 4a. Filters
  - 4b. Interpretive rules
  - 5a. Phonology and stylistic rules
  - 5b. Conditions on binding rules

For our purposes, the following aspects of the OB theory are of special importance.

- (i) The conditions on binding in 5b. are the Opacity Condition and the NIC, that is, the OB binding conditions.
- (ii) Apart from the rule "Move  $\alpha$ ", the transformational component includes the Case principles, discussed in § 5.7 above.
- (iii) The OB theory incorporates the indexing conventions discussed in § 5.5 above.
- (iv) Among the filters in 4a. is the \* $[\hat{t}at-t]$  filter. Chomsky and Lasnik (1977:451) formulate this filter as follows:

(2) \* $[\bar{S} \text{ that } [_{NP} e] \dots ]$ , unless  $\bar{S}$  or its trace is in the context:  $[_{NP} \text{ NP } \underline{\quad} \dots ]$  {68}

The filter (2) will block structures such as (3a), but not (3b-c).

- (3) a. \*who do you think  $[\bar{S} \text{ that } [ [_{NP} e] \text{ saw Bill} ] ]$  {63'}
- b. the man  $[\bar{S} \text{ that } [ [_{NP} e] \text{ saw Bill} ] ]$  {67a}
- c. a book  $t$  arrived  $[\bar{S} \text{ that } [ [_{NP} e] \text{ may interest you} ] ]$  ( $t$  the trace of  $\bar{S}$ ) {67b}

Chomsky (1981a:18) uses the term "S-structure" to refer to the output of the transformational rules. The term "surface structure" is used to refer to the actual labelled bracketing of an expression at the level of phonetic form/PF. This terminological convention is adopted in the discussion below.

When viewed from the perspective of binding theory, the major differences between the OB and GB theories can roughly be summarized as follows.

- (4) a. The GB theory incorporates a new binding theory, in which the notion 'government' plays a central role.

The OB binding conditions follow from this new binding theory.

- b. The GB theory incorporates a new principle, the "empty category principle"/ECP.
- c. The indexing theory of the GB theory consists of the simplest possible convention, namely free (or random) indexing, as opposed to the complex indexing conventions of the OB theory.
- d. The \* $[\hat{t}hat-t]$  filter is not part of the GB theory.

As will become clear from the discussion below, it is these differences between the GB theory and the OB theory that are responsible for the fact that the GB theory succeeds in overcoming (at least some of) the conceptual and empirical problems of the OB theory. The main emphasis in the following sections will be on the GB binding theory, since it is this component of the GB theory that incorporates the OB binding conditions.

There are two factors that complicate the attempt to provide an account of the GB binding theory and the ECP. The first is the highly modular nature of the GB theory as a whole. Chomsky (1981a:135) characterizes this modularity as follows.

- (5) "The system that is emerging is highly modular, in the sense that the full complexity of observed phenomena is traced to the interaction of partially independent subtheories, each with its own abstract structure."

For example, the GB binding theory interacts closely with Case theory and government theory. At least some aspects of these theories must be presented in order to explicate the binding theory itself. Moreover, some of the conceptual problems of the OB theory are solved, not by the GB binding theory itself, but by other components of the GB theory, or by the GB binding theory in conjunction with such other components. The strategy that

I will follow is to provide only as much information about these interacting theories and principles as is needed for an understanding of the GB binding theory, referring the reader to the relevant sections of (Chomsky 1981a) for more detailed information.

The second factor that complicates the attempt to provide an account of the GB binding theory is the fact that several versions of the GB binding theory are presented in the relevant works by Chomsky. In the discussion that follows I will explicitly indicate, where relevant, which version of the GB binding theory it is that is being discussed.

### 6.2.2 The formulation of the GB binding theory

Chomsky (1981a:188) formulates the binding principles of the GB binding theory as follows:

- (6) "(A) An anaphor is bound in its governing category  
(B) A pronominal is free in its governing category  
(C) An R-expression is free." {12}

In § 6.2.3 the various notions that feature in (6) will be explicated. In § 6.2.3.1 I present Chomsky's definitions of the notions 'bound' and 'free'. In § 6.2.3.2 the three classes of nominal expressions referred to in the binding principles, viz. 'anaphor', 'pronominal', and 'R-expression', are defined. The notion 'governing category' is discussed in § 6.2.3.3.

The discussion in §§ 6.2.3 - 6.2.4, and the illustration of the application of the GB binding theory in § 6.2.5, are of necessity highly technical. For this I can offer no apology. Without this technical background Chomsky's claims about the merits of the GB binding theory, as well as the differences among the various versions of the GB binding theory, cannot be fully appreciated.

### 6.2.3 Definitions of central notions

#### 6.2.3.1 'Bound' and 'free'

Chomsky (1981a:184) distinguishes between two types of binding: A-binding and  $\bar{A}$ -binding. A-binding holds when the binder is in an A-position, while  $\bar{A}$ -binding holds when the binder is in an  $\bar{A}$ -position. He (1981a:184) provides the following formal definitions of 'X-bound' and 'X-free', where "X" can be replaced by "A" or " $\bar{A}$ ".

- (7) " $\alpha$  is X-bound by  $\beta$  if and only if  $\alpha$  and  $\beta$  are coindexed,  $\beta$  c-commands  $\alpha$ , and  $\beta$  is in an X-position." (3.2.3 (5i))
- (8) " $\alpha$  is X-free if and only if it is not X-bound." (3.2.3 (5ii))

A-positions are those positions in which arguments may appear in D-structure. Chomsky (1981a:35) uses the term "argument" to refer to expressions that are assigned  $\theta$ -roles, that is, thematic roles such as agent-of-action.<sup>1)</sup> Arguments thus include names (for example, *John*), variables (for example, the trace of a *wh*-phrase), anaphors (for example, *each other*), and pronouns (for example, *he*). Excluded are idiom chunks (for example, *too much* as in *too much has been made of this problem*), and elements inserted to occupy an obligatory position of syntactic structure (for example, *it* as in *it is certain that John will win*, and existential *there* as in *there are believed to be unicorns in the garden*). A-positions include the subject position, and the complements to  $\bar{X}$ .

Non-A-positions/ $\bar{A}$ -positions include the head of X, and adjuncts of any sort. If it is assumed that the operation performed by *wh*-Movement involves adjunction of the *wh*-phrase to COMP, then it follows that the position occupied by a *wh*-phrase in COMP is an  $\bar{A}$ -position.

The distinction between A-binding and  $\bar{A}$ -binding can be illustrated with the following structure.

(9)  $who \left[ {}_S t \text{ seemed } \left[ {}_S t' \text{ to have been killed } t'' \right] \right]$  {3.2.3 (4)}

$t$  is  $\bar{A}$ -bound by *who*, since *who* is in an  $\bar{A}$ -position.  $t$  is in an A-position, namely that of subject. Thus,  $t'$  is  $\bar{A}$ -bound by *who* and A-bound by  $t$ . Similarly,  $t''$  is  $\bar{A}$ -bound by *who*, and A-bound by  $t$  and  $t'$ .

The GB theory is a theory of A-binding. That is, the terms "bound" and "free" in (6) are synonymous with "A-bound" and "A-free" respectively.

#### 6.2.3.2 'Anaphor', 'pronominal', 'R-expression'

Principle (6A) of the GB binding theory applies to anaphors. Chomsky (1981a:188) declares that "intuitively anaphors are NPs that have no capacity for 'inherent reference'". Two types of anaphors are distinguished: lexical anaphors, such as reciprocals, and NP-trace.

Principle (6B) of the GB binding theory applies to pronominals. Chomsky (1981a:102) informally characterizes pronominals as elements that have "the features gender, number and person, and perhaps other grammatical features, but not those of overt anaphors or R-expressions". Two classes of pronominals are distinguished: pronouns, which have a phonological matrix, and PRO, which lacks a phonological matrix.<sup>2)</sup>

Principle (6C) of the GB binding theory applies to R-expressions. According to Chomsky (1981a:102), this category includes "noun phrases with heads that are in some intuitive sense 'potentially referential' (for example, *John*, *wood*, *sincerity*, *book*; etc.) and variables . . .". Chomsky (1981a:185) defines the notion 'variable' as follows:

- (10) "α is a variable if and only if (3.2.3 (6))
- (i) α = [NP e]
  - (ii) α is in an A-position (hence bears an A- GF)
  - (iii) there is a β that locally  $\bar{A}$ -binds α."<sup>3)</sup>

According to Chomsky (1981a:102, 185), the possible binders β of α include *wh*-phrases and their traces, quantifier expressions (for example, *everyone*) and an empty element in COMP. Chomsky (1981a:185) provides the following definitions of the notions 'locally bound' and 'locally X-bound'.

- (11) "α is locally bound by β if and only if α (3.2.3 (5iii))  
is X-bound by β, and if γ Y-binds α then  
either γ Y-binds β or γ = β."
- (12) "α is locally X-bound by β if and only if (3.2.3 (5iv))  
α is locally bound and X-bound by β."

In (11) "X" and "Y" may be independently replaced by "A" or " $\bar{A}$ ". In (12) "X" may be replaced by "A" or " $\bar{A}$ ". In the structure (9), *t* is a variable: it is [NP e], it is in an A-position, and it is locally  $\bar{A}$ -bound by the *wh*-phrase in COMP.

### 6.2.3.3 'Governing category'

The notion 'government' plays an important role in the GB theory, and various definitions of this notion are considered in (Chomsky 1981a:§3.2.1).<sup>4)</sup> I will consider only the final definition presented by Chomsky (1981a:§3.2.1), namely the definition presented in (11). Where it becomes relevant in subsequent sections, I will compare the consequences of this definition with those of earlier definitions.

According to Chomsky (1981a:165), α governs γ in (13).

(13) "[ $\beta \dots \gamma \dots \alpha \dots \gamma \dots$ ], where (3.2.1 (11),

- (i)  $\alpha = X^0$
- (ii) where  $\phi$  is a maximal projection, if  $\phi$  dominates  $\gamma$  then  $\phi$  dominates  $\alpha$ .
- (iii)  $\alpha$  c-commands  $\gamma$ ."

In terms of (13i),  $\alpha$  is N, V, A, or P. Chomsky (1981a:25) assumes that the expansion of S in English is "NP INFL VP". INFL may in principle be analyzed as [ $\pm$  Tense], (AGR)]. AGR represents the complex of features person, gender, and number. It is identified with PRO, and hence is a lexical category, N.<sup>5)</sup> AGR is thus also a proper choice for  $\alpha$  in (13). In terms of (13ii), the maximal projections ( $\bar{S}$ , NP, AP, PP, VP) are absolute barriers to government.<sup>6)</sup> No such maximal projection may dominate  $\gamma$ , unless it also dominates  $\alpha$ , the governor. 'c-command' is defined as follows.

(14) " $\alpha$  c-commands  $\beta$  if and only if (3.2.1 (12))

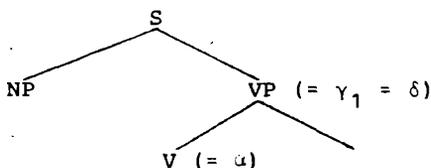
- (i)  $\alpha$  does not contain  $\beta$
- (ii) Suppose that  $\gamma_1, \dots, \gamma_n$  is the maximal sequence such that

- (a)  $\gamma_n = \alpha$
- (b)  $\gamma_i = \alpha^j$
- (c)  $\gamma_i$  immediately dominates  $\gamma_{i+1}$

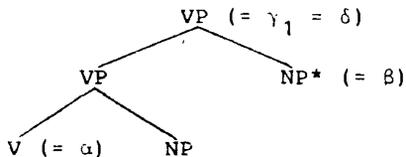
Then if  $\delta$  dominates  $\alpha$ , then either (I)  $\delta$  dominates  $\beta$ , or (II)  $\delta = \gamma_i$  and  $\gamma_i$  dominates  $\beta$ ."

This notion 'c-command' can be illustrated with reference to the following structures.

(15)



(16)



In (15) V does not c-command NP, because VP (=  $\gamma_1 = \delta$ ) does not dominate NP. In (16) V c-commands NP\*, since VP (=  $\gamma_1 = \delta$ ) dominates this NP (=  $\beta$ ). V in (16) also c-commands NP.

Given these definitions, instances of government such as those in (17) can be distinguished.<sup>7)</sup>

- (17) a.  $[\text{VP } V \text{ NP } (\text{NP}')] \quad (3.2.1 \text{ (ii)})$   
 b.  $[\text{PP } P \text{ NP}] \quad (3.2.1 \text{ (iii)})$   
 c.  $[\text{S } \text{for } [\text{S } \text{NP}_1 \text{ to } [\text{VP } V \text{ NP}_2]]]] \quad (3.2.1 \text{ (iii)})$

In (17a) V governs both NP and NP'. In (17b) P governs NP. In (17c) the prepositional complementizer *for* governs NP<sub>1</sub>. *for* does not govern NP<sub>2</sub>, since the maximal projection VP dominates NP<sub>2</sub> but not *for*. NP<sub>2</sub> is governed by V.

The GB binding principles (6) specify the conditions for boundedness within *governing* categories. Chomsky (1981a:188) defines 'governing category' as follows.

- (18) "α is the governing category for β if and only if α is the minimal category containing β and a governor of β, where α = NP or S." 3.2.3 (11)

In (17c), for example, S is the governing category for NP<sub>2</sub>, since S is the minimal NP or S that contains both NP<sub>2</sub> and its governor, V.

Like the GB binding theory, the GB Case theory is closely linked to government theory. Chomsky's (1981a:170) formulations of

the fundamental/ . . .

the fundamental properties of Case assignment clearly demonstrate the link between Case theory and government theory.

- (19) " (i) NP is nominative if governed by AGR {3.2.2 (1)}  
(ii) NP is objective if governed by V with the subcategorization feature:  
    \_\_\_ NP (i.e., transitive)  
(iii) NP is oblique if governed by P  
(iv) NP is genitive in [ NP \_\_\_  $\bar{X}$  ]  
(v) NP is inherently Case-marked as determined by properties of its [ - N ] governor."

The Case theory also includes the following Case filter.<sup>8)</sup>

- (20) "\*NP, where NP has a phonetic matrix but no Case." {3.2.2 (15)}

Chomsky (1981a:183) states that Case theory actually forms part of the theory of government. As he puts it, "the basic and central instances of Case assignment are instances of government by a Case-assigner".

The fact that both the GB binding theory and the GB Case theory are closely linked to government theory, enables the GB theory to overcome some of the conceptual problems of the OB theory, as will be shown in § 6.3.3 below.

#### 6.2.4 Indexing in the GB theory

The GB theory incorporates a very simple indexing theory. It is assumed (Chomsky 1981a:185) that coindexing of a moved element and its trace is, by convention, part of the rule "Move  $\alpha$ ". All other indexing is free. According to Chomsky (1981a:186), it might even be assumed that traces and moved elements are freely indexed at S-structure. All cases of improper indexing are

ruled out/ . . .

ruled out by independent conditions, including the binding conditions, Subjacency, and the ECP.<sup>9)</sup>

Chomsky (1981a:186-187) claims that the same indexing theory applies to pronouns. Thus pronouns are "proximate" when co-indexed with an antecedent (for example, *his* in *John lost his way*), and "obviative" if not coindexed with an antecedent (for example, *him* in *John saw him*). The GB indexing theory thus differs from the OB indexing theory with respect to the indexing of pronouns. In terms of the OB theory, anaphoric and referential indices are assigned to pronouns and names to account for the proximate-obviative distinction, and more generally, for disjoint reference. Only referential indices, in the sense of the OB theory, are assumed under the GB theory.<sup>10)</sup>

## 6.2.5 An illustration of the GB binding theory

### 6.2.5.1 General remarks

Having defined the notions that feature in the GB binding theory, it is now possible to take a closer look at the application of this theory. In § 6.2.5.2 the application of this theory in clauses is illustrated and in § 6.2.5.3 its application in NPs. Differences between the predictions made by the OB binding theory and those made by the GB binding theory will be noted where relevant. The main focus in both these sections is on principles (6A) and (6B), since it is from these principles that the SSC/Opacity Condition and NIC follow.

One further preliminary point: Chomsky (1981a:225, fn. 35) stresses that the binding principles as formulated in (6) do not presuppose that  $\alpha$  has a governing category. That is, the principles in (6) must be understood as follows.

(21) "Let  $\beta$  be a governing category for  $\alpha$ . Then

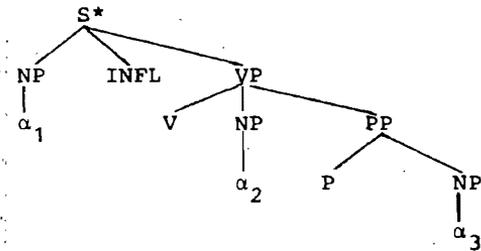
(A) / . . .

- (A) if  $\alpha$  is an anaphor, it is bound in  $\beta$
- (B) if  $\alpha$  is a pronominal, it is free in  $\beta$
- (C) if  $\alpha$  is an R-expression, it is free."

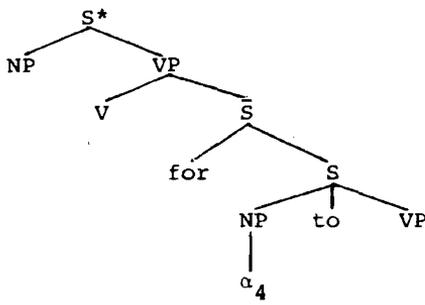
6.2.5.2 The application of the GB binding theory in clauses

The GB binding theory applies in clauses with the following basic structures, where  $\alpha_n$  stands for any of the categories to which the binding principles apply.

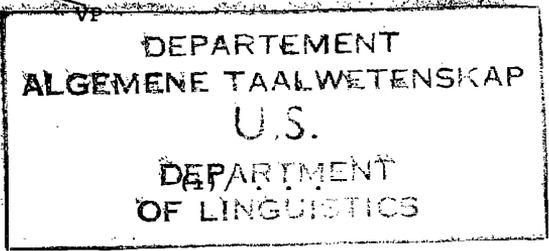
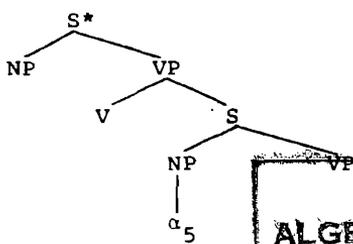
(22) a. (3.2.3 (13i))



b. (3.2.3 (13ii))



c. (3.2.3 (13iii))



(i) *The overt anaphors*

Overt anaphors, such as *each other*, have phonetic content. They must therefore be assigned Case, by the Case filter (20). Overt anaphors must thus have a governing category in all the structures of (22). By binding principle (6A), any overt anaphor must be bound in its governing category.

Suppose  $\alpha_1 = \textit{each other}$ ; INFL = [ [+ Tense] AGR ]. Then *each other* is governed by INFL, and its governing category is S\*.<sup>11)</sup>

(23) \*we thought [ S\* *each other* gave the books to Bill ]<sup>12)</sup>

In violation of (6A), *each other* is free in S\*. Consequently, (23) is unacceptable. Note that sentences such as (23) are also ruled out by the NIC.

Suppose that *each other* is the object of a verb or preposition in VP' - i.e.,  $\alpha_2$  or  $\alpha_3$  in (22a).

- (24) a. \*they expected [ S\* me to introduce *each other* to Bill ]  
 b. \*they expected [ S\* me to point the gun at *each other* ]

In (24a) *each other* is governed by the verb *introduce*, and in (24b) by the preposition *at*. In both cases, therefore, S\* is the governing category. In both (24a) and (24b) *each other* must be bound by the subject of S\*, *me*. However, *me* is not a possible antecedent for *each other*. Consequently, (24a) and (24b) are ruled out by principle (6A) of the binding theory. Note that (24a, b) are ruled out by the SSC/Opacity Condition as well.

Consider next sentences with *each other* in the subject position of an infinitive.



from positions  $\alpha_3$  and  $\alpha_4$  of (22) by other conditions.<sup>16)</sup> Consider the following sentences, with  $t = \alpha_1$  in (27),  $t = \alpha_2$  in (28), and  $t = \alpha_5$  in (29).<sup>17)</sup>

- (27) \*they are believed [<sub>S\*</sub> t are competent] (2.4.4 (17iv))
- (28) a. [<sub>S\*</sub> John was killed t] (2.4.4 (18ia))  
 b. \*they seem [<sub>S\*</sub> Bill likes t] (2.4.4 (17v))
- (29) a. [<sub>S\*</sub> they seem [t to be competent] (2.4.4 (17i))  
 b. [<sub>S\*</sub> they are believed [t to be competent] (2.4.4 (17ii))

In (27)  $t$  is governed by INFL, with  $S^*$  the governing category. In violation of principle (6A)  $t$  is free in this governing category. Thus (27) is unacceptable. This sentence is also ruled out by the NIC.

In (28)  $S^*$  is the governing category,  $t$  being governed by  $V$  in each case. In (28a)  $t$  is bound in  $S^*$ , as required by principle (6A). Thus (28a) is acceptable. In (28b)  $t$  is free in  $S^*$ , in violation of principle (6A). Thus (28b) is unacceptable. (28b) is also ruled out by the SSC/Opacity Condition.<sup>18)</sup>

In (29)  $t$  is governed by the matrix verb, thus  $S^*$  is the governing category. In both cases  $t$  is bound in  $S^*$ , as required by principle (6A). Thus (29a) and (29b) are acceptable.

The examples (27) - (29) illustrate that the NIC and SSC/Opacity Condition follow from principle (6A) of the GB binding theory in the case of NP trace in clauses.

(iii) *The overt pronominals, that is, pronouns*

A pronoun is necessarily Case-marked, because it has phonetic content. It thus has a governing category, in which it must be

free/ . . .



(iv) *The non-overt pronominal PRO*

Let us now consider the case of a pronominal without a phonetic matrix, that is, PRO. According to Chomsky (1981a:191), it is reasonable to regard PRO as a *pronominal anaphor*, since it is like overt pronouns in some respects, and like anaphors in others.<sup>19)</sup> PRO is like the overt pronouns in that it never has an antecedent within its own clause or NP. PRO is like the anaphors in that it has no intrinsic referential content, but is either assigned reference by an antecedent or is indefinite in interpretation, lacking specific reference. If PRO is indeed a pronominal anaphor, then it is subject to both binding principle (6A) and binding principle (6B): to (6A), because it is an anaphor, and to (6B), because it is a pronoun. By (6A) PRO must be bound in its governing category. By (6B) PRO must be free in its governing category. We thus have a contradiction if PRO has a governing category. Therefore, PRO cannot have a governing category, i.e., PRO must be ungoverned. The following principle, which expresses the "essential property" of PRO, is thus derived from the GB-binding theory.<sup>20)</sup>

(31) PRO is ungoverned

{3.2.3 (20)}

The binding theory also determines that the positions of PRO are essentially those of the other anaphors. To see in more detail how the GB binding theory determines the basic properties of PRO, consider the following sentences with the structures of (22), where  $\alpha$  = PRO.

- (32) a. \*it is unclear [how [<sub>S\*</sub> PRO solved the problem] ]  
           (PRO =  $\alpha_1$ ) (2.4.2 (26i))
- b. \*it is unclear [how [<sub>S\*</sub> to solve PRO] ]  
           (PRO =  $\alpha_2$ ) (2.4.2 (26ii))
- c. \*it is unclear [what [<sub>S\*</sub> to give t to PRO] ]  
           (PRO =  $\alpha_3$ ) (2.4.2 (26iv))

d./ . . .

- d. \* $[S^* \text{ I'm eager } [for \ [PRO \text{ to take part}]]]$   
 (PRO =  $\alpha_4$ )
- e. \* $[S^* \text{ I believe } [PRO \text{ to be incompetent}]]]$   
 (PRO =  $\alpha_5$ )
- f. John tried  $[PRO \text{ to win}]$   
 (PRO =  $\alpha_5$ ) (2.4.2 (8))

In (32a-e) PRO is governed: in (32a) by INFL, in (32b) by V, in (32c) by the preposition *to*, in (32d) by the prepositional complementizer *for*, in (32e) by the matrix verb *believe*. In all these cases PRO thus has a governing category. (32a-e) are thus ruled out by the GB binding theory, since it follows from this theory that PRO cannot have a governing category. In (32f) PRO is ungoverned. As predicted, this sentence is acceptable.

(v) *R-expressions*

Principle (6C) of the GB binding theory applies to R-expressions, i.e., to names and variables. Principle (6C) stipulates that R-expressions must be free. The application of (6C) to names is illustrated in (33), and its application to variables in (34).

- (33) a. he said that John would win (3.2.3 (25))  
 b. John said that John would win
- (34) a. who did he say Mary had kissed (3.2.3 (26))  
 (for which  $x$ , he said Mary had kissed  $x$ )  
 b. who did he say had kissed Mary  
 (for which  $x$ , he said  $x$  had kissed Mary)

If there is no emphatic stress, the embedded occurrence of *John* in (33) is understood as distinct in reference from the matrix subject. That is, this occurrence of *John* is free, as required by (6C).

In (34) / . . .

In (34), *he* cannot be replaced by the variable *x* in the associated LF-representation. That is, (35a, b) are not possible interpretations of (34a, b), respectively.

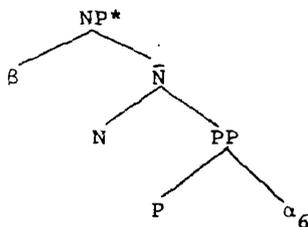
- (35) a. for which *x*, *x* said Mary had kissed *x*      {3.2.3 (27)}  
 b. for which *x*, *x* said *x* had kissed Mary

This follows from principle (6C), which stipulates that variables must be free.

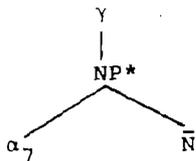
### 6.2.5.3 The application of the GB binding theory in NPs

The GB binding theory applies in NPs with the following basic structures.

- (36) a. {3.2.3 (56i)}



- b. {3.2.3 (56ii)}



According to Chomsky (1981a:207), principle (6C) of the GB binding theory applies unproblematically to arguments in NP. The present discussion will be mainly concerned with the principles (6A) and (6B) of this binding theory, from which the OB binding conditions are supposed to follow. I will now illustrate the application of these two principles of the GB binding theory to arguments in NP. The predictions made by these

principles/ . . .

principles will be compared with those of the OB binding theory. Notice that only the SSC/Opacity Condition is relevant here, since there is no Tense within NP.

(i) *Anaphors*

The non-overt anaphor, NP-trace, is excluded from the positions  $\alpha_6$  and  $\alpha_7$  by independent conditions.<sup>21)</sup> Only overt anaphors, such as *each other*, need therefore be considered.

Consider, firstly, sentences with *each other* in the position of  $\alpha_6$ .<sup>22)</sup>

- (37) a. [NP\* their stories about each other] (3.2.3 (57i))  
b. \*we heard [NP\* his stories about each other]<sup>23)</sup>  
c. we heard [NP\* some stories about each other] (3.2.3 (57iii))  
d. we heard [NP\* the stories about each other (that are being circulated)] (3.2.3 (57iv))  
e. we thought [that [NP\* pictures of each other] would be on sale] (3.2.3 (58))

The SSC/Opacity Condition of the OB binding theory makes the following predictions about these sentences. In (37a) *each other* is bound in the domain of the subject  $\beta$  (= *their*) of NP\*. The SSC/Opacity Condition thus correctly predicts its acceptability. In (37b) *each other* is free in the domain of the subject  $\beta$  (= *his*) of NP\*. The SSC/Opacity Condition correctly predicts its unacceptability. In (37c) and (37d) NP\* has no subject. *each other* can thus be free in NP\*. The SSC/Opacity Condition correctly predicts the acceptability of these sentences. Similarly in the case of the more complex example (37e). NP\* has no subject, and it is itself the subject of the embedded sentence. The SSC/Opacity Condition thus correctly predicts the acceptability of (37e), with *each other* free in NP\*.

Principle (6A) of the GB binding theory makes rather different predictions about these sentences. In each case *each other* is governed by P. NP\* is thus the governing category of *each other*. By principle (6A) *each other* must be bound in NP\*. In (37a) *each other* is bound in NP\*. (6A) thus correctly predicts the acceptability of this sentence. In (37b) *each other* is free in NP\*. (6A) correctly predicts the unacceptability of this sentence. In (37c-e) *each other* is free in its governing category NP\*. Principle (6A) thus incorrectly predicts that these sentences will be unacceptable. The GB binding theory in fact makes the right predictions only in those cases where NP contains a subject.

Chomsky (1981a:208) states that (37e) is "perhaps somewhat marginal and may be a marked construction, as consideration of some other languages suggests . . .". If (37e) is marked, then the wrong prediction of the GB binding theory with regard to it does not represent a real problem for this theory. However, Chomsky claims that (37c) "in English it surely has a different status from such violations of the SSC as (59) (= (38) - M.S.)".

(38) we thought [that [John's pictures of each other] would  
be on sale] (59)

The predictions of the OB and GB binding theories about the sentences in (37) are presented schematically in (39), (where "\*" before a number indicates unacceptability of the relevant sentence, "/" indicates a correct prediction of (un)acceptability, and "x" indicates a wrong prediction).

(39) / . . .



{3.2.1 (11)}, presented as (13) above, does not require that  $\alpha$  must be an immediate constituent of  $\beta$ . The same holds for definition {3.2.1 (6)}.<sup>24</sup> Definition {3.2.1 (4)},<sup>25</sup> however, does require that  $\alpha$  must be an immediate constituent of  $\beta$ .

Suppose that  $\alpha_7$  is ungoverned, and thus lacks a governing category (though it has Case). Principle (6A) then makes the right prediction for (40a). NP\* is not a governing category for *each other*, so that *each other* can be free in NP\*. In (40b) *each other* has no governing category. Principle (6A) thus incorrectly predicts that (40b) will be acceptable.

Suppose instead that  $\alpha_7$  is governed by the head of  $\bar{N}$ . *each other* then has a governing category, NP\*, in which it must be bound by principle (6A). In both (40a) and (40b) *each other* is free in NP\*. Principle (6A) thus incorrectly predicts that (40a) will be unacceptable, and correctly predicts that (40b) will be unacceptable, though, according to Chomsky (1981a:208), (40b) is barred "for what seem to be the wrong reasons".

Problems also arise when we have [<sub>VP</sub> V-ing ...] in place of  $\bar{N}$  in (36b).

(42) \*they preferred [<sub>NP\*</sub> each other's reading  
the book] {3.2.3 (62)}

As was pointed out above, the SSC/Opacity Condition makes the wrong prediction about sentences such as (42). *each other* is not in the domain of the subject of NP\*, and it is bound in the domain of the subject (= *they*) of the sentence. The SSC/Opacity Condition thus incorrectly predicts that (42) will be acceptable. The GB binding theory also makes a wrong prediction. Under none of the concepts of government considered by Chomsky, is *each other* governed in (42), VP being a barrier to government. Thus, *each other* has no governing category in (42). Principle (6A) thus incorrectly predicts that (42) will be acceptable.

Chomsky (1981a:208) suggests that examples such as (42) "may not be crucial, since it might relate to the plurality requirement for reciprocals . . .". In footnote 57 Chomsky (1981a:228) briefly discusses and illustrates the latter requirement in English. He points out that sentences such as (43) are unacceptable, because of a kind of plurality requirement elsewhere in the sentence imposed by *each other*.

- (43) a. \*they read each other's book  
 b. \*they saw a picture of each other  
 c. \*they turned the child against each other

All these become acceptable if *book*, *picture*, and *child* are made plural. The situation in English is more complex, however. Sentences such as (43c), with other lexical material, can be acceptable.

- (44) they kicked the ball towards each other

Apparently there is also the possibility of interlanguage variation with regard to the plurality requirement. Referring to personal communications by Lauri Carlson and Tarald Taraldsen, Chomsky notes that this requirement does not hold for Finnish and Norwegian. Chomsky (1981a:228, fn. 57) states that "further investigation is necessary to determine the character and parameters of these constructions". Chomsky (1981a:§5.2) makes some further comments about these constructions - see § 6.7 below for a discussion of these comments.

There is one further problem concerning reciprocals in the position of  $a_7$  in constructions such as (36b) that Chomsky (1981a: 222, fn. 3) notes. As we have seen, sentences like (40b) are correctly ruled out by the SSC/Opacity Condition, and by the GB binding theory, under one possible definition of 'government'. In Dutch, however, the analogue to (40b) is acceptable.

The predictions/ . . .

The predictions of the OB and GB binding theories about the sentences (40) and (42) are presented schematically in (45).

(45)

	OB binding theory	GB binding theory	
		$\alpha_7$ governed	$\alpha_7$ ungoverned
(40a)	✓	x	✓
* (40b)	✓	✓	x
* (42)	x	x	x

The SSC/Opacity Condition thus does not follow from the GB binding theory in the case of overt anaphors in NP.

(ii) *Pronominals*

Consider, firstly, the non-overt pronominal, PRO.

- (46) a. \*they expected that [<sub>NP</sub> pictures of PRO]  
would be on sale (PRO =  $\alpha_6$ ) {3.1 (7i)}
- b. \*I like [<sub>NP</sub> PRO book] (PRO =  $\alpha_7$ ) {2.4.2(3i)}
- c. I'd much prefer [<sub>NP</sub> PRO going to a movie]  
(PRO =  $\alpha_7$ , and [<sub>VP</sub> V-ing ...] in place  
of  $\bar{N}$ ) {2.4.2 (1ii)}

The OB binding theory, specifically the SSC/Opacity Condition, makes the wrong predictions about (46a) and (46b). In both sentences PRO is bound in the domain of the subject of the sentence (*they* and *I*, respectively), and not free in the domain of the subject of NP. The SSC/Opacity Condition thus incorrectly predicts that (46a) and (46b) will be acceptable. The SSC/Opacity Condition correctly predicts that (46c) will be acceptable. PRO is not in the domain of a subject of NP, and is bound in the domain of the subject *I* of the sentence.

The GB binding theory makes the correct prediction about (46a).

In this/ . . .

In this sentence PRO is governed by *of*, and thus has a governing category. The GB binding theory also makes the right prediction about (46b), if it is assumed that the position  $\alpha_7$  in (46b) is governed. If, however, this position is ungoverned, the GB binding theory wrongly predicts the acceptability of (46b).<sup>26)</sup> The GB binding theory makes the right prediction about (46c). The position of PRO is ungoverned. PRO thus has no governing category and it is correctly predicted that (46c) will be acceptable. The predictions of the OB and GB binding theories about the sentences (46) are presented schematically in (47).

(47)

	OB binding theory	GB binding theory
* (46a)	x	✓
* (46b)	x	✓ ≠
(46c)	✓	✓

(≠ This is the prediction if PRO is governed. If PRO is ungoverned, the GB binding theory also makes the wrong prediction. Chomsky himself does not mention this possibility. He (1981a: 208) simply says that PRO is excluded "from the governed positions  $\alpha_6$  and  $\alpha_7$  . . .")

Consider, secondly, the overt pronominals, i.e., pronouns. In (48), pronouns appear in the position  $\alpha_6$  of (36a), with *him* proximate to *John*.

- (48) a. John saw [<sub>NP\*</sub> my picture of him] {3.2.3 (63)}  
 b. \*I saw [<sub>NP\*</sub> John's picture of him]  
 c. \*John saw [<sub>NP\*</sub> a picture of him]  
 d. John thought I saw [<sub>NP\*</sub> a picture of him]

Referring to sentences like (48c), Chomsky (1981a:222, fn. 2) observes that "for some reason, disjoint reference seems less

than obligatory/ . . .

than obligatory in many such cases . . .". However, it is clear from his remarks on p. 209, directly below the sentences (63)/(48), that he considers (48) to be "ungrammatical".

The SSC/Opacity Condition makes the right predictions about all the sentences in (48). In (48a) *him* is free in the domain of the subject *my* of NP\*. In (48b) *him* is bound in the domain of the subject of NP\*, given that it is proximate to *John*. In (48c) *him* is bound in the domain of the subject of the sentence. In (48d) *him* is free in the domain of the embedded subject *I*.

The GB binding theory makes the right predictions about (48a, b, d), but the wrong prediction about (48c). In all cases *him* is governed by *of*, with NP\* as its governing category. By principle (6B), *him* must be free in NP\*. In (48a) and (48d) *him* is free in NP\*, and the GB binding theory correctly predicts that these sentences will be acceptable. In (48b) *him* is bound in NP\*, and the theory correctly predicts that this sentence will be unacceptable. In (48c) *him* is free in NP\*, and the GB binding theory incorrectly predicts that (48c) will be acceptable with *him* proximate to *John*.

In (49) *his* appears in the position of  $\alpha_7$  of (36b), and *his* is proximate to *John*. In (49c)  $\bar{N}$  of (36b) is replaced by VP.

- (49) a. John read [NP\* his book] . . . (3.2.3 (64))  
 b. John thought I saw [NP\* his book]  
 c. \*John preferred [NP\* his reading the  
 book] . . . (3.2.3 (65))

The SSC/Opacity Condition incorrectly predicts that (49a) will be unacceptable in the relevant interpretation, since *him* is bound in the domain of the subject *John*. It correctly predicts the acceptability of (49b), with *his* free in the domain of the embedded subject *I*. In (49c) *his* is bound in the domain of the subject *John*, and the SSC/Opacity Condition correctly predicts that this sentence will be unacceptable.

Let us/ . . .

Let us now consider the predictions which the GB binding theory makes about these sentences. Suppose that *his* is governed in (49a, b), i.e., that NP\* is a governing category for *his*. By principle (6B), *his* must then be free in NP\*, which is the case. The GB binding theory thus correctly predicts the acceptability of (49a, b). If *his* is ungoverned, the same predictions are made. *his* will then have no governing category. The GB binding theory predicts that (49c) will be acceptable, since *his* is not governed in NP\*, and thus has no governing category. However, (49c) poses no real problem for the GB binding theory, since its acceptability follows from another principle, the Avoid Pronoun Principle.<sup>27)</sup>

The predictions of the OB and GB binding theories about the sentences (48) and (49) are presented schematically in (50).

(50)

	OB binding theory	GB binding theory
(48a)	✓	✓
* (48b)	✓	✓
* (48c)	✓	x
(48d)	✓	✓
(49a)	x	✓
(49b)	✓	✓
* (49c)	✓	Avoid Pronoun

The SSC/Opacity Condition thus does not follow in full from the GB binding theory in the case of pronouns in NP. In some cases, viz. (46a), (46b), and (49a), where the OB binding theory makes the wrong predictions, the GB binding theory makes the right predictions. In the case of (48c), however, the situation is reversed.

### 6.3 The GB binding theory and the empirical and conceptual problems of the OB binding theory

#### 6.3.1 General remarks

Chomsky (1981a:157-161) and (1981d:128-132) discusses six so-called conceptual problems of the OB theory, the solution of which motivated the development of an alternative theory.<sup>28)</sup> These conceptual problems are all in some way related to the OB binding theory. In § 6.3.3 these problems are analyzed, and it is considered whether the GB binding theory succeeds in solving these problems. Since the discussion of the conceptual problems in questions in (Chomsky 1981a) is virtually identical to that in (Chomsky 1981d), I will refer only to the former work.

From the discussion in (Chomsky 1981d:128ff.) and (Chomsky 1979b: 7), it is clear that the above-mentioned conceptual problems, rather than any empirical problems of the OB theory, played the major role in motivating the search for an alternative to this theory. However, this does not imply that empirical considerations played no role. Thus Chomsky (1981d:128) refers to the "inevitable problems of empirical adequacy" of the OB theory. The latter work in fact contains a fairly detailed examination of empirical differences between the OB theory and the GB theory. For example, it is argued (Chomsky 1981d:140) that the GB binding theory overcomes one of the empirical problems of the OB binding theory. Chomsky (1981a:§3.1) contains a brief discussion of some empirical problems of the OB theory, in addition to the conceptual problems of the theory. In § 6.3.2 I briefly outline these problems, and consider to what extent the GB binding theory succeeds in solving these problems.

#### 6.3.2 Some empirical problems of the OB binding theory

Chomsky (1981a: 3.1) mentions what he calls "certain technical

problems/ . . .

problems" of the OB theory. Closer analysis reveals that these "technical" problems are in fact empirical problems. This interpretation is supported by Chomsky's (1981a:187) reference to the conceptual and *empirical* problems discussed in § 3.1 of his book.

The first empirical problem mentioned by Chomsky (1981a:155) concerns structures of the form  $[\gamma \dots [_{NP} \alpha [_{VP} V\text{-ing} \dots]]]$  with an overt anaphor in the position of  $\alpha$ . (51) is an example of such a sentence.

(51) \*we preferred  $[_{NP}$ \* each other's reading the {3.1 (3)} book]

*each other* (=  $\alpha$ ) is not free in the domain of the subject (= *we*) of  $\gamma$  (= the matrix S), since it is coindexed with *we*. The OB binding theory thus incorrectly predicts that (51) is acceptable.

As was pointed out in § 6.2.5.3 - see in particular the discussion immediately following (42) - the GB binding theory also makes a wrong prediction about such sentences. However, Chomsky (1981a:208) suggests that there is an alternative explanation available for sentences like (51), in the form of the plurality requirement. If this alternative explanation is adopted, then sentences such as (51) and (42) does not constitute counter-examples to either the OB binding theory or the GB binding theory.

With  $\alpha$  in structures such as  $[_{NP} \alpha \bar{N}]$  a pronoun, the OB theory once again makes the wrong predictions.<sup>29)</sup>

(52) they read [their books]

*their* (=  $\alpha$ ) in (52) is in the domain of the subject *they*. The SSC/Opacity Condition thus predicts that it must be free in (52). Nevertheless, *their* and *they* in (52) can be coindexed. As explained in § 6.2.5.3 - see especially the discussion immediately following (49) - the GB binding theory makes the correct predictions in such cases.

Chomsky (1981a:156) mentions some differences between PRO, on the one hand, and NP-trace and overt anaphors, on the other hand, that are left unexplained by the OB theory. The positions in which PRO appears are determined to a large extent by the binding principles for anaphors. Like other anaphors, PRO may appear as the subject of an infinitive or NP. However, in the case of "long distance control" the antecedent-anaphor relation of PRO is not subject to these binding conditions. Thus, consider the sentence (53), in which PRO is controlled by *they* over more than one clause boundary.

(53) they thought I said that [PRO <sup>{to feed}</sup><sub>{feeding}</sub> each other] would be difficult (3.1 (5))

In (53) PRO is bound by *they*, but is free in the domain of the subject *I*. The SSC/Opacity Condition thus wrongly predicts that (53) is unacceptable. Chomsky (1981a:222, fn. 3) claims that (53) contrasts with the unacceptable (54), with the overt anaphor *each other* in place of PRO.

(54) \*they thought I said that pictures of each other were on sale

Chomsky admits that judgments in the case of examples such as (54) are "not very solid". He nevertheless thinks that "there is a difference of judgment in the assumed direction".

The OB theory fails to explain why PRO can appear in certain positions from which trace is excluded. ("\*" indicates that trace is excluded from the relevant structure.)

- (55) a. John tried [PRO to win] (3.1 (6))
- b. \*John tried [t to win]
- c. it is difficult [PRO to win]
- d. \*John is difficult [t to win]
- e. John wanted [PRO to win]

- f. \*John wanted [t to win]
- g. \*John was wanted [t to win]
- h. [it is unclear [how PRO to solve the problem]]
- i. \*John is unclear [how t to solve the problem]

Similarly, the OB theory cannot explain why PRO is excluded from certain positions in which overt anaphors can appear, as in (56).

- (56) a. \*they expected that pictures of PRO would  
be on sale {3.1 (7)}
- b. they expected that pictures of each other  
would be on sale

The failure of the OB binding theory to explain the asymmetries between PRO and other anaphors illustrated in (55) and (56) constitutes an empirical shortcoming of this theory.

Chomsky (1981a:191-2) claims that all these problems relating to PRO are in fact solved by the GB binding theory. Since the relation of PRO to its antecedent (if there is one) is not determined by the binding principles, we can have long-distance control, as in (53) above. Since the relation of other anaphors to their antecedents is determined by the binding principles, the contrast between (53) and (54) follows.

The distinction between PRO and trace in (55) can also be explained by the GB binding theory. PRO can appear in these positions, since they are ungoverned. Trace is excluded from these positions because it must be governed.<sup>30)</sup> The exclusion of PRO from the relevant position in (56a) is also explained by the fact that it is a governed position.

In sum, then:

- (57) The GB binding theory has some empirical advantages over the OB binding theory. Firstly, the GB binding theory, in contrast with the OB binding theory, makes the correct

predictions/ . . .

predictions about sentences such as (52). Secondly, the GB binding theory, in contrast with the OB binding theory, can provide an explanation for the asymmetries between PRO and other anaphors.

It is interesting to note that while Chomsky (1981d:128) - written in 1979 - briefly refers to "the inevitable problems of empirical adequacy" of the OB framework, there is no systematic discussion of such empirical problems in the latter work. Also, no attempt is made to show that the GB binding theory overcomes a significant number of the empirical problems of the OB binding theory. In this respect the presentation of the GB binding theory in 1979 differs from the presentation in 1981.

When comparing the empirical success of the GB binding theory and the OB binding theory, it strikes one that there are quite a number of instances in which the OB binding theory apparently makes better predictions than the GB binding theory. A number of instances where the OB binding theory, in contrast with the GB binding theory, makes the correct predictions were noted in § 6.2.5.3 above - see in particular the schemas (39), (45), (50). These cases play a very interesting role in the development of the GB binding theory, and will be considered in detail in § 6.5 below.

### 6.3.3 The conceptual problems of the OB binding theory

#### 6.3.3.1 A redundancy in the theories of binding and Case

The first conceptual problem of the OB theory identified by Chomsky (1981a:157) concerns a redundancy in the theory, specifically in the theories of binding and Case. The three basic positions of NP in S are nominative subject of Tense, subject of an infinitive, and complement of a verb. Both the OB binding theory and the OB Case theory single out the subject position of an infinitive. The binding theory singles out this position

as the only/ . . .

as the only transparent domain in S. Case theory singles it out as the only position in S in which no Case is assigned.

This redundancy constitutes a conceptual problem for the OB theory in Chomsky's view. It is interesting to note that Chomsky (1980b:27) regarded this "implicit" redundancy between the OB binding theory and the OB Case theory as an improvement on the explicit redundancy between the OB binding theory and the \* $[\overline{\text{NP to VP}}]$  filter.<sup>31)</sup> Clearly, while the replacement of this explicit redundancy by an "implicit" redundancy represents an improvement for Chomsky, he regards even implicit redundancies as constituting conceptual problems.

Chomsky (1980b:fn. 30) mentioned the possibility of characterizing the properties of PRO in indirect questions and other control structures in terms of Case theory, rather than binding theory, by stipulating that PRO cannot have Case. The appearance of PRO would then be restricted to the only transparent position, namely the subject of an infinitive. In this way, the redundancy between Case theory and binding theory would be eliminated for PRO. However, this suggestion is rejected by Chomsky (1980b) because it does not generalize to the other cases of binding, and also because "the stipulation seemed rather *ad hoc*" (Chomsky 1981a:157). It is not quite clear what Chomsky means by "*ad hoc*" in this context. Chomsky (1981a:157) nevertheless says that "the similarity between the structural properties of Case and binding seems more than fortuitous, and raises the question whether opacity cannot somehow be reduced to Case theory", just as the TSC/PIC was reformulated in terms of considerations of Case as the NIC.

The above-mentioned redundancy in the theories of binding and Case does not exist within the GB theory. Within the latter framework, the theories of Case and binding are both formulated in terms of the notion 'government'. The subject position of an infinitive is an ungoverned position in the unmarked case

(the marked/ . . .

(the marked cases being an infinitive introduced by the complementizer *for*, or an infinitive in the complement of an  $\bar{S}$ -deletion verb, such as *believe*). In this way, then, the redundancy between Case theory and binding theory is "resolved in terms of their common component, the theory of government" (Chomsky 1981a:222).

Chomsky's characterization of the problem which the redundancy in the theories of binding and Case gives rise to as conceptual provides evidence that Chomsky adopts an empirical-conceptual distinction similar to the one set out in § 2.3.4.1 above. In terms of the latter distinction, a theory with a redundancy has a conceptual problem. In the case of Chomsky's linguistic theory, specifically, the existence of a redundancy in linguistic theory creates tension between this theory and Chomsky's general assumption that the language faculty is a simple system without redundancies.<sup>32)</sup>

#### 6.3.3.2 Absence of an explanation for the two opaque domains

A second conceptual problem of the OB theory identified by Chomsky (1981a:158) is the failure of the theory to provide an explanation for the fact that the two opaque domains are the subject of a tensed sentence and the c-command domain of the subject of any category. Chomsky comments that in (Chomsky 1980b (and earlier works)) the two opaque domains are in no way related, and neither is particularly well-motivated (except in terms of "rather vague" functional considerations).<sup>33)</sup> He concedes that some of the principles of UG must simply be stipulated, at least if the language faculty is an independent cognitive system. Moreover, the binding principles "do serve to unify a fairly impressive range of observations and also interact as one would hope with other principles . . .". He nevertheless considers it reasonable to ask "whether there are some more fundamental considerations from which the distribution of transparency and opacity derives".

The fact/ . . .

The fact that the OB theory cannot explain why the two opaque domains are the subject of a tensed sentence and the c-command domain of the subject of any category, indicates a certain lack of deductive depth in this theory. As we have seen, Chomsky regards deductive depth as a conceptual factor in theory appraisal. Chomsky has recently stressed the importance of deductive depth in linguistic theories.<sup>34)</sup> The role which deductive depth played in the development of the OB binding theory - see § 5.3 above - and in the development of the GB binding theory, underlines the importance attached by Chomsky to deductive depth.

Within the GB theory the explanation for the fact that the two domains - subject-of-AGR and domain of subject - are opaque is as follows. The PIC/NIC and SSC/Opacity Condition follow from principles (6A) and (6B) of the GB binding theory, formulated in terms of the notion 'governing category'. It is the latter notion which in fact unifies the PIC/NIC and SSC/Opacity Condition. The opaque positions are governed positions, that is, elements that appear in these positions have governing categories. The GB theory thus has greater deductive depth than the OB theory.

The question that naturally arises is whether it is possible to determine in a non-arbitrary manner when a specific lack of deductive depth gives rise to a conceptual problem, and when it must be accepted that the principle involved must be stipulated. The answer that emerges from an analysis of Chomsky's work is that, prior to the construction of an alternative theory with greater deductive depth, it cannot be determined whether a lack of depth creates an actual conceptual problem. This is also the answer implicit in Chomsky's (1981a:158, 160, 161) remarks on this issue. Any stipulation in a theory  $T_x$  that strikes the linguist as "strange", "in need of explanation", can give rise to a *potential* conceptual problem for  $T_x$ , and so motivate the search for an alternative theory,  $T_{x+1}$ , that

can provide/ . . .

can provide an explanation for the relevant stipulation. Once a new theory  $T_{x+1}$  has been formulated that can provide an explanation for the stipulation, the linguist will know that the potential conceptual problem constitutes an *actual* conceptual problem for  $T_x$ . Presumably, if all attempts to provide an explanation for the stipulation fails, the linguist would have to concede that the failure of  $T_x$  to explain this stipulation does not constitute an actual conceptual problem for it.<sup>35)</sup>

Note that these remarks also apply to complexities exhibited by a linguistic theory. That is, any complexity in a theory  $T_x$  noted by a linguist can give rise to a potential conceptual problem for  $T_x$ . Only when a simpler theory  $T_{x+1}$  which does not exhibit the relevant complexity has been formulated, the linguist will know that the potential conceptual problem constitutes an actual conceptual problem for  $T_x$ . As in the case of a lack of deductive depth, the failure of all attempts to formulate a simpler theory would indicate that the complexity of  $T_x$  does not give rise to an actual conceptual problem.

6.3.3.3 An asymmetry between the NIC and the SSC/Opacity Condition, and a contradiction regarding the NIC

The applicability of the binding conditions to the trace of *wh*-Movement leads to problems for the OB theory. Quoting Rizzi, Chomsky (1981a:158) provides the following Italian example to illustrate that in languages (such as Italian) that tolerate certain violations of the *wh*-Island Constraints,<sup>36)</sup> the SSC/Opacity Condition<sup>37)</sup> does not hold for the trace of *wh*-Movement.

- (58) a. tuo fratello, a cui mi domando [che storie  
abbiano raccontato t], era molto preoccupato  
b. ("your brother, to whom I wonder [which  
stories they told t], was very troubled") (3.1 (8))<sup>38)</sup>

In (58) the *wh*-phrase *a cui* moves in a single step to its S-

structure/ . . .

structure position from the position marked by *t*. This trace is free in the domain of the subject *abbiano* of the embedded clause, in violation of the SSC/Opacity Condition. According to Chomsky, the nonapplicability of the SSC/Opacity Condition to *wh*-traces appears "very natural", in the light of the similarity between variables and names, for example under the conditions of strong crossover.<sup>39)</sup> Freidin and Lasnik (1981) have pointed out that the similarity between variables and names with respect to strong crossover extends to the domain of Tense, i.e., to the NIC. Thus, in (59) the variable *t* and the pronoun *he* cannot be coindexed, i.e., they cannot be coreferential.

- (59) a. who did he say [Mary kissed *t*] (3.1 (9))  
 b. who did he say [*t* kissed Mary]

(59a) illustrates the nonapplication of the SSC/Opacity Condition to the *wh*-trace, and (59b) the nonapplicability of the NIC. The NIC and SSC/Opacity Condition are thus similar in that neither applies to variables, which behave like names in these constructions.

There are, however, examples which apparently indicate that the NIC does apply to *wh*-traces. The effect of the NIC on *wh*-Movement cannot be observed directly in Italian, because of an interaction with other principles. Referring to work by Sportiche,<sup>40)</sup> Chomsky (1981a:159) claims that the applicability of the NIC to *wh*-Movement is difficult to detect, since the relevant examples are all rendered unacceptable by the *wh*-Island Condition. There are, nevertheless, sentences which can be used to illustrate this effect. For example, compare (60) with the English translation (58b) of the Italian sentence (58a).

- (60) the men, who I wonder [which stories *t* told  
 to your brother], were very troubled (3.1 (10))

In (60) the *wh*-phrase *who* moves in one step from the position marked by *t*, just as *to whom* moves from the position marked by *t* in (58b). In (58b) the trace of *to whom* is free in the domain of the subject *they* of the embedded clause. In (60) the trace of *who* is free in the subject position of a tensed clause. Chomsky (1981a:159) claims that the status of the two sentences are clearly "quite different, even for dialects that mark (8) [= (58b) - M.S.] unacceptable because of a *wh*-island violation". The explanation for this difference in status appears to be that in (60) the NIC applies over and above the conditions that lead to *wh*-island violations (i.e., Subjacency with S and  $\bar{S}$  as bounding nodes), while in (58b) the SSC/Opacity Condition does not apply.<sup>41)</sup>

The examples (58b) and (60) thus illustrate an asymmetry between the NIC and SSC/Opacity Condition: while the former appears to apply to *wh*-traces in some manner, the SSC/Opacity Condition does not. Moreover, the examples (59b) and (60) apparently indicate the existence of a contradiction with respect to the NIC in the OB theory:<sup>42)</sup> whereas (60) appears to indicate that the NIC applies to *wh*-traces, (59b) indicates that it does not.

Chomsky (1981a:160) interprets these facts as follows. The *wh*-island Condition has two quite separate components. One of these relates to the choice of bounding nodes for Subjacency (and applies in the case of both (58) and (60)). The second component (which applies in the case of (60) but not of (58)) relates to something else, apparently the NIC. If this "something else" is in fact the NIC, then we have the unexplained asymmetry between the SSC/Opacity Condition and NIC. The problem is compounded by the fact that neither the SSC/Opacity Condition nor the NIC applies to *wh*-traces in strong crossover contexts. Chomsky concludes that the NIC expresses a spurious generalization, and that two distinct principles are involved in the phenomena that have been taken to fall under the NIC.

A possible solution to the problems sketched above is outlined

by Chomsky/ . . .

by Chomsky (1981a:160). The NIC must be restricted to the category of phenomena in which there is complete symmetry between the NIC and the SSC/Opacity Condition. Variables (including *wh*-traces) will thus be exempt from both conditions, and NP-traces will be subject to both. A distinct principle can then be formulated to account for the fact that *wh*-traces in sentences such as (60) are subject to something like the NIC.

This solution is in fact the one adopted within the GB theory. *wh*-traces, as variables, are not subject to either the PIC/NIC or the SSC/Opacity Condition. The reason for this is that while these conditions follow from principles (6A) and (6B) of the GB binding theory, *wh*-traces (as variables) fall under principle (6C) of this binding theory. Cases in which *wh*-traces apparently obey the NIC fall under an independent principle, the Empty Category Principle (ECP).

Chomsky (1981a:chapter 4) considers various possible formulations of the ECP. He also discusses various problematic consequences of these different formulations. Since our main concern is with the GB binding theory, I do not provide a detailed exposition of the ECP. I merely present one of the proposed formulations of the ECP, and illustrate how it explains the unacceptability of sentences such as (60) above, i.e., sentences in which *wh*-phrases are apparently subject to something like the NIC.

The ECP is formulated as follows by Chomsky (1981a:250).

(61) "ECP:  $[\alpha \ e]$  must be properly governed." (4.4 (11))

The notion 'proper government' that features in (61) is defined as follows.

(62) "α properly governs β if and only if α governs β  
[and α ≠ AGR]." (43)

Chomsky (1981a:250) extends the notion of 'government' defined in {3.2.1 (11)}, so that coindexed elements are also governors.

(63) "Consider the structure (i):

(i) [ $\beta \dots \gamma \dots \alpha \dots \gamma \dots$ ], where {4.4 (9)}

- (a)  $\alpha = X^0$  or is coindexed with  $\gamma$
- (b) where  $\phi$  is a maximal projection, if  $\phi$  dominates  $\gamma$  then  $\phi$  dominates  $\alpha$
- (c)  $\alpha$  c-commands  $\gamma$

In this case,  $\alpha$  governs  $\gamma$ ."

Consider again the sentence (60). Given the ECP, the unacceptability of (60) can be explained without any reference to the binding theory. *who* in (60) does not properly govern *t*, because the maximal projections  $\bar{S}$  and VP intervene. Government by AGR does not count as proper government, by (62). Apart from the *wh*-island violation involved in (60) the unacceptability of (60) is thus explained by the ECP.

In sum, then, the application of the OB binding theory to *wh*-traces gives rise to two distinct, but related, conceptual problems for the OB theory, both of which are solved by the GB theory. First, the OB theory exhibits an internal contradiction (or a logical inconsistency) in that *wh*-traces are subject to the NIC in some contexts, and not subject to the NIC in other contexts. It is true that Chomsky (1981a:232) points out that these facts about *wh*-traces and the NIC yield only "a near contradiction", since a special stipulation could help overcome the inconsistency. However, he states that he is "skeptical about any attempt to avoid the problem along these lines". Given Chomsky's scepticism about resolving the inconsistency that exists in the OB theory with regard to *wh*-traces and the NIC, this inconsistency can be regarded as a logical incon-

sistency/ . . .

sistency, or a real contradiction. This particular conceptual problem is an internal conceptual problem.

The second problem is that within the OB theory there is no explanation of why the NIC applies to *wh*-traces in some contexts, while the SSC/Opacity Condition never applies to *wh*-traces. As Chomsky (1981a:160) puts it, "it is not at all clear, within the OB-framework, why there should be this asymmetry". As this remark by Chomsky indicates, this second problem is one of lack of deductive depth.

#### 6.3.3.4 The strangeness of the \**[that-t]* filter

Chomsky (1981a:160) considers the "curious character of the \**[that-t]* filter" to constitute a fourth conceptual problem of the OB theory. He again notes that some properties of UG must be stipulated. Moreover, the filter is attractive in that it serves "to unify many phenomena related to 'long movement' of nominative subjects in an enlightening way".<sup>44</sup> Chomsky nevertheless claims that "the filter is so strange-looking that one would certainly want to derive it, if possible, from more natural principles".

The problem raised by the \**[that-t]* filter is in all relevant respects identical to the problem raised by the failure of the OB theory to provide an explanation for the two opaque domains - see § 6.3.3.2 above. That is, the problem is one of insufficient deductive depth. Let us now consider how the GB theory solves this problem.

There is a certain similarity between sentences such as (60) and (64) below, on the one hand, and sentences such as (65) below that fall under the \**[that-t]* filter, on the other hand.

(64) John, I wonder how well understands this book {3.1 (13i)}

(65) / . . .

(65) \*who do you think [<sub>S</sub> that [<sub>NP</sub> e] saw Bill] ]

The similarity lies in the fact that in each case an unacceptable sentence contains an element X in COMP, followed directly by the trace of another element Y. The sentences in (60) and (64) are those in which some version of the NIC is applicable. These examples suggest that the NIC and the \**[that-t]* filter are related.

Chomsky mentions three attempts in the literature to eliminate the filter by explaining the relevant phenomena in terms of the NIC: those by Taraldsen, Pesetsky, and Kayne.<sup>45)</sup> As in the case of these attempts, Chomsky (1981a) proposes to solve the problem posed by the \**[that-t]* filter by relating the filter phenomena and the NIC phenomena.<sup>46)</sup> As shown in § 6.3.3.3 above, within the GB theory those cases in which the NIC apparently applies to *wh*-traces are handled by the ECP. The conceptual problem raised by the \**[that-t]* filter is thus solved within the GB theory, since the filter need no longer be stipulated. Instead, its effects follow from another principle, the ECP.

A crucial assumption in Chomsky's claim that the ECP enables the GB theory to overcome the conceptual problem created by the \**[that-t]* filter is that the ECP is not open to the same criticism as the filter. Chomsky (1981a:251) mentions two considerations that apparently indicate that the ECP is indeed "better" than the \**[that-t]* filter. Firstly, the ECP is a general principle, covering a wide range of phenomena. The *that*-trace phenomena are only a subset of these phenomena.<sup>47)</sup> Secondly, the ECP "is a rather natural principle, as distinct from the \**[that-t]* filter or any of the alternatives to it that have been suggested in the literature, quite apart from its considerably wider scope". In connection with the alleged naturalness of the ECP Chomsky remarks that "it is not unreasonable that UG should require that the presence of an empty category be signalled in some manner by elements that are overtly present

(in this/ . . .

(in this case, the relevant governor, or where the governor is a trace in COMP, its antecedent)".

While there are still some unresolved problems surrounding the ECP, Chomsky's claim about the generality of the ECP must be accepted.<sup>48)</sup> However, his claim about the naturalness of the ECP is problematic. As noted in § 4.5 above, the notion 'naturalness' with which Chomsky operates is obscure. It is quite unclear in terms of what specific criteria natural principles of UG can be distinguished from unnatural principles. In effect, Chomsky's claim about the naturalness of the ECP is without any real content.

#### 6.3.3.5 The complexity of the OB-indexing conventions

The complexity of the indexing conventions incorporated in the OB theory raises a further conceptual problem for this theory. Chomsky (1981a:161) specifically mentions the desirability of eliminating the concept of an 'anaphoric' index entirely "in terms of some more basic and simple notion". The OB-indexing conventions were briefly outlined in § 5.5 above. It was shown that, within the OB theory, the complexity of the conventions (including the use of anaphoric indices) arises from the need to accommodate disjoint reference under the OB binding conditions.

It was pointed out in § 6.2.4 above that the indexing theory incorporated in the GB theory is very simple, namely, the random assignment of referential indices. The indexing of pronouns is exactly like that of anaphors, according to Chomsky (1981a:186). That is, they are "proximate" if they are coindexed with some other element and "obviative" if not coindexed with another element. As Chomsky (1981a:222) notes, if this simple indexing theory could be adopted, then the fifth conceptual problem of the OB theory would also be solved. The GB indexing theory is simpler than the OB indexing theory in that the

former/ . . .

former does not contain any principles for the assignment of anaphoric indices.

The crucial question in connection with the simple GB indexing conventions is whether it is as successful as the complex OB indexing conventions in handling disjoint reference. Chomsky (1981a) admits that not all properties of disjoint reference can be accounted for by the simple indexing theory of the GB theory. He (1981a:226, fn. 39) points out that the examples given in § 6.2.5.2 to illustrate the application of the GB binding theory to pronouns are restricted to distinct reference. Chomsky (1981a:§5.1) considers disjoint reference more generally, and discusses some cases that present problems for the GB indexing theory.<sup>49)</sup> Consider the following sentences.

- (66) a. \*we lost my way (5.1 (1ii))
- b. I lost my way (5.1 (3i))
- c. \*I lost his way (5.1 (3ii))
  
- (67) a. \*we expected me to like John (5.1 (1iii))
- b. we expected John to like me (5.1 (4))

In (66a) the idiom requires coindexing of *we* and *my*. This is illustrated by the contrast in acceptability between (66b), which has coindexing, and (66c), which does not. The question is how coindexing of *we* and *my* in (66a) must be interpreted. According to Chomsky (1981a:286), this example "indicates that we must take coindexed elements to be strictly coreferential, not merely overlapping in reference . . .". On this assumption, (66a) will be assigned its proper, ungrammatical, status.

Given this assumption about the interpretation of coindexing, consider now disjoint reference, as in (67a). Referring to Postal for the original observation, Chomsky (1981a:286) claims that (67a) clearly contrasts in acceptability with (67b). Within the GB theory of indexing there are two indexings possible

in the/ . . .

in the examples (67). The two pronouns may be either coindexed, or differently indexed. Suppose that *we* and *me* are coindexed in (67a, b). The GB binding theory will then correctly block (67a) and admit (67b). In (67a) the matrix S is the governing category in which *me* must be free, and in (67b) the embedded S is the governing category for *me*, thus allowing *me* to be coindexed with *we* in the matrix S. However, on this analysis *me* in (67b) is understood as overlapping in reference with *we* under coindexing. This is inconsistent with the analysis of (66a), where it is assumed that coindexed elements must not merely overlap in reference, but must be strictly coreferential.

Suppose then that *we* and *me* in (67a, b) are indexed differently. We then have consistency with the analysis of (66a). The GB binding theory now fails to explain the difference in status between (67a) and (67b), since *me* is free in both sentences. This, according to Chomsky (1981a:286), is "surely an incorrect result". He points out that the relevant examples do not present any problem for the complex indexing theory of the OB theory. In the latter theory, the referential indices of the pronouns will differ, and the anaphoric indices will indicate the required properties of overlapping and disjoint reference in (66a), (67a, b).

Chomsky (1981a:286) provides the following summary of the success which the GB theory has with respect to the fifth conceptual problem of the OB theory, namely, the complexity of the OB indexing theory.

- (68) "Clearly, then, the theory of indexing we have been using here is defective, and something more complex is required. The theory of anaphoric indices in the OB-framework overcomes these problems, with the exception of (1i) and (2). In § 3.1, I cited the complexity of this theory as one of the problems to be addressed in improving the OB-framework, and in the exposition above I have avoided all of these problems, but only by restricting myself to too narrow a class of examples. This problem, along with several others relating to the theory of indexing, therefore still stands, in contrast to the other problems raised, which receive a natural solution in the GB-framework." 50)

In § 7.2 below the consequences of the fact that the GB indexing can apparently solve the relevant conceptual problem of the OB theory only at the cost of a loss of empirical success will be examined.

#### 6.3.3.6 The asymmetry between pronouns and other anaphors

The sixth conceptual problem of the OB theory identified by Chomsky (1981a:161) is closely related to the fifth. The conditions under which pronouns enter into disjoint reference are essentially the same as those under which anaphors enter into coreference, namely, the conditions stipulated in the binding conditions. It is this asymmetry between pronouns and other anaphors that gives rise to the complexity of the indexing conventions of the OB theory and to the complexity of the notion "free (i)" defined in (Chomsky 1980b: Appendix), and explicated in § 5.5 above. The question arises why there is this asymmetry. Thus Chomsky asks, "why shouldn't pronouns have coreference, rather than disjoint reference, where, for example, reciprocals do?" Within the OB theory there is no explanation for the asymmetry between pronouns and anaphors. The failure of the OB theory to provide such an explanation indicates a lack of deductive depth in this theory. This lack of deductive depth gives rise to a conceptual problem that is in all relevant respects identical to the problems analyzed in §§ 6.3.3.2 - 6.3.3.4 above.

Within the GB framework there is an explanation for the above-mentioned asymmetry. Pronouns share with PRO the property of being pronominals. Hence they are subject to principle (6B) of the GB binding theory. In terms of (6B), pronouns must be free in their governing category. They thus differ from anaphors which, being subject to principle (6A) of the GB binding theory, must be bound in their governing category. The asymmetry between pronouns and anaphors thus follow from the fact that they fall under different binding principles.

#### 6.3.4 Summary of conclusions

The main points of the discussion in §§ 6.3.1 - 6.3.3 above can be summarized as follows.

- (69) a. The GB binding theory has some empirical advantages over the OB binding theory in that the GB binding theory, in contrast with the OB binding theory, makes the correct predictions about certain sentences and can provide explanations for certain facts.
- b. However, there are also cases in which the OB binding theory, but not the GB binding theory, makes the correct predictions.
- (70) a. The GB theory is claimed to have a number of conceptual advantages over the OB theory.
- (i) In two respects the GB theory is claimed to be simpler than the OB theory.
- (ii) In four respects the GB theory is claimed to have greater deductive depth than the OB theory.
- (iii) The GB theory avoids an internal contradiction exhibited by the OB theory.
- b. In the case of the conceptual problem caused by the complexity of the OB indexing conventions, the GB theory solves the problem at the cost of a loss of empirical success.
- (71) Actual, as opposed to potential, conceptual problems caused by complexities or lack of deductive depth in a theory cannot be identified prior to the formulation of an alternative theory/ . . .

tive theory that overcomes these conceptual problems.

- (72) Conceptual factors played a more important role than empirical factors in Chomsky's choice of the GB binding theory over the OB binding theory.

#### 6.4 The 1979 versus the 1981 versions of the GB binding theory

##### 6.4.1 General remarks

§ 6.3 above deals with the version of the binding theory presented in (Chomsky 1981a:183-209). The aim of § 6.4 is to compare this version of the binding theory with two earlier versions, namely those presented in (Chomsky 1979b) and (Chomsky 1981d). Both the latter versions date from 1979. In the discussion below I use the term "the LGB GB binding theory" to refer the version of the GB binding theory presented in (Chomsky 1981a: 183-209). The terms "the Pisa GB binding theory" and "the MCG GB binding theory" refer to the versions presented in (Chomsky 1979b) and (Chomsky 1981d), respectively.

##### 6.4.2 Three different formulations of the GB binding theory

Chomsky (1981d:134) formulates the GB binding theory as follows:

- (73) "A. If NP is lexical or a bound variable, then it is {7} free  
 B. If NP is pronominal, it is free in its governing category  
 C. If NP is an anaphor, it is bound in its governing category."

He provides the following explications of the terms that appear in (73).

- (i) "Pronominal" in (73B) refers to pronouns and PRO.

(ii) / . . .

- (ii) An argument is bound if it is c-commanded by a coindexed argument (where the argument positions are taken to be the NP positions within S or NP). If not bound, an argument is free.
- (iii)  $\alpha$  is the governing category for  $\beta$  if  $\alpha$  is the minimal category in which  $\beta$  is governed ( $\alpha = \text{NP}$  or  $\text{S}$ ).  $\gamma$  govern  $\beta$  if  $\gamma$  minimally c-commands  $\beta$  ( $\gamma =$  a lexical category or Tense); that is,  $\gamma$  c-commands  $\beta$  and there is no  $\phi$  c-commanded by  $\gamma$  and c-commanding  $\beta$  but not  $\gamma$ . If  $\beta$  has Case, then its governing category is the S or NP in which it is marked for Case.  $\bar{S}$  and NP are absolute barriers to government.

Principle (73A) of the MCG GB binding theory corresponds to principle (6C) of the LGB GB binding theory; (73B) corresponds to (6B); (73C) corresponds to (6A). There are two obvious differences between the formulations of the MCG GB binding theory and that of the LGB GB binding theory. Firstly, the binding principles of the MCG GB binding theory, but not those of the LGB GB binding theory, are formulated as conditionals. Secondly, principle (73A) of the MCG GB binding theory refers to lexical NPs and bound variables, while the corresponding principle (6C) of the LGB GB binding theory refers to R-expressions. Neither of these differences appear to have any significance - empirical or otherwise. As regards the last-mentioned difference, the discussion in § 6.2.3.2 above makes it clear that the term "R-expression" used in the LGB GB binding theory refers to the same class of arguments that "lexical NP" and "bound variable" refer to in the MCG presentation.

There is a third difference between the MCG and LGB GB binding theories that has empirical consequences. The two theories incorporate different notions of 'government'. Consequently, the class of governing categories of the two versions of the GB binding theory differs. The empirical consequences of this

difference/ . . .

difference are discussed in § 6.5 below. Chomsky's (1981a: §3.2.1) discussion of the concept of government makes it clear that the choice of a particular definition of this concept is an empirical matter.

The formulation of the GB binding theory presented by Chomsky (1979b:16) - the Pisa GB binding theory - differs from both the MCG and LGB versions.

- (74) A. If  $\alpha$  is an anaphor or lacks a phonetic matrix, then (i)  $\alpha$  is a variable or (ii)  $\alpha$  is bound in every governing category.
- B. If  $\alpha$  is Case-marked, then (i)  $\alpha$  is an anaphor or (ii)  $\alpha$  is free in every governing category.
- C. If  $\alpha$  is a pronominal, then it is free in every minimal governing category. 51)

Chomsky (1979b:8) provides the following definitions for the notions 'governing category', and 'minimal governing category'.

(75) " $\alpha$  is a governing category for  $\beta =_{\text{def}}$  there's some  $\gamma \{20\}$  such that  $\gamma$  governs  $\beta$  and  $\alpha$  contains  $\gamma$ ."

(76) " $\alpha$  is a minimal governing category for  $\beta =_{\text{def}}$   $\alpha$  is  $\{21\}$  a governing category which properly contains no governing category."

The notion 'minimal-governing category' that features in the Pisa GB binding theory is equivalent to the notion 'governing category' in the MCG and LGB versions. The notion 'government' incorporated in the Pisa GB binding theory is the same as that incorporated in the MCG GB binding theory. Chomsky (1979b:16, 20) makes it clear that the conditions on the boundedness of the different types of arguments contained in the Pisa GB binding theory are in fact the same as those contained in the MCG and GB versions. (74) above stipulates that anaphors must be bound in all governing categories, including their minimal governing category.

Lexical NPs and variables must be free. The question arises why the formulation of the Pisa GB binding theory differs so much from the formulations of the MCG and LGB GB binding theories. Having outlined the basic content of (74), Chomsky (1979b:20) provides the following answer to this question.

- (77) "That's roughly the content of that, although it is formulated in such a way as to make some other things follow. What follows is that PRO is ungoverned and that trace is a variable if it is case-marked. In particular it follows that the trace of NP movement is always not case-marked."

The ungoverned status of PRO follows from (74) in the following manner, according to Chomsky (1979b:17).

- (78) a. Assume PRO is governed.  
 b. Then there is a minimal governing category  $\alpha$  in which PRO is governed.  
 c. By (74C), PRO must be free in  $\alpha$ .  
 d. By (74A), since PRO is not a variable, it has to be bound in every governing category, including  $\alpha$ .  
 e. From the contradiction between (c) and (d) it follows that PRO has no governing category, i.e., PRO is ungoverned.

Chomsky (1981a:191) argues that the ungoverned status of PRO follows from the formulation (6) of the GB binding theory, given the assumption that PRO is a pronominal anaphor. The more complex formulation (74) is thus not needed to derive the principle that PRO is ungoverned. Chomsky (1981d:135) shows that the conclusion about the ungoverned status of PRO also follows from the MCG GB binding theory.

The principle that trace is a variable if it is Case-marked follows from the Pisa GB binding theory in the following manner.<sup>52)</sup>

(79) / . . .

- (79) a. Assume  $[\text{NP } e]$  has Case.  
b. Since  $[\text{NP } e]$  lacks a phonetic matrix, it is subject to (74A).  
c. Since  $[\text{NP } e]$  has Case, it is subject to (74B).  
d. By definition it is not an anaphor.  
e. Therefore,  $[\text{NP } e]$  with Case is a variable and free in every governing category.

(79) entails that the principle (80) follows from right to left in (Chomsky 1979b).<sup>53)</sup>

(80) " $[\text{NP } e]$  is a variable if and only if it has Case."

Since (80) follows from left to right from the Case filter,<sup>54)</sup>

(80) holds in its full generality in (Chomsky 1979b). In (Chomsky 1981a) (80) is not assumed in its full generality.

While it is assumed that variables always have Case, it is not assumed that every  $[\text{NP } e]$  with Case is a variable.<sup>55)</sup> The LGB binding theory thus differs from the Pisa GB binding theory in that it follows from the latter, but not the former theory, that every  $[\text{NP } e]$  with Case is a variable. Chomsky (1981a) drops the assumption that every Case-marked  $[\text{NP } e]$  is a variable for empirical reasons. He (1981a:272) argues that in (81a), which is the S-structure of the Italian sentence (81b), NP\* is PRO with Case. This PRO is not a variable.<sup>56)</sup>

- (81) a.  $[\text{S NP}^* [\text{VP copula - AGR NP}]]$  {4.5 (48i)}  
b. sono io {4.5 (47i)}  
("it's me")

The main points of this section can be summarized as follows.

- (82) a. There are three differences between the MCG GB binding theory and the LGB GB binding theory. Two of these differences are simply differences in formulation with no empirical or conceptual consequences. The third dif-

ference/ . . .

ference - in the definition of 'government' - has empirical consequences. It is on the basis of these empirical consequences that Chomsky chooses the LGB GB binding theory.

- b. The Pisa GB binding theory differs markedly in its formulation from both the MCG and LGB GB binding theories. Chomsky (1979b) claimed that the formulation of the Pisa GB binding theory was necessary to derive (i) the principle that PRO is ungoverned and (ii) the principle that trace is a variable if it is case-marked. However, it turned out that principle (i) also follows from the MCG and LGB GB binding theories, and that (ii) is wrong for empirical reasons.
- c. The choice of the MCG GB binding theory over the Pisa GB binding theory, as well as the choice of the LGB GB binding theory over both the earlier versions, were thus partly motivated on empirical grounds.
- d. Some of the differences between the three versions of the GB binding theory apparently have no empirical or conceptual consequences. In part, then, Chomsky's choices were without justification.

#### 6.5 The 1979 versus the 1981 interpretation of the empirical differences between the OB and GB binding theories

Chomsky (1981a:207-209) discusses certain differences between the predictions of the OB binding theory and those of the GB binding theory (in any of the versions discussed in § 6.4). These predictions specifically concern arguments in NP. In § 6.2.5 above the differences between the predictions of the two theories are set out in detail. The cases in which the two theories make different predictions are summarized in (83). Note that all the numbers in (83) are from § 6.2.5.3 above.

(83) a. Overt anaphor in the position of  $\alpha_6$  in (36a)

	OB	GB
(37c)	✓	x
(37d)	✓	x
(37e)	✓	x

b. Overt anaphor in the position of  $\alpha_7$  in (36b)

	OB	GB	
(40a)	✓	✓/x	≠
*(40b)	✓	✓/x	

≠ The predictions of the GB binding theory depend on the exact definition of 'government'.

c. PRO in position of  $\alpha_6$  in (36a)

	OB	GB
*(46a)	x	✓
*(46b)	x	✓

d. Pronoun in position of  $\alpha_6$  in (36a)

	OB	GB
*(48c)	✓	x

e. Pronoun in position of  $\alpha_7$  in (36b)

	OB	GB
(49a)	x	✓

f. Pronoun in position of  $\alpha_7$  in (36b), with

[<sub>VP</sub> V-ing] in place of  $\bar{N}$

	OB	GB	
*(49c)	✓	x	≠ ≠

≠ ≠ This prediction of the GB binding theory does not present an actual problem, since sentences like (49c) are ruled out by an independent principle, the Avoid Pronoun Principle.

The summary in (83) above shows that while the GB binding theory makes correct predictions in some cases where the OB binding theory makes wrong predictions, it also makes wrong predictions in some other cases where the OB binding theory makes correct predictions. Chomsky (1981a:209) presents the GB binding theory as "a considerable improvement over OB" on conceptual as well as em-

pirical/ . . .

pirical grounds. Cases where the GB binding theory, but not the OB binding theory, makes wrong predictions about the acceptability of sentences are clearly problematic for the theory. Such cases constitute potential counterexamples for the latter theory. Let us now consider what steps Chomsky took in connection with these problematic cases.

That there are cases in which the OB binding theory, but not the GB binding theory makes correct predictions about the acceptability of sentences, was already acknowledged in 1979, in both (Chomsky 1981d) and (Chomsky 1979b). Chomsky (1981d:141f), in particular, contains a fairly detailed discussion of the problems that *each other* in NP poses for the GB binding theory - see cases (83a) and (83b) above. In essence, the strategy adopted by Chomsky in 1979 with respect to these problems is to claim that the relevant predictions of the GB binding theory are actually correct, and that it is the predictions of the OB binding theory that are wrong. Consider in this connection the introductory remarks to (Chomsky 1979b:1). (The italics are mine.)

- (84) "At the GLOW talk [= (Chomsky 1981d) - M.S.], I discussed some conceptual problems that arise in a theory of the OB type. I suggested another approach [= the GB theory - M.S.] which I suggested would overcome to some degree, sometimes completely, sometimes not, these conceptual problems. I also mentioned that this approach has different empirical consequences. It's incomparable in empirical coverage in that it properly explains some things which were not explained in the other theory, but it doesn't cover some of the material in the other theory. *I then suggested that that was a good result since the things covered in this theory seem very central whereas the things covered in the other theory and not in this one seem rather peripheral.* We may look to the future Markedness theory to justify these differences."

Obscure though these remarks admittedly are, they nevertheless express the view that it is a "good result" that the GB theory fails to make correct predictions about the acceptability of those cases about which the OB theory makes correct predictions. The final remark quoted in (84) indicate that Chomsky appeals to the

notion 'markedness' in order to reinterpret the apparently wrong predictions of the GB binding theory as actually correct, and the apparently correct predictions of the OB binding theory as actually wrong. This point also emerges clearly from the discussion in (Chomsky 1981d:140-145). In order to understand exactly what Chomsky is claiming in (84), it is necessary to consider in detail the role of the notion 'markedness' in Chomsky's (1981d) interpretation of the empirical differences between the OB and GB binding theories. The sentences discussed by Chomsky (1981d:140-142) are the following.<sup>57)</sup>

- (85) John read [NP his books] (18)
- (86) \*They'd prefer [NP each other's writing the book] (20ii)
- (87) They read [NP each other's books] (21)
- (88) \*They found [NP some books [S for [S each other to read] ] ] (22)
- (89) a. \*They heard [NP my stories about each other] (23i)  
 b. They heard [NP the stories about each other] (that had been published last year) (23ii)  
 c. They heard [NP stories about each other] (23iii)
- (90) a. \*They expected that [NP my pictures of each other] would be on sale (24i)  
 b. They expected that [NP the pictures of each other] would be on sale (24ii)  
 c. They expected that [NP several books about each other] would be on sale (24iii)
- (91) a. They think [it is a pity that [NP pictures of each other] are hanging on the wall] (25i)  
 b. \*They think [he said that [NP pictures of each other] are hanging on the wall] (25ii)

- (92) a. I think [it pleased them that [NP pictures  
 of each other] are hanging on the wall] (26i)  
 b. They think [it pleased me that [NP pictures  
 of each other] are hanging on the wall] (26ii)
- (93) a. They think that [there are [NP some letters  
 for each other] at the post office] (27i)  
 b. \*They think that [he saw [NP some letters  
 for each other] at the post office] (27ii)

In (85) there is a pronoun in the position of  $\alpha_7$  in (36b). (85) thus corresponds to (49a) above. In (86) and (87) *each other*, an overt anaphor, appears in the position of  $\alpha_7$  in (36b). (86) corresponds to (42), and (87) to (40a). In (89) - (93) *each other* appears in the position of  $\alpha_6$  in (36a). These sentences correspond to those in (37c, d, e). In (88) *each other* appears in the position of  $\alpha_4$  in (22b). I will return to the status assigned to this sentence in (Chomsky 1981a) below. The differences between the predictions which the OB and GB binding theories make about these sentences, according to Chomsky (1981d:140f.), are summarized in (94).

(94)	OB	GB
(85)	x	✓
* (86)	x	✓
(87)	✓	x
* (88)	x	✓
* (89a)	✓	✓
(89b, c)	✓	x
* (90a)	✓	✓
(90b, c)	✓	x
(91a)	✓	x
* (91b)	✓	✓
(92a, b)	✓	x
(93a)	✓	x
* (93b)	✓	✓

Certain/ . . .

Certain aspects of these predictions are in need of explication.

- (i) The definition of 'government' adopted in 1979 in the MCG and GB Pisa binding theories differs from the definition adopted in 1981 in the LGB GB binding theory with respect to the class of categories that are considered to be barriers to government. On the 1979 definition  $\bar{S}$  and NP are taken as absolute barriers to government - see § 6.4.2 above. On the 1981 definition all maximal projections are taken as absolute barriers to government. This difference entails that the predictions made about sentences such as (86)/(20ii) by the MCG and Pisa and GB binding theories (the 1979 versions of the GB binding theory) differ from those made by the LGB GB binding theory (the 1981 version). As was noted in § 6.2.5.3 above, the 1981 version wrongly predicts that such sentences are acceptable since the VP internal to the NP blocks government - see for example, the discussion of (42) above. Thus, both the OB binding theory and the LGB GB binding theory make the wrong predictions about such sentences.
- (ii) As regards sentences such as (87)/(21), corresponding to (40a) above, the LGB GB binding theory makes either the wrong or the correct prediction, depending on the exact definition of 'government'. See the discussion following (40) above for details.
- (iii) Sentences such as (88)/(22) are regarded as unacceptable by Chomsky (1981d), with the GB binding theory making the correct prediction and the OB theory making the wrong prediction. Chomsky (1981a:216) claims that, while such sentences are unacceptable to him, most speakers tend to regard them as acceptable. If the latter judgment is correct, then the advantage which Chomsky (1981d) claims the GB binding theory has over the OB binding theory, becomes a disadvantage. Such sentences will be discussed in greater detail below.

(iv) / . . .

- (iv) Chomsky (1981d:142) claims that (89)/(23) and (90)/(24) illustrate that it is not a "definiteness restriction" that is involved in such sentences, as is the case in, for example, (95)/(28).<sup>58)</sup>

- (95) a. \*Who did they hear [ NP my stories  
about t ] (28)  
b. \*Who did they hear [ NP the stories  
about t ]  
c. Who did they hear [ NP stories about t ]

In (95b) the definiteness of the NP from which the *wh*-phrase is moved is responsible for the unacceptability of the sentence. In (89b) and (90b) *each other* appears within a definite NP, and is bound outside this NP. Nevertheless, these sentences are acceptable.

- (v) Chomsky (1981d:143) claims that (91)/(25), (92)/(26), and (93)/(27) illustrate that the subject that creates an opaque domain, i.e., that invokes the SSC/Opacity Condition, must be a possible argument. Thus, *it* and *there* do not invoke Opacity.

- (vi) Chomsky (1981d:143) claims that (92)/(26) illustrates that the notion 'subject' is crucial, and not the notion 'possible antecedent'. In (92b) *me* is in the position of a possible antecedent, but it does not create an opaque domain, so blocking coindexing of *they* and *each other*.

Chomsky interprets the facts presented in (94) as follows. The OB and GB binding theories are taken as making different predictions about the markedness of the sentences (85) - (93).<sup>59)</sup> The OB binding theory predicts that (85)/(18), (86)/(20ii) and (88)/(22) will be marked, and that all the others will be unmarked. The GB binding theory predicts that (87)/(21), (89b, c)/(23i, ii), (90b, c)/(24ii, iii), (91a)/(25i), (92a, b)/(26i, ii) and (93a)/(27i) will be marked and that all the others will be unmarked.

Chomsky (1981d:141) claims that the markedness predictions of the GB binding theory are correct, and that those of the OB binding theory are wrong. In support of this claim, Chomsky presents the following considerations. He (1981d:141) claims that (85)/{18} "is surely the normal case in the languages of the world", while "structures such as (21) [= (87) - M.S.] appear to be rare". Moreover, the OB principle that permits (87)/{21} does not extend to similar structures such as (86)/{20ii}, "which perhaps represents a more general case across languages". As regards the unstarred sentences in (89) - (93)/{23} - {27}, Chomsky (1981d:143) claims that they are marked because they "seem somewhat marginal", "judgments tend to vary", and "there appear to be differences in judgment depending on lexical choice".

In order to accommodate the cases that are marked under the GB binding theory, Chomsky (1981d:143) proposes that the grammar of English incorporate something like the following marked principle.

(96) *each other* may be free in its governing NP if {30} it is not free in the c-command domain of a lexical subject."

He notes that the exact formulation of the principle "depends on some rather questionable factual judgments involving not only *each other*, but also reflexives and pronouns". He also notes that (96) is in fact derivable from the GB binding theory, but that it is inoperative since a more restrictive condition - namely, that *each other* must be bound in all its governing categories - also follows. (96) applies only in special marked structures in which the general principles of the GB system are relaxed. Chomsky (1981d:145) claims that (96) may be "an example of the kind of 'analogic process' (in a rather abstract sense of the term) that we might expect to find outside of the central core of the system of grammar".

Chomsky's (1979b; 1981d) handling of the apparent counterexamples of the GB binding theory shows four obvious similarities with his

use of the notion 'markedness' as a protective device in earlier cases.<sup>60</sup> First, the complementary relation between Chomsky's use of idealizations - such as the idealization of core grammar - and his tolerant attitude toward apparent counterevidence can clearly be seen in the case presently under discussion. As explained in § 4.3 above, the adoption of the idealization of core grammar (like any other idealization) leads to a considerable complication of the relation between a UG and linguistic data, thus warranting a tolerant attitude to apparently refuting evidence. In order to determine whether a datum is relevant to a theory of core grammar, it must first be determined whether a datum bears on an unmarked aspect of language, or a marked aspect. Chomsky's reinterpretation of the data that apparently refute the GB binding theory illustrates this point very well.

Second, like the markedness claims discussed in §§ 4.3 and 4.4.5 above, the markedness claims discussed above are also hypothetical in nature, and thus in need of independent justification.

Third, like the markedness claims discussed in §§ 4.3 and 4.4.5 the markedness claims presently under discussion are completely speculative. Chomsky (1979b, 1981d) does present certain considerations in support of his markedness claims. He mainly refers to the commonness or rarity of the relevant constructions across the languages of the world. However, he fails to provide any evidence in support of claims such as "P is (un)common across the languages of the world". Chomsky's markedness claims can thus be regarded as *ad hoc* auxiliary hypotheses introduced to protect his theory from potential counterevidence.

Fourth, Chomsky's use of markedness claims that may be reconstructed as "P is (un)common among the languages of the world" again illustrates the point made in § 4.4.5 above, that the use of such claims necessitates the use of cross-linguistic data.

In spite/ . . .

In spite of the similarities mentioned above, there is an interesting difference between Chomsky's use of the notion 'markedness' as a protective device in the case presently under discussion, and his use of this notion in the earlier cases discussed in §§ 4.3 and 4.4.5. The strong link between the idealization of core grammar and the aim of developing a highly restrictive theory of UG was outlined in § 4.3 above. By assuming that there is a marked periphery of phenomena that fall outside the core, the linguist can develop a highly restrictive theory of this core. In all the earlier cases discussed above where the notion 'markedness' is used to protect general-linguistic hypotheses from apparent counterevidence, Chomsky's main objective was to retain as restrictive a UG as possible. For instance, by claiming that the French rule that must handle the peripheral *Tous-Movement* phenomena is marked, and thus outside the core grammar, Chomsky (1977c) is able to retain a very restrictive condition on the structural descriptions of the rules of core grammar.<sup>61)</sup> However, in the case of Chomsky's protection of the GB binding theory it is not the restrictiveness of UG that is at stake. Rather, what is at stake is the development of a UG with certain desirable conceptual properties that are independent from the consideration of restricted formal power. These desirable conceptual properties include the avoidance of redundancies, the avoidance of internal contradictions, and increased deductive depth.<sup>62)</sup> The independence of such conceptual properties from restrictedness of formal power is emphasized by Chomsky (1981a:15), who states that "a theory of UG with redundancies and inelegant stipulations may be no less restrictive than one that overcomes these conceptual defects". Chomsky's use of the notion 'markedness' to protect the GB binding, with its more desirable conceptual properties, from apparent counterevidence underlines the importance attached by Chomsky to considerations such as nonredundancy, deductive depth, etc., in theory appraisal.

In his analysis of Chomsky's protection of the GB binding theory Botha (1982a:39) claims that Chomsky's handling of the evidence

threatening/ . . .

threatening the GB binding theory "gives rise to the expectation that norms formulated in terms of such notions as 'deductive depth', 'unifiedness', 'principledness', 'naturalness', 'elegance', 'simplicity' and the like will play an increasingly important role in the validation of linguistic theories in time to come". The analysis presented above suggests that the relation between Chomsky's handling of the apparent counterevidence of the GB binding theory and the importance of notions such as 'deductive depth', 'unifiedness', 'simplicity', etcetera, in theory evaluation is rather the other way round. That is, Chomsky's handling of the evidence threatening the GB binding theory is a consequence of the importance attached by him to norms formulated in terms of notions such as 'deductive depth', 'unifiedness', 'simplicity', etc.

One of the most interesting points made by Botha (1982a) is that Chomsky makes use of what has been called "rhetorical tricks" in § 2.3.5 above in order to persuade others to accept his markedness claims.<sup>63)</sup> In particular, Botha (1982a:§4.4.3) argues that Chomsky uses certain methods of concealment to create the impression that anyone who resolves a conflict between the predictions of a linguistic theory and the acceptability of a native speaker with the aid of the notion 'markedness' in the manner outlined above, is doing a "normal and non-problematic thing". First, Chomsky's (1981d:140) use of the expression "In summary . . ." <sup>64)</sup> creates the impression that he is going to repeat the essence of something that has already been argued for. However, this expression is followed by claims about the markedness of the forms under discussion, a matter about which Chomsky (1981d) had nothing to say prior to the "summary". By creating the impression that he and his readers have always taken considerations of markedness into account when interpreting acceptability judgments, Chomsky's "In summary" formulation has the effect of partly concealing his reinterpretation of the data threatening the GB binding theory in terms of the notion 'markedness'.

Second, in the remarks under analysis Chomsky operates with an incorrect claim as if it were evidently true, namely the claim that the two alternative theories make predictions about markedness. Botha argues that neither the OB nor the GB theory, nor any other syntactic theory of Chomsky's, contains any of the principles of markedness that would be needed for deriving such predictions.

Third, Chomsky initially attempts to create the impression that there is no real need for any special justification for the markedness judgments, and that, consequently, the lack of such justification presents no problem. Later on Chomsky implicitly concedes that special justification is required when he (1981d: 141) portrays the markedness judgments about his {21} as an empirical assumption from which testable predictions can be derived.

By making use of the rhetorical tricks identified by Botha, Chomsky implicitly concedes that he has no good arguments to support his markedness judgments, and, by implication, no good arguments for the purpose of protecting the GB binding theory from the apparently threatening evidence. This point becomes very important in view of the further developments of the GB binding theory in (Chomsky 1981a). The approach adopted by Chomsky (1981a) toward those cases in which the GB binding theory apparently makes the wrong predictions differs substantially from his 1979 approach outlined above. Summarizing the relative merits of the OB and GB binding theories, Chomsky (1981a:209) claims that "the GB-framework is a considerable improvement over OB on empirical and conceptual grounds, *but there are still problems in the case of arguments within NPs . . .*" (the italics are mine). The "problems" referred to above are exactly those problems claimed by Chomsky (1981d) to have been solved by means of the notion 'markedness'. Chomsky (1981a) attempts to modify the GB binding theory by means of the notion 'accessible SUBJECT' so that this theory makes the correct predictions about the acceptability of the relevant sentences, just like the OB

binding/ . . .

binding theory. This modification of the GB binding theory is outlined in § 6.6 below.

What exactly is Chomsky's (1981a) position on the (un)markedness of these sentences which were claimed to be marked in (Chomsky 1979b, 1981d), and which now fall under the reformulated version of the GB binding theory? The answer is that Chomsky (1981a) does not make any definite claim about their markedness. That is, he leaves the matter open. Commenting on his discussion of possible modifications to the GB binding theory, Chomsky (1981a: 216) states that "it *may* be that this entire discussion properly belongs to the theory of markedness rather than of core grammar, and that the phenomena we have been discussing reflect marked properties of English". (The italics are mine.) Chomsky (1982a: 110) suggests that it might actually be the concept of accessibility that is marked.

- (97) "That is, you might argue that the unmarked case is where no notion of accessible subject enters and the marked case is where the notion of accessible subject does . . ."

However, Chomsky (1982a) also leaves open the question of whether the sentences in question are actually marked. Referring to (Chomsky 1981d), he (1982a:109) states that "what I suggested there was very heavily based on a factual assumption, that the picture noun phenomena are marked, *which they may very well be*". (The italics are mine.) Chomsky's (1981a, 1982a) position on the markedness of the sentences in question contrasts strikingly with his confident claims in (Chomsky 1981d) about their marked status. Note that Chomsky (1981a, 1982a) mentions no new considerations that cast doubt on the earlier claims about the markedness of the sentences. In fact, Chomsky (1982a:110) makes the following, rather surprising "confession" about his (1981d) claims on the markedness of the picture noun cases.

- (98) "I've always assumed they're a little odd in their behavior but *they really just didn't fall into the theory*

*I outlined! . . .*

*I outlined there at all, so I just had to say they're totally marked. I gave a half-baked argument about that, and there was some bad conscience, I must concede.*"  
{The italics are mine - M.S.}

In these remarks Chomsky actually admits that the only ground he (1981d) had for his claim that the picture noun cases are marked, was that they fell outside the GB theory of core grammar. The point is then not simply that Chomsky's (1981d) markedness claims have the status of *ad hoc* auxiliary hypotheses introduced solely to protect the GB binding theory from potential counter-evidence. The crucial point is that Chomsky tried to disguise their unjustifiedness, and other problematic features of these claims. In view of Chomsky's (1982a:110) remarks, the conclusion that Chomsky made use of rhetorical tricks in persuading others to adopt the GB binding theory is inescapable.

The main points of the discussion above can be summarized as follows.

- (99) a. Chomsky (1981d) protects the GB binding theory from certain potential counterexamples by claiming that the forms in question are marked, and thus fall outside the domain of a theory of core grammar.
- b. By using the notion 'markedness' to protect the GB binding theory, Chomsky is able to develop a UG with more desirable conceptual properties than earlier versions.
- c. The markedness claims of (Chomsky 1981d) are hypothetical and speculative, and must be regarded as *ad hoc* auxiliary hypotheses introduced solely to protect the GB binding theory from potential counterevidence.
- d. Chomsky (1981a) admits that his (1981d) markedness claims were unjustified, and based purely on theory-internal considerations.

- e. The speculative nature of the markedness claims is underlined by the fact that Chomsky (1981a; 1982a) adopts a different position on the markedness of forms conflicting with the GB binding theory. In the latter two works he leaves open the question of whether the relevant forms are actually marked.
- f. Chomsky (1981a) modifies the GB binding theory so that it can explain those cases which conflicted with earlier versions.
- g. Chomsky (1981d) made use of rhetorical tricks to persuade others to accept his markedness claims, and, consequently, his choice of the GB binding theory over the OB binding theory.

## 6.6 Some further modifications to the GB binding theory

### 6.6.1 General remarks

In § 6.4 above certain differences were outlined that exist among three different versions of the GB binding theory, namely the MCG, Pisa, and GB versions. In spite of these differences the three versions are similar in an important respect: the conditions under which the various types of arguments must be bound or free are the same for all three versions. In each case, the theory stipulates the boundedness, or otherwise, of an argument within the minimal NP or S that contains both the argument and its governor. Chomsky (1981a:209-216) argues that the GB binding theory must be reformulated in terms of the notion 'accessible SUBJECT'. In order to qualify as a governing category, a category must not only contain a governor, but also an accessible SUBJECT. Chomsky claims that this reformulation overcomes the majority of the empirical problems relating to arguments within NPs, as well as a conceptual problem of the earlier versions.

An exposition of Chomsky's formulation of the GB binding theory in terms of the notion 'accessible SUBJECT' is presented in § 6.6.2 below. In the discussion that follows the term "the GB SUBJECT binding theory" is used to refer to this reformulated version of the GB binding theory. The term "the GB governor binding theory" is used to refer collectively to the three versions discussed in § 6.4. In § 6.6.3 I discuss some additional modifications to the GB SUBJECT binding theory proposed by Chomsky.

#### 6.6.2 Reformulating the GB binding theory in terms of the notion 'accessible SUBJECT'

Chomsky (1981a:209-210) first illustrates how the SSC/Opacity Condition and some version of the PIC/NIC can be unified in terms of a notion 'SUBJECT'. He (1981a:210) points out that the version of the PIC/NIC involved in this unification is the one proposed by George and Kornfilt. This version takes agreement, rather than Tense, as the crucial element invoking opacity. In English there is no distinction, since there is a one-to-one correlation between the two. In other languages - for example, Turkish - where the two are disassociated, it is agreement, rather than Tense, that determines opacity. Chomsky remarks that "we have tacitly been assuming the accuracy of the George-Kornfilt theory all along, taking AGR - the governor of the subject - to be the crucial element determining opacity".

The SSC/Opacity Condition and the relevant version of the PIC/NIC can be unified in terms of the notion 'SUBJECT' in the following way. In an infinitive, NP, or small clause, the subject is the SUBJECT.<sup>65)</sup> In clauses where INFL contains AGR (as is the case in tensed clauses in English), AGR is the SUBJECT. In (100a) AGR is thus the SUBJECT. In the embedded clause of (100b) *John* is the SUBJECT, and AGR is the SUBJECT of the matrix clause.

(100) a. John [ INFL past AGR ] win [3.2.3 (67i)]

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- b. he [<sub>INFL</sub> present AGR] believes [JOHN  
to be intelligent] (3.2.3 (67iii))

Suppose that the SSC/Opacity Condition were reformulated in terms of the notion 'SUBJECT', stipulating that no anaphor may be free in the domain of SUBJECT. The PIC/NIC would then be reduced to this reformulated version of the SSC/Opacity Condition, i.e., to the case where SUBJECT is AGR. There would be only one opaque domain, namely the domain of SUBJECT.

Such a reformulation of the SSC/Opacity Condition and PIC/NIC has a conceptual advantage over the GB binding theory. The reformulated version of the SSC/Opacity Condition and PIC/NIC provides an answer to the question of why NP and S are the governing categories. In essence, the answer is that NP and S contain SUBJECTS, where a SUBJECT creates an opaque domain. The full answer is as follows. S is a governing category for  $\alpha$ , since it always contains a SUBJECT.<sup>66)</sup> NP is a governing category for  $\alpha$  only when it contains a SUBJECT  $\neq \alpha$ . Other categories, for example, adjective phrases, may also be governing categories if they contain subjects (hence SUBJECTS).<sup>67)</sup> Note that the conceptual problem in question is of the same type as the ones discussed in, for example, §§ 6.3.3.2, 6.3.3.3, 6.3.3.4, 6.3.3.6 above. That is, the problem arises from a lack of deductive depth in the theory.

Given the distinction made in the reformulated SSC/Opacity Condition between NPs with subjects and NPs without subjects, it follows that the predictions of this condition about arguments in NPs will differ from the predictions made by the GB binding theory. They will, however, make the same predictions about arguments in clauses.

Chomsky (1981a:211) claims that both the GB governor binding theory and the reformulated SSC/Opacity Condition have attractive features. The first solves many of the conceptual problems of

the OB binding theory, and the second solves a conceptual problem of the GB governor binding theory. Chomsky (1981a:211f.) therefore tries to amalgamate these two theories. The amalgamated theory, the GB SUBJECT binding theory, may be characterized in terms of the principles presented in (i) - (iv) below.

(i) The binding principles of the GB-governor binding theory - see (6) above - are taken over without any modification.

(ii) The principle (101) is adopted.

(101) "AGR is coindexed with the NP it governs" (3.2.3 (70I))

Given the general condition that a coindexed NP and pronominal (pronoun or PRO) must share the appropriate features, (101) accounts for the phenomenon of agreement. Recall that AGR = PRO.<sup>68</sup> (101) thus reduces the phenomenon of obligatory subject-verb agreement to general properties of proximate pronominals. According to Chomsky (1981a:216), the principle (101) is required in some form in any theory.

(iii) The GB SUBJECT binding theory incorporates a new definition of 'governing category'.

(102) " $\beta$  is a governing category for  $\alpha$  if and only if  $\beta$  is the minimal category containing  $\alpha$ , a governor of  $\alpha$ , and a SUBJECT accessible to  $\alpha$ ." (3.2.3 (70II))

It follows from (102) that  $\beta$  is a governing category only if it has a SUBJECT. S can thus always be a governing category, and NP can be a governing category when it has a subject. The same holds for small clauses. According to Chomsky (1981a:211), the choice of governing category receives "a rather natural characterization" in terms of (102). The conceptual problem faced by the earlier versions of the GB binding theory, namely, the problem of explaining why NP and S are the governing categories, is now solved. The question of whether S or  $\bar{S}$  should

be selected/ . . .

be selected as the governing category no longer arises. Note that in (Chomsky 1979b:8) no justification is provided for the decision to take S as the governing category.

(iv) The notion 'accessible' in (102) is defined in (104), in terms of the wellformedness condition (103).

(103) "\* [  $\gamma$  ...  $\delta$  ... ], where  $\gamma$  and  $\delta$  bear the same index." (3.2.3 (73))

(104) " $\alpha$  is *accessible* to  $\beta$  if and only if  $\beta$  is in the c-command domain of  $\alpha$  and assignment to  $\beta$  of the index of  $\alpha$  would not violate (73) (= (103) - M.S.)." (3.2.3 (74))

Chomsky (1981a:212) claims that (103) holds for a variety of constructions apart from those directly relevant in the present case. That is, he claims that there is some independent justification for (103). In all the cases of (105) this well-formedness condition is violated.<sup>69)</sup>

- (105) a. \* [  $NP_i$  the friends of [  $_i$  each other's ] parents ] (3.2.3 (75))  
 b. \* There is [  $NP_i$  a picture of [  $NP_i$  it-self ] ] on the mantelpiece  
 c. \* [  $NP_i$  the owner of [ [  $NP_i$  his boat ] ] ]  
 d. \* [  $NP_i$  the friends of [ [  $NP_i$  their ] parents ] ]

In (105b), for example, the NP *itself* (=  $\delta$ ) is contained in another NP *the friends of each other's parents* (=  $\gamma$ ), and  $\delta$  and  $\gamma$  bear the same index. (103) thus marks (105b) as ill-formed. Note that (104) refers to possible, not actual indexing of  $\beta$ . That is,  $\alpha$  becomes inaccessible to  $\beta$  if coindexing of  $\alpha$  and  $\beta$  would lead to a violation of (103).

The GB SUBJECT binding theory makes the same predictions about

arguments/ . . .

arguments in clauses as the GB governor binding theory. The nominative subject of a clause always has an accessible SUBJECT, namely the AGR element of INFL, which also governs the subject. Hence the clause is a governing category. The nominative anaphor in such a clause must therefore be bound in it, which is impossible. Pronominals must be free in this clause. (PRO can in fact never appear in the relevant position, since it is governed.) Since clauses must have subjects, hence SUBJECTS, which are accessible, the governing categories in the other cases reviewed remain the same.

However, different predictions are made about arguments within NPs. Consider firstly the case of overt anaphors, such as *each other*. These fall under binding principle (6A). In (106) - (115) I present the examples with *each other* in the position of  $\alpha_1$  discussed by Chomsky (1981a:216, 217). Wherever an example is the same as, or exactly analogous to, an example discussed in §§ 6.2.5.3 and 6.5 above, the number of the latter example is also provided. These numbers appear directly below the relevant example.

(106) a. \*they heard [<sub>NP</sub> my stories about each other] (3.2.3 (78))

(= (37b), (89a))

b. they heard [<sub>NP</sub> the stories about each other]

(= (37c), (37d), (89b))

c. they heard [<sub>NP</sub> stories about each other]

(= (37c), (89c))

(107) a. \*they expected [<sub>S\*</sub> me to hear [<sub>NP\*</sub> stories about each other]] (3.2.3 (79))

b. they expected that [<sub>S\*</sub> [<sub>NP\*</sub> pictures of each other] would be on sale]

(= (90c))

c. they expected that [<sub>S\*</sub> [<sub>NP\*</sub> {PRO feeding each other} PRO to feed each other] would be difficult]

- (108) \*they thought [<sub>S</sub>\* I expected that pictures  
of each other would be on sale] {3.2.3 (80)}  
(= (91b))
- (109) they thought [<sub>S</sub>\* I expected that  
{feeding each other} would be difficult] {3.2.3 (81)}  
to feed each other
- (110) a. they think it is a pity that pictures  
of each other are hanging on the wall {3.2.3 (82)}  
(= (91a))  
b. \*they think he said that pictures of each  
other are hanging on the wall {3.2.3 (83)}  
(= (91b))
- (111) a. they think there are [some letters for  
each other] at the post office {3.2.3 (86)}  
(= (93a))  
b. \*they think he saw [some letters for each  
other] at the post office  
(= (93b))
- (112) a. I think it pleased them that pictures of  
each other are hanging on the wall {3.2.3 (88)}  
(= (92a))  
b. they think it pleased me that pictures  
of each other are hanging on the wall {3.2.3 (89)}  
(= (92b))
- (113) they found [<sub>NP</sub> some books [<sub>S</sub> for each  
other to read]] {3.2.3 (87)}  
(= (88))
- (114) \*they preferred [<sub>NP</sub> each other's reading the  
book] {3.2.3 (90)}  
(= (42), (86))

(115) \*they thought [ S\* I preferred each other's  
 reading the book ] {3.2.3 (91)}

Chomsky shows that, with the exception of (114), the GB SUBJECT binding theory makes the correct predictions about all these cases. This contrasts with the GB governor binding theory, which makes the wrong predictions about (106b, c), (107b), (110a), (111a), (112a, b) - see §§ 6.2.5.3 and 6.5 above for details. Let me briefly explicate the predictions of the GB SUBJECT binding theory about the sentences (106) - (115).

In (106a), but not (106b, c), the object NP contains a SUBJECT accessible to *each other*. The GB SUBJECT binding theory thus requires that *each other* be bound in NP in (106a), but not in (106b, c). It thus correctly predicts the unacceptability of (106a) as opposed to the acceptability of (106b, c).

In (107a) NP\* contains a governor for *each other*, but no accessible SUBJECT. Therefore, NP\* is not a governing category for the anaphor. S\* does contain an accessible SUBJECT, namely the subject *me*. S\* is thus the governing category for *each other* in which it must be bound. The GB SUBJECT binding theory thus correctly predicts the unacceptability of (107a). In (107b) NP\* does not contain a SUBJECT accessible to *each other*. S\* does contain a SUBJECT, namely AGR. AGR is coindexed with NP\* by principle (101). It is thus not accessible to *each other* because of the well-formedness condition (103). It follows that S\* is not a governing category for *each other*. The matrix clause, however, is a governing category for *each other*, since it contains an accessible AGR element. (107b) is thus acceptable with coindexing of *they* and *each other*.

(107b) contrasts with (108). In (108) S\* contains a SUBJECT accessible to *each other*, namely the subject. The GB SUBJECT binding theory thus correctly predicts the unacceptability of (108), with *each other* free in S\*.

In (107c)/ . . .

In (107c) NP\* is a governing category for *each other*, since it contains the accessible SUBJECT PRO (coindexed with *they*). *each other* must therefore be bound in NP\*. The GB SUBJECT binding theory thus correctly predicts the acceptability of (107c), with *they* and *each other* coreferential. As regards PRO, the binding theory merely requires that it be ungoverned. This is the case in (107c), and also in (109).

Chomsky (1981a:214) remarks that the contrast between (110a) and (110b) may be attributed to a phenomenon "that has been frequently discussed in connection with the SSC, namely, that the nature of the subject that creates the opaque domain figures in determining the degree of violation of opacity, with agentive subjects inducing maximal violation and nonarguments minimal violation".<sup>70)</sup> He claims (1981a:214) that consideration of other examples, such as those in (116), suggests another approach.<sup>71)</sup>

- (116) a. \*they think [it bothered *each other* that S] (3.2.3 (84))  
 b. \*he thinks [it bothered *himself* that S]  
 c. he thinks [it bothered *him* that S]  
 (*him* proximate to *he*)

Chomsky argues that these examples indicate that it is not the agentivity of the subject *it* that explains the difference between (110a) and (110b). In (116) AGR in the embedded clause is a SUBJECT accessible to the italicized anaphor or pronominal. This clause is therefore the governing category for the anaphor or pronominal. By principle (6A) the anaphor must be bound in it (hence the unacceptability of (116a, b)). By principle (6B), the pronoun must be free in it (hence the possibility of interpreting (116c) with *him* proximate to *he*). The examples (116) thus fall together with (110b), and contrast with (110a). The problem is then to explain the difference between (110a) and (110b) on the one hand, and between (110a) and (116) on the other hand.

According/ . . .



The ability of the GB SUBJECT binding theory to provide an explanation for the contrast between (110a) and (110b) provides some confirmation for the fruitfulness of Chomsky's (1979a:188) policy of being willing "to put aside the counterexamples to a theory with some degree of explanatory force, a theory that provides a degree of insight, and to take them up again at a higher level of understanding".<sup>72)</sup> Sentences such as (110) were already noted as problematic for the SSC in (Chomsky 1973). As explained in § 3.2.7.4 above, sentences such as (110a) constitute potential counterexamples for the SSC. Chomsky (1973) only made a tentative suggestion about a possible solution to these counterexamples, in terms of the semantic notion 'agency'. He in effect "put aside" such counterexamples, until, at the "higher level of understanding" of the GB SUBJECT binding theory they are successfully taken up again. Note that a period of ten years has passed between the time when the class of counterexamples was first noted, and the time when they were successfully explained. The history of sentences such as (110) again underlines that Chomsky's tolerant attitude to potential counterevidence does not entail that counterevidence must be completely ignored. Rather, the explanation of such counterevidence remains part of the linguist's aim - even if a fairly long stretch of time passes between the first noting of the apparent counterexamples and their eventual explanation.

The acceptability of (112a) follows from the GB SUBJECT binding theory, in exactly the same way as the acceptability of (110a). This theory predicts that (112b) is also acceptable. According to Chomsky (1981a:216), judgments vary concerning (112b). But, since (112b) is much better than (110b), he takes the prediction of the GB SUBJECT binding theory to be correct.

The OB binding theory predicts the acceptability of (113), as does the GB SUBJECT binding theory. Only the matrix clause contains an accessible SUBJECT, and *each other*, being coindexed with *they*, is bound in this clause. The GB governor binding

theory/ . . .

theory predicts that (113) will be unacceptable. In 1979 Chomsky took the latter as the correct prediction - see (Chomsky 1981d). He (1981a:216) says that this accords with his own judgment. However, he recognizes that most speakers regard (112) as acceptable, in which case (112) does not pose a problem for the GB SUBJECT binding theory.

The GB SUBJECT binding theory makes the correct predictions about all the examples presented in (106) - (115), with the exception of (114). The OB binding theory also makes the wrong predictions about (114). However, Chomsky notes that it is possible that the unacceptability of (114) follows from a different requirement, namely the plurality condition for reciprocals - see § 6.2.5.3 above for a brief discussion of this condition. If this is indeed the case, then (114) is not an actual counterexample for either of these theories.

Chomsky (1981a:217) suggests that the GB SUBJECT binding theory may nevertheless be an improvement over the GB governor binding theory with regard to (114). The GB-governor binding theory does not prevent *each other* in (115) from being bound by *they*, since it is ungoverned in S\*. In the GB SUBJECT binding theory such coindexing is barred. AGR of S\* is an accessible SUBJECT for *each other*, and *each other* must therefore be bound in S\*. Under the GB SUBJECT binding theory, (115) is thus ruled out by both the binding theory and the plurality requirement. On these assumptions, one would expect (115) to be more unacceptable than (114). However, Chomsky admits that "one can hardly rely on such judgments". He suggests that "comparative evidence should prove relevant". The existence of parametric variation with respect to these constructions is noted in (Chomsky 1981a: 228, fn. 57).

Consider now examples with pronouns, which fall under principle (6B). In the examples of (118) *he* is proximate to *John*.

(118) / . . .

- (118) a. John saw [NP\* my picture of him] (3.2.3 (92))  
 (= (48a))
- b. \*I saw [NP\* John's picture of him]  
 (= (48b))
- c. \*John saw [NP\* a picture of him]  
 (= (48c))
- d. John thought [S\* I saw [NP a picture of him] ]  
 (= (48d))
- e. [S\* John read [NP his book] ]  
 (= (49a))
- f. John thought [S\* I saw [NP his book] ]  
 (= (49b))
- g. (?) [S\* John preferred [NP his reading the book] ]  
 (= (49c))

The GB governor binding theory makes the correct predictions about all these sentences, except (118c, e, f). (118g) is no real problem, since its status is determined by the Avoid Pronoun Principle. The GB SUBJECT binding theory makes the correct predictions about all the examples in (118), except (118e, g), with (118g) accounted for by the Avoid Pronoun Principle, as noted. NP\* and S\* are the governing categories. In (118a, d, f) *he* is free in its governing category. Thus these examples are acceptable. In (118b, c) *him* is bound in its governing category. Hence the unacceptability of these examples. What remains a problem, then, is (118e). Chomsky (1981a:217) points out that either (118e) or (119) does not fall under the binding theory, since in these constructions the pronoun is not free in the position where the anaphor is bound.

- (119) they read [NP each other's books] (3.2.3 (93))

Chomsky assumes that (119) falls under the binding theory. (118e) is then a potential counterexample for the binding theory. Chomsky points out that it has been suggested that *his* in (118e) is an obligatory variant of *himself's*, which is excluded from

(118e), contrary to the prediction of the binding theory. He (1981a:218) remarks that the near complementary distribution between proximate pronouns and reflexives is only partially captured within any of the approaches investigated in (Chomsky 1981a). Given differences among languages as to whether the analogue of *his* in (118e) can be regarded as proximate to the matrix subject, it is possible that something other than the binding theory is involved in these cases. It is then not clear, at present, whether (118e) is an actual counterexample to the binding theory.

The main points of the discussion above can be summarized as follows.

- (120) a. The GB SUBJECT binding theory has a conceptual advantage over the GB governor binding theory, in that the former overcomes a certain lack of deductive depth exhibited by the latter.
- b. The GB SUBJECT binding theory can handle the majority of those cases with arguments in NPs that apparently conflicted with the GB governor binding theory. The GB SUBJECT binding theory thus has greater empirical success than the GB governor binding theory.
- c. The ability of the GB SUBJECT binding theory to provide an explanation for the sentences in (110) provides an illustration of the fruitfulness of Chomsky's strategy of putting aside counterexamples until they can be taken up at a higher level of understanding.

### 6.6.3 Further modifications to the GB SUBJECT binding theory

Chomsky (1981a:219-221) considers two further modifications to the GB SUBJECT binding theory: (i) the addition of another principle to the theory of government, and (ii) a redefinition of the notion 'governing category'.

Following a suggestion by Hornstein, Chomsky (1981a:220) proposes

that the/ . . .

that the following principle be adopted as part of the theory of government.

- (121) "A root sentence is a governing category  
for a governed element." {3.2.3 (99)}

The adoption of the principle (121) makes it possible to overcome a problem concerning anaphors noted by Rizzi. In sentences such as (122), anaphors are governed, but lack governing categories, because they do not have accessible SUBJECTS.

- (122) a. [for each other to win] would be  
unfortunate {3.2.3 (97)}
- b. [for [<sub>i</sub> each other] to win] would  
be unfortunate for them<sub>i</sub> {3.2.3 (98)}

Both (122a) and (122b) are in fact unacceptable. The GB-SUBJECT binding theory incorrectly predicts that they are acceptable, since *each other* has no governing category in which it must be bound. If (122) is to be barred because it has no interpretation, it will make it impossible to adopt the simplest rule for the interpretation of *each other*, namely, apply the rule to any coindexed pair (NP, *each other*). While this rule will fail to assign an interpretation to (122a), it will (wrongly) assign an interpretation to (122b). The adoption of the principle (121) solves this problem. *each other* in (122) now has a governing category, namely the matrix clause. In both (122a) and (122b) *each other* is free in this governing category. In (122a) *each other* is not coindexed with another category, and in (122b) it is not coindexed with a c-commanding category - see the definitions of 'bound' and 'free' in (7), (8) above.

Cases such as (122a, b) are potential counterexamples for the GB-SUBJECT binding theory. The adoption of the principle in (121) as part of the theory of government is clearly a move aimed at protecting the GB SUBJECT binding theory from these potential

counterexamples/ . . .

counterexamples. Note that no independent justification is presented for the adoption of the relevant principle.

Chomsky (1981a:220f.) considers the possibility of simplifying the definition of 'governing category'. He proposes the elimination of the reference to government. The term "governing category" must then be replaced by something else. He proposes that it be replaced by "binding category", defined as in (123).

- (123) " $\beta$  is a *binding category* for  $\alpha$  if and only if  $\beta$  is the minimal category containing  $\alpha$  and a SUBJECT accessible to  $\alpha$ ." {3.2.3 (100)}

Binding principles (6A, B) are correspondingly reformulated as (124), and principle (121) is modified to (125).

- (124) (A) An anaphor is bound in its binding category  
(B) A pronominal is free in its binding category." {3.2.3 (101)}

- (125) "A root sentence is a binding category for a governed element." 3.2.3 (102)

In (125) Chomsky makes use of the concept 'root sentence', of Emonds (1976). Chomsky (1981a:220-221) claims that this proposed revision has "no meaningful consequences" in the case of overt elements or NP trace. Consider the case of PRO. The basic property of PRO is that it is ungoverned. As we have seen, this follows from the binding theory formulated in terms of 'government'. The ungoverned status of PRO no longer follows from the binding theory revised as in (124). What follows from (124) is that PRO lacks a binding category, which does not imply that PRO is ungoverned. However, the conclusion that PRO is ungoverned does follow from (124) in conjunction with (125). Note that (125) is also required in the unrevised theory. If PRO is governed, then by (125) it always has a binding category, in which it must be both bound and free by (124). Hence, PRO is ungoverned.

Sentences like (126) are problematic for the proposed simplification of the GB SUBJECT binding theory.

- (126) a. John expected [him to win] (3.2.3 (104))  
 b. John tried [ [PRO to win] ]  
 c. John knows [how [PRO to win] ]

As Chomsky (1981a:221) points out, *him* cannot be coindexed with *John*, because then (124B) will be violated. But exactly the same argument shows that PRO cannot be indexed with *John* in (126b, c), since the matrix clause is the binding category for PRO. This is the wrong result, however. The unrevised theory - i.e., with "governing category" in place of "binding category" - gives the correct result. In (126b, c) PRO has no governing category, and no requirement of unboundedness is thus made by principle (6B) of the binding theory. These examples indicate that it is necessary to introduce a crucial reference to government in the binding theory, i.e., that the simplification to (124) cannot be adopted. However, Chomsky (1981a:221) claims that the effects of such a reference to government "are so narrow as to suggest that an error may be lurking somewhere".

The elimination of the reference to government in the definition of 'governing category' would lead to a simplification of the theory, and thus to a conceptual improvement. However, the simplified version of the GB SUBJECT binding theory would face some potential counterexamples not faced by the unsimplified version of the theory. For this reason Chomsky does not actually adopt the simplified version of the theory.

In the case of the GB indexing theory, discussed in § 6.3.3.5 above, Chomsky adopted the simpler version of the theory in spite of the fact that it led to a loss of empirical success. The question then naturally arises why, in the present case, Chomsky made a different decision. This issue will be discussed in § 7.2.3.6 below.

The main points of the discussion above can be summarized as follows.

- (127) a. The principle in (121) is adopted in order to protect the GB SUBJECT binding theory from potential counter-evidence.
- b. The principle in (121) is without independent justification, and thus *ad hoc*.
- c. Chomsky considers replacing the definition of 'governing category' by the definition in (123) of 'binding category'.
- d. The proposed replacement would be justified on the grounds that the latter definition is simpler than the former.
- e. Chomsky does not actually adopt the proposed simplification, because the replacement would lead to a loss of empirical success.

#### 6.7 Remaining problems for the GB binding theory

Chomsky (1981a) mentions several empirical problems in the form of unexplained counterevidence which faces the GB-SUBJECT binding, empirically and conceptually the most successful version of binding theory to date. In some cases Chomsky merely mentions the existence of a problem, without giving much detail. In other cases he provides a detailed exposition of the exact nature of the problem. In several of these cases Chomsky tries to provide an explanation for the counterevidence, or at least to make a tentative suggestion regarding a possible explanation. In this section a brief overview is provided of the problems facing the GB-SUBJECT binding theory which are discussed by Chomsky (1981a). Suggestions by Chomsky about possible solutions will also be noted.

(i) *The status of reflexives*

Chomsky (1981a:218) notes that the concept 'anaphor' "has been left rather vague in the preceding discussion . . .". Informally, an anaphor is characterized as "an NP with no intrinsic reference". Reflexives fall under this characterization. Their cross-linguistic status with respect to the binding conditions is problematic, however. Chomsky (1981a:229, fn. 62) points out that the analogue to the reflexive in languages such as Japanese and Korean does not obey the binding conditions. It has been argued that, in some languages at least, such elements can be subject to pragmatic control. That is, they can be without antecedents.

The behaviour of these elements thus provides potential counter-evidence for the binding conditions. It has been suggested by Chomsky that the binding conditions may exhibit parametric variation with regard to what counts as an anaphor.<sup>73)</sup> This would provide a possible solution to the problem posed by the elements in question. No detail is provided, however.

(ii) *Pronouns in NP*

As was noted in § 6.6.2 above, no version of the GB binding theory, including the GB SUBJECT binding theory, makes the correct predictions about all cases of pronouns in NPs. For example, the GB SUBJECT binding theory incorrectly predicts that *his* in (128) (presented as (118e) above) must be free.

(128) [ <sub>S\*</sub> John read [ <sub>NP</sub> his book ] ]

The problem which (128) poses for the GB SUBJECT binding theory is thus left unsolved by Chomsky.

(iii) / . . .

(iii) *Constructions to which the binding theory must be extended*

Chomsky (1981a:225 fn. 36; 230:fn. 73) points out that there are cases to which the GB theory, including GB SUBJECT, must be extended. He mentions constructions with left-dislocated items, topics, predicate nominals, heads of relatives, and subjunctives. Only in the case of the latter does he (1981a:230, fn. 73) give some indication of the nature of the problems involved for the GB binding theory. He claims that, although they contain no overt AGR element, subjunctives behave like tensed clauses with respect to binding. (Unfortunately, Chomsky does not provide an example to illustrate this point.) For the present, he says, it must simply be stipulated that there is a null AGR in English subjunctives. This stipulation has the status of an *ad hoc* auxiliary hypothesis.

Chomsky (1981a:219) also points out that certain elements that do not function as anaphors in the narrow sense that applies to NP-trace, *each other*, PRO, etc., appear to obey the binding conditions. The sentence presented in (129) illustrates that the trace of extraposition is apparently subject to the binding conditions.

- (129) \* $\left[ \text{John's novel } t \right]$  arrived last week  
            $\left[ \text{that you ordered} \right]$  {3.2.3 (95i)}

In (129) the subject NP is the governing category for the trace of the extraposed clause. The unacceptability of (129) thus follows from the fact that this trace is free in its governing category.<sup>74)</sup> Chomsky refers to works by Fiengo and Lasnik and Quicoli, in which evidence is presented that the relation between an NP and a displaced quantifier (or maybe the relation between the trace of the quantifier and the quantifier) is also subject to Opacity.<sup>75)</sup> Chomsky points out that these cases are relevant to "a proper understanding of the notion 'anaphor'". No attempt is made in (Chomsky 1981a) to develop a notion 'anaphor' which would include all these cases.

(iv) *The AUX-to-COMP case in Italian*

Chomsky (1981a:225, fn. 36) briefly refers to an Italian construction derived by AUX-to-COMP movement with a nominative subject. This construction is discussed in an unpublished paper by Rizzi. Chomsky (1981c) also discusses the relevant construction, and the problems which it presents for the GB SUBJECT binding theory. The details that follow are from the discussion in the latter work.

Chomsky (1981c:3) assumes that the gerundive construction is of the form (130).

(130) NP G VP {5}

G in (130) is a realization of INFL, with the property that it may or may not be a governor. Italian examples with the structure of (130) are presented in (131).

(131) a. Gianni avendo telefonato, . . . {14i}  
 b. PRO avendo telefonato, . . . {14ii}

Italian has an AUX-to-COMP rule, that applies to structures such as (130), yielding structures such as (132).

(132) AUX - G [ NP .. ] {19}

Although Chomsky does not spell this out, it follows that in (130) there appears a COMP to the left of NP, and an AUX in G. Chomsky (1981c:7) claims that G in (132) must be a governor, with the NP governed by AUX in COMP. PRO is thus excluded from the NP position in (132), since PRO must be ungoverned. Consequently, the AUX-to-COMP rule derives (133a) from (131a), but not (133b) from (131b).

(133) a. avendo Gianni telefonato, . . . {21i}  
 b. \*avendo PRO telefonato, . . . {21ii}

According/ . . .

According to Rizzi, the nominative subject position in gerundive structures derived by the AUX-to-COMP rule is opaque. This follows quite straight-forwardly from the NIC. Chomsky (1981a:225, fn. 36) states that the island properties of the relevant structure also follow from the GB-governor binding theory. If G is indeed a governor, as is claimed by Chomsky (1981c:7), then the clause dominating the structure (132) is a governing category for the subject. By principle (6C) R-expressions must be free in this subject position.

Under the GB SUBJECT binding theory, an additional assumption is needed. The relevant clause must not only be a governing category for the subject NP. It must also contain an accessible SUBJECT for this NP. Chomsky (1981c:8) states that the latter assumption implies that G in COMP must have the same property as AGR, that is, it must be an accessible SUBJECT. He states that "while this is not an unreasonable conclusion in the framework of the binding theory there proposed [i.e., in (Chomsky 1981a) - M.S.], it is surely one that requires careful consideration". He does not elaborate on the latter point.

The assumption that G in COMP is an accessible SUBJECT has the status of an *ad hoc* auxiliary hypothesis, introduced to enable the GB SUBJECT binding theory to make the correct predictions about structures like (130).

(v) *A c-command failure for overt anaphors*

In terms of the definition of 'bound' adopted by Chomsky (1981a: 184), a binder must c-command the element bound by it. In constructions of the form (36a) with *each other* in the position of  $\alpha_6$ , this requirement is apparently violated. Consider, for example, the sentences in (134).

- (134) a. The rumors about each other irritated  
           the men (were annoying to the men)           (2.4.3 (18))

b./ . . .

## b. stories about each other disturbed the men

In both (134a) and (134b) *each other* is bound by the object of the verb, namely *the men*. However, *the men* does not c-command *each other* in (134).<sup>76)</sup> Chomsky (1981a:229 fn. 64) suggests that such cases may require "a slight modification of binding theory, relaxing the notion of c-command". No further detail is provided. Chomsky also claims that none of the binding theories reviewed in (Chomsky 1981a) can accommodate these cases.

## (vi) Arguments in PPs

There are several problems relating to the status of arguments within PPs.<sup>77)</sup> Consider firstly sentences like (135), with *each other* in the NP position of PP.

- (135) a. I spoke to the men about each other  
 b. \*I spoke about the men to each other<sup>78)</sup>

Chomsky (1981a:225, fn. 37) claims that the order of the two PPs in (135) is free, with a preference for the *to*-phrase preceding the *about*-phrase. Only in (135a) can the NP of the first PP be the antecedent of the anaphor. The unacceptability of (135b) follows from the GB binding theory, since *the men*, which must bind *each other*, does not c-command it. This leaves unexplained the acceptability of (135a). Chomsky proposes that (135a) might be the result of a rule of reanalysis applying to *speak to*, so that *the men* c-commands *each other*. He then considers various consequences of this proposal, some of which seem plausible, while others are wrong. He concludes that "it is not clear whether this approach is on the right track".

Consider, secondly, sentences with pronouns in the NP position of PPs. Chomsky (1981a:289) discusses an observation by Jean-Yves Pollock, that in Romance languages pronouns in PP can regularly be coindexed with NP antecedents in the same clause, as in (136).

(136) Jean m'a parlé de lui (5.2 (1))  
 ("John spoke to me about himself")

According to the different versions of the GB binding theory, including GB SUBJECT, the full clause in (136) is the governing category for the pronoun *lui*. In (136) *lui* is bound in its governing category, being coindexed with *Jean*. Principle (6B) is thus violated. Chomsky (1981a:289) considers three possible approaches to the problem raised by sentences such as (136). A first possibility is that PP serves as a governing category. This possibility is rejected on "theory-internal considerations". In terms of the GB SUBJECT binding theory, governing categories have accessible SUBJECTS. According to Chomsky, there is "little reason" to assume that PP in Romance languages has a SUBJECT while its analogue in English does not have a SUBJECT.

A second possibility is to relate the binding properties of pronouns in PPs to limitations on the distribution of reflexives. There is (near) complementary distribution between reflexives and proximate nouns. Chomsky rejects this approach as "unlikely" to be correct, without giving any particulars.

The third possibility, which "seems more plausible" to Chomsky (1981a:289), is to relate the appearance of proximate pronouns in sentences such as (136) to the option of cliticization in the Romance languages. It has been observed that clitics in the Romance languages behave in the manner of the English pronouns with regard to disjoint reference. According to Chomsky, this suggests that it is clitics, rather than full pronouns, which fall under principle (6B) in those languages that allow cliticization. He points out that in general full pronouns do not observe principle (6B). A possible explanation is that, in languages with the clitic option, full pronouns should be regarded as "somehow emphatic, thus immune to principle (6B) of the binding theory". This proposal has the status of an *ad hoc* auxiliary hypothesis.

Chomsky (1981a:290) provides English examples similar to (136).

- (137) a. John always keep his wits about him {5.3 (2)}  
 (\*himself, \*Bill)  
 b. the melody has a haunting character to it  
 (\*itself, \*Bill)  
 c. John likes to take his work home with him  
 (\*himself, \*Bill)
- (138) a. John pushed the book away from him {5.2 (3)}  
 b. John drew the book towards him  
 c. John turned his friends against him  
 d. John saw a snake near him

In the examples of (137) a proximate pronoun is obligatory, while in (136) it is optional. Chomsky points out that judgments vary as to whether a proximate pronoun or reciprocal should be used in some of the examples of (138), and "obscure factors enter into preference one way or another . . .". Thus, (138) contrasts with (139).

- (139) John turned the argument against himself {5.2 (4)}  
 (\*him where the reference is to John)

Sentences such as (137) and (138) pose a problem for the GB binding theory similar to the one posed by (136). In all cases the matrix clause is the governing category for the pronoun. By principle (6B) the pronoun must be free in its governing category. In each case, however, the pronoun is bound in the matrix clause, thus violating principle (6B).

One possible approach to these cases, according to Chomsky (1981a:290), is to argue that in cases where reflexives are excluded, PP is a governing category. If this is correct, reciprocals should also be excluded. In some cases, the correlation is "reasonably straightforward". Compare, for instance, (138c) and (139) with (140a, b), respectively.

- (140) a. \*they turned their friends against  
each other {5.2 (5)}
- b. they turned the arguments against  
each other

The relevant interpretation of (140a, b) is with *each other* bound by the subject. Chomsky notes that often in such cases judgments are "unclear". He states that "a proper theory dealing with these matters should explain the choice of elements and also the haziness of the judgments concerning them in many cases".

Chomsky (1981a:290) observes that it is "tempting" to suppose that such examples as (138) should be treated as analogous to (141), with the proximate interpretation of the pronoun.

- (141) a. John considers Mary angry at him  
(\*himself, \*her, herself) {5.2 (6)}
- b. John strikes Mary as angry at himself  
(\*him, her, \*herself)

It is assumed that these sentences have the representations (142) with embedded clauses at the level of LF (and in fact at every level of representation).<sup>79)</sup>

- (142) a. John considers [Mary angry at him] {5.2 (7)}
- b. John<sub>i</sub> strikes Mary [t<sub>i</sub> as angry at himself]

According to Chomsky, one could argue that, correspondingly, (138) has the representation (143) at LF, where *John* controls PRO, the subject of the predication *near him*.

- (143) John saw a snake [S PRO near him] {5.2 (8)}

Referring to work by Manzini, Chomsky rejects this approach. Manzini's own proposal is that PP, like other categories, has a kind of PRO subject which functions as an agreement element.

PP should then be a governing category, allowing pronouns and anaphors accordingly. It follows from this assumption that (144) should be acceptable in English, with the reciprocal bound by the subject.

(144) they saw snakes near each other (5.2 (11))

Chomsky notes that, as throughout this category of examples, judgments "tend to be uncertain". He himself does not make a firm suggestion about (138).

The main points of this section can be summarized as follows.

- (145) a. The GB SUBJECT binding theory, though empirically the most successful version of binding theory, nevertheless faces several empirical problems, including potential counterexamples.
- b. Chomsky (1981a) makes suggestions about possible solutions for some of these problems, but several problems remain unsolved.
- c. The examples discussed above illustrate both Chomsky's attitude of epistemological tolerance towards negative evidence threatening his theory and his willingness to search for solutions to empirical problems threatening his theory.
- d. The examples discussed above also illustrate that Chomsky takes data from languages other than English into account when formulating his theory.

## 6.8 The elimination of structure-building rules in the GB framework

In § 5.6 above I outlined a problem for the OB binding theory

noted in/ . . .

noted in (Chomsky 1980b). The problem concerned sentences like (146).<sup>80)</sup>

- (146) a. They regard me as very much like each other (them). {(38)a, b}
- b. I impress them as very much like each other (them).

In both cases *each other* cannot be bound by *they*, and *them* can be coindexed with *they*, exactly as if these sentences contained a subject that invokes opacity. However, Chomsky (1980b:17) observes that there seems to be no syntactic motivation for assigning "anything beyond the obvious surface structure" to such sentences. In particular, there is no subject in the surface structures of these sentences that could invoke Opacity. Chomsky solves this problem by adopting structure-building rules that assign to sentences such as (147) representations such as (147) at the level of LF.<sup>81)</sup>

- (147) a. they regard me as [<sub>S</sub> PRO be very much like each other (them)] {(39a, b)}
- b. I impress them as [<sub>S</sub> PRO be very much like each other (them)]

In (147a) PRO is controlled by *me*, and in (147b) by *I*. At the level of LF sentences such as (146) thus do contain a subject that can invoke Opacity. Since it is assumed in (Chomsky 1980b) that the binding conditions apply at the level of LF, the problem posed for the OB binding theory by sentences such as (146) is thus solved.

Chomsky (1981a:29) adopts the projection principle, presented in (148), as a general principle of UG.

- (148) "Representations at each syntactic level (i.e., {2.1 (38)} LF, and D- and S-structure) are projected from the lexicon, in that they observe the subcategorization properties of lexical items."

He (1981a:32) points out that a theory which incorporates structure-building rules violates the projection principle. It follows that structure-building rules are not allowed within the GB framework. The question then arises how the GB binding theory would be able to make the correct predictions about sentences such as (146). In essence, the answer is that within the GB-framework verbs like *regard* and *impress* take clausal *as*-complements. By the projection principle, sentences such as (146) will thus contain an embedded subject at every level of representation.

Chomsky (1981a:109-110) argues that the structure of sentences with *regard* is of the form (149a), while the structure of sentences with *impress* is of the form (149b). The analysis (149a) follows from the projection principle, while (149b) follows from the projection principle and Case theory.<sup>82)</sup>

- (149) a. John regards [Bill as foolish] (2.6 (25ii))  
 b. John<sub>i</sub> impressed me [t<sub>i</sub> as intelligent]

Given these assumptions, Chomsky (1981a:198) provides the following examples that correspond to the sentences in (146).

- (150) a. they<sub>i</sub> regard [Bill as too critical of them<sub>i</sub> (\*themselves, \*each other)] (3.2.3 (37ii))  
 b. Bill regards [them<sub>i</sub> as too critical of themselves (\*them<sub>i</sub>, each other)] (3.2.3 (37iii))  
 c. I impressed them<sub>i</sub> [t as too critical of them<sub>i</sub> (\*themselves, \*each other)] (3.2.3 (37iv))  
 d. they<sub>i</sub> impressed me [t as too critical of themselves (\*them<sub>i</sub>, each other)] (3.2.3 (37v))

The GB binding theory yields the correct results in each case. The embedded clause is the governing category throughout. In (150a, c) *them* is free in its governing category, as required by principle (6B), while *themselves* and *each other* are free in their

governing/ . . .

governing category in violation of principle (6A). In (150b, d) *themselves* and *each other* are bound in their governing category, as required by principle (6A), while *them* is bound in its governing category in violation of principle (6B).

Note that Chomsky (1981a:196) argues that binding theory applies at the level of S-structure, rather than at the level of LF. This revision has an effect on the applicability of principle (6C) only, i.e., it has no effect on the binding of anaphors and pronominals. Thus, the GB binding theory will yield the correct results for (150), irrespective of whether it applies at S-structure or at LF. The reason is that the sentences in question will have essentially the same structure at the level of S-structure and the level of LF, given the projection principle.

As was noted above, Chomsky's decision to eliminate structure-building rules was based on the fact that such rules are incompatible with the projection principle, a principle which Chomsky regards as having considerable support.<sup>83)</sup> The elimination of structure-building rules was thus based on a conceptual consideration, viz., the avoidance of an internal inconsistency in linguistic theory.

The main points of this section can be summarized as follows.

- (151) a. Chomsky eliminates structure-building rules in order to overcome a potential conceptual problem, namely, an internal contradiction resulting from the incompatibility of structure-building rules with the projection principle.
- b. Within the GB theory the independently justified projection principle provides a solution to the problem which sentences like (146) pose for the binding theory.

Footnotes to chapter 6

1. Cf. Chomsky 1981a:§§2.2, 2.6 for an exposition of  $\theta$ -theory.
2. Cf. Chomsky 1981a:330 for formal characterizations of 'pronominal' and 'PRO'.
3. Chomsky (1981a:47) stipulates that the grammatical functions/GFs determined in A-positions are called "A-GFs". Cf. Chomsky 1981a:42 for further discussion of the notion 'grammatical function'.

Chomsky (1981a:185) observes that the definition (10) of the notion 'variable' is inadequate in cases such as (3.2.3 (7)).

- (i) the man [to whom I gave the book t]
- (ii) the man [whose picture I saw t]
- (iii) John, [a picture of whom I saw t yesterday]

In these cases the trace  $t$  is not the variable bound by the phrase in COMP. Chomsky leaves this problem open.

4. Cf. also Chomsky 1981a:§4.4 for a further modification.
5. Cf. Chomsky 1981a:52 for further discussion of the properties of INFL and AGR. Cf. also Chomsky 1981a:164 for the possibility of regarding INFL itself as the governor.
6.  $S$  is not regarded as a maximal projection. Cf. Chomsky 1981a:164.
7. Cf. Chomsky 1981a:162 for more examples.
8. If the principle (i) were adopted, then the Case filter could be reformulated as the Extended Case Filter (ii).

(i)/ . . .

(i)  $[\text{NP } e]$  is a variable if and only if it has Case (3.2.2 (16))

(ii)  $*[\text{NP } \alpha]$  if  $\alpha$  has no Case and  $\alpha$  contains a phonetic matrix or is a variable. (3.2.2 (17))

Cf. Chomsky 1981a:175 for discussion of this point.

9. Cf. for example the discussion of {3.2.3 (9)} by Chomsky (1981a:186).
10. Chomsky (1981a:§5.1) presents evidence that this simple indexing theory is in fact inadequate. Cf. the discussion in § 6.3 below.
11. In some of the examples discussed in §§ 6.2.5.2 and 6.2.5.3 I indicate more structure than Chomsky does. This is done in order to make certain points clearer. Since the "additional" structure indicated by me does not conflict with the structure assumed by Chomsky, I do not comment on it in individual cases.
12. Cf. Chomsky 1981a:154 for this example, and also for (24b), (25b).
13. Cf. for example, Chomsky 1981a:66f. for a discussion of exceptional Case-marking in such instances.
14. If the principle {3.2.2 (16)} holds, the Case-less status of NP-trace would follow. Cf. footnote 8 above for {3.2.2 (16)}. Cf. also Chomsky 1981a:334, 345, footnote 5 for further discussion of this issue.
15. Cf. the discussion of the ECP in (Chomsky 1981a:Chapter 4) in this connection.

16. NP-trace is excluded from position  $\alpha_3$  by conditions on preposition stranding. Cf. Chomsky 1981a:292f. for a discussion of the latter phenomenon. NP-trace is excluded from position  $\alpha_4$  because it is an ungoverned position. Cf. Chomsky 1981a:Chapter 4 for a discussion of the ECP, from which it follows that NP-trace must be properly governed.
17. Not all the examples presented below are discussed in § 3.2.3 of (Chomsky 1981a). In cases where there are "gaps" in the data discussed in the latter section, I have tried to fill them with examples discussed in other subsections of (Chomsky 1981a).
18. Note that in (27) and (28b) the governor of NP-trace is a Case-assigner, which, strictly speaking, is not allowed.
19. Cf. Chomsky 1981a:330 for formal definitions from which it follows that PRO is both a pronominal and an anaphor.
20. Cf. Chomsky 1981a:§2.4 for a discussion of the ungoverned status of PRO.
21. Cf. Chomsky 1981a:156 for these conditions.
22. Cf. Chomsky 1981d:140ff. for more examples. These are discussed in § 6.4.3 below.
23. Cf. Chomsky 1981a:154 for this example. There is clearly an error in {3.2.3 (57ii)}. As printed, *each other* is bound by *their* in NP\*, while the intention clearly is that *each other* must be free in NP\*.
24. Definition {3.2.1 (6)} reads as follows:  
" [  $\beta$  ...  $\gamma$  ...  $\alpha$  ...  $\gamma$  ... ], where  
(i)  $\alpha = X^0$   
(ii) where  $\phi$  is a maximal projection,  $\phi$  dominates  $\alpha$  if and only if  $\phi$  dominates  $\gamma$ ."

$\alpha$  then governs  $\gamma$ .

25. Definition (3.2.1 (4)) reads as follows:

" $[\beta \dots \gamma \dots \alpha \dots \gamma \dots]$ , where

(i)  $\alpha = X^0$

(ii) where  $\phi$  is a maximal projection, if  $\phi$  dominates  $\gamma$  then  $\phi$  dominates  $\alpha$ .

(iii)  $\alpha$  is an immediate constituent of  $\gamma$ ."

$\alpha$  then governs  $\gamma$ .

26. Cf. Chomsky 1981a:165 for a discussion of these possibilities

27. Cf. Chomsky 1981a:65 for a discussion of this principle.

28. Some of these problems are also briefly and informally mentioned in (Chomsky 1981b). Since this work provides only an informal (and incomplete) account of the ideas contained in the other works, no further reference is made to it.

29. Cf. Chomsky 1981a:155 for the example (2a).

30. The requirement of government for trace follows from the Empty Category Principle.

31. Cf. the discussion in § 5.7 above for details of this explicit redundancy.

32. Cf. § 5.3 above for more discussion on the link between the elimination of redundancies and Chomsky's general assumption about the simplicity of the language faculty. For a detailed analysis, cf. § 7.2 below.

33. Cf. in this connection the remarks in (Chomsky 1977c:111) on the "naturalness" of the SSC and PIC.

34. Cf., for example, Chomsky 1978a:16, 1981b:48, 50.
35. In connection with the issue of the identification of actual conceptual problems, it is interesting to note Kayne's (1981b) attempt to provide an explanation for the c-command requirement stipulated in Chomsky's binding theory, in terms of the notion 'unambiguous path'. For Kayne this stipulation represents a conceptual problem.
36. These are the violations resulting from taking only  $\bar{S}$ , and not S, as a bounding node for Subjacency. Cf. Rizzi 1981 for more similar examples, and a discussion of the problems raised by them.
37. Chomsky (1981a) uses the term "SSC" for the pre-1978 formulations of the relevant condition, as well as for its reformulated version, the Opacity Condition. I will use the notation "SSC/Opacity Condition" to refer to the relevant condition.
38. Examples are separately numbered in each subsection of (Chomsky 1981a). I will refer to these numbers by indicating the relevant subsection and number in curly brackets. Thus, {3.1 (8)} refers to number (8) of subsection 3.1 of (Chomsky 1981a).
39. Cf. Chomsky 1976a:335f. for a discussion of the similarity between names and variables.
40. Cf. Sportiche 1981.
41. Chomsky (1981a:159f.) explains the contrast between {3.1 (11)} and {3.1 (13)} in the same way. Cf. Chomsky 1981d:8 for additional examples.
42. Since one might assume that the NIC holds only for the

referential/ . . .

referential indices of variables but not their anaphoric indices (the cases of crossover), or that the NIC does not hold in the crossover cases for some other reason there is only a near contradiction. Chomsky continues that he is sceptical about any attempt to avoid the problem along these lines, since the conclusion that variables are not subject to the NIC seems right on other grounds, namely in view of the "conceptual" relation between names and variables, which would exclude variables from the NIC in principle, a conclusion supported by the general applicability of principle (6C) of the binding theory to variables.

43. Chomsky (1981a:250) explains that pro-drop languages exclude the condition " $\alpha \neq \text{AGR}$ " from their notion of proper government.
44. Cf. Chomsky and Lasnik 1977:450f. for a discussion of this filter.
45. Cf. Taraldsen 1978, Pesetsky 1982, Kayne 1980.
46. Chomsky (1981a:161) mentions a further fact relating to the \* $[\text{that}-t]$  filter that must be explained. This filter does not apply in languages that allow missing subjects, i.e., languages that have the "pro-drop parameter". Apparent violation of the \* $[\text{that}-t]$  filter is one of a clustering of properties related to pro-drop in these languages. Cf. Chomsky 1981a:240. Chomsky (1981a:161) says that "we want to explain this clustering, if possible, in terms of a single parameter, which should be related to RES(NIC)".
47. Cf. also Chomsky 1982a:54 for some remarks by Chomsky on the generality of the ECP. Cf. also Kayne 1981a for a highly positive evaluation of the ECP.
48. Cf. the discussion in Chomsky 1981a:§5.3 and the references cited there.

49. Chomsky (1981a:314, fn. 1) thanks Howard Lasnik for the observations discussed in his § 5.1.
50. The sentences (1i) and (2) referred to in (68) are the following:  
  
(1i) John<sub>i</sub> told Bill<sub>j</sub> that they<sub>k</sub> should leave  
(2) John<sub>i</sub> told Bill<sub>j</sub> that he<sub>k</sub> should leave  
  
Cf. Chomsky 1981a:285 for an explication of the problems raised by these sentences for the indexing theory.
51. Cf. also Chomsky 1981a:224, fn 35 for this formulation.
52. Cf. Chomsky 1979b:17; 1981a:225, fn. 35 for this argument.
53. Cf., for example, Chomsky 1981d:133 for an explication of this point.
54. Cf., for example, Chomsky 1981d:133 for an explanation of this point.
55. Cf. Chomsky 1981a:275, 334 for a discussion of these points.
56. Cf. in this connection also Chomsky 1982b:81 for the distinction between the empty categories PRO and *pro*.
57. Cf. Chomsky 1979b:26 for a brief discussion of similar cases with pronouns, for example,  
John saw [a picture of him]
58. Cf. Chomsky 1973:239, fn. 19 for a discussion of such examples. Cf. also § 3.2.7.3 above.
59. Chomsky also refers to (85) - (93) as "structures". Cf. for example, Chomsky 1981d.

60. Cf. §§ 4.3 and 4.4.5 for discussion of such cases.
61. Cf. § 4.3 for details of this case.
62. Cf. the discussion in § 6.3 above for the conceptual problems of the OB theory that partly motivated the development of the GB binding theory.
63. Botha (1982a) argues that the episode in the development of the binding theory presently under discussion strongly resembles Galileo's attempt to defuse the so-called Tower Argument against the Copernican theory of the earth's diurnal rotation. Hence, Botha calls this episode "a Galilean episode in Chomskyan linguistics".
64. The full text of Chomsky's "In summary" paragraph is as follows:

"In summary, in accordance with the GB system the sentences (18) and (20ii) are unmarked and the sentence (21) is marked, whereas in the OB system the conclusions are reversed. Thus the two theories make different predictions concerning markedness."

Chomsky's {18} corresponds to (85) above, {20ii} corresponds to (86), and {21} corresponds to (87).

65. Cf. Chomsky 1981a:105f, 167f for the analysis of small clauses.
66. Chomsky (1981a:25) adopts the base rule {2.1. (25)} -  
S → NP INFL - from which it follows that S will always contain a subject.
67. Cf. the references cited in footnote 65 above.
68. Cf., for example, Chomsky 1981a:52 for a discussion of the

latter/ . . .

latter point. Note also that AGR is not a binder with respect to the binding theory. Cf. Chomsky 1981a:211-212.

69. Chomsky (1981a:229, fn. 63) discusses the possibility that (103) as it stands, is too strong, and that an "unless"-condition should be added to it.
70. Cf. Chomsky 1973:239, fn. 19, Koster 1978b:166f., and the discussion in § 3.2.7.4 above for more detail.
71. Chomsky (1981a:230, fn. 67) says that the discussion of these examples is based on suggestions by Tim Stowell and Dominique Sportiche.
72. Cf. § 3.3.5 above for some discussion of these remarks, and for an analogous case.
73. Cf. Chomsky 1980b:15 for a discussion of the problematic status of reflexives, and of the possibility of parametric variation with regard to what counts as anaphors for the binding theory. Cf. also the discussion in § 5.2 above.
74. Cf. Chomsky 1981a:219 for more detail on this interpretation of the unacceptability of (129).
75. Cf. the discussion in § 3.3.4 above on the applicability of the SSC to Quantifier Movement.
76. Cf. Chomsky 1981a:166 for an explanation of why there is no c-command in this case.
77. The cases discussed directly above in fact also involve arguments within PPs.
78. Cf. Chomsky 1981a:225, fn. 37 for these examples.

79. Cf. Chomsky 1981a:109f. for a discussion of these assumptions
80. These examples are presented as 5. (32a, b) above.
81. These structures are presented as 5. (34a, b) above.
82. Cf. Chomsky 1981a:109, 110 for more detail.
83. The discussion by Chomsky (1982b:8-10) gives a good indication of the scope of the projection principle. Cf. also Chomsky 1981a:29-34 for some discussion of the empirical and conceptual support available for this principle.

## Chapter 7

### THE RATIONALITY OF CHOMSKY'S LINGUISTICS

#### 7.1 Introduction

Chapters 3 - 6 contain a detailed metascientific description of the developmental history of binding theory - from the first version of the SSC and TSC proposed in the early seventies up to the most recent version of binding theory. The primary aim of chapter 7 is to develop a model of what constitutes rationality in Chomsky's linguistics, as this rationality is instantiated in the developmental history of binding theory. The task of constructing such a model, and of determining how successful it is in accounting for the developmental history of binding theory, is undertaken in § 7.2 below.

In addition to the primary aim outlined above, this chapter has three secondary aims.

The first secondary aim is to determine whether Chomsky's rationality exhibits any internal problems, for example in the form of internal contradictions. It will also be determined how serious these problems are (§ 7.3).

The second secondary aim is to compare Chomsky's rationality with the models of scientific rationality proposed by Laudan and Newton-Smith (§ 7.4). It must be determined what conflicts there are between Chomsky's rationality and Laudan's and Newton-Smith's models of scientific rationality. The question of how such conflicts must be interpreted will also be considered in § 7.4.

The third secondary aim is to determine the accuracy of the characterizations provided by others - including Chomsky himself - of the method employed in Chomsky's linguistics (§ 7.5). In addition to Chomsky's own metascientific comments, the recent accounts by Cook (1981) and Lightfoot (1982) will be critically appraised.

## 7.2 A minirat account of theory choice in Chomsky's linguistics

### 7.2.1 General remarks

In accordance with the view presented in § 2.2 above about the structure of a model of rationality, the model of Chomsky's rationality to be developed here consists of two components:<sup>1)</sup>

- (i) a specification of the goal of Chomsky's linguistics,
- (ii) a specification of the principles of theory appraisal and their appropriate use.

The main aim of § 7.2 is to develop a model in terms of which a minimal rational account - in Newton-Smith's sense - of the developmental history of binding theory can be provided.<sup>2)</sup> Consequently, no critical questions regarding Chomsky's rationality will be raised in § 7.2. A critical appraisal of Chomsky's rationality will be postponed until §§ 7.3 and 7.4.

### 7.2.2 The model: A preliminary formulation

#### 7.2.2.1 The goal of Chomsky's linguistics

No systematic attention was paid in §§ 3 - 6 to the question of the goal of Chomsky's linguistics. The main emphasis in these sections is on the factors that play a role in theory choice. Consequently, chapters 3 - 6 contain very little textual evidence from Chomsky's writings to support any claim about his goal. The focus in the present section will be on metascientific comments by Chomsky which shed light on his goal.

The goal of Chomsky's linguistics is to find truth, in particular, the truth about the language faculty, one component of the human mind. In the case of the theory of UG the goal is to find the truth about the initial state of the language faculty. In the case of theories

about/ . . .

about specific mental grammars the goal is to find the truth about the various final states of the language faculty. Since the present study is concerned with the development of a component of UG, the focus will be on UG in the following discussion.

One of the clearest statements by Chomsky on the goal of his linguistics is in (1980b:2). (In (1) "task for" can be replaced by "goal of", and the phrase "to discover the true nature of" can be read as "to discover the truth about".)

- (1) "The task for linguistic theory is to discover the true nature of the biological endowment that specifies the general structure of the language faculty."

Another clear statement occurs in (Chomsky 1979a:180). Comparing structuralism with generative grammar, he (1979a:178-179) makes it clear that he regards generative grammar, as opposed to structuralism, as a field with "real intellectual content". Commenting on the possible use of procedural methods in fields with "real intellectual content", Chomsky (1979a:179) makes the following remarks.

- (2) "There are no 'methods' in this sense in a field having real intellectual content. *The goal is to find the truth.* How to do that, nobody knows. There are no procedures that can be outlined in advance for discovering scientific truth." (The italics are mine - M.S.)

Since it is clear from the context of the discussion that Chomsky regards generative grammar as a field "with real intellectual content", the remarks quoted in (2) about scientific truth apply also to Chomsky's linguistics, a form of generative grammar.<sup>3)</sup>

In his (1980a:104-109) discussion of the question of whether there is a distinction between psychological reality and truth, the assumption that Chomsky's linguistics aims to construct true theories of the language faculty features prominently.<sup>4)</sup> In this discussion Chomsky takes it for granted that linguistic

theories aim at the truth. The only issue he is concerned with in this discussion is whether evidence for the truth of a theory is also evidence for the psychological reality of this theory. The idea that to find the truth is the goal of Chomsky's linguistics can also be found in many of his earlier works. For instance, Chomsky (1972a:18), commenting on grammars that represent underlying linguistic competence, states that "the problem of determining the character of such grammars and the principles that govern them is a typical problem of science, perhaps very difficult, but in principle admitting of *definite answers that are right or wrong as they do or do not correspond to the mental reality*". (The italics are mine.) Given that at that stage Chomsky equated mental/psychological reality with truth, these remarks provide additional support for the claim that Chomsky regards the finding of truth as the goal of his linguistics.

There is an important qualification that must be added to the claim that to find the truth is the goal of Chomsky's linguistics.<sup>5)</sup> The goal is not simply to discover *any truth*, but to discover truth that can provide explanations, that is, *explanatory truth*. For instance, the goal of Chomsky's linguistics cannot be reached by providing an accurate catalogue of all the observable properties of utterances.

There is strong textual evidence for the claim that Chomsky is interested in *explanatory truth*, that is, in developing theories with explanatory power. Chomsky's metascientific comments contain numerous references to the importance of explaining facts, the value of depth of explanation as opposed to gross coverage of data.

The following passage from (1980a:11) is typical in its emphasis on explanatory theories and explanations.

- (3) "I am interested, then, in pursuing some aspects of the study of mind, in particular, such aspects as lend themselves to inquiry through the construction of abstract *explanatory*

*theories/ . . .*

*theories* that may involve substantial idealization and will be justified, if at all, by success in *providing insight and explanations*. From this point of view, substantial coverage of data is not a particularly significant result; it can be attained in many ways, and the result is not very informative as to the correctness of the principles employed. It will be more significant if we show that certain fairly far-reaching principles interact *to provide an explanation for crucial facts . . .*" {The italics are mine - M.S.}

The importance of explanation is also stressed in Chomsky's earlier works. Consider, for example, (Chomsky 1965:25f; 1972a: 27).

There is a second qualification that must be added to the claim that the aim of Chomsky's linguistics is to find truth. Chomsky explicitly states that all current linguistic theories are in many respects false, as well as incomplete. Consider, for instance, his remarks on this issue in (Chomsky 1978a:24; 1980b: 2-3; 1981a:4; 1981e:8). Nevertheless, in his recent works Chomsky expresses confidence that progress has been made. Consider in this connection his remarks in (Chomsky 1981a:3, 344; 1982b:3, 89). His (1981a:344) remarks are quoted in (4).

- (4) "Whatever the defects of current theories may be - and they are sure to be severe - recent developments seem to me to open up new and exciting prospects, and may point the way to new and deeper understanding of the nature of language, with non-trivial implications over a considerably broader range."

Clearly some qualification is needed in order to reconcile the claim that to find truth is the goal of Chomsky's linguistics with the claim that progress has been made even though all current theories are false.

The problem is, of course, not unique to Chomsky's linguistics. It arises in any account of a scientific enterprise which posits truth as the goal. As Newton-Smith (1981:14, 183) explains, there is strong inductive support for the conclusion that "all

theories/ . . .

theories which have been or will be propounded are strictly speaking false". If there is reason to believe that truth is unattainable, how can truth then be posited as the goal of science? One well-known solution to this problem is to assume that while all current and future theories are false, they are nevertheless capturing more and more truth about the world. That is, theories are increasing in verisimilitude.<sup>6)</sup>

There are many philosophical problems surrounding the notion of increased verisimilitude.<sup>7)</sup> A discussion of these problems, and their possible solutions, falls outside the scope of the present study. It is sufficient to note here the need for some qualification to reconcile the claim that the goal of Chomsky's linguistics is to discover truth with the probable falseness of all current and future linguistic theories, and that the notion of increased verisimilitude has been offered by some philosophers of science as a possible solution to this problem.

In sum, then, the main claim being made here about the goal of Chomsky's linguistics can be formulated as follows.

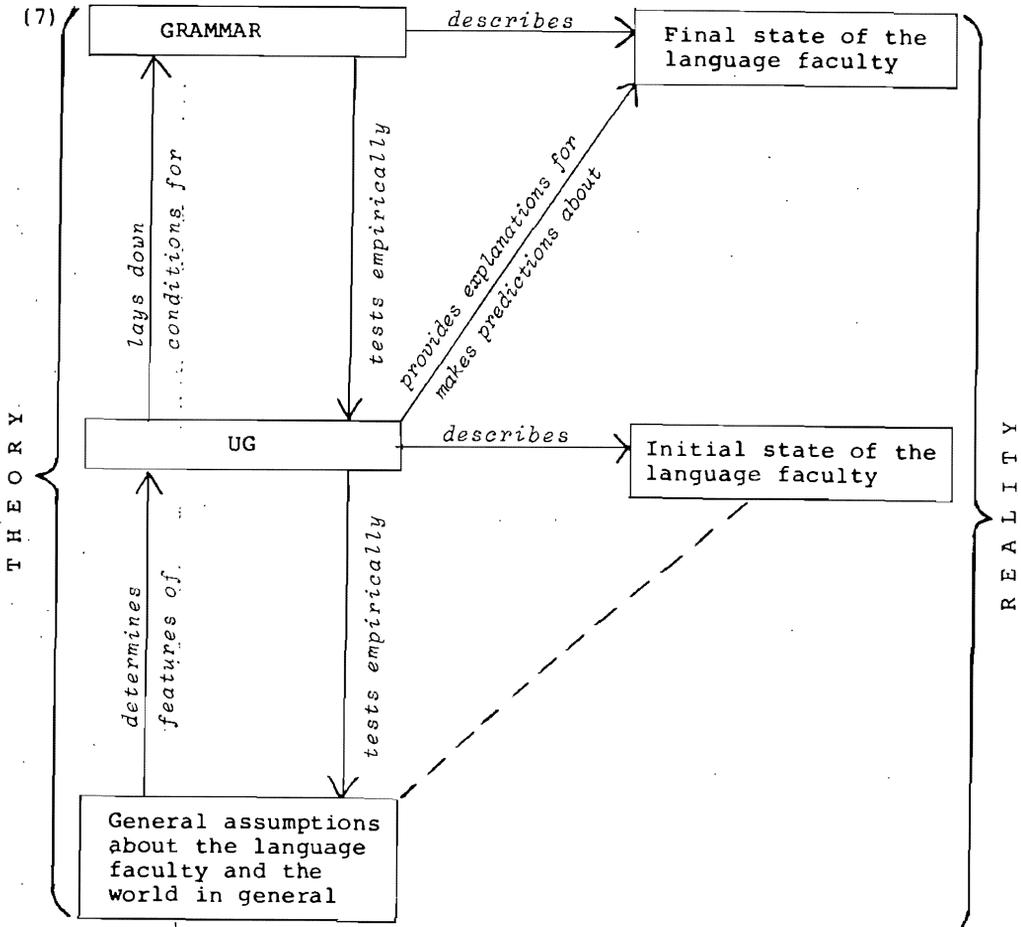
- (5) The goal of Chomsky's linguistics is to find the truth about the language faculty.

This claim must be qualified in two respects. These qualifications are presented in (6).

- (6) a. The goal is to find explanatory truth, and not just any truth.
- b. Since there is reason to assume that all current and future linguistic theories are false, Chomsky's linguistic theories must be seen as aiming to get nearer to the truth, or as aiming to capture more and more truth.

7.2.2.2 The principles of theory appraisal

In explicating the various principles of theory appraisal that have featured in the developmental history of binding theory, it will be useful to keep in mind some of the relationships that exist among a general theory of language, as represented in a specific version of UG, a proposed grammar of a natural language, the initial and final states of the language faculty, and certain general assumptions made by Chomsky about the nature of the language faculty. In (7) a highly simplified schematic representation of the relations which are relevant for the present discussion is provided. 8)



The representation/ . . .

The representation of the general assumptions about the language faculty and the world in general held by Chomsky in the schema (7) is not quite satisfactory. For one thing, the schema creates the impression that the relation between these general assumptions and the theory of UG is identical to the relation between UG and specific grammars. These general assumptions would more accurately be represented as being in a different dimension from UG and specific grammars. Another shortcoming is that (7) fails to reflect the relationship between these general assumptions and the world. The dotted line linking these assumptions to the initial state of the language faculty merely suggests that the assumptions are held to correspond to aspects of reality in some way. However, in spite of these shortcomings, the schema in (7) can form a useful background for the following explication of Chomsky's principles of theory appraisal.

Note that the term "grammar" is frequently used by linguists with a systematic ambiguity to refer both to a description of the final state of the language faculty, and to the final state itself. This usage will also be adopted below. Where the context does not make it clear which sense is intended, the expression "descriptive grammar" will be used to indicate the first sense distinguished above, and "mental grammar" to indicate the second sense.

There is one further point to be considered here before I can proceed with an explication of the various principles of theory appraisal that guided Chomsky in making the various theory changes analyzed above, namely, the relative importance of these changes. Intuitively, the various changes analyzed in chapters 3 - 6 differ as regards their importance. For instance, the replacement of the SSC and PIC by the OB binding conditions strikes one as being more important than the adoption of the auxiliary hypothesis that Picture-Noun Reflexivization falls outside the domain of sentence grammar.

Several/ . . .

Several factors might contribute towards these intuitive judgments of importance, but only one factor will be discussed here. The principle formulated in (8) captures one of the most significant contributing factors, if not the most significant factor.

- (8) The more fundamental a change is, the more important it is, where the fundamentality of a change is a function of the extensiveness of the effects of a change on components of the theory not directly involved in the change.

The more extensive the effects of a specific change are on components of a theory not directly involved in a change, the more fundamental the change is. The more "local" a change is in terms of its effects on other components of the theory, the less fundamental it is. The replacement of the SSC and PIC by the OB binding conditions had several effects on components of the theory other than the conditions themselves. For instance, the introduction of the OB binding conditions led to a change in the manner of application of the rules of construal. While previously, under the SSS and PIC, their manner of application had been restricted in certain respects, they now applied freely. Also, the class of structures generated by the rules of construal underwent a change, in that many ill-formed structures were now generated. The adoption of the auxiliary hypothesis that Picture-Noun Reflexivization falls outside the domain of sentence grammar had no such effect on other components of the theory. In terms of (8) the replacement of the SSC and PIC by the OB binding conditions is thus more important than the adoption of the auxiliary hypothesis in question.

The principles of theory appraisal formulated in the rest of § 7.2.2 are the most fundamental principles of theory appraisal to be incorporated in a model of Chomsky's rationality. These principles specify what properties of theories of UG are relevant to their appraisal. Or, to use Newton-Smith's terminology, they indicate what are the good-making features of Chomsky's theo-

ries/ . . .

ries of UG. Additional principles which specify, for example, the conditions for the appropriate use of these fundamental principles will be formulated in § 7.2.3.

(i) *Restrictedness of formal power*

One of the factors identified in chapters 3 - 6 which guided Chomsky during the developmental history of binding theory is that of restrictedness of formal power. Within Chomsky's linguistics, restrictedness of formal power is a good-making feature of UG. The following principle of theory appraisal in Chomsky's linguistics, based on the notion 'restricted formal power', can then be formulated. T in (9) is a theory of UG.

- (9) If the formal power of  $T_{x+1}$  is more restricted than that of  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ .

Note that in formulating (9), as well as the other principles of theory appraisal formulated in § 7.2.2.2, the simplifying assumption is made that all other things are equal. Of course, other things are quite often not equal. In § 7.2.3 I turn to the question of how conflicts among the principles are to be resolved.

Chomsky did not use the principle (9) to justify any of the specific changes which the SSC and the TSC/binding conditions have undergone during their developmental history. However, Chomsky used the principle (9) to justify the claim that the conditions under discussion should be incorporated in UG. In fact, the analyses of chapters 3 - 6 show that one of Chomsky's main arguments for the incorporation of the conditions in UG is that they make a significant contribution towards restricting the formal power of UG.<sup>9)</sup> At several points during the developmental history of the conditions Chomsky emphasized their contribution towards restricting the formal power of UG. As noted in § 3.2.5, Chomsky (1973) argued that the incorporation of the SSC and TSC in UG made it possible to uphold the simple string condition on transformational rules. Chomsky (1976a) argued that the SSC

and TSC/PIC/ . . . .

and TSC/PIC (together with other conditions on rule application) made possible the adoption of an even stronger restriction on the form of transformational rules, namely, the condition of minimal factorization - see § 3.3.2. And Chomsky (1980b) claimed that the SSC and TSC/PIC made it possible to restrict the formal power of rules of construal to the same extent as the formal power of transformational rules - see § 5.4.

In sum, then, the principle of restricted formal power (9) plays an important role in justifying the following choice made by Chomsky.

- (10)  $T_x + T_{x+1}$ , where  $T$  is UG,  $T_x$  is a version of UG which does not incorporate some version of the SSC and TSC/PIC, and  $T_{x+1}$  is a version of UG which does incorporate the SSC and TSC/PIC.

The choice outlined in (10) is the most fundamental change in Chomsky's theory discussed in chapters 3 - 6. In terms of the criterion formulated in (8) the fundamentality of a specific theory change is a function of the extent of the effects of this change on components of the theory not directly involved in the change. In the case of the change referred to in (10) the components directly involved in the change are the conditions themselves. As documented in chapters 3 - 6, the incorporation of the conditions in UG had extensive effects on various other components of the theory. For instance, the incorporation of the conditions affected the application of both transformational rules and interpretive rules, as well as the form of both rule types. In a sense the history presented in chapters 3 - 6 is a testimony to the fundamental status of the conditions within Chomsky's theory, and, by implication, to the fundamentality of the change which led to their incorporation in UG.

Let us now consider how Chomsky sees the link between the desirability of restrictedness of formal power in UG and his goal of

discovering/ . . .

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discovering the true nature of the language faculty.<sup>10)</sup> The fundamental problem of Chomsky's linguistics is: How is it possible for a child to acquire knowledge of language? In broad outlines, Chomsky's answer to this question is as follows. There is a vast discrepancy between the impoverished data available to the language learner and the rich structure of a human language. In order to explain how knowledge of a language can be acquired, it must then be assumed that much of the structure of human language is genetically determined. To put it differently: Chomsky claims that the initial state of the language faculty is rich and restrictive. Given the link between the assumption that the initial state of the language faculty is rich and restrictive and the fundamental problem of Chomsky's linguistics, it follows that this assumption is the most fundamental general assumption about the nature of the language faculty made within Chomsky's linguistics. Note that the argument from the poverty of stimulus provides empirical support for this assumption about the initial state of the language faculty.

UG is a theoretical description of the initial state of the language faculty. If this initial state is indeed rich and restrictive, then UG must in some way capture this property of the initial state. UG can capture this property by restricting the class of accessible grammars. The smaller the class of grammars made accessible by a UG, the richer and more restrictive the initial state of the language faculty described by this UG. Restricting the formal power of UG is a mechanism for restricting the class of grammars made accessible by UG. It follows that, within Chomsky's linguistics, UG must have restricted formal power if it is to be a true description of the initial state of the language faculty. To put it differently, restrictedness of formal power in UG is an indicator of truth.

It was explained in § 3.2.3 that restrictedness of formal power is in a dual sense empirical. First, in terms of Chomsky's general assumptions about the nature of language and language acquisition, UG must have restricted formal power to

enable it to provide a basis for an explanation of how language acquisition can take place. Second, restrictions on the formal power of UG are subject to empirical test. Consider the two versions of UG,  $T_x$  and  $T_{x+1}$ , where the class of grammars made accessible by  $T_{x+1}$  is a subclass of the grammars made accessible by  $T_x$ . That is,  $T_{x+1}$  has less formal power than  $T_x$ . There is then a class of grammars,  $C$ , which  $T_x$  predicts to be accessible and  $T_{x+1}$  predicts to be inaccessible. If the descriptively adequate grammar of a natural language falls within the class  $C$ , then one would have to conclude that the formal power of  $T_{x+1}$  is too restricted, and that  $T_{x+1}$  does not provide a correct description of the initial state of the language faculty. However, in the absence of such evidence, Chomsky would choose  $T_{x+1}$ , rather than  $T_x$ .

In addition to its empirical aspect, restrictedness of formal power also has a conceptual aspect, as was briefly noted in § 3.2.3 above. This conceptual aspect derives from the relation between a specific UG and the general assumption about the nature of the language faculty held by Chomsky, namely, that the initial state of this faculty is rich and restrictive. A UG with excessive formal power is in conflict with this general assumption, since the UG fails to provide a restrictive characterization of the initial state of the language faculty. This conflict gives rise to a conceptual problem for UG. By restricting the formal power of UG, this conceptual problem can be overcome. By the same token, any increase in the formal power of UG would create tension between UG and the general assumption in question.

(ii) *Success in providing descriptively adequate grammars for natural languages*

A large number of the theory changes discussed in chapters 3 - 6 was aimed at making UG consistent with the grammars of natural languages. Before these changes are listed, it will be useful to consider why Chomsky is concerned with consistency between UG

and specific/ . . .

and specific grammars. Within Chomsky's linguistics, UG must meet two basic requirements. The first - discussed directly above - is that UG must provide the basis for an explanation of language acquisition. It is in order to meet this requirement that UG must provide a restrictive characterization of the class of accessible grammars. The second requirement is that UG must provide a descriptively adequate grammar for each possible natural language. The genetically determined initial state of the language faculty makes it possible for a human being to acquire any natural language, provided that the necessary triggering experience is available. UG, as a description of the initial state of the language faculty, must thus provide descriptively adequate grammars for all possible natural languages.

In the schematic representation (7) it is specified that UG provides explanations for and makes predictions about specific mental grammars. Consider two versions of UG -  $T_x$  and  $T_{x+1}$  - where  $T_{x+1}$  has greater success than  $T_x$  in providing explanations for the properties of specific grammars and/or in making correct predictions about the properties of specific grammars. Within the framework of the assumptions outlined above, the greater explanatory and/or predictive success of  $T_{x+1}$  is an indication that  $T_{x+1}$  is closer to the truth than  $T_x$ .

To make explicit the role which UG's success in providing explanations for and making correct predictions about specific grammars plays in the theory choices made by Chomsky, the following principle must be incorporated in our model of his rationality. T in (11) is UG, or a subset of the theoretical principles incorporated in UG.

- (11) If  $T_{x+1}$  has more success than  $T_x$  in providing explanations for and making correct predictions about the mental grammars of individual languages, as described in descriptively adequate grammars of these languages, then  $T_{x+1}$  is better than  $T_x$ .

The formulation/ . . .

The formulation in (11) expresses the notion that it is in fact descriptive grammars which provide the empirical test for UG, a notion also expressed in the schema (7).

In terms of (11), the merit of a specific UG depends on how well it "fits in" with the facts of specific languages. Given that Chomsky's aim is finding truth, the discussion above indicates that Chomsky operates with a correspondence view of truth. In the correspondence view of truth, truth consists in a correspondence with the facts. Caws (1965:15) formulates the essence of this theory as follows.

- (12) "The most straightforward way of defending the truth of a statement about the world of experience is to point to the state of affairs it describes; if the state of affairs corresponds to what has been asserted, everybody will agree that the statement is a true one."

In this view, the extent to which a theory "fits the facts" is then an indicator of the truth of the theory.<sup>11)</sup> There is also textual evidence that Chomsky does indeed operate with a correspondence view of truth. Consider his (1972a:18) reference to answers that are "right or wrong as they do or do not correspond to the mental reality", and his (1980a:104-109) discussion of the possible difference between the truth of a linguistic theory and the psychological reality of a linguistic theory.

The role which restrictedness of formal power plays in Chomsky's linguistics also illustrates that Chomsky operates with a correspondence view of truth. UG must have restricted formal power in order to "fit in" with the facts of language acquisition.

It is now possible to specify what the empirical success of UG - in the sense of 'empirical success' defined in § 2.3.4.1 - consists of. On the one hand, the empirical success of UG consists of its success in providing explanations for the acquisition of language. It is in order to enable it to provide such explanations that UG must have restricted formal power. On the other hand, the empirical success of UG consists of its success in pro-

viding explanations for and making correct predictions about the properties of specific grammars. The familiar term "descriptive adequacy" is frequently used to refer to this second component of the empirical success of UG.

Consider a version of UG,  $UG_x$ , which fails to provide explanations for, or makes the wrong predictions about, the properties of a specific mental grammar, as described by a descriptive grammar,  $G_x$ . For Chomsky, who wishes to restrict the formal power of UG as much as possible, there are two basic ways to handle such a situation. First, Chomsky can modify  $UG_x$  - without increasing its formal power - in such a way that the modified  $UG_x$  provides explanations for, and makes the correct predictions about the relevant mental grammar. Second, Chomsky can argue that  $G_x$  is not the descriptively adequate grammar of the language in question, and thus try to modify  $G_x$  so that it no longer conflicts with  $UG_x$ . (A combination of the two methods is, of course, also possible.) During the developmental history of binding theory Chomsky in some cases modified UG, and in other cases the proposed descriptive grammar of a natural language, in order to increase the explanatory and predictive success of UG. In (13) all the changes made at the level of UG are listed, and in (14) all those changes made at the level of specific grammars.<sup>12)</sup> For some of the changes listed in (13) T is UG, while in others T is a specific principle or a set of principles incorporated in UG, such as binding theory.

Note that in (13t) reference is made to the *absence* of change, rather than change. This is symbolized as "  $\neq$  ". As Newton-Smith (1981:5) points out, while it is usually change which we want to explain, there are instances where it is the absence of change that needs explaining. Chomsky's decision not to replace the definition of 'governing category' by the simpler definition of 'binding category' needs explaining just as much as any of the other choices he made during the development of binding theory.

- a.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is a version of UG which does not incorporate the SSC and TSC, and  $T_{x+1}$  is a version which does incorporate these conditions - §§ 3.2.4, 3.2.5.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is a version of UG, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that S, but not S', is the domain of cyclic rules - § 3.2.7.2.
- c.  $T_x \rightarrow T_{x+1}$ , where T is a version of UG, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that an element in COMP can be moved into another COMP only - § 3.2.7.2.
- d.  $T_x \rightarrow T_{x+1}$ , where T is a version of UG, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that *wh*-Movement applies cyclically - § 3.2.7.2.
- e.  $T_x \rightarrow T_{x+1}$ , where T is a version of UG, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that movement rules (including *it*-Replacement and *wh*-Movement in English) leave traces - § 3.2.7.2.
- f.  $T_x \rightarrow T_{x+1}$ , where T is the TSC, and where  $T_{x+1}$  is  $T_x$  plus a stipulation exempting a Y in COMP - § 3.2.7.1.
- g.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is a version of the SSC which does not include controlled traces as specified subjects, and  $T_{x+1}$  is a reformulated version of  $T_x$  which does include controlled traces as specified subjects - § 3.2.7.2.
- h.  $T_x \rightarrow T_{x+1}$ , where T is the SSC, and where  $T_{x+1}$  is a reformulation of  $T_x$  in terms of the feature [+ definite] - § 3.2.7.3.
- i.  $T_x \rightarrow T_{x+1}$ , where T is the SSC, and where  $T_{x+1}$  is a reformulation/ . . .

reformulation of  $T_x$  in terms of the semantic notion 'agent' - § 3.2.7.4.

- j.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the definition for 'involve' in the SSC and TSC, and where  $T_{x+1}$  is a modified version of  $T_x$  which includes the case where  $X$  is a constant context for some change - § 3.3.4.
- k.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the definition for 'involve' in the SSC and TSC, and where  $T_{x+1}$  is a modified version of  $T_x$  which includes a subclass of the rules of interpretation only, namely the rules of construal - § 4.4.3.
- l.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the PIC, and  $T_{x+1}$  is  $T_x$  plus the stipulation that  $\alpha$  must be the cyclic node which immediately dominates  $Y$  - § 4.4.4.
- m.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the SSC and TSC/PIC, and  $T_{x+1}$  is the OB binding conditions (the Opacity Condition and the NIC) - § 5.3.
- n.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is an indexing theory that does not assign anaphoric indices, and  $T_{x+1}$  is the OB indexing theory that does assign anaphoric indices - § 5.5.
- o.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the OB binding theory and  $T_{x+1}$  the GB governor binding theory - § 6.3.
- p.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the Pisa GB binding theory and  $T_{x+1}$  the MCG GB binding theory - § 6.4.2.
- q.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the MCG GB binding theory and  $T_{x+1}$  is the LGB GB binding theory - § 6.4.2.

r./ . . .

- r.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the GB governor binding theory and  $T_{x+1}$  the GB SUBJECT binding theory - § 6.6.2.
  - s.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the GB SUBJECT binding theory and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that a root  $S$  is a governing category - § 6.6.3.
  - t.  $T_x \neq T_{x+1}$ , where  $T_x$  is the GB SUBJECT binding theory, incorporating the notion 'governing category' and  $T_{x+1}$  is the GB SUBJECT binding theory incorporating the notion 'binding category' - § 6.6.3.
  - u.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is some version of binding theory, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that the notion 'anaphor' is subject to parametric variation - §§ 4.3 and 6.7.
  - v.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the GB SUBJECT binding theory, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that  $G$  in COMP has the same properties as AGR, and is thus an accessible SUBJECT - § 6.7.
  - w.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the binding theory, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that in languages with the cliticization option full pronouns are immune to principle B of binding theory - § 6.7.
- ) a.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that English has a rule of PRO-Replacement - § 3.2.7.2.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that Picture-Noun Reflexivization falls outside the domain of sentence grammar - § 3.3.5.

c./ . . .

- c.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that the rule relating an NP and *the other* falls outside the domain of sentence grammar - § 3.3.5.
- d.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that Coreference Assignment falls outside the domain of sentence grammar - § 3.3.5.
- e.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of French, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that part of *Tous-Movement* falls outside the domain of core grammar - § 4.3.
- f.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and where  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that VP-deletion falls outside the domain of sentence grammar - § 4.4.2.
- g.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the proviso exempting a  $Y$  in COMP from the SSC and the PIC, and the auxiliary hypothesis that COMP-COMP movement falls outside the domain of core grammar - § 4.4.5.
- h.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that English has certain structure-building rules - § 5.6.
- i.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that those constructions about which the GB governor binding theory apparently makes the wrong predictions fall outside the domain of core grammar - § 6.5.

j./ . . .

- j.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that subjunctives in English contain a null AGR - § 6.7.

The number of theory changes justified in terms of the principle (11) is far greater than the number of changes justified in terms of any other principle of theory appraisal employed by Chomsky during the developmental history of binding theory. The changes listed in (13) and (14) also include very fundamental changes - for example, (13a, e, m, o) - in addition to less fundamental, "local" changes - for example, (13h, i, j), (14a, b, c, d, e, f).

(iii) *Simplicity*

Chomsky justified three of the changes he made to binding theory by arguing that the modified version  $T_{x+1}$  of the theory avoids a redundancy exhibited by the earlier version  $T_x$ . These changes are listed in (15).

- (15) a.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the SSC and PIC, and  $T_{x+1}$  is the OB binding conditions (the Opacity Condition and the NIC) - § 5.3.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is a version of UG which incorporates the \* $[\text{NP to VP}]$  filter, and  $T_{x+1}$  is a version without the \* $[\text{NP to VP}]$  filter - § 5.7.
- c.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the OB binding theory, and  $T_{x+1}$  is the GB binding theory - § 6.3.3.1.

For Chomsky, a redundancy exhibited by a theory represents a form of complexity. By eliminating the redundancy, the theory is simplified. The link between the elimination of redundancies in linguistic theory and the desirability of simplicity in linguistic theory is, for example, explicitly made by Chomsky (1981a: 14). Nonredundancy is not the only type of meta-theoretical sim-

plicity/ . . .

plicity which Chomsky used in justifying his theory choices during the developmental history of binding theory. Consider the following two choices discussed above.

- (16) a.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the formulation 5.(8) of the NIC which contains a stipulation referring to S, and  $T_{x+1}$  is the formulation 5.(2) of the NIC, which does not contain the stipulation referring to S - § 5.3.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the OB indexing theory and  $T_{x+1}$  is the GB indexing theory - § 6.3.3.5.

Chomsky justified both these choices by claiming that  $T_{x+1}$  is simpler than  $T_x$ . In the case of (16a),  $T_{x+1}$  is simpler than  $T_x$  in that  $T_{x+1}$  does not contain a stipulation referring to S. In the case of (16b),  $T_{x+1}$  is simpler than  $T_x$  in that  $T_x$  contains no principles for the assignment of anaphoric indices. On the basis of the cases listed in (16), it must then be concluded that the simplicity of a linguistic theory, or a subcomponent of a linguistic theory, in part depends on the number of stipulations which it contains. By eliminating a stipulation, the theory is made more simple. Chomsky's (1981a:338) remarks on the elimination of "special conditions" (in addition to the elimination of redundancies) in the search for simplicity provides textual evidence for this interpretation.

The following two principles of theory appraisal must then be added to (9) and (11). In (17) and (18) T is UG, or a subset of the principles incorporated in UG, such as binding theory.

- (17) If  $T_{x+1}$  contains fewer redundancies than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ .
- (18) If  $T_{x+1}$  contains fewer stipulations than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ .

Note/ . . .

Note that the notion of theoretical simplicity on which (18) is based also covers the potential simplification which Chomsky (1981a:220) considered in connection with the definition of 'governing category' in the GB-SUBJECT binding theory. This simplification consisted in eliminating the stipulation that the governing category of  $\alpha$  contains the governor of  $\alpha$ . As explained in § 6.6.3 above, Chomsky did not actually adopt this simplification, since it would have led to a loss of empirical success.<sup>13)</sup>

Chomsky's grounds for pursuing theoretical simplicity is (i) that the pursuit of theoretical simplicity is one of the factors which has contributed to the great success of the natural sciences, and (ii) that up to the present it has proved to be a productive policy in the study of language. Both these points are mentioned by Chomsky (1981a:14, 339; 1982a:30-31) in his discussion of the role which simplicity ought to play in the development and appraisal of linguistic theories.

Chomsky assumes that theoretical simplicity in UG - in the senses of (17) and (18) - directly reflects a property of the language faculty described by UG, namely simplicity. This direct link between simplicity as a metatheoretical property of UG and simplicity as a property of the language faculty is made quite explicitly by Chomsky (1981a:14). Thus, he states that the search for simple principles might be mistaken since biological systems - of which the language faculty is one in Chomsky's view - "often exhibit redundancy and other forms of complexity . . ." The same point is made by Chomsky (1982a:30). Underlying Chomsky's search for theoretical simplicity there is then the assumption that the language faculty itself is simple. This assumption is stated clearly by Chomsky (1981a:339).

(19) ". . . it seems to me that work of the past few years suggests that it makes a good deal of sense to pursue the working hypothesis that the theory of core grammar, for reasons that are not at all obvious, does have some of the prop-

erties/ . . .

properties of the systems studied in the more fundamental natural sciences, and that for some reason neural structures at least in this domain instantiate a perhaps surprisingly simple and unified system of principles."  
{The italics are mine.}

Recall that UG is a theory of core grammar. The reference to "neural structures" in (19) raises problems which will be considered in § 7.3 below.

As was noted in §§ 5.3, 5.7, 6.3.3.1, and 6.3.3.5 above, Chomsky regards lack of simplicity in UG, or, alternatively, the presence of a complexity in UG, as giving rise to a conceptual problem for UG. Such a problem is in effect caused by a conflict between a specific UG and the general assumption that the language faculty itself is simple (given, of course, that theoretical simplicity in UG reflects the simplicity of the language faculty). By eliminating the complexity in UG - for example, by eliminating a redundancy or a stipulation - the conflict is resolved, and the conceptual problem overcome.

This analysis of the role which considerations of simplicity play in theory appraisal in Chomsky's linguistics illustrates that Chomsky adopts a coherence norm of truth, in addition to the correspondence norm discussed above. In the words of Caws (1966: 15-16), the coherence theory is "the view that truth consists in a fitting in with other truths".

UG must be simple in order to "fit in" with the assumption that the language faculty is simple. Note that Laudan (1981:147) also links conceptual factors in theory appraisal with the adoption of what he calls "coherentist constraints" on theories.

Insofar as it has a conceptual aspect, the principle of restricted formal power (9) also illustrates the role of a coherence norm of truth in Chomsky's linguistics. A UG must have restricted formal power in order to "fit in" with the hypothesis that the initial state of the language faculty is rich and complex.

(iv) *Unifying a wide range of phenomena*

In his recent works Chomsky frequently uses the term "unify" and the related terms "unifying", "unified", "unification" in connection with linguistic theory. One of the senses in which Chomsky uses these terms can be paraphrased as follows: A principle is unifying if it governs a wide range of phenomena. For instance, Chomsky (1981b:66) claims that "the principles of opacity and DR [= Disjoint Reference - M.S.] serve as general unifying principles with many consequences . . ."

Of the OB binding principles Chomsky (1981a:158) says that they "do serve to unify a fairly impressive range of observations . . .", and of the \**[that t]* filter he (1981a:160) says that "it does serve to unify many phenomena related to 'long movement' of nominative subjects in an enlightening way".

It was stated in § 3.2.4 above that Chomsky adopts a principle of methodological generality, which specifies that theories should be of maximal generality. The generality of a theory is a function both of the number of facts explained by the theory and of the variety of facts explained by it.<sup>14)</sup> In one sense, then, a unifying principle is a general principle. Its success in unifying a wide range of phenomena, that is, its generality, is presented by Chomsky as one of the features of binding theory which provides support for this theory. In addition to the remarks quoted above, see also Chomsky's (1977c:89) remarks on the generality of the SSC, the PIC, and the Subjacency Condition in comparison with Ross' island constraints (§ 4.5), and his (1980b:10) reference to the SSC and PIC as "abstract principles governing a wide range of phenomena". The following principle of theory appraisal based on the notion 'unifying/generality' can thus be formulated. In (20) T is a principle or subset of principles incorporated in UG.

- (20) If  $T_{x+1}$  is more general than  $T_x$ , that is, if  $T_{x+1}$  unifies  
a wider/ . . .

a wider range of phenomena than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ .

The role which generality, in the sense of (20), plays in the appraisal of linguistic theories in part underlines the importance of empirical success in appraising the merit of linguistic principles. In terms of (20), the merit of a theoretical principle depends on the range of facts which it can explain (and about which it makes the correct predictions). However, within Chomsky's linguistics generality, or success in unifying a wide range of phenomena, also has a second, conceptual, aspect. There is an obvious link between the desirability of having unifying/general principles and the aim to develop a theory which is simple, in the sense that it contains a limited number of principles - see (18) above. If linguistic theory as a whole should comprise only a limited number of principles, then each individual principle would have to be unifying/general, in the sense that it governs a wide range of phenomena. Thus, Chomsky (1980b:10) characterizes a unifying theory as one in which "a few abstract principles govern a wide range of phenomena". Generality, or success in unifying a wide range of phenomena, is then a conceptual factor in the appraisal of linguistic principles. Moreover, "unifying-ness" in the sense of generality, in conjunction with deductive depth, determines another conceptual property of linguistic theories. This point is taken up immediately below.

(v) *Deductive depth*

In terms of the characterization provided in § 4.2 above, a theory  $T_{x+1}$  has greater deductive depth than another version  $T_x$  if a principle which must be stipulated in  $T_x$  can be deduced from a more general principle in  $T_{x+1}$ . Several of the theory choices which Chomsky made during the developmental history of binding theory were justified (or partly justified) in terms of greater deductive depth. The developmental history of binding theory thus provides evidence that Chomsky adopts the following

principle/ . . .

principle of theory appraisal.  $T$  in (21) is UG, or a subset of principles incorporated in UG, such as binding theory.

- (21) If  $T_{x+1}$  has greater deductive depth than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ .

The theory choices discussed above which were justified in terms of (21) are listed in (22).

- (22) a.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the SSC and the TSC/PIC as conditions that restrict the application of both transformational rules and rules of semantic interpretation, and  $T_{x+1}$  is the SSC and TSC/PIC as conditions that restrict the application of rules of semantic interpretation only - § 4.2.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is a version of UG which incorporates the SSC and PIC as conditions on rule application, and  $T_{x+1}$  is a version of UG which incorporates the binding conditions, which are conditions on a level of representation - § 5.3.
- c.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is a version of UG which incorporates the OB binding theory and  $T_{x+1}$  is a version of UG which incorporates the GB governor binding theory - § 6.3.
- d.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the GB governor binding theory, and  $T_{x+1}$  is the GB SUBJECT binding theory - § 6.6.2.

In each of these cases Chomsky claimed that  $T_{x+1}$  has greater deductive depth than  $T_x$ . The relevant difference between  $T_x$  and  $T_{x+1}$  can conveniently be characterized in the following terms: while in  $T_x$  it must be stipulated that  $p$ ,  $T_{x+1}$  can provide an answer to the question "why  $p$ ?" In the case of (22a) the "why"-question is "Why are both transformational rules and rules of

semantic interpretation subject to the SSC and TSC/PIC?" In the case of (22b),  $T_{x+1}$ , but not  $T_x$ , can provide answers to the following questions: (i) Why do transformational rules apply cyclically? and (ii) Why can an element escape from a tensed clause or a clause with a specified subject via the COMP of that clause? In the case of (22c),  $T_{x+1}$ , but not  $T_x$ , can provide an answer to the question of why the two opaque domains are the subject of a tensed sentence and the c-command domain of the subject of any category. In the GB governor binding theory, the phenomena accounted for by the \* $[that-t]$  filter are accounted for by the ECP, a more abstract principle which covers a very wide range of phenomena. The ECP - and thus  $T_{x+1}$  in (22c) - can thus provide an answer to the question of why the string *that t* is not allowed. In the case of (22c),  $T_{x+1}$ , but not  $T_x$ , can also provide an answer to the question of why *wh*-traces are in some instances subject to the NIC, but never to the SSC.  $T_{x+1}$  can answer this question since, in addition to the GB binding principles, it contains the Empty Category Principle. In the case of (22d),  $T_{x+1}$ , but not  $T_x$ , can provide an answer to the question of why NP and S are the two governing categories.

In his recent works Chomsky frequently stresses the importance of deductive depth in linguistic theory. For instance, Chomsky (1978a:16) states that to be a "deep unifying" principle, a principle must "ground" a variety of generalizations "in a system that has a certain degree of deductive structure". Consider also in this connection Chomsky's comments on deductive depth/structure in (Chomsky 1978a:24, 26; 1980b:2; 1981b:66-67; 1982a:75). However, to fully appreciate the importance which Chomsky attaches to deductive depth in linguistic theory, it is necessary to consider again Chomsky's use of the terms "unifying"/"unified".

It was argued above that Chomsky uses the terms "unifying"/"unified" to refer to principles which are general, that is, principles which cover a wide range of observations. However, Chomsky also uses these terms in a second sense, in which there is

a crucial/ . . .

a crucial link between unifiedness, or theoretical unification, and deductive depth. There are several passages in Chomsky's recent works which indicate that he makes a distinction between principles which are unifying in that they are general, and principles which are unifying/unified in that they have deductive depth, in addition to being general. Thus, Chomsky (1978a:17) characterizes the Subjacency Condition as "a genuine unifying principle . . . that is, a number of island constraints can be deduced from it". In Chomsky's (1981b:50) comments on the Subjacency Condition he uses the term "unifying" to refer to a principle which is both general and has deductive depth. Thus, he claims that the Subjacency Condition is "an example of a genuine unifying principle that incorporates a number of generalizations concerning islands and provides explanations for a wide range of facts . . ."

That unifying-ness in the sense of generality should be distinguished from unifiedness in the sense of deductive depth is also clear from Chomsky's (1982a:75) comparison of the binding theory and the bounding theory. The bounding theory represents the Subjacency Condition. As the remarks quoted immediately above indicate, in Chomsky's view the bounding theory is general and has deductive depth. Remarks were also quoted above to the effect that in Chomsky's view the binding theory up to and prior to the OB-binding theory, was unifying, in the sense of being general. Commenting on the development of the GB framework - called the "Pisa-framework" by him - Chomsky (1982a:75) states that this "is in part an attempt to try to develop the kind of deductive structure for the binding theory that the *Conditions*-framework tried to do for the bounding theory". The implication is clear. Prior to the GB framework/Pisa-framework, the binding theory could not compare with the bounding theory as regards deductive structure depth. Chomsky (1982a:75) then proceeds with the following comments on the development of the bounding theory versus that of the binding theory.

- (23) "Thinking of the period from *Conditions on Transformations* through 'On *wh*-movement' at least, the theory [that is, the bounding theory - M.S.] developed, right or wrong, a limited deductive structure, with some principles like subjacency that had interesting consequences. The binding theory on the other hand, was more or less descriptive. The Pisa framework is an attempt at further unification, deriving properties of binding from simpler principles and extending a more unified theory to new domains."

The crucial point to note in connection with these remarks, when read in connection with Chomsky's remarks about the development of deductive structure for the binding theory within the Pisa-framework, is the link between improving the deductive structure of the binding theory and developing a more unified theory. By increasing the deductive depth of the theory, the theory becomes more unified.

There is no need to formulate a separate principle of theory appraisal based on this second notion of unifiedness. This second notion is a derivative concept based on the notion of generality (that is, the first notion of unifiedness incorporated in (20)) and of deductive depth.

Chomsky's grounds for pursuing unifiedness, in the second sense defined above, in UG are in essence the same as his grounds for pursuing theoretical simplicity in UG. That is, the pursuit of unifiedness, or unification, has contributed to the success of the natural sciences, and has proved to be a productive strategy in the study of language. In addition to Chomsky's (1982a:14, 339) comments in this connection, see also his (1981b:66-67, 70) comments on the importance of deductive depth, which is the crucial component of this second notion of unifiedness.

Let us now consider how the pursuit of deductive depth is linked to Chomsky's aim of discovering the true nature of the language faculty. Recall that if  $T_{x+1}$  has greater deductive depth than  $T_x$ , then  $T_{x+1}$  can provide an explanation for a principle stipulated in  $T_x$ . Increased deductive depth thus leads to increased

success/ . . .

success in providing explanations. The facts explained through an increase in deductive depth would be relatively far removed from the observational level, with a high theoretical content. If it is indeed the case that there is no definite observational-theoretical distinction, then theoretical success - including the explanation of a theoretical principle - would count as empirical success. 15)

However, it would be wrong to assume that increased deductive depth in Chomsky's linguistic theories could be completely reduced to empirical success. Chomsky himself regards a lack of deductive depth in a theory as constituting a *conceptual* problem. The fact is that for Chomsky the pursuit of deductive depth in his linguistic theories is linked to an assumption about the nature of the language faculty. To understand this point, it is necessary to consider Moravcsik's (1980) comments on 'deep' theories, and Chomsky's response to these comments.

Chomsky (1981a:15) states that deductive depth "is a concomitant of what Moravcsik (1980) calls 'deep' as opposed to 'shallow' theories of mind, and is an indication of success in developing such theories". Moravcsik (1980:28) draws the following distinction between "deep" and "shallow" theories.

- (24) "I shall label as 'deep' (without implying any depth in a normative sense) the theories that refer to many layers of unobservables in their explanations, and I shall regard even some of the fundamental facts to be accounted for as lying beneath the level of observability. Such theories are guided by the intuition that the observable appearances can be explained adequately only by the examination of the underlying unobservable aspects of nature. ('Nature does not wear its essence on its sleeves'.) What I label 'shallow' theories are those that try to stick as close to the observable as possible, aim mostly at correlations between observables, and posit something unobservable only when this seems unavoidable - even then, such theories demand some direct relationships between the observable and the unobservable."

Moravcsik (1980:28) also comments on the success of "deep" theories in certain domains of study.

(25) "The history of the natural sciences like physics, chemistry, and biology is a clear record of the success story of 'deep' theories. The more sophisticated and complex the underlying system of unobservables becomes in physics or chemistry, the more we seem to be able to account for. In fact, even the application to the practical - i.e. the rise of technology - was made possible only after the considerable 'deepening' of physics and chemistry".

In his response to Moravcsik (1980), Chomsky (1980d:48) states that "Moravcsik's commentary helps considerably to clarify the issues that lie at the core of these discussions . . ." Chomsky thus endorses Moravcsik's views on "deep" and "shallow" theories quoted above. Note that, according to Moravcsik, the construction of "deep" theories are guided by an assumption about nature, namely, that nature is structured in such a way that a rich and complex system of unobservables underlies the observable appearances. According to Moravcsik, what makes Chomsky special is that he extends this assumption to the mind, and the language faculty, in particular. Consequently, Chomsky aims at "deep" linguistic theories, that is, theories with a great degree of deductive depth. As Moravcsik correctly notes, Chomsky's argument for "deep" theories of mind (and the language faculty in particular) is based on the analogy between physics and other successful sciences. Consider, in this connection, Chomsky's (1981a:339) remarks quoted in (19) and also his (1981a:14-15) discussion of the issue.

In sum, then, deductive depth in UG reflects a hypothetical property of the language faculty, namely, that the language faculty is "deep" in the sense of being composed of a rich and complex set of unobservables. Within Chomsky's linguistics, deductive depth is then an indicator of the truth, or truthlikeness, of UG.

(vi) *Naturalness as principles of mental computation*

Chomsky explicitly justified two of the theory choices .

$T_x + T_{x+1}$  discussed above by claiming that  $T_{x+1}$  is more "natural" than  $T_x$ . These choices are listed in (26).

- (26) a.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is Ross' island conditions, and  $T_{x+1}$  is the SSC, the TSC/PIC, and the Subjacency Condition - § 4.5.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the \* $\left[ \textit{that } t \right]$  filter and  $T_{x+1}$  is the Empty Category Principle - § 6.3.3.4.

As was explained in § 4.5 above, Chomsky expects the principles of linguistic theory to be "natural as principles of mental computation". In essence, this notion of naturalness concerns the relationship between a theory of the language faculty and other theories of mental computation. A theory of the language faculty is natural to the extent that it is compatible with other theories of mental computation. It is not clear whether this relationship of compatibility is regarded by Chomsky to be any stronger than a relationship of mutual plausibility.<sup>16)</sup>

There is then both textual evidence from Chomsky's work and evidence from the developmental history of binding theory that Chomsky employs the following principle of theory appraisal. T in (27) is UG.

- (27) If  $T_{x+1}$  contains principles that are natural as principles of mental computation, and  $T_x$  contains principles that are not natural in this sense, then  $T_{x+1}$  is better than  $T_x$ .

Chomsky has nothing explicit to say about the link between the naturalness of linguistic principles and his goal of discovering the true nature of the language faculty. However, there are reasons to assume that the following is a reasonable reconstruction of the link as Chomsky would see it. All theories of mental computation aim at discovering truth. As in the case of the physical world, it is assumed that the world of mental computation is unified.<sup>17)</sup> Consequently a theoretician of the mind expects either to be able to unify diverse theories of mental computation into a single all-encompassing theory or to have a family of mutually supporting theories. Chomsky's comments on

the modularity of the mind and of language, in particular - see, for example, (Chomsky 1980a:28, 89-90, 225) - indicate that he adheres to something like this second position. On this view, lack of support between two theories of mental computation - for example, in the form of joint implausibility - would indicate that at least one of the theories is not true. Chomsky's insistence that the mind should be studied in the same way as the physical world also provides support for this reconstruction of the link between the naturalness of linguistic principles and Chomsky's aim of discovering the true nature of the language faculty.

The above reconstruction of Chomsky's views on the naturalness of linguistic principles again indicates that he adopts a coherence norm of truth. A theory of the language faculty must "fit in" with other theories of mental computation.

(vii) *Absence of internal contradictions in UG*

Chomsky justified two of the changes  $T_x + T_{x+1}$  discussed above by arguing that  $T_{x+1}$  avoids an internal tension exhibited by  $T_x$ . These changes are listed in (28).

- (28) a.  $T_x + T_{x+1}$ , where  $T_x$  is the version of UG incorporating the OB binding conditions, and  $T_{x+1}$  is the version of UG incorporating the GB binding theory and the ECP - § 6.3.3.3.
- b.  $T_x + T_{x+1}$ , where  $T_x$  is a version of UG which incorporates structure-building rules, and  $T_{x+1}$  is a version without structure-building rules, - § 6.8.

In the case of the change listed in (28b), the internal tension clearly takes the form of a logical inconsistency. The incorporation of the Projection Principle prohibits any rule which builds structure during the derivation of a sentence. (28a) is more complicated. The OB theory does exhibit a logical inconsistency,

in that/ . . .

in that some data indicate that *wh*-traces are subject to the NIC, while other data indicate that *wh*-traces are not subject to the NIC. Chomsky (1981a:232) points out that these facts about *wh*-traces and the NIC yield only "a near contradiction", since a special stipulation could help overcome the inconsistency. However, he states that he is "sceptical about any attempt to avoid the problem along these lines". Given Chomsky's scepticism about solving the inconsistency regarding *wh*-traces and the NIC within the OB-framework, this inconsistency must be regarded as an inconsistency analogous to the inconsistency which motivated the theory change described in (28b).

The theory changes listed in (28) thus provide evidence that Chomsky operates with the principle of theory appraisal (29).

- (29) If  $T_{x+1}$  avoids an internal contradiction exhibited by  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ .

There is a standard argument for the avoidance of logical inconsistencies, or internal contradictions, in a theory. A theory with logical inconsistencies contains each sentence of the meta-language of the theory. And as Newton-Smith (1981:229) explains, no theory of verisimilitude ("truthlikeness") would be acceptable that did not assign the lowest degree of verisimilitude to a theory which contained every sentence of the theory's language as well as the negation of every such sentence. Although Chomsky does not explicitly comment on the reasons why one should avoid logical inconsistencies in a theory, there is no reason not to assume that the argument set out above also applies to inconsistencies in Chomsky's linguistic theories.

(viii) *Compatibility with the autonomy thesis*

Chomsky justified the change presented in (30) below by arguing that the modified version,  $T_{x+1}$ , is compatible with a stronger version of the autonomy of syntax thesis than the unmodified version  $T_x$ .

- (30)  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the SSC and TSC/PIC as conditions that restrict the applicability of both transformational rules and rules of semantic interpretation, and  $T_{x+1}$  is the SSC and TSC/PIC as conditions that restrict the applicability of rules of semantic interpretation only - § 4.2.

As explained in § 4.2 above, a UG which contains semantic conditions on syntactic transformations is not logically inconsistent with the autonomy of syntax thesis. The autonomy of syntax thesis is a thesis about the structure of *specific* grammars. In the case of specific grammars the desirability of an autonomous syntax can be reduced to the necessity of restricting the formal power of UG. In the case of UG, the (non)-autonomy of the syntactic component does not affect the formal power of the theory. However, if it is the case that at the level of UG too the syntactic subcomponent is autonomous from the semantic subcomponent, then the *plausibility* of the autonomy thesis is increased. By implementing the change listed in (30) the autonomy thesis can be strengthened, to exclude universal semantic conditions on syntactic transformations. The possibility of the change (30) thus provides support for the autonomy thesis.

In justifying the choice listed in (30) Chomsky thus employed a principle of theory appraisal which can be formulated as in (31).  $T$  in (31) is UG.

- (31) If  $T_{x+1}$  is compatible with a stronger version of the autonomy thesis than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ .

The principles of theory appraisal (9), (11), (17), (18), (20), (21), (27), (29), and (31) identify the various features of UG on which Chomsky based the theory choices which he made during the developmental history of binding theory. These principles thus represent the most fundamental principles of rational theory choice in Chomsky's linguistics. However, these principles cannot on their own provide a complete *minirat* account of the development of binding theory. In § 7.2.2.3 several additional principles which guided Chomsky's theory choices will be formulated.

As in the case of the principles formulated above, the primary evidential basis for these principles is provided by the developmental history of binding theory.

### 7.2.3 Extending the model

#### 7.2.3.1 Explaining a choice $T_x \rightarrow T_{x+1}$

Recall that the model of Chomsky's rationality developed in § 7.2 must provide minirat accounts for the various theory choices made by Chomsky during the developmental history of binding theory. That is, the model must provide explanations for the various steps in the development of binding theory in terms of Chomsky's own beliefs about the goal of linguistics and the principles of theory appraisal.

Consider all the changes  $T_x \rightarrow T_{x+1}$ , where Chomsky judged  $T_{x+1}$  to be better than  $T_x$  in terms of one or more of the principles of theory appraisal formulated in § 7.2.2.2. The model developed in § 7.2.2 can provide partial explanations for all such choices. The nature of this explanation can be illustrated by the replacement of the SSC and PIC by the OB binding theory. Chomsky judged the OB binding theory -  $T_{x+1}$  - to be better than the SSC and PIC -  $T_x$  - in that  $T_{x+1}$  had more success than  $T_x$  in providing explanations for and making correct predictions about specific grammars,  $T_{x+1}$  avoided a redundancy exhibited by  $T_x$ , and  $T_{x+1}$  had greater deductive depth than  $T_x$ . In terms of Chomsky's assumptions, a  $T_{x+1}$  with greater explanatory and predictive success than  $T_x$  and/or fewer redundancies than  $T_x$  and/or greater deductive depth than  $T_x$  is closer to the truth than  $T_x$ . Given that the goal of Chomsky's linguistics is finding the truth, Chomsky consequently chooses  $T_{x+1}$  over  $T_x$ . The explanation provided by the model of rationality for this change is schematically reconstructed in (32).

(32) a. The goal of Chomsky's linguistics is to find the truth.

b./ . . .

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- b. Within Chomsky's linguistics the following features of UG are regarded as indicators of truth, or closeness to truth: (i) the explanatory and predictive success of UG with regard to specific mental grammars; (ii) the avoidance of redundancies by UG; (iii) the deductive depth of UG.
  - c. The OB binding conditions have greater explanatory and predictive success than the SSC and PIC, the OB binding conditions avoid a redundancy exhibited by the SSC and PIC, and the OB binding conditions have greater deductive depth than the SSC and PIC.
  - d. Therefore, Chomsky judged the OB binding conditions to be better than the SSC and PIC.
  - e. Therefore, Chomsky replaced the SSC and PIC by the OB binding conditions.

While the principles formulated in § 7.2.2 can provide partial explanations for many of the theory choices made by Chomsky during the developmental history of binding theory, these principles do not constitute a complete model of rational theory choice in Chomsky's linguistics, as instantiated by the development of binding theory. The need for additional principles to be incorporated in our model of Chomsky's rationality becomes evident when one considers that the principles formulated above do not on their own provide answers to the following questions that can be raised about the various theory choices made by Chomsky during the developmental history of binding theory.

- (33) a. How does Chomsky resolve conflict among the principles of theory appraisal listed in § 7.2.2?
- b. Under what conditions is it appropriate for Chomsky to use *ad hoc* devices to protect his theories from potential negative evidence?

- c. Under what conditions is it appropriate for Chomsky to restrict the domain of his linguistic theory in order to exclude potential negative evidence, and under what circumstances is it appropriate for him to extend the domain of linguistic theory?
- d. Under what conditions is it appropriate for Chomsky to adopt an attitude of epistemological tolerance towards potential negative evidence threatening his theory?
- e. Under what conditions is the use of data from a wide range of languages necessary for the appraisal of linguistic theory in Chomsky's view?

A minimal rational account of the developmental history of binding theory ought to provide answers to at least the questions listed in (33). In §§ 7.2.3.2 - 7.2.3.7 these questions, together with other related questions, are examined in detail against the background of the developmental history of binding theory, as set out in chapters 3 - 6 above. In the course of the discussion a more complete picture of theory choice in Chomsky's linguistics will emerge. Several principles will be formulated which must be incorporated in our model of what constitutes rationality in Chomsky's linguistics, along with the principles of § 7.2.2.

#### 7.2.3.2 Conflict between restricting the formal power of UG and achieving descriptive adequacy

In the formulation of the principles of theory appraisal presented in § 7.2.2 the simplifying assumption was made that all other things are equal. In actual fact, other things are not always equal, and the various principles of theory appraisal formulated above do in some cases point in different directions. In §§ 7.2.3.2 and 7.2.3.3 the question of how conflict among the various principles of theory appraisal is resolved by Chomsky will be considered.

As was noted in § 3.2.3 above, there is a certain tension between improving the explanatory adequacy of UG and improving its descriptive adequacy. While explanatory adequacy often requires restricting the formal power of UG, descriptive adequacy often seems to require an increase in the formal power of UG. In terms of the principles of theory appraisal formulated in § 7.2.2, this means that the principle of restricted formal power (9) may in certain instances be in conflict with the principle (11), which is based on the explanatory and predictive success of UG with respect to specific grammars - descriptive adequacy, for short.

Binding theory does not in itself restrict the formal power of UG. It only indirectly makes possible the introduction of restrictions on the formal power of UG. Consequently, one would not expect many direct conflicts between the principle of restricted formal power (9) and the principle of descriptive adequacy (11) in the various changes made to binding theory. However, since the development of binding theory is so closely related to attempts to restrict the formal power of UG, the possibility of conflict between restricting the formal power of UG and improving the descriptive adequacy of UG does in a general way bear on the developmental history of binding theory. A few comments on the light which the developmental history of binding theory sheds on Chomsky's handling of this conflict are thus in order.

As was noted above, the possibility of restricting the formal power of UG featured prominently in Chomsky's argumentation for the incorporation of the SSC and TSC/PIC (and their later versions) in UG. The structure of Chomsky's argumentation is, in essence, as follows. In order to achieve explanatory adequacy, the formal power of UG must be restricted. If the formal power of UG is restricted - for instance, through the adoption of the simple string condition and the principle of minimal factorization - then there is an apparent loss of descriptive adequacy. Since UG must also achieve descriptive adequacy, steps must be

taken/ . . .

taken to overcome this loss of descriptive adequacy. That is, it must be shown that a UG with restricted formal power can achieve descriptive adequacy. Binding theory, together with various other principles, is introduced in order to ensure that UG with its restricted formal power is descriptively adequate.<sup>18)</sup> Clearly, the success of any proposed restriction on the formal power is determined by the success of the various principles introduced to ensure the descriptive adequacy of UG. Any negative evidence threatening these last-mentioned principles indirectly threatens the proposed restrictions on the formal power of UG.

The formal power of recent versions of UG is severely restricted. However, several problems of descriptive adequacy remain unsolved. In chapters 3 - 6 above it was shown that binding theory is threatened by potential negative evidence, both in the form of unexplained phenomena and counterevidence. This is true even for the most successful version of binding theory developed up to now - see § 6.5 for details. Conflict between the principle of restricted formal power (9) and the principle of explanatory and predictive success with respect to specific grammars (11) is thus present in the developmental history of binding theory. The fact that Chomsky retains the restrictions on the formal power of UG in spite of these problems of descriptive adequacy, indicates that, in general, restrictedness of formal power is a weightier factor in the appraisal of theory choices than descriptive adequacy.

Of course, it should not be concluded that Chomsky restricts the formal power of UG without taking into account the effect of such restrictions on the descriptive adequacy of UG. The developmental history of binding theory, and in fact the developmental history of Chomsky's linguistic theory as a whole since the sixties, testifies that Chomsky's aim is to *reconcile* restrictions on the formal power of UG with descriptive adequacy. That is, the aim is to develop a UG which is better than previous versions in terms of both (9) and (11).

It also/ . . .

It also does not follow that Chomsky will never expand the formal power of UG in order to increase its descriptive adequacy. The introduction of structure-building rules is a case in which Chomsky proposed an (allegedly minimal) expansion of the formal power of UG in order to increase the descriptive adequacy of UG. Specifically, Chomsky increased the formal power of UG in order to increase the explanatory success of binding theory with respect to the grammar of English - see § 5.6 for details.

The question naturally arises whether there is a rule which guides Chomsky in the handling of conflict between the principle of restrictive formal power (9) and the principle of explanatory and predictive success with respect to specific grammars (11). That is, is there a rule on the basis of which Chomsky decides when to resolve a conflict in favour of restricted formal power, and when to resolve a conflict in favour of descriptive adequacy? This question is in fact but a special case of a more general question that arises in connection with Chomsky's work, namely, whether there is a rule on the basis of which Chomsky decides when to modify his linguistic theory in the face of potential negative evidence, and when to put such evidence aside. In essence, then, the question of the resolution of conflict between the principles of theory appraisal (9) and (11) can be reduced to a question about the conditions which guide Chomsky's adoption of an attitude of epistemological tolerance. This question will be considered in § 7.2.3.6 below.

### 7.2.3.3 Conflict between the simplicity of UG and the explanatory and predictive success of UG

In the case of some of the theory choices  $T_x \rightarrow T_{x+1}$  discussed above there is a conflict between the principle of theory appraisal (11) and the principle (18). In (11) the explanatory and predictive success of UG with respect to specific grammars is specified as a virtue of UG. In (18) a specific type of metatheoretical simplicity is specified as a virtue of UG. In particular,

(18) specifies that the fewer the number of stipulations which a theory or subcomponent of a theory contains, the better it is. The various choices in which there is a conflict between the explanatory and predictive success of UG and the simplicity (in the sense of (18)) of UG are listed in (34).

- (34) a.  $T_x \rightarrow T_{x+1}$ , where T is the TSC, and where  $T_{x+1}$  is  $T_x$  plus a stipulation exempting a Y in COMP - § 3.2.7.1.
- b.  $T_x \rightarrow T_{x+1}$ , where T is the definition for 'involve' in the SSC and TSC, and where  $T_{x+1}$  is  $T_x$  plus a stipulation which includes the case where X is a constant context for some change - § 3.3.4.
- c.  $T_x \rightarrow T_{x+1}$ , where T is the PIC, and  $T_{x+1}$  is  $T_x$  plus the stipulation that  $\alpha$  must be the cyclic node which immediately dominates Y - § 4.4.4.
- d.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is an indexing theory that does not assign anaphoric indices, and  $T_{x+1}$  is the OB indexing theory that does assign anaphoric indices - § 5.5.
- e.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the PIC,  $T_{x+1}$  is the formulation 5.(8) of the NIC, and  $T_{x+1}$ , but not  $T_x$ , contains a stipulation referring to S - § 5.3.
- f.  $T_x \rightarrow T_{x+1}$ , where  $T_x$  is the OB indexing theory that assigns anaphoric indices and  $T_{x+1}$  is the GB indexing theory that does not assign anaphoric indices - § 6.3.3.5.
- g.  $T_x \rightarrow T_{x+1}$ , where T is the GB SUBJECT binding theory, and  $T_{x+1}$  is  $T_x$  plus the principle stipulating that a root S is a governing category for a governed element - § 6.6.3.

- h.  $T_x \neq T_{x+1}$ , where  $T_x$  is the GB SUBJECT binding theory incorporating the notion 'governing category', and  $T_{x+1}$  is the GB SUBJECT binding theory incorporating the notion 'binding category' - § 6.6.3.
- i.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the GB SUBJECT binding theory, and  $T_{x+1}$  is  $T_x$  plus the stipulation that G in COMP has the same properties as AGR, that is, it is an accessible SUBJECT - § 6.7.
- j.  $T_x \rightarrow T_{x+1}$ , where  $T$  is UG, and  $T_{x+1}$  is  $T_x$  plus a principle stipulating that in languages with the cliticization option full pronouns are immune to principle B of binding theory - § 6.7.

In the case of all the changes listed in (34) - with the exception of (34f) and (34h) -  $T_x$  is simpler than  $T_{x+1}$  in the sense of (18). That is,  $T_{x+1}$  is  $T_x$  plus an additional stipulation. In each case Chomsky introduced the complication in order to increase the explanatory and predictive success of UG with respect to specific grammars. The stipulation referred to in (34a) was introduced in order to prevent the TSC from making the wrong predictions about clause external *wh*-Movement. The stipulation referred to in (34b) was introduced in order to enable the conditions to account for Q-float. The stipulation referred to in (34c) was made in order to enable the TSC/PIC to make the correct predictions about cases with anaphors in subjectless NPs. The change listed in (34d) - that is, the addition of principles for the assignment of anaphoric indices - was made in order to enable the OB binding conditions to account for Disjoint Reference. The stipulation referred to in (34e) was introduced in order to prevent the NIC from making the wrong predictions about *wh*-traces in COMP. The stipulation referred to in (34g) was introduced in order to prevent the GB SUBJECT binding theory from making the wrong predictions about the binding of anaphors in the subject position of sentential subjects. The stipulation referred to in (34i)

was introduced in order to enable the GB SUBJECT binding theory to make the correct predictions about the AUX-to-COMP rule in Italian. The stipulation referred to in (34j) was introduced in order to enable binding theory to make the correct predictions about the binding of pronouns in PPs in the Romance languages.

In the case of (34f),  $T_{x+1}$  is simpler than  $T_x$ , in that  $T_{x+1}$  contains no clauses referring to anaphoric indices. However,  $T_{x+1}$  - the GB indexing theory - fails to account for cases which are accounted for by  $T_x$  - the OB indexing theory. In this case Chomsky chose  $T_{x+1}$ , the simpler version.

In the case of (34h),  $T_{x+1}$  is also simpler than  $T_x$ . The definition of 'binding category' is simpler than that of 'governing category', in that the latter, but not the former, contains a clause referring to a governor of  $\alpha$ . In this case Chomsky chose the more complex  $T_x$ , because the simpler  $T_{x+1}$  faced potential counterexamples not faced by  $T_x$ .

In the case of the choices listed in (34c, d, e, f, h) Chomsky explicitly commented on the relative simplicity of the two versions  $T_x$  and  $T_{x+1}$ . In the case of the other changes no explicit comments by Chomsky on the relative simplicity of the two versions are available. However, as indicated above, the latter changes clearly represent complications in the sense of (18). It is then reasonable to assume that the incorporation of the stipulations referred to in (34a, b, g, i, j) gives rise to complications analogous to the complications referred to in (34c, d, e, f, h).

The changes listed in (34a, b, c, d, e, g, i, j) all have the following feature in common: a complication was introduced into UG in order to increase the explanatory and predictive success of UG with respect to specific grammars. In all these cases the conflict between the principle (11) of explanatory and predictive success and the principle (18) of simplicity was resolved in favor of (11). That is, given the choice between a  $T_x$  and a

$T_{x+1}$ , so that  $T_{x+1}$  is more highly valued than  $T_x$  in terms of (14) and  $T_x$  is more highly valued than  $T_{x+1}$  in terms of (18), Chomsky chose  $T_{x+1}$ . In the case of (34h) the conflict is also resolved in favour of explanatory and predictive success. Since the simplification in question would have led to a loss of predictive success, Chomsky did not adopt the simplification.

In the case of (34f) Chomsky apparently resolved the conflict between the explanatory and predictive success of UG with respect to specific grammars and simplicity in the sense of (18) in favour of simplicity. However, it is not clear that (34f) represents a genuine conflict between explanatory and predictive success and simplicity. It is true that the OB indexing conventions, in contrast to the GB indexing conventions, make the correct predictions about sentences such as 6.(67). As explained in § 6.6.3, there are also sentences that can be accounted for by the GB indexing theory, but which are problematic for the OB indexing theory. While the replacement of the OB indexing theory by the GB indexing theory thus leads to a reduction of explanatory and predictive success in one domain, there is an increase in empirical success in a different domain. Chomsky (1981a:289) sums up his discussion of the relative explanatory and predictive success of the two indexing theories by claiming that "an apparent advantage of the OB-framework appears illusory". The latter remark suggests that there is no real conflict between explanatory and predictive success and simplicity in the replacement of the OB indexing theory by the GB indexing theory, since the former does not really have greater explanatory and predictive success than the latter. To put it differently, Chomsky is apparently suggesting that the gain in explanatory and predictive success of the GB indexing theory in one domain compensates for its loss of explanatory and predictive success in another domain. If this interpretation of Chomsky's (1981a:289) remarks is correct, then it follows that in replacing the OB indexing theory by the GB indexing theory Chomsky did not really opt for simplicity at the cost of explanatory and predictive success.

All the above-mentioned cases of conflict between the principle of theory appraisal (11) and the principle (18) support the conclusion that in the theory choices which Chomsky made during the developmental history of binding theory, the explanatory and predictive success of UG with respect to specific grammars weighed heavier than the simplicity of UG (in the sense of (18)). There is then some justification for adding the following principle to the principles of theory appraisal formulated in § 7.2.2.

- (35) In the case of a conflict between the explanatory and predictive success of UG with respect to specific grammars (principle (11)) and the simplicity of UG (principle (18)), the conflict is to be resolved in favour of the explanatory and predictive success of UG.

It would be wrong to conclude on the basis of the theory choices listed in (34) and the principle (35) that considerations of simplicity did not play an important role in the developmental history of binding theory. It is a fact that considerations of simplicity - including the principle (18) - feature prominently in Chomsky's justification for the various changes he made to binding theory since 1978. Consider in this connection the changes to the OB binding theory and the GB binding theory listed in (15) and (16) above. The importance of simplicity in guiding theory choices is underlined by the fundamentality of the changes justified in terms of it.

The further history of the complications referred to in (34a, c, d, e) provides additional evidence for the importance of simplicity in the sense of (18) as a factor guiding theory choice in Chomsky's linguistics. In each of these cases the relevant complication was eliminated from UG at some later stage in the development of UG, and in each case Chomsky regarded the elimination of the complication as an advantage. First, as regards the stipulation in (34a) that Y is not in COMP, Chomsky (1977c:85) argues that this is a language specific stipulation belonging to

the grammar/ . . .

the grammar of English, and not to UG. As explained in § 4.4.5, Chomsky presents this complication within the context of markedness. Such a complication of the grammar of English is, of course, quite compatible with the notion of markedness. By incorporating the stipulation that Y is not in COMP in the grammar of English, the PIC, and thus also UG, becomes simpler in the sense of (18). Second, as regards the stipulation referred to in (34c), the replacement of the SSC and the PIC by the OB binding conditions eliminates the need for this stipulation - see § 5.3. Third, as regards (34d), Chomsky (1981a) argues that the complex OB indexing conventions can be replaced by the simpler GB indexing conventions. Fourth, as regards (34e), the reference to S was eliminated, once it was assumed that the trace in COMP of a *wh*-moved subject is not nominative.

These cases indicate a concern on Chomsky's behalf to eliminate complexities introduced to increase the explanatory and predictive success of UG at some stage in the developmental history of the theory, when further developments in the theory made it possible to do so without the loss of the explanatory and predictive success originally gained by the introduction of the complication. Given this concern, it would certainly be wrong to deduce from (35) that simplicity did not play an important role in the developmental history of binding theory.

While (35) can account for the theory choices listed in (34), the generality of (35) can be questioned. It is not at all clear from the developmental history of binding theory that Chomsky will *always* resolve conflict between explanatory and predictive success and simplicity in favour of the former. Suppose that Chomsky must choose between two versions,  $T_x$  and  $T_{x+1}$ , of linguistic theory.  $T_x$  is much simpler than  $T_{x+1}$ , that is,  $T_x$  is rated much higher than  $T_{x+1}$  in terms of the principles of theory appraisal (17) and (18).  $T_{x+1}$  has slightly better explanatory and predictive success than  $T_x$ , that is,  $T_{x+1}$  is rated slightly higher than  $T_x$  in terms of the principle of theory appraisal (11). Would the principle (35) apply in this case? That is, would Chomsky actually choose  $T_{x+1}$ ? As in the case of

the conflict between restricted formal power and descriptive adequacy discussed in § 7.2.3.2 above, the question about the rule which guides the resolution of conflict between explanatory and predictive success and simplicity is but a special case of a more general question: Is there a rule on the basis of which Chomsky decides when to modify his linguistic theory in the face of threatening negative evidence, and when to put such evidence aside? To fully appreciate this point, it is necessary to consider the relationship between UG and the various general assumptions about the language faculty made by Chomsky.

In the schematic representation (11) it is specified that, on the one hand, these general assumptions in some sense lay down conditions for UG. For instance, given the assumption that the language faculty is simple, UG - as a description of the language faculty - must reflect this simplicity. On the other hand, UG in some sense puts to test the correctness of the general assumptions made by Chomsky about the language faculty. It is this second feature of the relation between UG and the general assumptions which are now of special interest.

In the discussion in § 7.2.2.2 above of the principles of theory appraisal (17) and (18), it was argued that for Chomsky the pursuit of metatheoretical simplicity of UG in the senses of (17) and (18) is based on an assumption about the nature of the language faculty, namely, that the language faculty itself is simple. Chomsky's reasons for making this assumption, and thus for pursuing metatheoretical simplicity, is that an analogous assumption has contributed to the great success of the natural sciences, and that up to the present it has proved to be a fruitful assumption in the study of language.

As was pointed out in § 7.2.2.2 above, Chomsky only tentatively assumes that the language faculty is simple. Thus, Chomsky (1981a:14) states that the "guiding intuition" that the language faculty is simple might be mistaken. If Chomsky at present

leaves open/ . . .

leaves open the question of the correctness of the assumption about the simplicity of the language faculty, how can its correctness then eventually be determined? The following remarks by Chomsky (1981a:15) provide the answer.

- (36) "It is pointless to adopt *a priori* assumptions concerning these matters {including the question of the simplicity of the language faculty - M.S.}, though one's intuitive judgments will, of course, guide the course of inquiry and the choice of topics that one thinks merit careful investigation. The approach I will pursue here can be justified only in terms of its success in unearthing a more 'elegant' system of principles that achieves a measure of explanatory success. To the extent that this aim is achieved, it is reasonable to suppose that the principles are true, that they in fact characterize the language faculty, since it is difficult to imagine that such principles should merely hold by accident of a system that is differently constituted."

From the remarks quoted in (36), it is clear that the explanatory success of a UG provides the test for the correctness of, amongst other things, the assumption that the language faculty is simple. The explanatory success of UG thus also provides the test for the appropriateness of employing the principles (17) and (18) in appraising the merit of linguistic theories.

UG's success in providing explanations for the properties of specific grammars clearly forms an important component of the explanatory success of UG. The fact that Chomsky (1981a:15) refers only to explanatory success, and not also to predictive success, is, as far as can be determined, without any significance within the present context.

To return to the schema (7), UG thus provides a test for the correctness of the assumption that the language faculty is simple in the following way. If a UG which fits in with this simplicity assumption achieves explanatory (and predictive) success, then this should be interpreted as indicating the correctness of the simplicity assumption. If a UG which fits in with the simplicity

assumption/ . . .

assumption fails to achieve explanatory (and predictive) success, then this should be interpreted as an indication that the simplicity assumption is incorrect. Given the link between Chomsky's assumption that the language faculty is simple and his employment of the principles of theory appraisal (17) and (18), UG provides a similar test for the appropriateness of employing these principles of theory appraisal.

It should now be clear why it was claimed above that the question of the resolution of conflict between explanatory and predictive success and simplicity could be reduced to a question about Chomsky's tolerance towards potential negative evidence which threatens his theory. Given that Chomsky advocates a tolerant attitude towards potential negative evidence threatening a theory, one would not expect him to blindly give up a simple theory - that is, a theory that fits in well with the assumption that the language faculty is simple - in the face of potential negative evidence, and to replace it with a theory which is less highly valued in terms of the principles of theory appraisal (17) and (18). The question of whether there is a rule which guides Chomsky in deciding when to modify his theory (in this case through replacing simple principles with more complex principles) in the face of potential negative evidence and when to set such negative evidence aside, is discussed in § 7.2.3.6 below. It will be argued there that the introduction of the complications listed in (34) can, with two exceptions, be explained in terms of a general constraint on epistemological tolerance. There is then no need for a separate principle such as (35). Given the doubts which can be raised about the generality of (35), it would clearly be desirable not to incorporate (35) in a model of Chomsky's rationality.

For Chomsky, the explanatory success of UG provides a test not only for the correctness of the simplicity assumption, but for all the general assumptions which he makes about the language faculty. More generally, Chomsky regards specific theories of the

mind as providing the test for the correctness of his general assumptions about the mind. This is stated quite explicitly by Chomsky (1980a:3). In these introductory comments to (Chomsky 1980a) Chomsky refers to the assumptions which he makes about "human cognitive capacities and the mental structures that serve as the vehicles for the exercise of these capacities". He continues with the following comments on the evaluation of these assumptions.

- (37) "In the end, the best way to clarify these assumptions and to evaluate them is to construct specific models guided by them in particular domains, *then to ask how these models fare when interpreted as explanatory theories*. If the leading ideas are appropriate, they will be sharpened and justified by the success of explanatory theories that *develop them in a specific way*." [The italics are mine - M.S.] 19)

In the discussion above the focus was on conflict between metatheoretical simplicity of UG and the explanatory and predictive success of UG with respect to specific grammars. Conflict between metatheoretical simplicity of UG and UG's explanatory success with respect to the facts of language acquisition is also possible. In terms of the principles of theory appraisal formulated in § 7.2.2.3, this would take the form of conflict between the principle of restricted formal power (9) and the principles of simplicity (17) and (18). Chomsky (1981a:15) indirectly refers to the possibility of such conflict when he points out that the search for a more restrictive UG is independent from the search for a UG which is, for example, simpler than older versions. Thus, he states that "a theory of UG with redundancies and inelegant stipulations may be no less restrictive than one that overcomes these conceptual defects".

No concrete instances of conflict between restricting the formal power of UG and improving its metatheoretical simplicity occurred during the developmental history of binding theory. Consequently, the developmental history of binding theory provides no clue as to

how Chomsky would resolve such conflicts. It may be noted in passing that there is one factor which may play a role in the resolution of such conflicts, namely, the differing status, in Chomsky's view, of the consideration of restricted formal power from considerations of simplicity, deductive depth, and so on. Chomsky's (1981a:15) comments on the difference in status between the search for theories of UG which are simple, natural, and so on, and the search for more restrictive theories of UG provide some support for this view. He states that the former "has a rather different status, and much less obvious validity, than the search for more restrictive theories of UG, which is dictated by the very nature of the problem faced in the study of UG". These remarks suggest that in case of conflict, restricted formal power would weigh heavier than, for example, simplicity in determining a theory choice.

#### 7.2.3.4 Ad hoc protective devices

Several cases have been discussed in chapters 3 - 6 above in which Chomsky made use of *ad hoc* (= without independent justification) devices to protect his theory from potential counterevidence. These cases are summarized in (38) below. Note that the protective devices referred to in (38) include auxiliary hypotheses as well as modifications to the conditions themselves.

- (38) a.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the TSC, and  $T_{x+1}$  is  $T_x$  plus a stipulation exempting a  $Y$  in COMP - § 3.2.7.1.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the SSC, and where  $T_{x+1}$  is a re-formulation of  $T_x$  in terms of the feature [+ definite] - § 3.2.7.3.
- c.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the SSC, and where  $T_{x+1}$  is a re-formulation of  $T_x$  in terms of the semantic notion 'agent' - § 3.2.7.4.

d. / . . .

- d.  $T_x \rightarrow T_{x+1}$ , where T is the definition for 'involve' in the SSC and TSC, and where  $T_{x+1}$  is  $T_x$  plus a stipulation which includes the case where Y is a constant context for some change - § 3.3.4.
  
- e.  $T_x \rightarrow T_{x+1}$ , where T is the PIC, and  $T_{x+1}$  is  $T_x$  plus the stipulation that  $\alpha$  must be the cyclic node which immediately dominates Y - § 4.4.4.
  
- f.  $T_x \rightarrow T_{x+1}$ , where T is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that English has certain structure-building rules - § 5.6.
  
- g.  $T_x \rightarrow T_{x+1}$ , where T is the GB SUBJECT binding theory, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that a root S is a governing category - § 6.6.3.
  
- h.  $T_x \rightarrow T_{x+1}$ , where T is some version of binding theory, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that the notion 'anaphor' in the binding theory is subject to parametric variation, so that reflexives in languages such as Japanese and Korean do not count as anaphors - §§ 4.3 and 6.7.
  
- i.  $T_x \rightarrow T_{x+1}$ , where T is the binding theory, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that subjunctives contain a null AGR - § 6.7.
  
- j.  $T_x \rightarrow T_{x+1}$ , where T is the GB SUBJECT binding theory and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that G in COMP (in Italian) has the same properties as AGR, and is thus an accessible SUBJECT - § 6.7.
  
- k.  $T_x \rightarrow T_{x+1}$ , where T is the GB binding theory, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that in languages with the cliticization option full pronouns are immune to principle B of binding theory - § 6.7.

Note that/ . . .

Note that several of the changes listed in (37) also appear in the list (34) of complications introduced into UG, presented in § 7.2.3.3 above. Given the notion of simplicity on which the principle of theory appraisal (18) is based, this is not surprising. However, §§ 7.2.3.3 and 7.2.3.4 deal with different aspects of these changes. In § 7.2.3.3 the focus was on these changes as changes that involve *complications* introduced into UG. In § 7.2.3.4 the focus is on the *lack of independent justification* for the relevant changes.

It was argued above that, given the lack of any real justification for Chomsky's markedness claims, all such claims should also be regarded as *ad hoc* auxiliary hypotheses introduced to protect the theory. These claims are listed in (39) below.

- (39) a. The French rule that must explain the peripheral *Tous-*Movement phenomena of Kayne (1975:63-64) is a marked rule - § 4.3.
- b. COMP-COMP *wh*-Movement is a marked rule - § 4.4.5.
- c. The constructions about which the GB governor binding theory, but not the OB binding theory apparently makes the wrong predictions, are actually marked - § 6.5.

The claims listed in (39b, c) differ from the other *ad hoc* protective devices listed above, in that Chomsky attempted to create the impression that these markedness claims have some independent justification. However, as argued in §§ 4.4.5 and 6.5 above, the considerations presented by Chomsky in support of these claims provide no real support for them. The significance of the fact that Chomsky attempted to create the impression that the claims of (39b, c) are independently justified, while no real justification was in fact presented for them, will be considered in §§ 7.2.3.6 and 7.3 below.

The role/ . . .

The role played by the devices listed in (38) and (39) in the protection of binding theory provides evidence that the principles of rationality adhered to by Chomsky do not rule out the use of *ad hoc* protective devices to overcome potential problems of explanatory and predictive success which threaten a theory which is in other respects highly valued. The protected theory in the cases listed in (38) is the binding theory, a theory which Chomsky values highly. By incorporating the principle (40a) and the constraint (40b) on the use of *ad hoc* devices in our model of Chomsky's rationality, the role of *ad hoc* auxiliary hypotheses in Chomsky's linguistics, and the developmental history of binding theory in particular, is made explicit.

- (40) a. In cases where UG is threatened by potential negative evidence, *ad hoc* devices may be introduced to protect the theory.
- b. *Ad hoc* devices may be used to protect a theory if the threatened theory is in other respects highly valued, for instance, if it has considerable explanatory and predictive success.

A question arises at this point concerning the link between Chomsky's use of *ad hoc* protective devices and his attitude of epistemological tolerance towards negative evidence threatening his linguistic theories. Why did Chomsky, in the cases noted above, introduce *ad hoc* protective devices instead of simply setting the negative evidence aside? This question will be considered in § 7.2.3.6, as part of an analysis of Chomsky's attitude of epistemological tolerance.

It should not be concluded on the basis of (40) that Chomsky attaches no value to independent justification for auxiliary hypotheses and modifications introduced to protect his theory from potential negative evidence. In chapters 3 - 6 several cases were noted where Chomsky did provide independent justification

for proposed/ . . .

for proposed protective devices, or where he referred to works by other linguists in which such justification is provided. These include the auxiliary hypotheses discussed in § 3.2.7.1 about sentence structure and the nature of *wh*-Movement, the hypothesis discussed in § 3.2.7.2 that movement rules leave traces behind, and the claims discussed in §§ 3.3.5 and 4.4.2 that certain rules do not belong to sentence grammar.

It is also the case that many of the *ad hoc* protective devices referred to in (38) (and in (39)) were eliminated at a later stage of theory development. The escape hatch status of COMP was eventually derived in a principled manner from the OB binding theory - see § 5.3 above. The need for a reformulation of the SSC in terms of the notion 'agent' is overcome within the framework of the GB SUBJECT binding theory - see § 6.6.2 above. The need for a stipulation that  $\alpha$  in the PIC must be the cyclic node which immediately dominates Y is overcome in the OB binding theory - see § 5.3 above. Within the GB framework structure-building rules are no longer required (and in fact no longer permitted) - see § 6.8 above. The GB SUBJECT binding theory does not require the markedness claims referred to in (39c) - see § 6.6.2 above.

Can the fact that these protective devices are eliminated be taken as further evidence that Chomsky values non-*ad hoc* ways of dealing with negative evidence higher than *ad hoc* ways? Caution should be exercised in drawing such a conclusion. In each of the cases mentioned above other factors were involved. For instance, the elimination of the COMP escape hatch led to a UG with greater deductive depth. The elimination of structure-building rules also eliminated an internal inconsistency in UG. Chomsky's metascientific comments do not make clear what role the lack of independent justification for these devices played in their elimination. Only in the case of the claims referred to in (39c) did Chomsky explicitly refer to their lack of independent justification as a reason for eliminating them. Consider in this

connection/ . . .

connection Chomsky's (1982a:110) comments on the issue. With the exception of (39c), then, it is not easy to determine how important lack of independent justification was in the elimination of these *ad hoc* protective devices.

There are some general comments by Chomsky on the use of *ad hoc* hypotheses (or "statements") which indicate that the *ad hoc*-ness of a protective device becomes a liability only if an alternative analysis is available which does not require *ad hoc* statements. Chomsky and Lasnik (1978:272) state that in order to establish that an analysis is incorrect, "it does not suffice to show that this analysis contains *ad hoc* statements". What is required, according to them, is "a further demonstration that under some other analysis the idiosyncracies disappear". These remarks support the conclusion drawn above, namely, that within Chomsky's linguistics the use of *ad hoc* protective devices is permitted. At the same time, these remarks indicate that a theory which requires an *ad hoc* device in order to account for some phenomena is less highly valued than an alternative theory which does not require such an *ad hoc* hypothesis, all other things being equal. This may explain Chomsky's concern to eliminate *ad hoc* devices introduced to protect his theory when later developments in the theory make it possible to do so. However, as argued above, there is no clear evidence that in those instances where Chomsky eliminated *ad hoc* stipulations initially introduced to protect binding theory, it was in fact their *ad hoc*-ness which motivated their elimination. For this reason, no principle stipulating the desirability of eliminating *ad hoc* protective devices will be incorporated in the present model of Chomsky's rationality.

#### 7.2.3.5 Restricting the domain of the theory

One of the prominent features of the developmental history of binding theory, as outlined in chapters 3 - 6, is that in many cases Chomsky restricted the domain of UG in order to exclude

potential/ . . .

potential negative evidence threatening his theory. In (41), (42), and (43) these cases are listed.

(41) *Cases where Chomsky restricted the domain of UG by claiming that a rule representing a potential counterexample for the theory falls outside the domain of sentence grammar.*

- a.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that Picture-Noun Reflexivization falls outside the domain of sentence grammar - § 3.3.5.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that the rule relating an NP and *the other* falls outside the domain of sentence grammar - § 3.3.5.
- c.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that Coreference Assignment falls outside the domain of sentence grammar - § 3.3.5.
- d.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that VP-deletion falls outside the domain of sentence grammar - § 4.4.2.

(42) *Cases where Chomsky restricted the domain of UG by claiming that a rule representing a potential counterexample for the theory falls outside the domain of core grammar.*

- a.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of French, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that part of *Tous-Movement* falls outside the domain of core grammar - § 4.3.
- b.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$

is/ . . .

is  $T_x$  plus the auxiliary hypothesis that COMP-COMP movement falls outside the domain of core grammar - § 4.4.5.

- c.  $T_x \rightarrow T_{x+1}$ , where  $T$  is the grammar of English, and  $T_{x+1}$  is  $T_x$  plus the auxiliary hypothesis that the picture noun constructions about which the GB governor binding theory apparently makes the wrong predictions fall outside the domain of core grammar - § 6.5.

- (43) *Cases where Chomsky restricted the domain of binding theory by claiming that a rule representing a potential counterexample for the conditions belong to a component of the grammar not subject to the conditions.*

$T_x \rightarrow T_{x+1}$ , where  $T$  is the definition for 'involve' in the SSC and TSC/PIC, and where  $T_{x+1}$  is a modified version of  $T_x$  which includes a subclass of the rules of interpretation only, namely the rules of construal - § 4.4.3.

As a result of the latter change in the definition of 'involve' the *not-many* rule, the *such that* rule, and the interpretive rule of Hebrew relativization fall outside the domain of the conditions.

The restrictions listed in (41) - (43) all have the effect of excluding a potential counterexample to the SSC and PIC from the domain of these conditions. By excluding the rules in question from the domain of the conditions, the conditions are, of course, no longer required to account for them. As argued above, the explanatory and predictive success of UG with respect to specific grammars played a significant role in the developmental history of binding theory. A large number of changes in binding theory - some of them quite fundamental - were made in order to increase the explanatory and predictive success of UG. The question now arises how this aim of increased explanatory and predictive success

can be/ . . .

On the innermost cycle *wh*-Movement applies to (68a) to give (68b). On the next cycle - assuming that *each*-Movement precedes *wh*-Movement - (68c) is first derived by applying *each*-Movement, and then (68d) is derived by *wh*-Movement. On the last cycle, *wh*-Movement (together with the obligatory rules of Auxiliary Inversion and Case Assignment) derives (68e). The assumption that *each*-Movement precedes *wh*-Movement therefore does not suffice to rule out the derivation of the unacceptable (68e). In fact, (68e) constitutes a potential counterexample to the proposed analysis of (67), an analysis that incorporates the assumption that *each*-Movement precedes *wh*-Movement.

In view of the empirical criticisms that can be raised against the assumption that *each*-Movement precedes *wh*-Movement, Chomsky rejects this assumption. Instead, he assumes that *wh*-Movement - like *it*-Replacement - leaves behind a trace. In the case of *wh*-Movement, this trace is controlled by the moved *wh*-phrase. In (68b) *who* will thus control its trace in the subject position of the lowest embedded clause. Because of the presence of this controlled subject, the SSC will prohibit *each*-Movement from moving *each* into the embedded clause, thus blocking the derivation of (68e). In the same manner the trace of *who* in (67b) will prevent the application of *each*-Movement to give (67c). The SSC can then explain the nonapplication of *each*-Movement in (67c) and (68c), if it is assumed that *wh*-Movement leaves a controlled trace behind.

Recall that Chomsky's (1973) aim is to restrict the formal power of transformations. In order to achieve this aim Chomsky must show that constraints on the applicability of transformations can be explained in terms of general - i.e., universal - conditions on rules. Cases in which these general conditions fail to explain the constraints on rule application are then clearly problematical. In fact, such cases can be regarded as potential counterexamples to the system of conditions proposed in (Chomsky 1973) - for short, the "Conditions"-framework. Cases such as

(64b) and/ . . .

(64b) and (68e), in which the conditions fail to explain the non-applicability of *each*-Movement, thus constitute potential counter-examples to the "Conditions"-framework. When viewed against this background, it becomes obvious that the hypothesis that *it*-Replacement and *wh*-Movement leave behind controlled traces are very important within the context of (Chomsky 1973).

One striking feature of Chomsky's (1973) presentation of the notion that transformational rules leave behind controlled traces, is the extent to which crucial issues are left unclear. To mention but a few examples: Chomsky does not specify exactly what class of movement rules leave behind traces. While he (1973: 269, fn. 4) does suggest "that every rule that moves an item from an obligatory category (in the sense of Emonds (1970))<sup>69)</sup> leaves a trace", it is by no means clear whether these are the only rules that leave traces. Also very little information is provided about the nature of traces, and the ways in which they interact with conditions other than the SSC. No information is provided as to how traces can be associated with the correct moved phrase, a problem that obviously arises in structures where there is more than one trace. Trace theory, as presented in (Chomsky 1973), is thus in crucial respects obscure.

Despite this obscurity in its content, Chomsky (1973) tries to show that there is some independent justification for the notion that (certain) movement rules leave traces. He (1973:269, fn. 49) claims that this notion makes it possible to explain the obligatory character of NP-Preposing in Passive in sentences versus its optional character in noun phrases. Of crucial importance is the fact that this explanation incorporates the assumption that in simple N-V-N sentences the subject position is filled by a full NP in the underlying structure.<sup>70)</sup> It is thus possible to avoid the assumption made by Emonds, namely that the subject position of such sentences is obligatorily empty in the underlying structure. The latter assumption is claimed by Chomsky to be problematical, although he provides no reasons for his judgment.

tools in making progress towards depth of understanding.<sup>22)</sup> Abstraction and idealization are particularly important, according to Chomsky (1980a:218), where the behaviour of some organism is believed to be determined by the interaction of several systems operating under conditions of great variety and complexity. Chomsky claims that "progress in such an inquiry is unlikely unless we are willing to entertain radical idealization, to construct abstract systems and to study their special properties, hoping to account for observed phenomena directly in terms of properties of the systems postulated and their interaction". Chomsky argues that it is reasonable to assume that both the human mind and the language faculty are modular in structure.<sup>23)</sup> Consequently, as Chomsky (1980a:219) claims, "a significant notion of 'language' as an object of rational inquiry can be developed only on the basis of rather far-reaching abstraction".

The role which the idealizations of sentence grammar and core grammar played in restricting the domain of UG must be seen against the background set out above. The idealization of sentence grammar enables Chomsky to abstract from all aspects of knowledge of language that extend beyond the level of the sentence in developing UG.<sup>24)</sup> All phenomena which reflect such knowledge should thus be excluded from the domain of UG. The idealization of core grammar enables Chomsky to abstract from the effects of the heterogeneous character of the actual experience in real speech communities, and the "periphery of borrowings, historical residues, inventions, and so on" incorporated in each actual language.<sup>25)</sup> Phenomena which reflect such factors should thus also be excluded from the domain of UG.

In order to make explicit the role which restrictions in the domain of a theory played in the developmental history of binding theory, the following principle must be incorporated in our model of rational theory choice in Chomsky's linguistics.

(46) In cases where UG is threatened by potential counterexam-

ples, the/ . . .

ples, the domain of the theory may be restricted to exclude the rules constituting the counterexamples.

Close examination of the restrictions listed in (41) - (43) suggests that there are three conditions which govern the appropriate use of (46). First, (46) may be used only if the domain of UG can be restricted in a systematic way, in terms of a well-defined notion or conceptual distinction, and not by listing an arbitrary set of unrelated rules. In the cases of (41) the domain of UG is restricted in terms of the distinction between sentence grammar and non-sentence grammar. In the cases of (42) the domain of UG is restricted in terms of the distinction between the marked periphery and the unmarked core. In (43) the domain of binding theory is restricted in terms of the distinction between rules of construal and other rules of semantic interpretation.

Second, the fact that a theory  $T_1$  fails to account for a specific phenomenon,  $P$ , does not in itself establish that  $P$  falls outside the domain of  $T_1$ . Evidence must be available that  $P$  does indeed fall within the domain of another theory,  $T_2$ . Thus, for each of the rules mentioned in (41), Chomsky presented evidence that the rules do indeed apply across sentence boundaries, or he referred to works by others in which such evidence is presented. The situation with respect to the markedness claims of (42) is more complicated. As was suggested in §§ 4.3, 4.4.5, 6.5 above, the considerations which Chomsky presented in support of these claims in fact provide no support for them. However, the crucial point for the purposes of the present discussion is that Chomsky did mention certain considerations which he presented as providing support for the markedness claims referred to in (42). That is, Chomsky at least tried to create the impression that there is evidence that the rules in question fall outside the domain of core grammar, and inside the domain of the marked periphery. An appraisal of the lack of real justification for the markedness claims listed in (42) must be postponed until §§ 7.2.3.6 and 7.3 below. The significance of the fact that

Chomsky later developed the GB SUBJECT binding theory which could account for the phenomena excluded from the domain of UG in (42c), will also be considered there.

The third condition which governs the appropriate use of (46) is similar to one which governs the use of *ad hoc* devices to protect a theory from potential negative evidence. In the case of each of the restrictions listed in (41) - (43) the theory whose domain Chomsky restricted has considerable merit in Chomsky's view. Chomsky justified the incorporation of binding theory in UG in terms of several of the principles of theory appraisal presented in § 7.2.2. In the case of (42c), where the choice is between two versions of binding theory, Chomsky argued that the GB governor binding theory has several conceptual advantages over the OB binding theory.

In sum, then, the following conditions govern the use of the principle (46), in terms of which the domain of a linguistic theory may be restricted to exclude potential counterevidence.

- (47) a. The domain of a theory may be restricted to exclude potential counterevidence if the threatened theory is in other respects highly valued, for instance, if it has considerable explanatory success, or attractive conceptual properties.
- b. The domain of a theory may be restricted only if this can be done in a systematic way, that is, in terms of a general concept or conceptual distinction, and not in terms of an arbitrary list of unrelated rules.
- c. A rule may be excluded from the domain of a theory if there is independent justification that the rule falls outside the domain of this theory, and inside the domain of another theory.

It is/ . . .

It is interesting to compare Chomsky's willingness to restrict the domain of application of binding theory with Pullum's reaction to such restrictions. As argued above, Chomsky's willingness to restrict the domain of his theory to those areas where it "works" is in part a consequence of his view that the domain of a theory is not pretheoretically clear, and in part a consequence of his view that the fundamental aim of inquiry is depth of understanding rather than gross coverage of data. From Chomsky's point of view, then, evidence that his theory fails in large areas, and is in fact restricted to a small domain, would be relatively unimportant.

Exactly the opposite is true for Pullum. He (1979a:136-138) cites numerous examples of "processes" in English and other languages that violate the SSC. The processes - nearly forty - cited by Pullum cover a wide range, including pronominal anaphora, reflexive anaphora, coreferential deletion, deletion anaphora, movement rules, triggered morphological processes. Pullum (1979: 139) discusses one type of reaction to the challenges posed by such potential counterevidence for the SSC, namely, that of restricting the kind of data to which the condition is relevant. Pullum remarks that "methodologically, this kind of defence of a hypothesis is of the worst kind, amounting to a determination to weaken the claims of the SSC until no facts remain in its scope which can refute it". The significance of Pullum's reaction is that he is operating within a Popperian falsificationist framework, which demands that hypotheses should be testable (= falsifiable), and which attaches great importance to attempts to refute hypotheses. That is why it is objectionable to Pullum to restrict the domain of the SSC to precisely those cases which it can account for. For Chomsky, on the other hand, this is a rational thing to do, since it makes it possible to achieve some depth of understanding, even if it is only in a restricted domain.

Of course, it must not be concluded that Chomsky is uninterested in extending the domain of data successfully accounted for by his

theory/ . . .

theory. Chomsky (1982a:82) explicitly states that he has "consciously tried to extend the range of data . . ." The developmental history of binding theory also provides evidence of Chomsky's concern to extend the range of data accounted for by his theory. At several points during the developmental history of binding theory Chomsky took steps to enable binding theory to explain phenomena previously left unexplained by it, or conceded the need for such steps. Consider, for instance, the reformulation of the SSC to include controlled traces as specified subjects (see § 3.2.7.2), the proposed incorporation of the notion [+ definite] in the SSC (see § 3.2.7.3), the modification of the definition of 'involve' to include the case where X is a constant context for some change (see § 3.3.4), and Chomsky's (1981a) proposals about the constructions to which binding theory must be extended, including left-dislocated items, topics, predicate nominals, heads of relatives, subjunctives, extraposition (see § 6.7). These examples provide evidence that Chomsky is also concerned to extend the domain of phenomena accounted for by his theory.

#### 7.2.3.6 Epistemological tolerance

As noted above, for instance in § 3.2.7.6, in his recent works Chomsky explicitly advocates the adoption of a tolerant attitude in cases where a theory is threatened by potential negative evidence. Thus, Chomsky (1980a:9-10) expresses the opinion that linguists should exhibit "readiness to tolerate unexplained phenomena or even as yet unexplained counterevidence to theoretical constructs that have achieved a certain degree of explanatory depth in some limited domain". Other recent works in which Chomsky argues for the adoption of a tolerant attitude towards potential negative evidence include (Chomsky 1978a:10; 1978b:14; 1979a:188). In these works Chomsky depicts a tolerant attitude towards unexplained phenomena and potential counterexamples as a feature of the so-called "Galilean style of inquiry", a style of inquiry which he argues ought to be adopted by linguists.

This connection/ . . .

This connection between epistemological tolerance and Chomsky's conception of the "Galilean style of inquiry" was also explicated in § 2.4 above.

In his (1980a:9-10) remarks quoted above, Chomsky distinguishes two types of cases towards which a tolerant attitude is justified: (i) "unexplained phenomena", and (ii) "unexplained counterevidence". This distinction is incorporated in the present analysis of Chomsky's attitude of epistemological tolerance. In terms of this analysis, a first type of case in which an attitude of epistemological tolerance may be adopted, includes cases where a theory fails to explain certain phenomena, because it says nothing about them. The second type of case in which an attitude of epistemological tolerance may be adopted, includes cases where a theory says the wrong thing about certain phenomena, that is, where the theory makes wrong predictions.

The developmental history of binding theory provides evidence that Chomsky does in practice adopt an attitude of epistemological tolerance in cases where his theory is threatened by potential negative evidence. Chomsky openly acknowledges that binding theory (as well as other components of UG) faces potential negative evidence, both in the form of unexplained phenomena and unexplained counterevidence. In (48) a list is presented of specific cases discussed above in which some version of binding theory is threatened by negative evidence, either in the form of unexplained phenomena or unexplained counterevidence. The common feature of these cases is that Chomsky adopted an attitude of epistemological tolerance towards the negative evidence threatening binding theory, that is, Chomsky simply set aside the negative evidence.

- (48) a. The SSC, reformulated to include controlled traces, still fails to explain the unacceptability of 3.(63b)  
 - \* *the men are easy for each other to please* -  
 § 3.2.7.2.

- b. The TSC and SSC make the wrong predictions about Coreference assignment - § 3.2.7.5.
- c. The SSC makes wrong predictions about several processes cited by Postal (1976) - § 3.3.4.
- d. In some cases certain versions of binding theory make the wrong predictions about pronouns in NPs - § 6.7.
- e. Binding theory fails to explain the properties of constructions with left-dislocated items, topics, predicate nominals, heads of relatives - § 6.7.
- f. Binding theory makes the wrong predictions about the binding of anaphors in the position of  $\alpha_6$  in 6.(36a), where the c-command requirement is violated - § 6.7.
- g. Binding theory makes wrong predictions about the binding of anaphors and pronouns in PPs in English - § 6.7.

One further case discussed above in which Chomsky adopted an attitude of epistemological tolerance must be mentioned here. As noted in § 6.3.3.5 above, Chomsky (1981a:285) observed that the simple GB indexing theory makes some wrong predictions about disjoint reference. In spite of these wrong predictions, Chomsky did not give up the GB indexing theory, or modify it. The difference between this illustration of Chomsky's epistemological tolerance and the cases listed in (48), is that the threatened theory in this instance is not the *binding* theory, but the *indexing* theory.

(48) contains two types of cases. First, in some of the cases listed in (48) Chomsky did not even mention possible solutions to the relevant problem - see for instance (48b, c, e). Second,

in some/ . . .

in some of the cases Chomsky discussed possible solutions to the relevant problem, but pointed out that the solutions themselves are for some reason or another problematic - see for instance (48f, g). In these latter cases, as in the former cases, Chomsky in effect fails to propose a solution which he regards as adequate, and the potential negative evidence is thus not explained. For this reason cases such as (48f, g) are also regarded as cases in which Chomsky adopted an attitude of epistemological tolerance towards potential negative evidence threatening binding theory.

It could be argued that the list of cases in which Chomsky adopted a tolerant attitude towards potential negative evidence threatening binding theory should be extended to include cases in which Chomsky made only a tentative and vague suggestion in connection with the negative evidence. The reason for including such cases would be that Chomsky has not shown that the relevant empirical problems can in fact be overcome in terms of his vague, tentative suggestion. Possible candidates for inclusion in (48) under this less strict view of what comprises epistemological tolerance are the proposals listed as (38b, c, h, i, k). In § 7.2.3.4 above these proposals were interpreted as proposals for *ad hoc* devices used to protect Chomsky's theory. If such proposals were added to the list (48) of cases in which Chomsky exhibited an attitude of epistemological tolerance, the content of the claims made below about Chomsky's epistemological tolerance would not be affected. The same is true for the claims made in § 7.2.3.4 about Chomsky's use of *ad hoc* protective devices. The only effect of including cases such as (38b, c, h, i, k) in (48) would be to strengthen the arguments for the claims made below about Chomsky's epistemological tolerance. For this reason no final choice will be made here as to whether cases such as (38b, c, h, i, k) should be regarded as instances in which Chomsky made use of *ad hoc* devices to protect his theory, or whether in these instances Chomsky in fact adopted an attitude of epistemological tolerance.

In addition/ . . .

In addition to the potential negative evidence for binding theory discussed in chapters 3 - 6 above, there is additional potential negative evidence presented in the literature. Such negative evidence is presented in, for instance, (Bach and Horn 1976), (Bach 1977), (Brame 1977, 1978), (Grosu 1978), (Iwakura 1980), (Nanni and Stillings 1978), (Pullum 1975, 1979). It is reasonable to assume that Chomsky was aware of at least some of the potential negative evidence presented in these works. The fact that Chomsky did not reconstruct his theory in order to overcome this potential negative evidence provides additional support for the claim that epistemological tolerance is a feature of Chomsky's work.

Of course, not *all* cases in which Chomsky failed to modify his theory in the face of potential negative evidence presented in the literature need to be explained in terms of epistemological tolerance. The schema (7) shows that the predictions of UG about a specific mental grammar must be tested empirically via a descriptive grammar, which is a characterization of this mental grammar. This fact has an important consequence for the testing of UG. Chomsky (1977c:74) puts this point as follows.

- (49) "To find evidence to support or to refute a proposed condition on rules, it does not suffice to list unexplained phenomena; rather it is necessary to present rules, i.e., to present a fragment of a grammar. The confirmation or refutation will be as convincing as the fragment of grammar presented." 26)

Some of the "evidence" presented in the literature against Chomsky's theory, and specifically the binding theory presently under discussion, takes the form of lists of unexplained phenomena, and not justified fragments of grammar. This is true, for instance, for at least some of the alleged negative evidence presented by Postal (1976) and Pullum (1975; 1979).<sup>27)</sup> Failure on Chomsky's part to modify his theory in order to overcome such potential negative evidence should not be characterized as epistemological tolerance. In such cases Chomsky is not actually

setting potential negative evidence aside. He is rather rejecting the alleged negative evidence as inappropriate or irrelevant.<sup>28)</sup>

In order to make explicit the role which epistemological tolerance plays in Chomsky's reaction to potential negative evidence threatening his theories, the following principle must be incorporated in our model of his rationality.

- (50) When UG (or some component of UG) is threatened by negative evidence - derived from either unexplained phenomena or potential counterevidence - an attitude of epistemological tolerance may be adopted towards this negative evidence.

Various questions arise concerning the role of epistemological tolerance in Chomsky's work. For instance, how does Chomsky reconcile his tolerant attitude towards negative evidence with the adoption of the principle of theory appraisal (11), which states that one of the factors relevant to the appraisal of UG is its success in providing explanations for and making correct predictions about specific grammars? Also, how can the epistemological tolerance exhibited in the cases mentioned above be reconciled with the many serious attempts made by Chomsky during the developmental history of binding theory to overcome potential negative evidence? And are there conditions which govern the use of the principle of epistemological tolerance in reaction to potential negative evidence? That is, are there circumstances under which it is not appropriate to adopt an attitude of epistemological tolerance towards negative evidence threatening a theory? In the rest of § 7.2.3.6 an attempt will be made to provide answers to these questions.

Firstly, let us consider the reasons why, in general, Chomsky regards epistemological tolerance to negative evidence as an appropriate response.<sup>29)</sup> Chomsky's argument for the adoption of an attitude of epistemological tolerance is based on two considera-

tions/ . . .

tions. In the first place, Chomsky claims that this attitude worked very well in the case of Galileo, and in fact in the natural sciences in general. Thus Chomsky (1978a:10) attributes "the great successes" of the modern natural sciences "to the pursuit of explanatory depth which is frequently taken to outweigh empirical inadequacies". Chomsky's argument is then that the adoption of a similar attitude in linguistics might facilitate progress in linguistics just as it did in the natural sciences. In the second place, Chomsky claims that at this stage in the development of linguistics, linguists often do not know what kind of evidence is relevant to linguistic theories. He (1980a:10) spells out the consequence of this fact for the evaluation of potential counter-evidence for a linguistic theory in the following striking way.

- (51) "As for the matter of unexplained apparent counterevidence, if someone were to descend from heaven with the absolute truth about language or some other cognitive faculty, this theory would doubtless be confronted at once with all sorts of problems and 'counterexamples', if only because we do not yet understand the natural bounds of these particular faculties and because partially understood data are so easily misconstrued."

These remarks by Chomsky tie in with his remarks quoted in § 7.2.3.5 above on the problems involved in determining the domain to which a phenomenon belongs. As Botha (1982a:12) explains, epistemological tolerance complements the use made of abstraction and idealization in defining the scope of a theory. If not just any problematic datum needs to be explained by a theory, then not just any linguistic datum can constitute real negative evidence for this theory.

The second main issue to consider in this analysis of Chomsky's epistemological tolerance is how the adoption of an attitude of epistemological tolerance can be reconciled with the many serious attempts made by Chomsky during the developmental history of binding theory to overcome negative evidence threatening binding theory. Part of the answer to this question is

as follows/ . . .

as follows: The claim that epistemological tolerance is an appropriate response to potential negative evidence threatening a theory does not imply that negative evidence becomes irrelevant for the appraisal of that theory. Consider, for example, the remarks by Chomsky (1979a:188), where it is made clear that, while it is reasonable to set aside potential counterexamples to a theory "with some degree of explanatory force", ultimately all counterexamples must be explained. All other things being equal, the elimination of negative evidence threatening a linguistic theory constitutes a step forward in Chomsky's linguistics. The developmental history of binding theory provides ample evidence for this. A great number of the changes made in binding theory were (partly or completely) justified on the grounds that the change enabled the theory to overcome negative evidence, either in the form of unexplained phenomena or unexplained counterevidence. Consider in this connection the changes listed in (13) and (14) above, with the exception of (13a). The developmental history of binding theory in fact shows that within Chomsky's linguistics, attempts to overcome potential negative evidence can play a positive role in improving linguistic theories. Chomsky (1982b:75-76) explicitly acknowledges the positive role which counterevidence can play in improving a theory.

Chomsky's search for explanatory depth rather than gross coverage of data - a search which, in his view, requires epistemological tolerance - also does not imply that the data become unimportant, or are disregarded. This point is made pertinently by Chomsky (1980a:11-12). Having stressed the importance of finding principles which can provide explanations for "crucial facts", he continues as follows.

- (52) "It is a mistake to argue, as many do, that by adopting this point of view one is disregarding data. Data that remain unexplained by some coherent theory will continue to be described in whatever descriptive scheme one chooses, but will simply not be considered very important for the moment."

The attention paid by Chomsky to negative evidence threatening the various versions of binding theory, and to possible ways to overcome this negative evidence, provide support for the claim that he does not blindly disregard recalcitrant data.

The answer to the question of how the adoption of an attitude of epistemological tolerance can be reconciled with attempting to explain potential negative evidence has a second component, namely, that there are limits to Chomsky's epistemological tolerance. This brings us to the third issue to be considered here in connection with Chomsky's epistemological tolerance, namely, whether the adoption of such an attitude towards negative evidence is guided by any rule, or rules. That is, is there a rule on the basis of which Chomsky decides when it is appropriate to set aside negative evidence threatening a theory, and when the threatened theory must be modified or given up? In his comments on the issue, Chomsky claims that there is no precise rule on the basis of which it can be calculated that the negative evidence for a particular theory has accumulated to such an extent that the theory must be modified or abandoned. Consider in this connection the two sets of remarks presented in (53a) and (53b) below. The italics are mine.

- (53) a. "One must try to assess the relative importance of the phenomena or rules that contradict one's hypotheses, as compared with the evidence supporting them. Then, one will either put aside counterevidence to be dealt with later, or else decide that the theory is inadequate and must be reconstructed. *The choice isn't easy. There is no algorithm.* And as this kind of problem arises constantly in the course of research, *it is an intuitive judgment whether or not one should persevere within a given framework* - because of the positive results and in spite of the apparent counterexamples." (Chomsky 1979a:188.)
- b. "Apparent counterexamples and unexplained phenomena should be carefully noted, but it is often rational

to put/ . . .

to put them aside pending further study when principles of a certain degree of explanatory power are at stake. *How to make such judgments is not at all obvious; there are no clear criteria for doing so.*" (Chomsky 1980b:2.)

These remarks suggest that the adoption of an attitude of epistemological tolerance towards potential negative evidence is not governed by precise rules. If so, then one would have to conclude that non-rule governed judgment plays a role in theory appraisal in Chomsky's linguistics.<sup>30)</sup> However, even if it were granted that there is an element of non-rule governed judgement in Chomsky's decisions whether or not to adopt an attitude of epistemological tolerance towards potential negative evidence, it would not follow that there are no factors which influence his decisions. Close examination of the cases listed in (48) reveals that there are at least two factors which influence these decisions.

The first factor is Chomsky's own appraisal of the threatened theory in terms of the principles of theory appraisal presented in § 7.2.2 above. For Chomsky, epistemological tolerance is an appropriate response when a successful, interesting theory is threatened by potential negative evidence. In his explicit comments on the appropriateness of adopting a tolerant attitude towards potential negative evidence, Chomsky refers to "theoretical constructs that have achieved a certain degree of explanatory depth in some limited domain" (Chomsky 1980a:9), and to "principles of a certain degree of explanatory power" (Chomsky 1980b:2). Chomsky clearly regards binding theory, which is the threatened theory in the cases listed in (48), as a theory with explanatory power. The case of the GB indexing theory indicates that it is not only explanatory power that makes a theory threatened by negative evidence worth preserving. The reason why Chomsky chose the GB indexing conventions to the OB indexing conventions in spite of potential negative evidence threatening the GB indexing conventions, was the greater

metatheoretical simplicity (in the sense of (18)) of these latter conventions. There is then some evidence that the following constraint on the use of the principle (50) in Chomsky's linguistics should be incorporated in our model of his rationality.

- (54) A tolerant attitude may be adopted towards negative evidence threatening a theory, if the theory is in other respects highly valued, for instance, if it has considerable explanatory success, or attractive conceptual properties.

Important though the success of the threatened theory undoubtedly is in influencing a decision to adopt a tolerant attitude towards negative evidence, this factor does not make it possible to distinguish between cases in which Chomsky adopted a tolerant attitude towards evidence threatening binding theory and cases in which Chomsky modified his theory in order to explain such negative evidence. A factor which does make it possible to make an interesting distinction between the two types of cases is that of the preservation of empirical success. The developmental history of binding theory provides strong evidence that the following principle plays a role in the theory changes made by Chomsky.

- (55) Given a chronologically ordered series of different versions of UG, a version  $T_x$  is better than a version  $T_{x+1}$  if  $T_x$ , but not  $T_{x+1}$ , can preserve the explanatory and predictive success of their predecessors, unless  $T_{x+1}$  has explanatory and predictive success in a new area which compensates for its loss of success.

(55) can be interpreted as a further specification of the principle of theory appraisal (11), which identifies the explanatory and predictive success of UG with respect to specific grammars as one of the factors which guides theory choice in Chomsky's linguistics. Note also that (55) must be interpreted so that a restriction of the domain of a theory does not count as an

instance/ . . .

instance in which there is a loss of empirical success - provided, of course, that the restriction is justified. If a phenomenon does not actually fall within the domain of a theory, then there is no genuine loss of empirical success if a version  $T_{x+1}$  of a theory, in contrast with an earlier version  $T_x$ , cannot account for this phenomenon.

If (54) were indeed a principle guiding Chomsky's theory choices, then one would expect this principle to constrain Chomsky's epistemological tolerance. Specifically, one would not expect Chomsky to adopt a tolerant attitude towards negative evidence threatening a version  $T_{x+1}$  of his theory in a case where an earlier variant  $T_x$  could explain the data from which the evidence is derived, unless  $T_{x+1}$  has some success in an area in which  $T_x$  fails to compensate for its loss. An examination of the cases listed in (48) reveals that in none of these cases - with the exception of (48d) - the evidence threatening binding theory was successfully accounted for by an earlier variant of binding theory. It will be argued below that (48d) is a case where there is a loss as well as a gain of empirical success, and that (48d) supports the inclusion of the "unless"-clause in (55). The cases listed in (48) thus provide strong evidence for the claim that the principle (55) should be incorporated in a model of Chomsky's rationality, and, more specifically, for the claim that the preservation of empirical success constrains Chomsky's epistemological tolerance.

Let us now consider (48d) in more detail, in order to see how (48d) provides support for the inclusion of the "unless"-condition in (55). In (48d) reference is made to the fact that some versions of binding theory make the wrong predictions about the interpretations of pronouns in NP. Consider the following sentences, with an overt anaphor in NP in (56a) and a pronoun in NP in (56b).<sup>31)</sup>

(56) a. they read [NP each other's books]

b. John read [ NP his book ]

The success of the various versions of binding theory in making the correct predictions about the interpretation of *each other* and *his* in such constructions is summarized in (57).<sup>32)</sup>

(57)	(56a)	(56b)
OB	✓	x
GB governor	x	✓
GB SUBJECT	✓	x

In the series of different versions of binding theory presented in (57), two points can be identified at which there is apparently a loss in explanatory and predictive success. First, in the transition from the OB binding theory to the GB governor binding theory, there appears to be a loss of success with respect to (56a). Second, in the transition from the GB governor binding theory to the GB SUBJECT binding theory there appears to be a loss of success with respect to (56b). As regards the failure of the GB governor binding theory - in contrast with the OB binding theory - to account for cases such as (56a), Chomsky did not adopt a tolerant attitude towards the negative evidence in question. Rather, Chomsky claimed that cases such as (56a) are marked, and thus fall outside the domain of core grammar. It is true that these markedness claims are problematic, and they will be examined in greater detail below. However, the general point should be clear. To react to negative evidence threatening a theory by arguing that the phenomena from which the evidence derives fall outside the domain of the threatened theory is not to adopt a tolerant attitude towards the threatening evidence, but to attempt to provide an explanation for it.

As regards the failure of the GB SUBJECT binding theory to account for (56b), it is interesting to note that the GB SUBJECT binding theory makes the same predictions about (56) as the OB binding theory. The crucial point to note in connection with

the summary/ . . .

the summary presented in (57) is that no version of binding theory succeeds in making the correct predictions about both (56a) and (56b). Chomsky (1981a:217-218) explicitly makes this point. The problem, as Chomsky explains, is that in these constructions the pronoun is not free where the anaphor is bound. All versions of binding theory predict that a pronoun must be free where an anaphor is bound. The GB SUBJECT binding theory, like all its predecessors, in fact fails to account for "the near complementary distribution between proximate pronouns and reflexives." The success of the GB SUBJECT binding theory in making the correct predictions about cases such as (56a) is off-set by its failure to make the correct predictions about cases such as (56b). Or, to put it differently, the failure of the GB SUBJECT binding theory to make the correct predictions about (56b) is compensated for by (56a).

The replacement of the OB indexing conventions by the GB indexing provides further evidence that the principle for the preservation of empirical success must make provision for a loss of success in one area to be compensated for by a gain of empirical success in another area. The GB indexing theory fails to make the correct predictions about all cases of disjoint reference. As noted above, Chomsky adopts a tolerant attitude towards the negative evidence which disjoint reference provides for the GB indexing conventions. From the perspective of the current discussion, the crucial point to note in connection with this negative evidence threatening the GB indexing theory, is that the GB indexing theory fails in cases in which the OB indexing theory succeeds in making correct predictions. Apparently, then, the replacement of the OB indexing conventions by the GB indexing conventions led to a loss of explanatory and predictive success. However, Chomsky (1981a:288) argued that there are also cases which can be accounted for by the GB indexing theory, but which are problematic for the OB indexing theory. There is thus simultaneously a gain in explanatory and predictive success. Having discussed in detail the relative empirical

success/ . . .

success of the two indexing theories, Chomsky (1981a:289) concludes that "an apparent advantage of the OB-framework appears illusory". Chomsky's efforts to show that the GB indexing theory succeeds in cases where the OB indexing theory fails, together with his conclusion about the relative merit of the two theories, provide some support for the idea that a loss of empirical success in one area can be compensated for by a gain of empirical success in another area.

Neither of the two cases discussed above in which a loss of empirical success in some area is apparently compensated for by a gain in another area provides any evidence that there is a rule on the basis of which it can be decided that a particular gain compensates for a particular loss. If this conclusion is correct, then the weighting of gains and losses of empirical success is an area in which non-rule governed judgment plays a role in Chomsky's theory choices.

There is also some textual evidence to support the claim that the failure of other versions of his theory to account for certain phenomena influences Chomsky in deciding to adopt a tolerant attitude towards the negative evidence which such phenomena provides for a specific version proposed by him. Thus Chomsky (1973:238) pointed out that Coreference Assignment violated not only the TSC, but also some of Ross' island constraints, which were the immediate predecessors of the 1973-conditions. Chomsky (1981a:217-218), in commenting on the failure of the GB SUBJECT binding theory to make correct predictions about the interpretation of pronouns in cases such as (56b), stated that the near complementary distribution between proximate pronouns and reflexives "is only partially covered by any of the approaches we have been investigating here . . .". Chomsky (1981a:229, fn. 64) also points out that none of the versions of binding theory reviewed in (Chomsky 1981a) can account for cases in which the binder does not c-command the element bound by it.

It was/ . . .

It was claimed above that the principle of the preservation of empirical success (55a) makes it possible to distinguish between cases in which Chomsky adopted a tolerant attitude towards negative evidence threatening his theory and cases in which he took special steps to overcome such evidence. In (34), (38), and (41) - (43) several cases are listed in which Chomsky took special steps to overcome negative evidence threatening binding theory. The nine cases listed in (34) are of special interest, since they involve a conflict between two principles of theory appraisal adopted by Chomsky: the principle of explanatory and predictive success with respect to specific grammars (11) and the principle of simplicity (18). In (34) above nine cases are listed in which Chomsky chose a more complex version of his theory over a simpler version, in order to overcome potential negative evidence threatening the simpler version. Chomsky thus chose a version less highly valued in terms of the principle (18) than an alternative version of his theory. The question was raised in § 7.2.3.3 why Chomsky did not adopt a tolerant attitude towards the threatening evidence in these cases. The principle of the preservation of empirical success (55) provides an interesting answer to this question in seven of the nine cases listed in (34). In the case of (34a, b, d, e, g, h, i) the special stipulation was introduced not to increase the explanatory and predictive success of the theory, but to preserve the success of an earlier variant of the theory. Let us now consider each of these cases in detail, in order to substantiate the claim that the stipulations served to preserve the explanatory and predictive success of earlier variants.

Ross' so-called island theory is, as far as the conditions component of UG is concerned, the immediate predecessor of the 1973 "Conditions"-framework. This island theory correctly predicted that clause external *wh*-Movement can take place. Without the COMP escape hatch referred to in (34a) (and of course the auxiliary hypotheses discussed in § 2.2.7.1 above) the 1973

"Conditions"-theory/ . . .

"Conditions"-theory would have wrongly predicted that *wh*-phrases cannot be moved out of clauses. The incorporation of the COMP-escape hatch thus enabled the 1973-conditions to preserve the empirical success of Ross' island theory with respect to clause external *wh*-Movements.

As explained in § 3.3.4 above, the change in the definition of 'involve' to include the case where X is a constant context for some change - see (34b) - was made in order to enable the SSC to apply to Q-float. The importance of Q-float derived from the fact that Postal (1976) had argued that Q-float shows that the formal power of transformational rules must be enriched to enable transformational rules to refer to grammatical functions. Fiengo and Lasnik (1976:188) rejected Postal's argument, and claimed that there is a "reasonably adequate analysis of Q-floating" which is consistent with a restrictive theory of transformations that prohibits reference to grammatical functions. In terms of the assumptions made by Chomsky (1977c) about the structure of embedded clauses, Fiengo and Lasnik's analysis of Q-float no longer works. Consequently, Chomsky (1977c) tried to show that within the framework of his different assumptions Q-float can also be accounted for without reference to 'subject' in the rule. (34b) thus also represents a case in which Chomsky tried to preserve the empirical success of an earlier variant of his theory. In particular, he tried to preserve the empirical success of the version proposed by Fiengo and Lasnik (1976) with respect to Q-float.

The OB binding conditions - formulated as conditions on the (un)-boundedness of anaphors at some level of representations - replaced the SSC and PIC - formulated as conditions on the application of rules of construal. Without the indexing theory which assigns anaphoric indices referred to in (34d), the OB binding theory would not have preserved the success of the SSC and PIC with respect to Disjoint Reference.

The clause/ . . .

The clause referring to S - see (34e) - was added to the NIC in order to enable the NIC to preserve the explanatory and predictive success of the PIC with respect to *wh*-traces in COMP. Chomsky dropped this clause from the NIC only when it became clear that, given certain other assumptions of the OB-theory, this clause was not really required by the NIC to account for *wh*-traces in COMP.

The GB SUBJECT binding theory replaced the GB governor binding theory. Without the stipulation about root S's referred to in (34g), the GB SUBJECT binding theory would have failed to preserve the success of the GB governor binding theory with respect to anaphors in the subject position of tenseless sentential subjects.

In (34h) reference is made to Chomsky's decision not to replace the notion "governing category" in the GB SUBJECT binding theory with the simpler notion 'binding category'. If this simplification were introduced, the explanatory and predictive success of the version incorporating the notion 'governing category' with respect to the binding of PRO would not have been preserved completely.

The assumption about G in COMP referred to in (34i) was required to enable the GB SUBJECT binding theory to preserve the explanatory and predictive success of earlier versions - including the GB governor binding theory - with respect to the Italian AUX-to-COMP construction.

The fact that the principle of the preservation of empirical success (55) can provide explanations for Chomsky's decisions to introduce the complications listed in (34a, b, d, e, g, h, i) in his theory, instead of setting the relevant negative evidence aside, provides additional justification for the claim that the principle (55) constrains Chomsky's epistemological tolerance. Recall that there is an overlap between the cases listed in (34)

of complications/ . . .

of complications introduced into the theory and the cases listed in (38) of *ad hoc* protective devices employed by Chomsky. For instance, (38a) is also listed as (34a), (38d) is also listed as (34b), (38g) is also listed as (34g), and (38j) is also listed as (34i). In the case of (38a, d, g, j) the principle of the preservation of empirical success can thus also provide an explanation of why Chomsky decided to employ a protective device. In two of the cases in which Chomsky reacted to negative evidence threatening binding theory by claiming that the phenomena from which the negative evidence derives fall outside the domain of the theory, the effect of the restriction was to avoid a genuine loss of empirical success. Consider in particular the restrictions listed in (42b) and (42c). The ways in which Chomsky's proposals served to avoid a loss of empirical success were outlined above.

In contrast with the cases discussed above, where Chomsky took steps in order to preserve the success of an earlier version, several cases were discussed above in which Chomsky took steps to overcome negative evidence threatening his theory even though no earlier version of his theory could account for the evidence. See for instance (34c, j), and (38b, c, e, f, h, i, k). Note that (34j) is also listed as (38k). The fact that there are cases in which Chomsky took special steps to overcome negative evidence which also threatened earlier versions of his theory, does not pose a real threat to the principle (55). (55) does not entail that Chomsky will take steps to overcome negative evidence threatening some version of his theory *only* in order to preserve the explanatory and predictive success of an earlier version of the theory. (55) only entails that Chomsky will take steps in order to prevent a loss of success. There is no reason to assume that the preservation of empirical success is the only factor which plays a motivating role in Chomsky's decision to take steps to explain negative evidence threatening his theory. Chomsky is after all also concerned with extending the domain of the theory. It is then not

unreasonable/ . . .

unreasonable to assume that if there is a satisfactory solution available for an empirical problem threatening his theory, Chomsky will adopt it - especially if the solution leaves the threatened theory itself essentially unmodified. It is interesting to note how frequently a proposal made by Chomsky in connection with threatening evidence is based on work done by others. That is, a "ready-made" solution was already available. In the case of (34c) Chomsky took over a suggestion made by Vergnaud. In the case of (34j) (= (38k)) Chomsky referred to work done by other linguists on clitics in the Romance languages. In several of the cases in which Chomsky solved the problem posed for binding theory by potential negative evidence by claiming that the phenomena from which the evidence derives fall outside the domain of the theory, he also took over proposals made by other linguists about the proper domain of the phenomena in question. Consider in this connection his claims that Picture-Noun Reflexivization (see (41a)), Coreference Assignment (see (41c)), and VP-deletion (see (41d)) fall outside the domain of sentence grammar. In each of these cases Chomsky relied on work done by other linguists in connection with the exact status of the rules.

It should be kept in mind that the number of cases in which Chomsky took steps to overcome negative evidence which also threatened earlier versions of his theory may in fact be smaller than indicated above. The possibility was mentioned above that the characterization of epistemological tolerance should be relaxed to include cases in which only vague and tentative suggestions regarding possible solutions to empirical problems are made. The proposals referred to in (38b, c, h, i, k) are very vague and tentative. (Note that (38k) is also listed as (34j)). If these vague and tentative suggestions are indeed manifestations of an attitude of epistemological tolerance, and not of a concern to explain potential negative evidence, then the cases (38b, c, h, i, k) would have to be added to the list (48) of cases in which Chomsky adopted an attitude of epistemological tolerance.

In sum/ . . .

In sum, then, there is clear evidence that the principle of the preservation of empirical success (55) constrains the adoption of an attitude of epistemological tolerance by Chomsky. In none of the cases in which Chomsky adopted such an attitude (under either the strict or the lax characterization of epistemological tolerance) was there a loss of empirical success. The principle of the preservation of empirical success (55) also makes it possible to explain for a wide range of cases why Chomsky took special steps to explain negative evidence threatening some version of his theory, rather than setting the negative evidence aside. Of special importance is the fact that in terms of the principle (55) it can be explained for all but two cases why Chomsky decided to introduce a complication into UG. Note that under the lax interpretation of epistemological tolerance, the complication referred to in (34c) would actually be the only one not explained in terms of (55). The preservation of empirical success is, of course, not the only factor which motivates Chomsky to take special steps to explain negative evidence. There are cases in which Chomsky took such special steps even though the relevant negative evidence threatened earlier variants of the theory too. The availability of a solution - for instance, from work done by other linguists - is also a significant factor in explaining why Chomsky took special steps in a particular instance, rather than putting the negative evidence aside.

There is a further aspect of the developmental history of binding theory which provides support for the principle of the preservation of empirical success (55). If this principle is incorporated in the model of Chomsky's rationality, then it becomes possible to explain the rather curious role which Chomsky's notion 'markedness' played in the development of the various versions of the GB binding theory. Chomsky's (1981d) main argument for replacing the OB binding theory by the GB governor binding theory was that the GB governor binding theory overcomes certain conceptual problems faced by the OB binding theory.<sup>33)</sup>

However/ . . .

However, the GB governor binding theory, in contrast with the OB binding theory, made the wrong predictions about the interpretation of arguments in NPs. Chomsky's (1981d) response to the counterexamples threatening the GB governor binding theory was to claim that the relevant phenomena are marked, and in fact fall outside the domain of core grammar. To support his claim about the markedness of the phenomena, Chomsky referred to the rarity of the phenomena across the languages of the world, and claimed that lexical choice plays a role in determining the acceptability of the constructions in question.<sup>34)</sup>

Let us set aside for the moment the question of whether such considerations can provide the required support for claims about the markedness of phenomena.<sup>35)</sup> By claiming that the phenomena about which the GB governor binding theory made the wrong predictions are marked, Chomsky in effect claimed that the relevant phenomena fall outside the domain of his theory, and that they thus cannot provide real negative evidence for the GB governor binding theory. If one were to accept Chomsky's (1981d) markedness claims, then the change from the OB binding theory to the GB governor binding theory would have to be analyzed as follows. The change represented an improvement in terms of the principles of theory appraisal (17), (18), and (21). Phenomena which provide potential counterevidence for the GB governor binding theory actually fall outside the domain of this theory, and thus do not provide real counterevidence for it. The replacement of the OB binding theory by the GB governor binding theory is thus in several respects an improvement, and there is no disadvantage or "loss" involved.

However, there are two reasons why such an account of the replacement of the OB binding theory by the GB governor binding theory would be wrong. The first is that Chomsky (1981d) did not provide any real justification for his claims about the markedness of the phenomena about which the GB governor theory made the wrong

predictions/ . . .

predictions. Moreover, Chomsky later openly admitted this. It was explained in § 6.5 above that Chomsky's claims about the distribution of the relevant constructions across the languages of the world are completely unsubstantiated, and such claims can thus provide no support for claims about the markedness of these constructions. Chomsky (1982a:110) makes the following "confession" about his (1981d) claims on the markedness of the picture noun cases.

- (58) "I've always assumed they're a little odd in their behaviour, but *they really just didn't fall into the theory I outlined there at all, so I just had to say they're totally marked. I gave a half-baked argument about that, and there was some bad conscience, I must concede.*" (The italics are mine - M.S.)

In the light of these remarks, one must conclude that Chomsky (1981d) chose the GB governor binding theory in preference to the OB binding theory even though the GB governor binding theory was threatened by negative evidence which did not also threaten the OB binding theory. This conclusion, together with Chomsky's (1982a:110) reference to "a half-baked argument", confirms Botha's (1982a) claim that Chomsky made use of what amounts to "rhetorical tricks" to persuade others to accept the replacement of the OB binding theory by the GB governor binding theory. By trying to create the impression that the phenomena in question are undoubtedly marked and that there is no need for extensive justification of his markedness claims, Chomsky tried to mislead others about the shortcoming of the GB governor binding theory. The obvious question that arises is why the phenomena about which the GB governor binding theory made the wrong predictions were so important that Chomsky took such special steps in connection with them. That is, why did Chomsky not set the evidence aside, in accordance with the attitude of epistemological tolerance advocated by him? I will return to this question below.

There is a second reason why the change from the OB binding theory to the GB government cannot be analyzed in the manner outlined above.

Although/ . . .

Although Chomsky (1981d) claimed that the cases about which the GB governor binding theory makes the wrong predictions fall outside the domain of core grammar, he (1981a:209) still regarded these cases as presenting a problem for binding theory. The development of the GB SUBJECT binding theory was partly motivated by a desire to account for these cases. The fact that the GB SUBJECT binding theory makes the correct predictions about them, is one of the considerations in terms of which Chomsky justified his decision to replace the GB governor binding theory by the GB SUBJECT binding theory. The introduction of the latter version of the GB binding theory also indicates that the analysis of the change from the OB binding theory to the GB governor binding theory presented above is in fact incorrect.

The question that lies at the heart of any attempt to reconstruct the change from the OB binding theory to the GB governor binding theory to the GB SUBJECT binding theory, is the following: Why did Chomsky not set aside the negative evidence threatening the GB governor binding theory in the hope that later versions of the theory might be able to account for it? If it is assumed that Chomsky adheres to the principle of the preservation of explanatory and predictive success (55), then there is an answer to this question. In the choice between the OB binding theory and the GG governor binding theory there was a conflict between the principle of the preservation of explanatory and predictive success (55), on the one hand, and the principles of increased metatheoretical simplicity (17) and (18) and the principle of greater deductive depth (21), on the other hand. Chomsky resolved this conflict in favour of the latter principles. That is, he chose the GB governor binding theory. Especially in view of remarks such as those quoted in (53), it is reasonable to assume that this choice was a matter of non-rule governed judgment. That is, in making this choice Chomsky did not actually break a rule stipulating that the preservation of empirical success must always outweigh other factors, such as increased simplicity and deductive depth. Note also that this choice is the choice one would

expect/ . . .

expect him to make in view of his adoption of the "Galilean style of inquiry".<sup>36)</sup> Chomsky's (1981d) controversial markedness claims, and the rhetorical tricks which he employed in connection with them, must then be seen as instruments used by him to persuade others that in the choice of the OB binding over the GB governor binding theory there was no real conflict between preserving the empirical success of UG and improving the conceptual properties of UG. That is, he tried to persuade others that the conceptual improvements in binding theory did not involve any loss of empirical success.

#### 7.2.3.7 Cross-linguistic data

In the principle of theory appraisal (11) it is specified that the explanatory and predictive success of UG with respect to specific grammars is one of the factors which determine its merit. Descriptively adequate grammars provide the empirical test for proposed UGs. A question which frequently cropped up in chapters 3 - 6 is what role data from a variety of languages - cross-linguistic data, for short - should play in the appraisal of general-linguistic hypotheses. In this section the focus is on what role cross-linguistic data actually played in the developmental history of binding theory, and how this role fits in with Chomsky's explicit views on the matter. In a sense, the discussion that follows is a further clarification of the role which the principle (11) played in determining the theory choices made by Chomsky in connection with binding theory.

It is well-known that Chomsky holds the view that one can gain insight into linguistic universals without taking into account data from a wide range of languages. For Chomsky, the in-depth study of a single language (or a small set of languages) represents a fruitful approach towards the study of UG.<sup>37)</sup> Chomsky's view that the study of a single language can yield insight into linguistic universals is closely linked to his acceptance of the validity of the argument from poverty of the

stimulus/ . . .

stimulus in the study of language.<sup>38)</sup> In a recent work Chomsky (1981a:6) presents his view on this issue as follows.<sup>39)</sup>

(59) "A valid observation that has frequently been made (and often, irrationally denied) is that a great deal can be learned about UG from the study of a single language, if such study achieves sufficient depth to put forth rules or principles that have explanatory force but are underdetermined by evidence available to the language learner. Then it is reasonable to attribute to UG those aspects of these rules or principles that are uniformly attained but underdetermined by evidence."

A complete appraisal of Chomsky's use (or non-use) of cross-linguistic data would have to include a detailed analysis of the role which the argument from poverty of the stimulus could play in the study of mind. Such an analysis falls outside the scope of the present study. Thus, the aim of the following discussion is to clarify what use Chomsky actually made of cross-linguistic data during the developmental history of binding, rather than to critically appraise Chomsky's position.<sup>40)</sup>

The developmental history of binding theory provides a good illustration of Chomsky's method of approaching the study of UG through the in-depth study of a single language. Throughout the developmental history of binding theory the main focus was on data from English. The importance of data from English in the developmental history of binding theory becomes evident when one compares the first major work in which Chomsky presented the SSC and TSC namely, (Chomsky 1973), with the most recent work in which extensive modifications to binding theory are proposed, namely, (Chomsky 1981a). In (Chomsky 1973) the SSC and TSC are justified exclusively by reference to data from English. In (Chomsky 1981a) the claims about the various versions of the GB binding theory too are justified almost exclusively with reference to English data.

However, as implied by the use of "almost" above, Chomsky also took data from languages other than English into consideration

in his developing of binding theory. In (60) below a list is provided of all the cases discussed above in which Chomsky considered data from languages other than English in justifying and revising binding theory.

- (60) a. Since their introduction in (Chomsky 1973), Chomsky kept open the possibility of parametric variation in the conditions if they were to account for a wider variety of languages. Thus:
- (i) Chomsky (1973) mentioned the possibility that  $\alpha$  in the TSC might be a language-specific parameter - see § 3.2.4. Chomsky (1977c) specifically mentions Korean, which has no distinction between tensed and non-tensed clauses, in this connection - see § 4.3.
  - (ii) Chomsky (1977c) mentions the possibility that the notion 'subject' in the SSC might have to be defined differently for different types of languages - see § 4.3.
  - (iii) Chomsky (1977c) mentions the possibility that the class of cyclic nodes referred to in the PIC and SSC might be subject to parametric variation - see § 4.3.
  - (iv) Although reflexives fall under the definition of 'anaphor', reflexives in Japanese and Korean do not function as anaphors with respect to binding theory. Chomsky (1980b, 1981a) suggests that this might be a point of parametric variation - see §§ 5.2 and 5.7.
- b. In some instances Chomsky took special steps to account for potential counterevidence from languages other than English/ . . .

than English. In addition to the cases mentioned in (a) above, the following cases were discussed in chapters 3 - 6 above.

- (i) Chomsky claims that peripheral *Tous*-Movement in French, which constitutes a potential counterexample for the PIC, belongs to the marked periphery, and thus falls outside the domain of the conditions - see § 4.3.
  - (ii) Chomsky claims that Hebrew relativization, which constitutes a potential counterexample for the conditions, falls outside their domain if the conditions are restricted to rules of construal - see § 4.4.3.
  - (iii) Chomsky introduces an auxiliary hypothesis to the effect that G in COMP has the same properties as AGR, and can thus function as an accessible SUBJECT, in order to enable the GB SUBJECT binding theory to make the correct predictions about AUX-to-COMP movement in Italian - see § 6.7.
  - (iv) Chomsky considers possible solutions to the problems raised by the binding of pronouns in PP in the Romance languages for binding theory - see § 6.7.
- c. Rizzi's work on *wh*-Movement in Italian played an important role in Chomsky's identification of one of the conceptual problems of the OB binding theory - see § 6.3.3.3.
  - d. In formulating the GB SUBJECT binding theory, Chomsky took into account George and Kornfilt's work on

Turkish, in which agreement does not always coincide with Tense - see § 6.6.2.

- e. When he claimed that COMP-COMP *wh*-Movement is a marked phenomenon Chomsky referred to the absence of this phenomenon in German and Russian to justify his claim - see § 4.4.5. Chomsky also made an (unsubstantiated) claim about the distribution of certain constructions across the languages of the world when he made claims about the markedness of the picture noun cases - see § 6.5.

The cases listed in (60) show that although Chomsky's work on binding theory was based mainly on data from English, cross-linguistic data did also play a role in the development of this theory.<sup>41)</sup> When one examines the role of cross-linguistic data in the appraisal of linguistic theories, it must, of course, be kept in mind that there is a methodological consideration which limits the use of cross-linguistic data in such appraisal. This is the availability of descriptively adequate analyses from a variety of languages. Consider in this connection Chomsky's (1977c:74) remarks, quoted in (49) above. It is significant to note that as more descriptively adequate analyses from a wider variety of languages have become available, Chomsky has increasingly taken cross-linguistic data into account when constructing and revising his theories. Even a cursory look at two of the most recent technical works in which Chomsky presents proposals about UG - (Chomsky 1981a) and (Chomsky 1982b) - will provide support for this view.<sup>42)</sup> Note also that the cross-linguistic data taken into account in these works do not come from the Romance languages only, but from a much more diverse range of languages, including Arabic, Chinese, Finnish, Hebrew, Greek, Japanese, Russian, Turkish.

Three distinct roles which cross-linguistic data played in the developmental history of binding theory can be distinguished.

First, cross-linguistic data provided evidence for the further testing, justification, and revision of binding theory, a theory initially justified exclusively with reference to data from English. Consider in this connection for instance (60b, d) above. There are many passages in his work in which Chomsky explicitly states the view that cross-linguistic data provides a basis for the further testing of hypotheses about linguistic universals. For instance, Chomsky (1976b:47) explains that there are cases in which "we may plausibly postulate that *P* is a property of universal grammar on the basis of investigation of a single language". He goes on to explain that "the argument rests on the alleged fact that something is known without relevant experience so that knowledge must be attributed to the language faculty itself, a faculty common to the species". In commenting on this argument, Chomsky states that "the argument is nondemonstrative and is therefore *open to refutation by broader inquiry into the same language or other languages*". (The italics are mine.)<sup>43</sup>

In a response to Cell rier during the 1975 conference of Chomsky's and Piaget's views on language learning, Chomsky is even more insistent on the importance of cross-linguistic data in the testing of hypotheses about linguistic universals.

- (61) "An innatist hypothesis is a refutable hypothesis. Any hypothesis which says that such and such a property of language is genetically determined is subject to the most immediate refutation of the strongest kind. Such hypotheses have been refuted over and over again in the past by just looking at the next phenomenon in the same language or the next language . . .

If the hypothesis is refuted for the next language then it is wrong. Assuming, of course, the uniformity of species (I am just taking that for granted, that there aren't subspecies of humans), then if somebody proposes the property *P* and says that all he can suggest is that property *P* is genetically determined, then he will be subject to the most immediate refutation by looking at the next language where somebody may show that it doesn't conform to the property *P*." (Piatelli-Palmarini 1980:80).

Of course/ . . .

Of course, the testing of hypotheses about linguistic universals is somewhat more complicated than suggested by Chomsky in the remarks quoted above. The possibility of parametric variation in universal principles, as well as the distinction between the marked periphery and the unmarked core, obviously complicate the interpretation of any conflict between a proposed universal and the data from any specific language. However, the main point remains unaffected. Chomsky acknowledges that data from languages other than English have a role to play in the testing of hypotheses about linguistic universals, and the developmental history of binding theory reflects this view.

Second, cross-linguistic data was used to determine parametric variation in universal principles. Chomsky (1981a:6) singles out "study of closely related languages that differ in some clustering of properties" as "particularly valuable for the opportunities it affords to identify and clarify parameters of UG that permit a range of variation in the proposed principles". A well-known example is work on the Romance languages, which made it possible to determine that the binding category in the Subjacency Condition is an open parameter, with NP, S, and  $\bar{S}$  possible values.<sup>44</sup> Note that Chomsky (1981a:6) does not exclude the possibility that work on unrelated languages might also yield insight into parametric variation. During the developmental history of binding theory Chomsky also made use of data from languages other than English to identify possible parameters in binding theory. Consider in this connection the cases listed in (60a). The yielding of insight into parametric variation thus represents a second role which cross-linguistic data played during the developmental history of binding theory.

The developmental history of binding theory suggests that a third specific role for cross-linguistic data should be distinguished. Such data are apparently required for the justification of markedness claims. Consider in this connection (60e). This role of cross-linguistic data reflects on Chomsky's (1981a:9)

view on the issue of how the domain of core grammar can be delimited from the marked periphery. Chomsky states that at present the linguist is "compelled to rely heavily on grammar-internal considerations and *comparative evidence*, that is, on the possibilities of constructing a reasonable theory of UG and *considering its explanatory power in a variety of language types . . .*" (The italics are mine.) In Chomsky's view, then, cross-linguistic data provide necessary evidence for the testing of markedness claims. Chomsky's reference to cross-linguistic data in justifying the markedness claims referred to in (60e) is, of course, problematic. Chomsky's use of such data in justifying markedness claims will again be considered in § 7.3 below, where certain problematic aspects of Chomsky's rationality are outlined.

The principle of theory appraisal (11) states that the merit of a theory of UG is determined by its explanatory and predictive success with respect to specific grammars. Clearly, the greater the number of grammars with respect to which a specific version of UG has explanatory and predictive success, the more highly valued it is in terms of the principle of theory appraisal (11). As argued above, this principle played a prominent role in the justification of theory changes made by Chomsky during the developmental history of binding theory. It is against this background that the attention paid by Chomsky to cross-linguistic data should be seen.

7.2.4 Summary and some conclusions

In § 7.2.2 nine principles of theory appraisal employed by Chomsky during the developmental history of binding theory were formulated. These principles are repeated in (62) - (70) below.

(62) If the formal power of  $T_{x+1}$  is more restricted than that of  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ . (= (9))

(63) / . . .

- (63) If  $T_{x+1}$  has more success than  $T_x$  in providing explanations for and making correct predictions about the mental grammars of individual languages, as described in descriptively adequate grammars of these languages, then  $T_{x+1}$  is better than  $T_x$ . (= (11))
- (64) If  $T_{x+1}$  contains fewer redundancies than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ . (= (17))
- (65) If  $T_{x+1}$  contains fewer conditions or stipulations than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ . (= (18))
- (66) If  $T_{x+1}$  is more general than  $T_x$ , that is, if  $T_{x+1}$  unifies a wider range of phenomena than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ . (= (20))
- (67) If  $T_{x+1}$  has greater deductive depth than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ . (= (21))
- (68) If  $T_{x+1}$  contains principles that are natural as principles of mental computation, and  $T_x$  contains principles that are not natural in this sense, then  $T_{x+1}$  is better than  $T_x$ . (= (27))
- (69) If  $T_{x+1}$  avoids an internal contradiction exhibited by  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ . (= (29))
- (70) If  $T_{x+1}$  is compatible with a stronger version of the autonomy thesis than  $T_x$ , then  $T_{x+1}$  is better than  $T_x$ . (= (31))

In each of these principles  $T$  is UG, or a component of UG. It was argued in § 7.2.3 that the following principles, in addition to those listed in (62) - (70), determined the theory choices made by Chómsky during the developmental history of binding theory.

- (71) a. In cases where UG is threatened by potential negative evidence, *ad hoc* devices may be introduced to protect the theory. (= (40a))
- b. *Ad hoc* devices may be used to protect a theory if the threatened theory is in other respects highly valued, for instance, if it has considerable explanatory success. (= (40b))
- (72) a. In cases where UG is threatened by potential counterexamples, the domain of the theory may be restricted to exclude the rules constituting the counterexamples. (= (46))
- b. The domain of a theory may be restricted to exclude potential counterevidence if the threatened theory is in other respects highly valued, for instance, if it has considerable explanatory success, or attractive conceptual properties. (= (47a))
- c. The domain of a theory may be restricted only if this can be done in a systematic way, that is, in terms of a general concept or conceptual distinction, and not in terms of an arbitrary list of unrelated rules. (= (47b))
- d. A rule may be excluded from the domain of a theory if there is independent justification that the rule falls outside the domain of this theory, and inside the domain of another theory. (= (47c))
- (73) a. When UG (or some component of UG) is threatened by potential negative evidence, a tolerant attitude may be adopted towards this negative evidence. (= (50))

- b. A tolerant attitude may be adopted towards negative evidence threatening a theory, if the threatened theory is in other respects highly valued, for instance, if it has considerable explanatory success, or attractive conceptual properties. {= (54)}

- (74) Given a chronologically ordered series of different versions of UG, a version  $T_x$  is better than another version  $T_{x+1}$  if  $T_x$ , but not  $T_{x+1}$ , can preserve the explanatory and predictive success of their predecessors, unless  $T_{x+1}$  has explanatory and predictive success in a new area which compensates for its loss of success. {= (55)}

The principles listed as (62) - (70) above are the most fundamental principles which guided Chomsky in the theory choices analyzed above, in the sense that these principles identify the various properties of UG which Chomsky regards as relevant to the appraisal of different versions of UG. These properties are: (i) restricted formal power, (ii) explanatory and predictive success with respect to specific grammars (descriptive adequacy, for short), (iii) metatheoretical simplicity in the sense of nonredundancy, (iv) metatheoretical simplicity in the sense of a limited number of stipulations (v) unifiedness, in the sense of generality, (vi) deductive depth, (vii) naturalness as principles of mental computation, (viii) absence of inconsistencies (ix) degree of compatibility with the autonomy thesis. As explained in § 7.2.2, all these properties are regarded as indicators of truth, or truthlikeness, in UG.

Of the principles proposed in § 7.2.7, the principle of the preservation of empirical success (55)/(74) is undoubtedly the most fundamental. This principle should be interpreted as a further specification of the principle (11)/(63). The principle (55)/(74) constrains Chomsky's epistemological tolerance, and can in

many instances explain why Chomsky reacted to negative evidence by introducing additional stipulations and *ad hoc* auxiliary hypotheses, or by restricting the domain of the theory. Of special interest is the fact that (55)/(74) can explain in a large number of cases why Chomsky introduced a complication into his theory, that is, why he chose a version less highly valued in terms of (18)/(65) than another available version.

The other principles of § 7.2.3 all bear on Chomsky's handling of potential negative evidence - derived from both unexplained phenomena and unexplained counterexamples. In a sense these principles also represent further specifications of the principle (11)/(63).

As regards the question of conflict among the fundamental principles of theory appraisal listed in (62) - (70) above, all the actual cases discussed above took the form of a conflict between the principle of explanatory and predictive success with respect to specific grammars (11)/(63) and some other principle - specifically, the principle of restricted formal power (9)/(62), and the principle of simplicity (18)/(65). It was argued above that the question of how Chomsky resolves such conflicts can be reduced to a question about the conditions governing the adoption of a tolerant attitude towards negative evidence threatening a theory. Two conditions were identified that constrain the adoption of an attitude of epistemological tolerance in specific instances. These conditions are (54)/(73b) and (55)/(74). However, these conditions do not fully determine the decision to set aside negative evidence. As argued above, there is some evidence that an element of non-rule governed judgment is involved in Chomsky's decisions to set aside threatening evidence. It then follows that the resolution of conflict between the principle (11)/(62) and some other principles of theory appraisal is also not completely governed by rules, but is in part a matter of non-rule governed judgment.

Note that the formulation of some of the principles are such that the formulation itself reflects the fact that non-rule governed judgment is involved in the application of the principles. For instance, the stipulation in the principles (40b)/(71b), (47a)/(72b), and (54)/(73b) that the threatened theory must be highly valued in terms of the principles of theory appraisal listed in (62) - (70) is not precise. Neither the developmental history of binding theory nor Chomsky's metascientific comments provide any evidence that the conditions for applying the principles (40a)/(71), (46)/(72a) and (50)/(73a) can be made more precise. It was also suggested that the decision as to whether a gain of empirical success in one area compensates for a loss of success in another - see (74) - is not completely rule-governed.

There is some additional evidence not considered above that non-rule governed judgment plays a role in the theory choices made by Chomsky. In the introductory remarks to (Chomsky 1981a), Chomsky makes a distinction between "leading ideas" and "the execution of leading ideas". Chomsky's "leading ideas" include the notions of government, Case, and binding. He (1981a:1-2) comments as follows on the execution of leading ideas.

(75) "In applying these leading ideas, it is always necessary to make a number of empirical assumptions that are only partially motivated, at best. The leading ideas admit of quite a wide range of possibilities of execution . . . . Often I will make some decision for concreteness in order to proceed, though leading ideas may not be crucially at stake in such decisions."

Chomsky (1981a:2-3) further elaborates on the distinction between leading ideas and their execution. However, the remarks quoted above are sufficient to support the following claim: In the execution of a leading idea - for example, the notion of binding - Chomsky must make choices for concreteness, even though his choices cannot always be justified in terms of his principles of theory appraisal. To put it differently: Chomsky must in

some instances/ . . . .

some instances make choices, even though his "rules" for making theory choices provide no guidance.

During the developmental history of binding theory Chomsky did in some instances make choices such as those characterized above. One example is the choice between two definitions of the notion 'subject', a central notion in the SSC. Chomsky had to choose between the following two definitions of 'subject': (i) the subject of  $S$  is also the subject of  $\bar{S}$ , and (ii) the subject of  $S$  is not the subject of  $\bar{S}$ . In (Chomsky 1973) the latter definition was adopted, and in (Chomsky 1977c) the former. Of special interest is Chomsky's (1977c:130, fn. 37) comment that the choice was made "largely for expository reasons". That is, he did not justify his choice in terms of the usual considerations. Of course, it is not the case that there are in principle no considerations which could bear on such a choice. Chomsky himself mentions a consideration which bears on the choice between the two definitions of 'subject', namely, the scope of bridge conditions. However, as Chomsky's (1977c:130, fn. 38) comments on the notion 'subject' underline, at that stage there were no clear grounds available for making a definite choice.

Chomsky's (1977c:85) discussion of the problem which COMP-COMP *wh*-Movement poses for the SSC and PIC provides another illustration of a choice made in the absence of the usual justification. Chomsky considered two solutions to the problem, both of which involve a language-specific mechanism: a complex language specific COMP-COMP movement rule, or a language specific proviso in the SSC and PIC. The details of these possible solutions are presented in § 4.4.5 above. Chomsky (1977c) chose the second solution, that is, the language specific proviso. The following comments by Chomsky (1977c:85) support the view that this choice was not justified in terms of any principle of theory appraisal, but was in fact a matter of non-rule governed judgment.

- (76) "Which of these approaches (that is, the approach in terms of the language-specific COMP-COMP movement rule and the approach in terms of the language-specific proviso in the SSC and PIC - M.S.) is preferable is unclear. I will assume the latter, without much reason."

It was pointed out in § 6.4.2 above that some of the differences among the Pisa GB binding theory, the MCG GB binding theory, and the LGB GB binding theory have no empirical and conceptual consequences. Chomsky's choice of the MCG GB binding theory in preference to the Pisa GB binding theory, and his choice of the LGB GB binding theory in preference to the MCG GB binding theory, were thus not justified in full. In part then, these choices must be analyzed as instances in which Chomsky had to make non-rule governed choices in connection with the execution of one of his leading ideas, namely, binding theory.

Given that non-rule governed judgment plays a role in Chomsky's theory choices, the principles of theory appraisal presented in (62) - (74) do not constitute an algorithm admitting of mechanical application. Nevertheless, these principles can provide minirat accounts for the theory choices made by Chomsky during the developmental history of binding theory. That is, if one assumes that Chomsky does indeed adhere to these principles of theory appraisals, and that they are related to his aim of discovering the truth in the manner outlined in §§ 7.2.2 and 7.2.3, then one can explain why Chomsky made the various choices described in chapters 3 - 6. In terms of the principles of theory appraisal set out above, Chomsky made the "best choice" in each case. Non-rule governed judgment enters the account in cases where the principles do not clearly identify a "best choice". In such cases Chomsky made a choice on the basis of non-rule governed judgment.

The model of Chomsky's rationality developed above is based on the assumption that theory appraisal is in fact a matter of theory comparison. Each of the fundamental principles listed in

(62) - (70) above, as well as (74), specifies a factor in terms of which the merit of one version of the theory *relative to* another version can be determined. The comparative nature of the model developed above is a natural consequence of the aim of the present study, namely, to determine what considerations guided Chomsky in choosing among different versions of binding theory. However, the comparative nature of the model also has an explanatory function. In § 6.3.3.2 above the question was raised when the presence of some stipulation in a linguistic theory gives rise to a conceptual problem - a problem of insufficient deductive depth and/or complexity. It was argued in § 6.3.3.2 that it is only when an alternative version of the theory is available which manages to avoid the need for this stipulation that it can be determined that the stipulation gives rise to a genuine conceptual problem. Note that this feature is captured directly by the principles of theory appraisal formulated above. In terms of these principles, the choice is always between a version of the theory which incorporates a stipulation *S* and an alternative version which does not require *S*.

During the discussions in §§ 7.2.2 and 7.2.3 brief mention was made of Chomsky's use of rhetorical tricks in presenting his theory choices. This issue will be taken up in more detail in §§ 7.3 and 7.4 below.

To conclude this exposition of what constitutes rationality in Chomsky's linguistics, let us turn to a question raised in § 2.4 above, namely, to what extent the method employed by Chomsky during the developmental history of binding theory instantiates "the (lax) Galilean style of inquiry". The latter style of inquiry, as defined by Botha (1982a:42), has four features. These features were presented in 2.(17) above, and are repeated here as (77) for ease of reference.

- (77) a. To make progress in the scientific study of language (and mind) we should set, as the fundamental aim of

inquiry/ . . .

inquiry, depth of understanding in restricted areas  
- and not gross coverage of data.

- b. To get serious inquiry started, we should make radical abstractions and idealizations in defining the initial scope of the inquiry.
- c. To capture the desired understanding or insight, we need unifying, principled theories deductively removed (perhaps far removed) from the primary problematic data.
- d. To keep up the momentum of the inquiry, we should adopt an attitude of epistemological tolerance towards promising theories that are threatened by still unexplained or apparently negative data.

The developmental history of binding theory, as described above, exhibits all four these features. § 7.2.3.6 contains a detailed analysis of Chomsky's epistemological tolerance. This discussion makes it clear that since 1973 epistemological tolerance formed an important feature of Chomsky's approach towards the development of binding theory. As regards (77c), many theory changes discussed above were justified in terms of greater unifyingness and increased deductive depth. The list of theory changes in (22) shows that a concern for unifying, deductively deep theories played a particularly prominent role in the development of the OB binding theory and the various versions of the GB binding theory. The fundamentality of these changes underlines the weight which Chomsky assigns to considerations of unifyingness and deductive depth in theory appraisal. The change listed in (22a) - the reinterpretation of the SSC and TSC/PIC to restrict rules of construal only - indicates that a concern for deductive depth also determined some of the earlier choices made by Chomsky.

The role/ . . .

The role which the idealizations of sentence grammar and core grammar played in determining the scope of UG, and of binding theory, in particular, instantiates the feature (77b). Details of these idealizations are presented in § 7.2.3.5. Taken in its entirety, the developmental history of binding theory can best be understood as an attempt to attain a certain depth of understanding in a restricted domain, rather than gross coverage of data. That is, Chomsky's aim in developing binding theory is similar to the aim specified in (77a).

However, not all the theory choices made by Chomsky during the developmental history of binding theory can be regarded as instantiations of "the lax Galilean style of inquiry". Several of the choices made by Chomsky should be explained in terms of a desire to "cover the data". Consider in this connection the introduction of special stipulations, auxiliary hypotheses, and so on, to overcome negative evidence threatening his theory, and the careful attention paid by Chomsky to data not yet fully explained by his theory. This feature of the developmental history of binding theory provides additional support for a point made by Botha (1982a:42-43) in connection with "the lax Galilean style of inquiry". Botha argues that while this style of inquiry is one of the major methodological tools of theoretical linguistics, it cannot be the sole methodological tool of theoretical linguistics. Botha's argument is that deep, unifying principles can be conceived of only in the event that there exist "things", typically empirical generalizations, to be unified. Allowance must thus be made for a mode of inquiry by means of which empirical generalizations can be established. The developmental history of binding theory shows that while Chomsky aims at developing a theory which meets the requirements incorporated in (77), he at the same time tries to establish empirical generalizations. It is interesting to note that, having established an empirical generalization - presented, for instance, in the form of a special stipulation added

to a theory which otherwise meets the requirements of the Galilean style of inquiry - Chomsky is still interested in reducing such stipulations to unifying, deep principles. Consider in this connection the elimination of the various complications in the theory discussed in §§ 7.2.3.3 and 7.2.3.4 above. Consider also the elimination of the need to incorporate the notion 'agentivity', discussed in § 6.5 above.

Adoption of the Galilean style of inquiry as an appropriate mode of inquiry thus does not rule out inquiry which leads to the establishment of empirical generalizations. It would in fact be wrong to interpret Chomsky's appeals to linguists to practise the Galilean style of inquiry in such a way that his appeals entail giving up inquiry which leads to the establishment of empirical generalizations. Chomsky's appeals should rather be interpreted as follows: The adoption of the Galilean style of inquiry is a *necessary*, but not *sufficient* methodological tool for progress in theoretical linguistics. The developmental history of binding theory shows that Chomsky himself does not exclusively practise the (lax) Galilean style of inquiry. He also practises a mode of inquiry which leads to the establishment of empirical generalizations, generalizations which must ultimately be reduced to unifying, deep principles, in accordance with the requirements of the Galilean style of inquiry.

### 7.3 Some problems with Chomsky's methodological beliefs

In § 7.2 a model of what constitutes rationality in Chomsky's linguistics was presented, a model which can provide a minimal rational account of the developmental history of binding theory. In accordance with Newton-Smith's concept of a minirat account of theory choice/change, § 7.2 does not contain a critical appraisal of the theory choices made by Chomsky, nor a critical appraisal of his beliefs regarding the goal of science and the factors which ought to guide theory choices. In §§ 7.3 and 7.4 the focus shifts from a non-critical description of Chomsky's

rationality towards a critical appraisal of Chomsky's rationality.

The issue in § 7.3 is whether Chomsky's beliefs about the goal of science and the principles which ought to guide theory choice - his methodological beliefs, for short - are minimally rational. In order to provide a minimally rational account of someone's beliefs, it must be shown that within the context those beliefs were justified.<sup>45)</sup> Or, as Newton-Smith (1981:254) puts it, someone is minimally rational in holding a certain belief if, by so doing, he is "following the dictates of reason". As argued in § 2.2 above, in the case of a contemporary scientist an appraisal of the reasonableness or unreasonableness of his methodological beliefs cannot be made without reference to an adequate contemporary model of scientific rationality. In § 7.4 below Chomsky's rationality, and specifically his beliefs about the goal of science and the principles of theory appraisal, will be critically appraised with reference to two recent models of scientific rationality. These are the models of Laudan and of Newton-Smith.<sup>46)</sup> The present section, § 7.3, contains only a very limited appraisal of the rationality of Chomsky's methodological beliefs on the basis of general norms which are not specific to any particular model of scientific rationality. Questions are raised below concerning inconsistencies in the set of beliefs held by Chomsky, his grounds for holding certain beliefs, and the exact content of certain beliefs.

A first set of problems with Chomsky's methodological beliefs arises in connection with his use of the distinction between an unmarked core and a marked periphery during the developmental history of binding theory.<sup>47)</sup> In recent expositions of his linguistic theory, Chomsky assigns a central role to the notions 'core', 'periphery', 'markedness', and the distinction between unmarked core and marked periphery. Consider for instance the centrality of these notions and distinction in Chomsky's (1981a: Chapter 1) exposition of the general structure of his linguistic theory. The distinction between unmarked core and marked peri-

phery/ . . .

phery not only forms a crucial part of Chomsky's accounts of the general structure of his linguistic theory. The developmental history of binding theory illustrates that this distinction also plays a role in highly specific theory choices made by Chomsky - see for instance the theory choices listed in (42) above. It appears then as if the notions 'core', 'periphery', 'markedness', and the distinction between unmarked core and marked periphery are quite central within Chomsky's linguistic theory. Given this central status of these notions and distinction, it is interesting to note that there is a certain tension between Chomsky's claims about the unmarked core and marked periphery, on the one hand, and his methodological beliefs, on the other. Botha's (1981:433) claims about the fundamental principles of Chomskyan generative grammar provide a useful basis for an explication of the nature of this tension.

Botha (1981:433) distinguishes four fundamental principles of Chomskyan generative grammar. One of these is the principle of *epistemological empiricism*, which stipulates that hypotheses must be testable in principle and justified in fact.<sup>48)</sup> There can be no doubt that epistemological empiricism is also a fundamental principle of Chomsky's linguistics.<sup>49)</sup> During the discussions above it was pointed out that Chomsky frequently comments on the importance of testing linguistic hypotheses, and on the need for empirical justification for such hypotheses. The analysis of the developmental history of binding theory provides strong support for the claim that Chomsky does in practice adhere to the principle of epistemological empiricism. This principle is, for instance, instantiated by the principle of theory appraisal (11).<sup>50)</sup> Explanatory and predictive success with respect to specific grammars was found to be one of the most significant factors guiding Chomsky in his theory choices.

The role of conceptual factors in Chomsky's theory appraisals in no way conflicts with the claim that epistemological empiricism is a fundamental principle of Chomsky's linguistics. The

developmental history of binding theory shows that for Chomsky conceptual factors *supplement* empirical factors in theory appraisal. Conceptual factors do not *replace* empirical factors. Moreover, as argued in § 7.2.3.3 above, the appropriateness of employing a specific conceptual factor in theory appraisal is itself open to empirical testing. The adoption of an attitude of epistemological tolerance by Chomsky also does not conflict with the principle of epistemological empiricism. As argued in § 7.2.3.6 above, the adoption of such an attitude does not entail that negative evidence becomes irrelevant in the appraisal of linguistic theories. Rather, epistemological tolerance is seen by Chomsky as a useful tool in the search for a successful theory, where one of the criteria for success is the ability of the theory "to fit the facts".

Chomsky's adherence to the principle of epistemological empiricism has an important consequence for his claim that there is a distinction between an unmarked core and a marked periphery, and more specifically, for his claims about the (un)markedness of particular rules or constructions. Markedness claims have the status of hypotheses. Unlike claims such as "This sentence is acceptable", or "This phrase is ambiguous", markedness claims are not based on the linguistic intuitions of native speakers. Given their hypothetical status, markedness claims must therefore be tested and justified, in accordance with the principle of epistemological tolerance.

It has been argued by, for example, Botha (1980:79-80) and Lightfoot (1979:77ff), that in order to test and justify claims about the (un)marked status of a structure *S* or a rule *R* the linguist must take into account external linguistic evidence. That is, claims about the (un)marked status of *S* and *R* must be tested and justified on the basis of evidence about the functioning of *S* and *R* in an account of such external phenomena and/or processes as non-idealized language acquisition, speech production and perception, language pathology, linguistic change, linguistic

variation, pidginization and creolization. Evidence from such sources is external in the following sense: In terms of the abstractions and idealizations employed by Chomsky in defining the aims of linguistic theory, it represents data about phenomena that fall outside the specific part of linguistic reality that Chomsky's theories initially have to account for.

In passages such as (Chomsky 1981a:9), Chomsky clearly assumes that markedness claims must be tested.<sup>51)</sup> As regards the role of external evidence in such testing, his position differs from that of Botha and Lightfoot. Chomsky (1981a:9) states that one would "hope" that such evidence would be "useful" in the testing of markedness claims.<sup>52)</sup> However, he stops short of conceding that such external linguistic evidence is *necessary* for the testing of markedness claims. Also, he claims that at present such evidence cannot provide much insight into the issue of whether a specific rule or construction belongs to the unmarked core or the marked periphery. Chomsky concludes that when faced with the problem of deciding how to delimit the domain of core grammar as distinct from marked periphery, the linguist is "therefore compelled to rely heavily on grammar-internal considerations and comparative evidence, that is, on the possibilities for constructing a reasonable theory of UG and considering its explanatory power in a variety of languages . . .".

The justification provided by Chomsky for the markedness claims he made in connection with binding theory illustrates this view. To support his claim that clause-external *wh*-Movement is a marked process, Chomsky referred to the fact that such movement is impossible in German and Russian. In an attempt to justify his claims about the (un)markedness of constructions with arguments in NPs, Chomsky referred to the distribution of the structures across different languages. Thus, Chomsky (1981d:141) stated that a certain construction "is surely the normal case in the languages of the world", and of another he states that it "perhaps represents a more general case across languages".

These claims are presented by Chomsky as providing support for the claim that the constructions in question are unmarked. Similarly, in support of a claim that some constructions are marked, Chomsky stated that such constructions "appear to be rare".

Evidence of the type referred to above does not fit very neatly into the internal-external distinction explicated above. While the comparative evidence referred to above is internal in the sense that it is derived from the linguistic intuitions of ideal speaker-hearers of natural languages, there is also an external element, namely, a comparison of the intuitions of speakers of different languages. None of the arguments below crucially depend on the exact status of comparative evidence of the type referred to above relative to the internal-external distinction. For the purpose of the present discussion, it will be assumed that such comparative evidence is internal linguistic evidence. If it should turn out that this assumption is wrong, the main points argued for below will remain unaffected.

Even if one does not insist, with Botha and Lightfoot, on the use of external linguistic evidence in the testing and justification of markedness claims, the kind of justification provided by Chomsky for his markedness claims is in itself problematic. It was pointed out in §§ 4.4.5 and 6.5 above that Chomsky's claims about the distribution of certain rules and constructions across different languages are in fact unsubstantiated. In the case of clause-external *wh*-Movement, Chomsky failed to cite any descriptively adequate analyses of German and Russian to support his claim about the marked status of this rule. In the case of the markedness claims made in connection with the GB governor binding theory, Chomsky did not even mention any specific languages. While Chomsky tried to create the impression that his markedness claims are justified, they are in fact without any justification. For this reason these markedness claims were analyzed as *ad hoc* auxiliary hypotheses introduced to protect Chomsky's theory.

It was argued above that Chomsky's rationality allows him to use *ad hoc* protective devices. There is no reason to assume that adherence to epistemological empiricism completely rules out in principle the use of *ad hoc* devices to protect one's theories from potential negative evidence. Both Laudan's and Newton-Smith's models of scientific rationality incorporate principle of epistemological empiricism in some form, while permitting the use of *ad hoc* protective devices.<sup>53)</sup> Viewed from this perspective, the unjustifiedness of the specific markedness claims made by Chomsky during the developmental history of binding theory does not constitute a serious problem for his rationality.

However, the markedness claims discussed above highlight another, serious, problem that exists in connection with Chomsky's markedness claims. Suppose that Chomsky's claims about the distribution of the various rules and constructions were substantiated. Would it then have followed that the markedness claims under discussion were justified (again not taking into account Botha's and Lightfoot's arguments for the necessity of employing external linguistic evidence in the testing and justification of markedness claims)? The answer to this question must be "no". At present it is not clear how comparative evidence is relevant for the appraisal of specific markedness claims. For instance, it is not clear what distribution pattern(s) is/are characteristic of a marked construction, and what of an unmarked construction. It is also not clear how many languages, and of what different language types, must be compared before claims may be made about the (un)marked status of a specific rule or construction. It is not only Chomsky's own work that fails to provide answers to such questions. Recent work done by other linguists on the distinction between unmarked core and marked periphery also fails to shed much light on the issues under discussion.<sup>54)</sup> The exact role of comparative evidence in the testing and justification of markedness claims is thus at present obscure.

The conclusion/ . . .

The conclusion that must be drawn in connection with the distinction between unmarked core and marked periphery within Chomsky's linguistics is clear. The distinction, as presently employed within Chomsky's linguistics, has no empirical justification, and, even worse, it is not clear what (internal) evidence could be used to test and justify claims about this distinction. Such a negative conclusion about the role of the distinction between unmarked core and marked periphery within Chomsky's linguistics is, of course, not new. What is new about the argumentation presented above, is that it shows that Chomsky's own explicitly stated views on the testing of markedness claims support this conclusion.

The current lack of clarity on what evidence could be used to test and justify markedness claims, and the consequent lack of justification for the distinction between unmarked core and marked periphery, conflict with the principle of epistemological empiricism. To put it differently: *It is not reasonable for Chomsky to adopt the principle of epistemological empiricism and at the same time to adopt the hypothesis that there is a distinction between an unmarked core and marked periphery in the grammar of a language, where the latter distinction is at present without justification, and even untestable.* The seriousness of this conflict within Chomsky's beliefs is magnified by the fact that the notions 'core', 'periphery', and 'markedness' interact with every principle of UG proposed by Chomsky. The current problems that exist in connection with the testing of claims about the distinction between unmarked core and marked periphery in effect carry over to Chomsky's theory as a whole. The analyses presented in §§ 4.4.5 and 6.5 above illustrate how the problems which exist in connection with the testing and justification of the relevant distinction affect the testing of proposed principles of UG, such as binding theory.

It could be argued in defence of the distinction between unmarked core and marked periphery that this distinction has facilitated

the development/ . . .

the development of a theory of UG with explanatory success and highly valued conceptual properties. That is, it could be argued that although the distinction has no direct justification, it is indirectly justified via the success of Chomsky's recent theories of UG. However, doubts can be raised about the validity of such a defence of the distinction between the unmarked core and the marked periphery. Recall that Chomsky adopts an attitude of epistemological tolerance, in terms of which it is appropriate to set aside negative evidence threatening a theory. Given Chomsky's adoption of this attitude, it is not at all clear that the distinction between unmarked core and marked periphery had a necessary role to play in the development of recent theories of UG. That is, there appears to be no reason why Chomsky could not simply have set aside the allegedly "marked" cases which provide negative evidence for his theory. Note that the principle of the preservation of empirical success (55), although it does constrain the adoption of a tolerant attitude towards negative evidence, does not constitute such a reason. As argued in §§ 7.2.3.7 and 7.4, Chomsky's principles of theory appraisal do not provide an algorithm for theory choice. In case of a conflict between two principles - including conflict between the principle of the preservation of empirical success (55) and some other principle - non-rule governed judgment will play a role in the resolution of the conflict. Within the overall context of Chomsky's rationality, there is no principled reason why such conflict should always be resolved in favour of the principle of the preservation of empirical success.

It was argued above - see § 6.5 above - that some of the markedness claims made by Chomsky during the developmental history of binding theory amount to no more than rhetorical tricks to disguise the fact that his theory is threatened by some negative evidence. Given the problems which currently exist in connection with the testing and justification of claims about distinction between unmarked core and marked periphery, it is not un-

reasonable to analyze this distinction itself as a rhetorical trick employed by Chomsky to disguise some of the empirical failings of his theory. Or, as Huybregts and Van Riemsdijk put it in (Chomsky 1982a:108):

- (78) "Reading the literature, one cannot escape the conclusion that notions such as markedness and periphery are being used as euphemistic terms to refer to phenomena that are not understood or do not fit into the core."

It is interesting to note that Chomsky's (1982a:108) response to Huybregts and van Riemsdijk's remark tends to support this rather negative conclusion about the current status of the relevant distinction, rather than to undermine it. Thus, Chomsky claims that he "just does not have any good ideas" about the structure of the "periphems" and the theory of markedness. As regards his (1981d) suggestion that in the periphery some of the conditions of core grammar may be relaxed, he states that he does "not really feel that there is any evidence". Chomsky (1982a:108) even questions the validity of the distinction between core and periphery.

- (79) "I do not even think it is clear whether we should make a sharp distinction between core and periphery. Maybe these are more closely related notions of some sort."

If one assumes that the principle of epistemological empiricism is too fundamental a principle of Chomsky's linguistics to be given up by him, then there are in essence two ways in which the tension between this principle and the current status of the distinction between unmarked core and marked periphery can be resolved. Firstly, Chomsky could give up this distinction. Phenomena previously claimed to belong to the marked periphery would then provide negative evidence for Chomsky's current UG, negative evidence towards which a tolerant attitude may be adopted. Eventually, UG would have to account for these phenomena, too. Note that this is in fact what happened with the development of the GB SUBJECT binding theory. This theory was

developed partly to explain the interpretation of arguments in NPs in constructions which had previously been claimed to be marked. Secondly, Chomsky could take the distinction between the unmarked core and marked periphery seriously. That is, he could try to solve the problems which exist in connection with the testing and justification of markedness claims, and to search for factual justification for such claims.

In spite of the current problematic status of the distinction between the unmarked core and the marked periphery, there is a good reason for Chomsky (and other Chomskyan linguists) to try and resolve the tension between this distinction and the principle of epistemological empiricism in the second way outlined above and not to precipitately abandon the distinction. If the distinction can be substantiated, then Chomsky's linguistics could benefit greatly. By working out this distinction, and developing a theory of markedness, new insight could be gained in the differences among languages. For instance, it could become possible to explain why certain types of constructions are rare in the languages of the world.<sup>55)</sup> The development of a theory of markedness could also yield new insight into external linguistic phenomena. Consider Botha's (1982a:35ff) argument in this connection.<sup>56)</sup> In view of the potential fertility of the distinction between the unmarked core and marked periphery, it would thus be wrong to insist on the immediate rejection of the distinction by Chomsky. Of course, it is possible that the distinction, while apparently fertile, will not bear fruit.<sup>57)</sup> If this should turn out to be the case, then Chomsky would eventually have to give up the distinction.

A second set of problems with Chomsky's methodological beliefs arises in connection with the role which considerations of simplicity play in theory appraisal in Chomsky's linguistics. The role which considerations of simplicity actually play in theory appraisal, and the roles which they ought (not) to play, have been the subject of great controversy within the philosophy of

science.<sup>58)</sup> No systematic and comprehensive attempt will be made here to analyze the role of considerations of simplicity in Chomsky's linguistics against the background of the various general metascientific analyses of considerations of simplicity in theory appraisal.<sup>59)</sup> Rather, I will only briefly discuss three aspects of Chomsky's use of considerations of simplicity in theory appraisal that are problematic within the context of recent views on simplicity in science.

The first problematic aspect arises in connection with Chomsky's reasons for regarding metatheoretical simplicity in the senses of (17) and (18) as relevant to the appraisal of linguistic theories. The clearest statement by Chomsky on why metatheoretical simplicity (in the senses of (17) and (18)) is relevant to the appraisal of linguistic theories, is in (Chomsky 1981a:339). The relevant remarks have been quoted in (19) above. As explained in § 7.2.2 above, Chomsky assumes that simplicity as a metascientific property of theories in some sense reflects simplicity as a property of the world. Of particular importance is Chomsky's remarks that "for some reason neural structures at least in this domain instantiate a perhaps surprisingly simple and unified system of principles". Chomsky (1982a:30) also makes a direct link between metatheoretical simplicity and simplicity as a property of the physical world. Note, however, that he (1982a:30) refers to "the brain", and not "neural structures". Suppose that, as suggested by these remarks, the assumption that neural structures/the brain underlying language are simple forms part of Chomsky's argument for employing metascientific considerations of simplicity (in the senses of (17) and (18)) in the appraisal of linguistic theories. A problem then arises. Chomsky presents his linguistic theories as theories which characterize the language faculty at an abstract level. Chomsky (1980a:197) identifies an important sense in which linguistic theories provide *abstract* characterizations of the language faculty. Linguistic theories consist of "abstract conditions that unknown mechanisms must meet". These theories do not describe "actual mechanisms"

functioning/ . . .

functioning in the brain. The relationship between the characterization at this abstract level and the more concrete level of the brain (and thus of neural structure) is far from clear. The following comments from recent works by Chomsky support this view. The italics are mine.

- (80) a. ". . . I . . . see no reason not to take our theories tentatively to be true at the level of description at which we are working, *then proceeding to refine and evaluate them and to relate them to other levels of description, hoping ultimately to find neural and biochemical systems with the properties expressed in these theories.*" {Chomsky 1980a:107}
- b. "So viewed, *linguistics is the abstract study of certain mechanisms, their growth and maturation. We may impute existence to the postulated structures at the initial, intermediate, and steady states in just the same way as we impute existence to a program that we believe to be somehow represented in a computer or that we postulate to account for the mental representation of a three-dimensional object in the visual field. Evidence bearing on empirical hypotheses such as these might derive from many and varied sources. Ultimately, we hope to find evidence concerning the physical mechanisms that realize the program . . .*" {Chomsky 1980a:188}
- c. ". . . *we are keeping to abstract conditions that unknown mechanisms must meet. We might go on to suggest actual mechanisms, but we know that it would be pointless to do so in the present stage of our ignorance concerning the functioning of the brain.*" {Chomsky 1980a:197}
- d. "What do we mean for example when we say that the brain really does have rules of grammar in it. We do not know exactly what we mean when we say that. We do not think there is a neuron that corresponds to 'move alpha'. So we are talking somehow about general structural properties of the brain, and there are real non-trivial questions about what it means to say that the brain, or any system, has general properties . . . I think there are really serious questions here that people should investigate, trying to get a better grasp of the notion of 'true statements' that attribute general abstract properties to complex systems." {Chomsky 1982a:32}

Note that Chomsky's views on the difficulties involved in relating

abstract linguistic theories to the brain/neural structure, are shared by other scholars.<sup>60</sup> Botha (1982b:34-35) mentions two scholars who express similar opinions: Colby (1978) and Marshall (1980). Thus, Colby (1978:177) claims that "the conceptual distance between symbolic rules and neurons is so great that it is difficult to propose how knowledge about one might contribute to knowledge about the other". Marshall (1980:125) states plainly that "we have no principled ideas about how language is coded by the brain".

The link which Chomsky draws between metatheoretical simplicity as a property of abstract linguistic theories and simplicity as a property of the brain/neural structure is clearly inconsistent with his views (and those of others) on the conceptual distance between the constructs of linguistic theory and the brain/neural structure. This inconsistency then creates a problem for Chomsky's methodological beliefs.

In addition to the problem raised by the "distance" between the abstract level characterized by linguistic theories and the concrete neurophysiological level of the brain, Chomsky's link between simplicity as a property of linguistic theories and simplicity as a property of the world faces another potential problem. As is evident from the discussion above, Chomsky's use of considerations of metatheoretical simplicity in theory appraisal is based on the (tentative) assumption that the world itself is simple. This assumption may be called "the thesis of ontological simplicity". Note that even where Chomsky does not link simplicity as a property of linguistic theory with simplicity as a property of neural structure, he explicitly assumes a link between metatheoretical simplicity of linguistic theory and simplicity in the world described by this theory. Thus he (1981a:14) directly links the elimination of redundancies in linguistic theory with the non-existence of redundancies in the language faculty.

. The thesis/ . . .

The thesis of ontological simplicity is quite controversial, and serious doubts have been expressed about its correctness. It is true that certain scientists, including for example Galileo and Einstein, claimed to have been guided by the belief that the world is simple.<sup>61)</sup> However, the thesis of ontological simplicity is by no means accepted by all scientists and philosophers of science.<sup>62)</sup> For instance, Bunge (1967a:283-4) claims that the ontological thesis that the world is simple "is refuted by the history of science, which shows that progress is, to a large extent, the discovery of complexities behind simple appearances". A similar view is expressed by Moravcsik (1980:28). Thus, he claims that "the more sophisticated and complex the underlying system of unobservables becomes in physics or chemistry, the more we seem to be able to account for". It will not do to react to criticism of the thesis of ontological simplicity by arguing that the success of recent simple theories in the natural sciences provides support for this thesis. The point is that it is not at all obvious that recent physical theories are simpler than older theories. Thus, Newton-Smith (1981:230) claims that "in so far as we have a grasp of the notion of relative simplicity, Quantum Mechanics looks more complicated than classical mechanics, and general relativity looks more complicated than Newtonian gravitational theory".

Within the philosophy of science the question of why science should be concerned with simple theories is also not always answered with reference to a property of the world described by scientific theories. The best-known alternative answer is probably that provided by Popper. Popper argues that scientists should seek simple theories, because greater simplicity leads to greater falsifiability.<sup>63)</sup>

The opinions quoted above do not establish that Chomsky's belief in the thesis of ontological simplicity, and his belief in the role of simplicity in the natural sciences are unreasonable, or wrong. What these opinions do establish is that

there are/ . . .

there are legitimate doubts which may be raised about the reasonableness of Chomsky's beliefs. A definite answer to the question of whether Chomsky's beliefs about the thesis of ontological simplicity and the role of simplicity in the natural sciences cannot be provided here. Such an answer must be based on an extensive analysis of current views on the role of simplicity in science. Such an analysis would form the topic of another full-scale study. For the purposes of the present study it is sufficient to note that there is a potential problem with the minimal rationality of Chomsky's beliefs about simplicity in science.

Chomsky's notion of metatheoretical simplicity also gives rise to problems. Chomsky's explicit comments on this issue do not throw much light on the precise content of the notion of metatheoretical simplicity which Chomsky uses in theory appraisal. In the reconstruction of Chomsky's views presented in § 7.2, two forms of metatheoretical simplicity which play a role in Chomsky's theory appraisals were distinguished: (i) A theory  $T_{x+1}$  is simpler than an alternative  $T_x$ , if  $T_{x+1}$  avoids a redundancy exhibited by  $T_x$  - see (17) above; (ii) A theory  $T_{x+1}$  is simpler than an alternative  $T_x$ , if  $T_{x+1}$  contains fewer stipulations than  $T_x$  - see (18) above. Two questions now arise in connection with Chomsky's notion of metatheoretical simplicity. First, is it the case that Chomsky believes that there are only two forms of metatheoretical simplicity which are relevant to the appraisal of linguistic theories, namely, the avoidance of redundancies and a restriction of the number of stipulations? Second, if these are indeed the only forms of metatheoretical simplicity which Chomsky believes should be used in the appraisal of linguistic theories, what grounds does he have for selecting these forms only, and excluding other forms of metatheoretical simplicity? Chomsky's metascientific comments on this work leave both these questions unanswered.

There are good reasons why these questions about Chomsky's use

of considerations of simplicity in theory appraisal should be answered. Within the philosophy of science various forms of metatheoretical simplicity are distinguished, and the inter-relationships among the different forms of metatheoretical simplicity are complicated. For instance, Bunge (1961:121) distinguishes four different forms of simplicity.<sup>64)</sup> *Syntactical simplicity* - or "economy of forms" - amongst other things depends on (i) the number and structure (that is, the degree) of the specific primitive concepts, and (ii) the number and structure of independent postulates. *Semantical simplicity* - or "economy of presuppositions" - depends on "the number of specifiers of meaning of the basic predicates". *Epistemological simplicity* - or "economy of transcendent terms" - depends on "closeness to sense-data". *Pragmatical simplicity* - or "economy of work" - depends on, amongst other things, psychological simplicity (that is, intelligibility), algorithmic simplicity (that is, ease of computation), experimental simplicity (that is, feasibility of design and interpretation of empirical tests). Bunge claims that the various forms of simplicity conflict with other desiderata for theories. For instance, simplicity conflicts with explanatory power, predictive power, and depth.<sup>65)</sup> Moreover, as Bunge (1961:122) points out, the various types of simplicity are not all compatible with one another. Greater simplicity in one form does not necessarily mean greater overall simplicity. This point can also be illustrated with reference to one type of simplicity, namely, syntactical simplicity. Syntactical simplicity is (in part) determined by the number and structure of the specific primitive concepts, and by the number and structure of independent postulates. Chomsky's notion of metatheoretical simplicity appears to be closely related to this notion of syntactical simplicity. Recall that for Chomsky too the number of stipulations determines the simplicity of a linguistic theory. Note, however, that a reduction in the number of independent postulates does not necessarily lead to greater overall syntactical simplicity. Such a reduction could be off-set by an increase in the complexity of the primitive concepts.<sup>66)</sup>

It is not only the complexities surrounding the notion of metatheoretical simplicity within the philosophy of science which underline the need for a clarification of Chomsky's views about metatheoretical simplicity, and his grounds for these views. Chomsky's own metascientific comments also underline this need. The following remarks by Chomsky (1972b:125), in which he plays down the role of simplicity in the appraisal of linguistic theories, apparently contrast with his recently expressed views on the importance of considerations of simplicity in theory appraisal. The italics are mine.

- (81) "A 'better theory', then, is one that specifies the class of possible grammars so narrowly that some procedure of choice or evaluation can select a descriptively adequate grammar for each language from this class, within reasonable conditions of time and access to data. *Given alternative linguistic theories that meet this condition, we might compare them in terms of general 'simplicity' or other metatheoretic notions, but it is unlikely that such considerations will have any more significance within linguistics than they do in any other field.*"

There are two possible interpretations for the apparent contrast between Chomsky's remarks quoted in (81) and his more recent comments on considerations of simplicity in theory appraisal. First, it is possible that the notion of metatheoretical simplicity used by Chomsky (1972b) does not crucially differ from the notion currently used by him. If this were the case, then one would have to conclude that Chomsky's views on the role of simplicity in the appraisal of linguistic theories have changed since the early seventies. The second possibility is that the notion of metatheoretical simplicity used by Chomsky (1972b) differs crucially from the notion currently used by him. In this case there would be no real conflict between the remarks quoted in (81) and his more recent comments on simplicity, and no change in his views on the issue. Either way, the apparent contrast between (81) and Chomsky's more recent comments highlights the need for a clarification by Chomsky of the exact content of the notion of metatheoretical simplicity

used by him, and of his grounds for adopting this notion.<sup>67)</sup>

The lack of clarity about Chomsky's grounds for pursuing meta-theoretical simplicity in the senses of (17) and (18) only, must at present be regarded as giving rise to potential problems for the minimal rationality of Chomsky's methodological beliefs, and not actual problems. The possibility cannot be ruled out that Chomsky could provide entirely satisfactory answers to the questions formulated above. If this should prove to be the case, then the problems identified above should properly be regarded not as problems with Chomsky's methodological beliefs, but as problems which exist in connection with his metascientific comments on his method. I return to this issue in § 7.5 below.

A third set of problems with the minimal rationality of Chomsky's methodological beliefs is connected with his notion 'unifiedness'.

In a passage quoted in (19) above, Chomsky (1981a:339) directly links unifiedness as a property of linguistic theory to unifiedness as a property of neural structure. As argued above, a similar connection made between simplicity as a property of linguistic theory and simplicity as a property of neural structure gives rise to a problem for Chomsky's beliefs about the role of considerations of simplicity in theory appraisal. The view that linguistic theory in some way reflects a general property of neural structure is inconsistent with Chomsky's views on the "distance" between the abstract level at which linguistic theory characterizes the language faculty and the concrete level of neural structure. The same inconsistency exists in connection with Chomsky's claims about the link between unifiedness as a property of linguistic theory and unifiedness as a property of neural structure. Given this inconsistency, it follows that the relevant beliefs held by Chomsky about unifiedness are not minimally rational.

The last problem with the minimal rationality of Chomsky's methodological beliefs to be considered here, concerns his use of the notion 'natural' in theory appraisal. As reconstructed in § 4.5 above, this notion - in so far as its content can be determined - bears on the relation between linguistic theory, as a theory of mental representations and mental computations, and theories of non-linguistic mental representations and computations. Given Chomsky's views on how linguistic theory should fit in within an overall theory of mind, no doubts can be raised about the reasonableness of Chomsky's belief that naturalness as principles of mental computation should be a factor in the appraisal of linguistic theories. However, there is a problem with Chomsky's current use of this factor in theory appraisal. There is an obvious necessary condition for a claim about the naturalness of a linguistic principle to be substantiated. It must be specified what other theories of mental representations and computations are used in determining the naturalness of the linguistic principle. Unless Chomsky could specify what other theories of mental representations and computations he uses in appraising the naturalness of linguistic principles, his claims about the naturalness of particular linguistic principles would be without content. Chomsky's recent works contain no specific references to non-linguistic theories of mind and non-linguistic principles of mental representation and computation which he employs in determining the naturalness of linguistic principles. It must then be concluded that at present Chomsky has no grounds for his claims about the naturalness of linguistic principles. A possible consequence of this conclusion will be considered in § 7.4 below.

Three actual problems which threaten the minimal rationality of Chomsky's methodological beliefs were identified above. These problems are listed in (82).

- (82) a. The current lack of clarity on the testing of claims about the distinction between unmarked core and marked

periphery, and the consequent unjustified status of Chomsky's claims in this connection, conflict with the principle of epistemological empiricism adopted by Chomsky.

- b. The link which Chomsky draws between, on the one hand, simplicity and unifiedness as properties of linguistic theories, and, on the other hand, simplicity and unifiedness as properties of neural structures/the brain, is in conflict with his views on the "distance" between the abstract level of the characterization provided by linguistic theory and the concrete level of neural structure/the brain.
- c. In the absence of any specification of what particular non-linguistic theories of mind are involved in determining the naturalness of linguistic principles it is not reasonable to employ a principle of theory appraisal based on the notion "naturalness as principles of mental computation" in appraising linguistic theories.

The first two problems take the form of an inconsistency within the total set of Chomsky's beliefs. In the case of the third problem, an obvious necessary condition for the reasonableness of actually applying a certain principle of theory appraisal is not met within Chomsky's work. In addition to the three actual problems listed in (82), three potential problems for the minimal rationality of Chomsky's methodological beliefs were also identified above. These potential problems all concern his beliefs about the role of simplicity in theory appraisal. The problem in each case is that, given the complexity of current theorizing on the role of simplicity in science, it is not clear exactly what Chomsky's beliefs are, or whether it is indeed reasonable at present to hold these beliefs. The three potential problems threatening the minimal rationality of Chomsky's methodological beliefs are listed in (83).

- (83) a. It is not clear that Chomsky's tentative adoption of the thesis of ontological simplicity, and his reliance on this thesis in justifying and explaining the pursuit of metatheoretical simplicity, are entirely reasonable.
- b. It is not clear what forms of metatheoretical simplicity Chomsky regards as relevant for the appraisal of linguistic theory.
- c. Suppose that, as suggested by the developmental history of binding theory, Chomsky believes that only two forms of metatheoretical simplicity - namely, the absence of redundancies and a limitation of the number of stipulations - are relevant for the appraisal of linguistic theories. It is then not clear why it would be reasonable to select only these two forms of metatheoretical simplicity.

The problems listed in (83) must be regarded as *potential* problems, rather than *actual* problems, since it cannot be determined without further investigation whether they really exist. In the case of (83a), a detailed analysis of current work on simplicity in science may show that it is at present entirely reasonable to hold the relevant beliefs. In the case of (83b, c), Chomsky may be able to clarify the exact content of his beliefs, as well as his grounds for holding these beliefs, in a satisfactory manner. That is, he may be able to show that his beliefs are in fact reasonable. In § 7.5 below I return to Chomsky's failure to comment explicitly and fully on his methodological beliefs.

#### 7.4 Chomsky's rationality and general models of rationality

One of the aims of the present study is to examine Chomsky's rationality relative to the general models of scientific

rationality/ . . .

rationality proposed by Laudan and by Newton-Smith. In § 2.3.6 a variety of issues related to the views of Laudan and Newton-Smith were isolated on which the developmental history of binding theory could, in principle, throw light. In the present section it is determined what light the developmental history of binding theory actually throws on these issues. The concepts and principles of Laudan's and Newton-Smith's models referred to below are explicated in § 2.3 above.

The fundamental question to be answered in § 7.4 is what conflicts, if any, there are between Chomsky's rationality, on the one hand, and the views on scientific rationality held by Laudan and Newton-Smith, on the other hand. Where such a conflict is found to exist, it must also be determined, as far as possible, what conclusions should be drawn on the basis of the conflict. For any conflict between Chomsky's rationality and a particular model of scientific rationality - MSR, for short - there are at least three possible interpretations that should be considered.

- (i) MSR contains an adequate characterization of the best method of science, and Chomsky fails to employ the best method.
- (ii) MSR contains an adequate characterization of the best method in the sciences in general, and Chomsky's employment of a different method is a reflection of the fact that linguistics differs in some principled respects from the other sciences.
- (iii) MSR is not an adequate characterization of scientific rationality, and the conflict between MSR and Chomsky's rationality is a result of this inadequacy.

(i) *Conceptual factors in theory choice*

Both Laudan and Newton-Smith emphasize the role which so-called

conceptual/ . . .

conceptual factors play in theory choice. In terms of the empirical-conceptual distinction adopted in § 2.3.4.1, the success of a theory in providing explanations for the facts in its domain and its success in making correct predictions about these facts, are empirical factors that feature in the justification of this theory. Conceptual factors bear on internal properties of the theory - such as internal inconsistencies and vagueness - and on the relation between this theory and other propositions/propositional systems adopted by the scientists involved. The latter include, amongst others, general principles of the research tradition to which the theory belongs and specific theories in other domains.

In terms of this empirical-conceptual distinction, the following factors which were found to have played a role in the justification of theory choices made during the developmental history of binding theory are conceptual in nature: (i) simplicity, (ii) unifying-ness (= generality), (iii) deductive depth, (iv) naturalness as principles of mental computation, (v) compatibility with the autonomy thesis, (vi) the elimination of internal inconsistencies. Within Chomsky's linguistics the desirability of greater simplicity (in the senses of (17) and (18)) in linguistic theory, and of greater unifying-ness and deductive depth in linguistic theory follows from the relation between specific theories of language proposed by Chomsky and certain general assumptions about the nature of the mind, and specifically the language faculty, made by him. Naturalness concerns the relation between a specific theory of the language faculty and specific theories of other, non-linguistic, components of the human mind. The fifth conceptual factor mentioned above concerns the relation between a specific theory of the language faculty and an assumption made by Chomsky about the relation among some of the components of this faculty. In contrast to the first five factors, which are all external in Laudan's (1977:49) sense, the sixth factor - the elimination of internal contradictions - is an internal conceptual factor.

In chapters/ . . .

In chapters 3 - 6, and in § 7.2.2, textual evidence was presented that the empirical-conceptual distinction adopted by Chomsky is identical to the distinction between empirical and conceptual factors adopted in § 2.3.4.1 above. Consider in this connection the fact that Chomsky labels the problems of the OB theory which arise from lack of simplicity and lack of deductive depth as "conceptual".<sup>68)</sup> These problems are contrasted with empirical inadequacies of the theory, which arise from failure of the theory to explain certain linguistic facts and to make the correct predictions about such facts.

It can then be concluded that the developmental history of binding theory, and Chomsky's linguistics in general, provide support for Laudan's and Newton-Smith's views on the importance of conceptual factors in theory choice. Any account of the various theory choices made during the developmental history of binding theory based solely on the success achieved by the theory in providing explanations for and making correct predictions about the facts in its domain would leave many important aspects of this history unexplained. An example of a feature that would be left unexplained on such an account would be the replacement of the OB binding theory by the GB governor binding theory.

In making his empirical-conceptual distinction, Laudan (1977:48) stresses that there is a continuum between empirical and conceptual problems. It is necessary to keep in mind that by classifying the six factors listed above as being conceptual in nature - in order to distinguish them from the explanatory and predictive success of the theory - it is not claimed that these conceptual factors are entirely independent from empirical considerations. For instance, as explained in § 7.2.2 above, the desirability of increasing the deductive depth of linguistic theory is, in Chomsky's work, connected with an (ontological) assumption made by Chomsky about the nature of the world. It is this relation which makes deductive depth a conceptual factor. However, increasing the deductive depth of a theory also results

in the/ . . .

in the explanation of a fact previously left unexplained. For this reason increasing the deductive depth of a theory is also in part an empirical consideration. Any increase in the deductive depth of a theory leads to an increase in the theoretical success of the theory. If there is no absolute observational-theoretical distinction, then theoretical success must be regarded as part of empirical success.<sup>69)</sup> The consideration of increased deductive depth is then partly conceptual, and partly empirical. It was argued in § 7.2.2, the consideration of restricted formal nature is also in part empirical, and in part conceptual.

While it can be argued that some of the conceptual factors discussed above also have an empirical aspect, there is a general principle of Chomsky's rationality that also links such conceptual factors to the empirical success - that is, the explanatory and predictive success - of linguistic theories. As explained in § 7.2.3.3 above, the use of factors such as simplicity (in the senses of (17) and (18)) and deductive depth, in the justification of theory choices is subject to some form of empirical test. Each such factor must contribute towards the development of a linguistic theory with increased empirical success - that is, increased explanatory and predictive success. This link between the conceptual factors discussed above and the empirical success of linguistic theory throws some light on a difference between Laudan's and Newton-Smith's models of scientific rationality.

(ii) *The status of conceptual factors*

As explained in § 2.3.4.4, Laudan's and Newton-Smith's models differ on the issue of why conceptual factors should play a role in theory choice. For Newton-Smith, success in avoiding conceptual difficulties is relevant to theory choice because such success is indicative of likely long-term observational (= empirical) success. If a specific conceptual factor is not

linked/ . . .

linked to increased observational success, then it should not feature in theory appraisal. Given the link which Newton-Smith claims to have established between observational success and verisimilitude, he can thus justify the role of conceptual factors in theory choice by reference to the truth-directedness of science.

For Laudan, in contrast, the elimination of conceptual difficulties is in itself a goal of science. For him, success in avoiding conceptual problems is constitutive of a good theory. No link is made between success in solving conceptual problems and success in solving empirical problems. In particular, Laudan does not justify the elimination of conceptual problems on the grounds that this would, in the long term, lead to increases in the ability of the theory to solve empirical problems. In contrast with Newton-Smith, Laudan (1977:123) also explicitly denies any connection between problem solving ability and truth.

The status of conceptual factors within Chomsky's linguistics is similar to the status assigned to them in Newton-Smith's model. As reconstructed in § 7.2.2, Chomsky also links the use of conceptual factors in theory appraisal with the search for truth. Moreover, there is also a direct link between conceptual factors in theory appraisal and the empirical success of linguistic theory. The empirical success of linguistic theory provides a test for the appropriateness of employing specific conceptual factors in the appraisal of linguistic theories. That is, the use of conceptual factors in theory choice is directly linked to increased empirical success. On the question of the status of conceptual factors that feature in theory choice, Chomsky's linguistics is then in conflict with Laudan's model.

It is interesting to note in this connection that one of the points of criticism raised against Laudan's model is that he cannot explain the relevance of conceptual problems in theory appraisal. It is in fact claimed that such an explanation

requires/ . . .

requires reference to truth, which would lead to an inconsistency in Laudan's model.<sup>70)</sup> It is then reasonable to assume that the conflict between Chomsky's rationality and Laudan's model of scientific rationality regarding the status of conceptual factors in theory choice results from a shortcoming of Laudan's model.

(iii) *Conflict between empirical factors and conceptual factors*

Neither Laudan's nor Newton-Smith's models specifies that conflict between empirical factors and conceptual factors in theory appraisal should be resolved in one direction only. That is, both models allow for the possibility that such conflict could in some cases be resolved in favour of empirical factors, and in other cases in favour of conceptual factors.

The developmental history of binding theory does not provide a clear answer to the question of how conflict between empirical and conceptual factors is resolved in Chomsky's linguistics. In particular, the evidence that conflict may be resolved in favour of conceptual factors is not clear-cut. In § 7.2.3.3 a number of cases were discussed in which the conceptual factor of simplicity (in the sense of (18)) conflicts with empirical success. With the exception of the replacement of the OB indexing theory by the GB indexing theory - see (34f) - Chomsky resolved these conflicts in favour of empirical success. The replacement of the OB indexing theory by the GB indexing theory is, however, not a clear case in which Chomsky resolved a conflict in favour of a conceptual consideration. As explained in § 7.2.3.3 above, Chomsky tried to argue that this change involved not only a loss of empirical success, but also a gain of empirical success.

In the case of the replacement of the OB binding theory by the GB governor binding theory, there is also a conflict between conceptual factors, on the one hand, and empirical factors, on the

other/ . . .

other hand. Conceptually the GB governor binding theory is in several respects better than the OB binding theory. However, the GB governor binding theory has less empirical success than the OB binding theory in accounting for the binding of NPs within NPs. The fact that Chomsky chose to replace the OB binding theory by the GB governor binding theory, apparently indicates that he resolved this particular conflict between empirical and conceptual factors in favour of conceptual factors.

Chomsky's (1981d) claims that those cases about which the GB governor binding theory makes the wrong predictions are marked, complicates the interpretation of the change from the OB binding theory to the GB governor binding theory. By making these markedness claims, Chomsky in effect denied that there was a genuine conflict between empirical and conceptual factors in the replacement of the OB binding theory by the GB governor binding theory. As argued in §§ 4.4.5 and 6.5 above, Chomsky's markedness claims had no justification. Moreover, Chomsky later admitted that they were unjustified, and that he made the claims only because the GB governor binding theory could not account for the cases in question. It was consequently argued in § 6.5 that these markedness claims must be seen as mere rhetorical tricks used by Chomsky to disguise the empirical problems of the GB governor binding theory. Given this analysis of Chomsky's (1981d) markedness claims, the replacement of the OB binding theory by the GB governor binding theory must then be seen as an instance in which conceptual factors outweighed empirical success. That is, in this case Chomsky resolved the conflict in favour of conceptual factors.

With the development of the GB SUBJECT binding theory, Chomsky managed to combine the empirical success of the OB binding theory with the conceptual success of the GB governor binding theory. An interesting, but apparently unanswerable, question that arises at this point is what would have happened if Chomsky should have failed to develop a new version of binding theory

which/ . . .

which combined the empirical success of the OB binding theory with the conceptual merits of the GB governor binding theory.

The developmental history of binding theory thus provides some evidence that conflicts between empirical factors and conceptual factors in theory appraisal are not always resolved in favour of empirical factors. Recall also that it was argued in § 7.2.4 above that, in general, there are no precise rules for the resolution of conflicts among the various principles of theory appraisal used by Chomsky. The resolution of such conflicts is in part a matter of non-rule governed judgment. The finding that conflicts between empirical and conceptual factors are not resolved in one direction only, fits in very well with the claim that non-rule governed judgment plays a role in theory appraisal.

(iv) *Principles belonging to Chomsky's research tradition*

With two exceptions, all the conceptual factors that played a role in the various theory choices made during the developmental history of binding theory bear on the relation between a specific version of UG and a general principle belonging to Chomsky's research tradition/general theory.<sup>71)</sup> The exceptions are naturalness - which concerns the relation between specific theories with different domains - and the elimination of inconsistencies - which bear on an internal property of the theory.

Both Laudan's and Newton-Smith's models of scientific rationality make provision for this type of consideration in theory appraisal. For Laudan the general principles in question must be analyzed as belonging to the ontological component of Chomsky's research tradition. Within Newton-Smith's model these general principles must be analyzed as metaphysical principles with specific content. However, Laudan and Newton-Smith differ in the emphasis which they place on conceptual considerations of this sort. Newton-Smith does not single out this type of conceptual consideration as being of particular importance. In contrast,

Laudan/ . . .

Laudan (1977:88) claims that the majority of the conceptual problems which a specific theory may face will be generated by tension between this theory and its associated research tradition. As the analysis of the developmental history of binding theory presented above shows, the majority of the conceptual problems faced by binding theory were generated by conflict with principles of Chomsky's general theory of language. This history thus provides support for Laudan's view on the importance of this class of conceptual factors in the appraisal of specific theories.

Laudan, as opposed to Newton-Smith, also makes explicit provision for the methodological rules, or principles, of a research tradition to generate conceptual problems that play a role in theory choice. According to Laudan (1977:58), methodological rules offer norms for scientific behaviour. That is, these rules tell us "what we should, or should not, do in order to achieve the cognitive, epistemic, and practical goals of the scientific enterprise". Clearly, the various principles of theory appraisal proposed in § 7.2 constitute methodological rules of Chomsky's linguistics. To claim that conflict between a specific theory and the norms contained in these principles of theory appraisal create a conceptual problem for the theory, amounts to an uninteresting tautological claim.

However, Laudan's claim about the role of methodological rules in science cannot be completely reduced to this empty claim. Laudan's methodological rules comprise more than principles of theory appraisal in the sense of § 7.2. According to Laudan (1977:79), the methodological rules (or principles) "will be wide-ranging in scope, addressing themselves to experimental techniques, modes of theoretical testing and evaluation, and the like".

The developmental history of binding theory provides some evidence that methodological rules, in this more general sense, do influence the appraisal of theories within Chomsky's linguistics.

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Consider in this connection Chomsky's rejection of "unanalyzed phenomena" as providing negative evidence for UG. Chomsky's view that only descriptively adequate analyses of specific languages can provide such evidence represents a methodological rule, in Laudan's sense. The current debate about the testing and justification of markedness claims also underlines the influence of methodological principles on theory appraisal within Chomsky's linguistics. Chomsky's participation in this debate shows that he himself recognizes the role of methodological norms in the appraisal of linguistic theories. Consider also Chomsky's recent comments on the need for linguists to adopt the "Galilean style" of inquiry, and in particular his claim that explanatory depth, rather than gross coverage of data, is important in the appraisal of linguistic theories.

The focus in the present study was on successive versions of binding theory, where each version was proposed by Chomsky at some point. More extensive evidence for the role of methodological rules in the appraisal of linguistic theories may emerge from a study with a less restricted data base. In particular, a detailed examination of cases in which the choice is between some version of Chomsky's theory and an alternative theory proposed by other linguists may provide such evidence. The difference of opinion between Chomsky and Lasnik (1978:272), on the one hand, and Postal and Pullum (1978), on the other hand, about the effect which the employment of *ad hoc* mechanisms has on the merit of trace theory, is a case in point. This difference of opinion can be attributed to the fact that Chomsky and Lasnik's methodological norms allow for the use of *ad hoc* protective devices, while Postal and Pullum's norms prohibit this. For Postal and Pullum, the presence of an *ad hoc* device in a theory would then generate a serious methodological conceptual problem for this theory, a problem which detracts from the merit of the theory. For Chomsky and Lasnik the presence of an *ad hoc* protective device in a theory does not give rise to such a conceptual problem. Consequently, Postal and Pullum's appraisal of a theory which contains an *ad hoc* protective device would differ

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from Chomsky's (and Lasnik's) appraisal of the same theory, and this difference would be directly attributable to different methodological norms adhered to by them.

Unlike Laudan, Newton-Smith does not explicitly make provision for methodological difficulties as a factor in theory appraisal. However, one cannot conclude that Newton-Smith makes no provision for such difficulties. He (1981:208) states that "a full discussion" of scientific method "would have to cover a multitude of topics, including the design of experiments, the theory of measurement and the role of mathematics in science". The discussion in chapter 9 of his book - in which Newton-Smith proposes his model of theory appraisal - is "largely restricted to the question of the possibility of giving an abstract characterization of the factors that ought to guide theory choice". Newton-Smith thus does not rule out disputes about "experimental techniques, modes of theoretical testing and evaluation" within science. Neither does he rule out the possibility that such disputes could affect the appraisal of scientific theories. Although the issue is admittedly not completely clear, it seems unlikely that Laudan's and Newton-Smith's models make different claims about the relevance of methodological norms for theory appraisal.

(v) *The preservation of empirical success*

It was argued above - see § 7.2.3.6 - that a principle of the preservation of empirical success guides theory choices in Chomsky's linguistics. In terms of this principle - see (55) above - a version  $T_{x+1}$  of a theory must preserve the explanatory and predictive success of earlier versions,  $T_x$ , unless  $T_{x+1}$  has explanatory and predictive success in a new area to compensate for the loss of success in another area. This principle of the preservation of empirical success is similar to Newton-Smith's notion of observational nesting, which he proposed as one of the goodmaking features of theories.

Laudan (1977:6) explicitly rejects the principle that theories should preserve the empirical success of their predecessors. In terms of Laudan's criterion for determining problem-solving effectiveness, a theory  $T_{x+1}$  could leave unsolved an empirical problem solved by an earlier variant,  $T_x$ , if the additional empirical and/or conceptual problems solved by  $T_{x+1}$  (but not  $T_x$ ) outweigh the empirical problem left unsolved. It appears then as if, on the issue of the preservation of empirical success, Laudan's model is in conflict with Chomsky's rationality.

However, it is not obvious that the developmental history of binding theory provides evidence of a real conflict between Laudan's model of scientific rationality and Chomsky's rationality as regards the preservation of empirical success. It is true that the principle of the preservation of empirical success played an important role in the development of binding theory. For instance, it was argued in § 7.2.3.6 above that this principle restricts the adoption of a tolerant attitude in cases where a theory is threatened by negative evidence. The principle of the preservation of empirical success does not, however, outweigh all the other principles of theory appraisal adopted by Chomsky. That is, in case of a conflict between the principle of the preservation of empirical success and another principle of theory appraisal - for instance, the principle of greater simplicity (18) or the principle of greater deductive depth (21) - the conflict will not necessarily be resolved in favour of the principle of the preservation of empirical success. As argued in § 7.2.4, the resolution of conflict between two principles of theory appraisal is in part a matter of non-rule governed judgment. Given that the markedness claims which Chomsky (1981d) made in connection with the GB governor binding theory are mere rhetorical tricks, then the change from the OB binding theory to the GB governor binding theory is a case in which the empirical success of the older version was not preserved.

Note that/ . . .

Note that Newton-Smith's model also does not entail that empirical success will always be preserved. Observational nesting - which incorporates the notion that theories must preserve the empirical success of their predecessors - is only one of the factors which play a role in theory appraisal. In case of a conflict between observational nesting and some other feature, the conflict may be resolved in favour of the latter.

In sum, then, it is not obvious that Laudan's and Newton-Smith's models do actually make conflicting claims on the issue of the preservation of empirical success. Both models are apparently compatible with the role of the principle of the preservation of empirical success (55) in Chomsky's linguistics: Although (55) plays a significant role in determining the theory choices made by Chomsky, this principle is not exceptionless. In particular, in certain instances some of the empirical success of a version of the theory is "sacrificed" for better conceptual properties.

(vi) *Ad hoc protective devices*

Both Laudan and Newton-Smith allow for the use of *ad hoc* devices for protecting a theory from potential negative evidence. As argued in § 7.2.3.4, the use of such *ad hoc* protective devices is also permitted within Chomsky's linguistics. On this issue, then, Chomsky's rationality fits in with the models of scientific rationality proposed by Laudan and Newton-Smith. It is interesting to note that the use of *ad hoc* protective devices is one of the points on which Chomsky's linguistics has in the past been criticized from the conventional falsificationist perspective. The existence of Laudan's and Newton-Smith's models - in which the assumption that science is rational is reconciled with the use of *ad hoc* protective mechanisms - thus provides us with a new perspective of Chomsky's use of *ad hoc* protective devices.

It was/ . . .

It was pointed out in § 2.3.4.8 that there is one difference between Laudan's and Newton-Smith's views on the employment of special protective devices. While there is nothing in Laudan's model to prevent the proliferation of such devices, Newton-Smith's model does contain a principle which rules out the proliferation of such devices. One of the factors to be used in theory appraisal, according to Newton-Smith, is smoothness. The smoothness of a theory is determined by the number of independent auxiliary hypotheses used to protect it. The greater the number of independent auxiliary hypotheses used to protect a theory, the less smooth the theory would be. The use of a number of auxiliary hypotheses to overcome empirical failures would thus adversely affect the smoothness of the theory. The more the failures of a theory can be shown to be systematic, and to fall under the same principle, the smoother the theory.

While the evidence is not clear, it nevertheless seems as if Chomsky is also anxious to show that different failures of his theory can be covered under a single (or a limited number of) principles. Consider in this connection his use of the distinction between sentence grammar and non-sentence grammar, and the distinction between core grammar and the periphery. This suggests that Chomsky also values smoothness in a theory. The value apparently placed by Chomsky on smoothness is clearly closely related to his principle of simplicity (18), which stipulates that the fewer the principles of a theory, the better the theory.

Even if a firm conclusion could be reached that Chomsky does indeed value smoothness, this would not necessarily mean that his rationality is in conflict with Laudan's model. It could be argued that the value which Chomsky places on smoothness is the result of a methodological principle adopted by Chomsky, that is, a methodological principle which belongs to Chomsky's research tradition. Recall that Laudan's model makes provision for methodological principles which belong to individual research traditions. What is unclear, is how Laudan can exclude the pos-

sibility of such specific methodological principles circumventing the general principles of his model. Suppose that a methodological principle which stipulates that theories must have the property of smoothness belongs to individual research traditions, and that this principle in effect circumvents the principle of Laudan's model which permits the use of large numbers of *ad hoc* auxiliary hypotheses. The question then arises how this general claim of Laudan's about *ad hoc* auxiliary hypotheses can be tested. The possibility that specific methodological principles may circumvent the general principles of Laudan's model, represents a serious weakness of the latter model. Given this possibility, Laudan's model actually makes only very weak claims about theory appraisal, contrary to Laudan's intention. It is interesting to consider in this connection Feyerabend's (1981:70) criticism of Laudan's model. Feyerabend refers to the fact that the "rules" of Laudan's model may be circumvented, and claims that as a consequence this model is trivial.

(vii) *Simplicity*

Metatheoretical simplicity - in the senses of (17) and (18) - plays a role in the theory choices made by Chomsky. As explained in § 7.2.2.3, Chomsky's use of metatheoretical simplicity in theory appraisal is based on the assumption that that part of the world described by linguistic theory is simple.

Laudan's model does not explicitly make provision for, or rule out, the use of simplicity in theory appraisal. Clearly, however, the role of simplicity in theory appraisal in Chomsky's linguistics is consistent with Laudan's model. On Laudan's model, considerations of increased metatheoretical simplicity could play a role in theory appraisal either on the basis of an ontological principle regarding the simplicity of the world (as in the present case) or on the basis of a methodological principle which places a value on metatheoretical simplicity without linking this to simplicity as a property of the world.

Newton-Smith's views on simplicity in theory appraisal are different from Laudan's. He denies that simplicity is an indicator of likely long-term observational success (that is, empirical success). Consequently, simplicity should not be employed in theory appraisal. Newton-Smith also states that there is at present no successful criterion for measuring the relative simplicity of theories. He (1981:231) concludes his discussion of simplicity with the following remarks.

- (84) "This does not mean that we should not continue to opt for simplicity given the choice in contexts in which the notion has hard content. The case for simplicity is pragmatic. It simply is easier to calculate with simpler theories. But there is no reason to see greater relative simplicity of this sort as an indicator of greater verisimilitude."

Superficially, the role of simplicity in theory appraisal in Chomsky's linguistics is in conflict with Newton-Smith's model. However, closer examination reveals that there is no genuine conflict. First, the notion of metatheoretical simplicity employed by Chomsky has a specific and restricted content, and is not equivalent to the notion of (overall) formal simplicity considered by Newton-Smith. Second, the content of the metatheoretical notion simplicity employed by Chomsky can be made quite precise. At least in the case of the theory choices discussed above, it was possible to decide on the relative simplicity - in the sense of (17) and (18) - of the different versions of the theory. Third, Chomsky's use of his restricted metatheoretical notion of simplicity is linked to his adoption of a general assumption about the nature of the world - in Newton-Smith's terminology, a metaphysical principle. The adoption of such a principle, and the consequent employment of a metatheoretical notion of simplicity which has a precise and restricted content, seem to be compatible with Newton-Smith's exclusion of overall formal simplicity as a good-making feature of theories.

It must be kept in mind that, apart from the question of compatibility between Chomsky's views on simplicity, and Laudan's and

Newton-Smith's views on this issue, there are problems with Chomsky's beliefs about the role of simplicity in linguistics. In § 7.3 above it was argued that it is doubtful whether a minimal rational account can be provided of Chomsky's beliefs about the role of considerations of simplicity in the appraisal of linguistic theories, given current views on simplicity as a property of scientific theories.

(viii) *Truth*

One of the most fundamental differences between Laudan's and Newton-Smith's models concerns the role which they assign to the notion 'truth' in an account of the scientific enterprise. Newton-Smith's account of scientific progress and rationality is based on the assumption that science is truth-directed. Laudan, on the other hand, argues that truth should not play a role in an account of the scientific enterprise. That is, he argues for a truth-independent account of the scientific enterprise. The assumption that Chomsky's linguistics is truth-directed plays a central role in the account presented above of Chomsky's rationality. For instance, it is argued in § 7.2.2.2 that the aim of Chomsky's linguistics is to discover the truth about the language faculty. On the issue of truth, then, Chomsky's rationality is in conflict with Laudan's model, but consistent with Newton-Smith's.

It was pointed out in § 2.3.4.2 above that Laudan's claims about the possibility of a truth-independent account of the scientific enterprise have been widely criticized, partly on the grounds that his own model requires an appeal to considerations of truth on various points. The conflict between Laudan's model and Chomsky's rationality on the issue of truth can then be seen as the result of an inadequacy in Laudan's model.

The account of Chomsky's rationality presented above can be used to illustrate one of the points of criticism levelled at Laudan,

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namely, that Laudan's claim that a research tradition has an ontological component becomes intelligible only on the assumption that science is truth-directed.<sup>72)</sup> Both Gutting (1980a:97) and McMullin (1979:634-635) question the role of the ontological component within the broader framework of Laudan's views. Gutting argues that while Laudan is correct in insisting that a research tradition incorporates an ontological component, the inclusion of such a component requires an appeal to truth as the goal of science. Thus Gutting (1980a:97) asks, "If a theory is not directed toward truth, why should it be required to solve problems in terms of a particular view of reality?" McMullin argues that Laudan's ontological component requires a realist interpretation of theories, which in turn presupposes that the search for truth plays a regulating role in the scientific enterprise. To be more specific: McMullin argues that the inclusion of an ontological component requires the adoption not only of the *semantic* aspect of realism, but also the *epistemic* aspect. This point can be clarified with the aid of Newton-Smith's (1981:43) characterization of his form of realism as comprising four components.

(i) *The ontological ingredient*

The sentences of scientific theories are true or false as the case may be in virtue of how the world is independently of ourselves.

(ii) *The causal ingredient*

Evidence that a theory is true or approximately true is evidence for the existence of whatever entities have to exist in order for the theory to be true or approximately true.

(iii) *The epistemological ingredient*

It is possible in principle to have good reasons for

thinking which of a pair of rival theories is more likely to be more approximately true.

(iv) *The thesis of verisimilitude*

The historically generated sequence of theories of a mature science is a sequence of theories being ever more approximately true.

Laudan accepts the ontological ingredient of this realist position, that is, he does not deny that theories are true or false. For this reason Newton-Smith (1981:30) classifies Laudan as an epistemological instrumentalist, and not as a semantical instrumentalist. Laudan does, however, reject the other components of the realist position set out above. Of particular interest is his rejection of the epistemological ingredient of realism and the thesis of verisimilitude. That Laudan does indeed reject these components emerge clearly from his (1977:123-127) discussion of the role of truth in science. McMullin's point is that the inclusion of an ontological component in research traditions requires the adoption not only of the ontological ingredient of realism, but also these other components explicitly rejected by Laudan.

Consider now the various ontological theses listed in § 7.2.2.3 that guide theory appraisal in Chomsky's linguistics. If Gutting's and McMullin's criticism of Laudan is correct, then one can explain why these principles are relevant only if one assumes that Chomsky adopts a realist interpretation of linguistic theories that comprise more than just the ontological component of realism. Adoption of, for example, the causal ingredient, the epistemological ingredient, and the thesis of verisimilitude in turn presupposes that science is truth-directed. To put it differently: If Gutting and McMullin are correct, then the relevance of these ontological principles for theory appraisal can be explained only on a strong realist interpretation of linguistic theories, which presupposes that linguistics is truth-

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directed. The issue of the ontological interpretation of Chomsky's linguistic theories is very complex, and no attempt will be made here to unravel all the issues. Whatever the precise content of Chomsky's realism, there can be no doubt that it comprises much more than the ontological ingredient.<sup>73)</sup> There can also be no doubt that Chomsky's adoption of a relatively strong realist interpretation of linguistic theories provides the explanation for the role which ontological assumptions play in theory appraisal in his work.

The need for a truth-directed account of Chomsky's linguistics can also be illustrated by reference to Chomsky's tolerant attitude to potential negative evidence which threatens his theories. In principle, Chomsky's epistemological tolerance is compatible with both Laudan's and Newton-Smith's models. However, when one considers Chomsky's *reasons* for adopting such an attitude, a point of conflict with Laudan's model emerges. One of Chomsky's reasons for advocating a tolerant attitude to negative evidence is that at the present stage of the development of linguistics, linguists often do not know what kind of evidence is relevant to linguistic theories. When one closely examines Chomsky's explication of this point in (Chomsky 1980a:10), it becomes clear that Chomsky's argument for epistemological tolerance involves a strong appeal to the notion of the truth-directedness of linguistic theories. The argumentation, and particularly his remarks quoted in (51) above, make sense only on the assumption that discovering truth (or at least approximate truth) is the aim of Chomsky's linguistic theories. It is precisely because the aim of his theories is truth, that Chomsky advocates an attitude of epistemological tolerance. As regards his reasons for adopting epistemological tolerance, then, Chomsky's rationality is also incompatible with Laudan's epistemological instrumentalism.

(ix) *Non-rule governed judgment*

As explained in § 2.3.4.7 above, Newton-Smith provides a role

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for non-rule governed judgment in the scientific enterprise. He denies that his good-making features - that is, the considerations that play a role in theory appraisal - constitute algorithms that can be applied mechanically. In cases of conflict among these features, the scientists involved must exercise their judgment, "since there is no way of weighting the relative importance of the differing factors". Newton-Smith (1981:225) also claims that it may not be clear whether one theory possesses a particular good-making feature to a higher degree than another. In such cases scientists must again exercise their judgment. Laudan, on the other hand, provides no role for non-rule governed judgment in theory appraisal. Instead, Laudan's model entails that there is a calculus of theory choice. As noted in § 2.3.4.7 above, Laudan's claim that his model is workable as a calculus of problem-solving effectiveness - and thus of the relative merit of theories - has been widely criticized.

The analysis of the developmental history of binding theory presented above reveals that there are several respects in which the choices that have been made by Chomsky are not fully guided by precise rules, that is, by a calculus. That is, there are several aspects of theory appraisal within Chomsky's linguistics which are in part subject to non-rule governed judgment. One such aspect is the adoption of a tolerant attitude in cases where linguistic theory is threatened by potential negative evidence. It was argued in § 7.2.3.6 above that there is no precise rule on the basis of which it can be calculated that the potential negative evidence for a theory has accumulated to such an extent that the theory must be abandoned. The adoption of a tolerant attitude towards potential negative evidence is thus in part a matter of non-rule governed judgment. As noted in § 7.2.3.6, Chomsky explicitly acknowledges this point. The application of the principle of the preservation of empirical success (55) - which to some extent constrains the adoption of a tolerant attitude towards potential negative evidence - is also subject to non-rule governed judgment. There is namely no precise rule on the basis of which it can

be calculated when a gain of empirical success in one area compensates for a loss of empirical success in another area. It was concluded in § 7.2.4 that the resolution of conflict between two (or more) principles of theory appraisal is also to some extent a matter of non-rule governed judgment. Also, while the empirical success of linguistic theory provides a test for the appropriateness of applying criteria formulated in terms of norms such as 'simplicity', 'deductive depth', 'unifiedness' in theory appraisal, there is no rule on the basis of which it can be calculated that the empirical success of linguistic theory no longer justifies the use of such criteria. In § 7.2.4 it was also argued that non-rule governed judgment enters into the decisions made by Chomsky in connection with the execution of his leading ideas.

On the issue of whether there is a calculus for theory appraisal, or whether non-rule governed judgment plays a role in such appraisal, Chomsky's rationality is then in conflict with Laudan's model, but compatible with Newton-Smith's. Given the criticism levelled at Laudan's model regarding the possibility of constructing a calculus of theory choice, it must be concluded that this conflict between Chomsky's rationality and Laudan's model is the result of an inadequacy in Laudan's model.

Newton-Smith deals only very cursorily with the role of non-rule governed judgment in science. One of the interesting points which Newton-Smith does make in this connection, is that it may in the long run be possible to appraise choices made on the basis of non-rule governed judgment in terms of the usual principles of theory appraisal. Some non-rule governed judgments may in the long run turn out to be better than others. The factors that could be used to justify a decision, although not available at the time of the decision, may become available at a later stage. At that stage, the correctness of the earlier non-rule governed judgment can then be appraised. Newton-Smith (1981:233) provides the following illustration of this point.

- (85) "It may be that in the long run some decisions turn out to be better than others. To take that favourite example of the whale: suppose that prior to the discovery of whales we thought of mammals as animals that live on the land and suckle their young. Fish live in water and do not suckle their young. We think that we might have decided in the face of the lack of help given by the rules that the whale was a fish. Later we would have found that this decision made life complicated. For our general theories about fish would require more caveats excluding whales than our general theories about mammals would require if the whale were counted as a mammal."

In § 7.2.4, where the role of non-rule governed judgment in the execution of leading ideas of Chomsky's linguistics is briefly discussed, it was also noted that the considerations needed to justify a non-rule governed choice may become available at some later stage.

A detailed analysis of the role of non-rule governed judgment in science in general, and in linguistics in particular, would constitute a study in its own right, and will therefore not be attempted here. However, I do wish to briefly relate the finding that non-rule governed judgment plays a role in theory appraisal within Chomsky's linguistics to earlier methodological studies of Chomskyan generative grammar which also concluded that non-rule governed judgment plays a role in this enterprise.

The topic discussed above was non-rule governed judgments made by linguists about the merit of a metascientific object, namely a linguistic theory. In previous studies it was established that linguists also have non-rule governed judgements about a different sort of object, namely, theoretically postulated aspects of natural language. In (Botha:1976) and Winckler (unpublished) such non-rule governed judgments are called "theoretic intuitions".<sup>74)</sup> The sort of non-rule governed judgments discussed above are called "metatheoretic intuitions" by Botha and Winckler. Botha (1976) argues that theoretic intuitions play a variety of roles in linguistic inquiry. Of special interest for the purposes of the present discussion is the fact that

(Botha 1976) contains numerous examples of such theoretic intuitions by Chomsky. Clearly, a comprehensive analysis of non-rule governed judgment in Chomsky's linguistics would have to take into account not only judgments about the merits of linguistic theories - that is, metascientific intuitions - but also these theoretic intuitions.

In sum: If Newton-Smith is correct, then non-rule governed judgment will necessarily be involved in at least some of the theory choices made within linguistics. According to Newton-Smith, the fact that controversies about competing theories cannot be settled by reference to an algorithmic set of rules for theory appraisal, does not indicate that science is irrational. The fact that non-rule governed judgment plays a role in theory appraisal within Chomsky's linguistics, does then not necessarily detract from the rationality of Chomsky's linguistics.

(x) *Changes in the criteria for theory appraisal*

As argued in § 2.3.4.11, both Laudan's and Newton-Smith's models of scientific rationality permit the criteria for theory appraisal used within a domain of inquiry to change. The question to be considered here is whether Chomsky's principles of theory appraisal underwent any changes during the period covered in chapters 3 - 6, namely, from the early seventies up to the present.

Since the late seventies Chomsky has frequently appealed to linguists to adopt the so-called "Galilean style" of inquiry.<sup>75)</sup> These recent appeals by Chomsky suggest that he himself may recently have changed from a non-"Galilean style" of inquiry to a "Galilean style" of inquiry. Any attempt to answer the question of whether such a change has taken place, is complicated by the fact that it is not easy to determine exactly

what content Chomsky assigns to the notion "the Galilean style of inquiry". The problems which exist in this connection are outlined by Botha (1982a), and are briefly referred to in § 2.4 above. Botha (1982a:42) concluded that if the historical implications of the expression "the Galilean style" are not taken too seriously, then there is a style of inquiry in Chomskyan linguistics which may be called "the *lax* Galilean style of linguistic inquiry". The four defining features of this style were presented in 2.(17) above, and are repeated as (77) above. The specific question to be considered here is then whether Chomsky has adopted "the lax Galilean style of inquiry" as a new mode of inquiry since 1973.

It was argued in § 7.2.4 that adoption of "the (lax) Galilean style of inquiry" as an appropriate mode of inquiry does not rule out the use of all other modes of inquiry. Specifically, the adoption of this style of inquiry does not rule out inquiry which leads to the establishment of empirical generalizations. It was pointed out that throughout the developmental history of binding theory Chomsky tried to establish empirical generalizations. The question is then not whether Chomsky *replaced* all other styles of inquiry with "the lax Galilean style of inquiry" during the developmental history of binding theory. The question rather is whether he introduced the latter style of inquiry as an *additional*, new mode of inquiry at some point since 1973. As argued in § 7.2.4, all four the main features of "the lax Galilean style of inquiry" are exhibited by the developmental history of binding theory. This is true not only for Chomsky's most recent work on the OB and GB binding theories, but also for his early work on the SSC and TSC/PIC, and on the 1973-Conditions-framework as a whole. On the basis of the data presented in chapters 3 - 6 above, it must then be concluded that Chomsky has not introduced "the lax Galilean style of inquiry" as a new mode of inquiry at some point since 1973.

The present study of the developmental history of binding theory cannot provide a definite answer to the question of when Chomsky started to conduct inquiry within the so-called "lax Galilean style". To answer the latter question, a longer period of Chomsky's work on linguistic theory than the one covered in chapters 3 - 6 would have to be analyzed. However, I wish to mention some considerations which bear on this question.

There is some evidence that at least three of the defining features of this style of inquiry are exhibited by work dating from much earlier than the work reviewed above. It was pointed out in § 7.2.3.5 above that Chomsky's recent emphasis on understanding and depth of explanation, rather than gross coverage of data - see (77a) - is not really something new in his work. Similarly, radical abstractions and idealizations - see (77b) - feature prominently in Chomsky's early work. Consider in this connection the role which the idealizations of the ideal speaker-hearer and the completely homogeneous speech-community played in Chomsky's (1965:3ff.) account of the aim of linguistic inquiry. Instances in which Chomsky adopted a tolerant attitude where a theory is threatened by potential negative evidence - see (77d) - can also be found in Chomsky's earlier works.<sup>76)</sup> For instance, Chomsky (1965:146) proposed a general condition on transformations, which prohibits the insertion of morphological material "into a configuration dominated by S once the cycle of transformational rules has already completed its application to this configuration".<sup>77)</sup> In connection with this constraint Chomsky (1965:146-147) pointed out that "there are a few examples that seem to conflict with this analysis . . . for reasons that I do not understand". He nevertheless states that "it provides an interesting confirmation of the theory of transformational grammar."

The one feature of "the lax Galilean style of inquiry" not mentioned above is the search for "unifying, principled theories

deductively/ . . .

deductively removed (perhaps far removed) from the primary problematic data" - see (77c). It seems as if this feature characterizes an important difference between work done before the introduction of the 1973-Conditions framework and work done since then. The Subjacency Condition, proposed in (Chomsky 1973), was the first example of a genuinely unifying, deductively deep syntactic principle developed by Chomsky. Chomsky's (1982a:41, 75) comments on the status of "Conditions on transformations" (1973) within the developmental history of his linguistic theory confirm this interpretation. Thus, Chomsky (1982a:75) agrees with Huybregts and Van Riemsdijk's statement, that "Conditions on Transformations clearly introduced a new era of linguistic theorizing with notions such as conceptual unification and deductive depth being keywords". The SSC and TSC of 1973, in contrast with the Subjacency Condition, did not qualify as genuine unifying, deductively deep principles. As Chomsky (1982a:75) points out, and is shown by the analyses presented in chapters 3 - 6, it is only very recently that real progress has been made with regard to the unification and deductive depth of binding theory. However, the recent work aimed at improving the deductive depth of binding theory does not reflect any change in Chomsky's method of inquiry.

In sum, then: Chomsky did not adopt "the lax Galilean style of inquiry" as an additional new mode of inquiry at some point during the developmental history of binding theory. At least three features of this style of inquiry - namely, the search for depth of understanding in restricted areas rather than gross coverage of data, the employment of radical abstractions and idealizations, and the adoption of an attitude of epistemological tolerance - are also found in work dating from before 1973. The fourth feature - the search for genuinely unified principles, that is, general, deductively deep principles - does distinguish work done before the 1973-Conditions framework and work done since then. The notion 'deductive depth', as defined in § 4.2 above, is indeed the key to an understanding of Chomsky's work on linguistic theory since 1973.

Chomsky's search for deductively deep theories represents a novel aspect not only of his own work on language. Moravcsik (1980: 28) claims that Chomsky's search for "deep" theories of mind represents a radical departure from key traditions within the social sciences. At the same time, according to Moravcsik, Chomsky's insistence on deep theories of mind brings his work in line with the successful natural sciences, like physics, chemistry, and biology. It seems then that the search for deductive depth is the crucial property of the style of inquiry which Chomsky currently advocates for linguistics, a style of inquiry which he calls "the Galilean style of inquiry".

Chomsky's use of the principles of theory appraisal (17) and (18) also gives rise to questions about changes in his views on theory appraisal. In the developmental history of binding theory, these two principles were first used to justify the choice of the OB binding theory over the SSC and PIC. The question arises whether this is a coincidence or whether it reflects a change in Chomsky's views on the role of considerations of simplicity in theory appraisal. Also, if Chomsky's views on the role of considerations of simplicity in theory appraisal have changed, the question arises in exactly what respects his views have changed. The lack of clarity on Chomsky's present and past views on the role of simplicity in theory appraisal poses a problem for any attempt to answer questions about possible changes in his views in this connection. This lack of clarity is in part the result of the inexplicitness of Chomsky's metascientific comments on the role of considerations of simplicity in theory appraisal. In § 7.3 above it was spelled out in what respects Chomsky's views are insufficiently clear. In spite of the above-mentioned problem, it is possible to draw some conclusions about possible changes in Chomsky's views on the role of considerations of simplicity in the appraisal of linguistic theories.

It is not the case that, prior to 1978, Chomsky regarded considerations of simplicity as irrelevant to the appraisal

of linguistic/ . . .

of linguistic theories, and UG in particular. Even in passages where he played down the importance of considerations of simplicity in theory appraisal, he conceded that such considerations have a role to play. The following passage from (Chomsky 1972b: 125) illustrates this point.

- (86) "Given alternative linguistic theories that meet this condition [= of restricted formal power - M.S.], we might compare them in terms of general 'simplicity' or other metatheoretic notions, but it is unlikely that such considerations will have any more significance within linguistics than they do in any other field."

Even in (Chomsky 1975c:119) - written in 1955 - the relevance of general considerations of simplicity for the appraisal of linguistic theory is acknowledged. Chomsky's (1957:34ff.) argumentation against phrase structure grammars provides strong evidence that he actually used considerations of simplicity in theory appraisal in his early work.<sup>78)</sup>

The recent increased emphasis on considerations of simplicity in the appraisal of linguistic theories may simply be a reflection of the fact that, in Chomsky's view, current versions of linguistic theory satisfy the most fundamental requirement which a linguistic theory should meet. That is, current versions of UG provide such a restricted characterization of the notion 'possible human language', that these theories can form the basis for an explanation of language acquisition. Consider in this connection Chomsky's (1981a:11; 1982b:13) claims that the formal power of current versions of UG is so restricted that they define only a finite set of core grammars. It is reasonable to assume that since the formal power of current versions of UG is regarded as being sufficiently restricted, other, less fundamental, considerations will play an increasingly prominent role in the appraisal of linguistic theories.

An aspect of Chomsky's views on the role of considerations of simplicity in theory appraisal which does appear to have changed,

is his/ . . .

is his interpretation of the status of such considerations. As explained in § 7.2.2.3 above, in some of his recent works Chomsky directly links simplicity as a property of linguistic theories and simplicity as a property of that part of the world described by a linguistic theory. It is only in works dating from 1981 or later - see (Chomsky 1981a, 1982a) - that Chomsky explicitly draws this direct link. Unfortunately, it is not completely clear whether Chomsky has in fact changed his views during the last three or four years, or whether he has only recently made explicit views held earlier by him.

The evidence that bears on the question of whether Chomsky's method has changed during the period from 1973 up to the present, is then not clear-cut. However, there is some evidence that the introduction of the 1973-Conditions framework coincided with a change in the criteria for theory appraisal within Chomsky's linguistics. In particular, since (Chomsky 1973), deductive depth has become a new, additional factor in the appraisal of linguistic theories. It is also possible that Chomsky's views on why considerations of simplicity are relevant for the appraisal of linguistic theories have changed.

The main aim of § 7.4 is to determine what conflicts, if any, there are between Chomsky's rationality, on the one hand, and the accounts of scientific rationality proposed by Laudan and by Newton-Smith, on the other hand. A problem which frequently cropped up in the attempt to compare Chomsky's rationality with the models of Laudan and of Newton-Smith, is that it is in some cases difficult to determine whether the two models do in fact make conflicting claims about theory appraisal. Several apparently interesting differences between the two models dissolved upon closer examination. This point was already made in § 2.3.4.11, in connection with the issue of changes in the method of science. The problem also cropped up above in connection with the role of methodological norms in theory appraisal, and the value of smoothness. In spite of these difficulties, it is

possible/ . . .

possible to answer the question of which of the two models of scientific rationality fits in best with Chomsky's rationality.

Several points were noted above on which Chomsky's rationality is in conflict with Laudan's model of scientific rationality. First, an account of Chomsky's rationality requires reference to the truth-directedness of Chomsky's linguistics. Second, in Chomsky's rationality there is a direct link between the use of conceptual factors in theory appraisal and the empirical success of theories, in that the empirical success of a theory provides a test for the appropriateness of using specific conceptual considerations in theory appraisal. Third, non-rule governed judgment plays a prominent role in the theory choices made within Chomsky's linguistics. On each of these points where Chomsky's rationality conflicts with Laudan's model, this model has been criticized on independent grounds. One can then conclude that these conflicts are the result of inadequacies in Laudan's model of scientific rationality.

With the possible exception of Chomsky's use of considerations of simplicity in theory appraisal, no conflicts between Chomsky's rationality and Newton-Smith's model of scientific rationality were found above. As argued above, it is not even clear that there is a direct conflict between Chomsky's rationality and Newton-Smith's model of scientific rationality with respect to the role of considerations of simplicity in theory appraisal. Suppose that one were willing to accept Newton-Smith's model of scientific rationality as an adequate account of contemporary scientific rationality. In terms of Newton-Smith's model Chomsky's beliefs about the goal of science and the principles of theory appraisal can then be positively endorsed. To put it differently: In terms of Newton-Smith's model a maximal rational account can be provided of Chomsky's beliefs about the goal of science and the principles which ought to guide theory appraisal.

As pointed out above, there are several points on which Laudan's

and Newton-Smith's/ . . .

and Newton-Smith's models are in agreement, apart from the points on which they differ. For instance, the models agree on the importance of a variety of conceptual factors in theory choice, the permissibility of putting aside potential negative evidence threatening a theory and the permissibility of employing *ad hoc* protective devices. On all these points Chomsky's rationality fits in with both of these models. What is interesting, is that these are all points on which Laudan's and Newton-Smith's models conflict with the older, conventional falsificationist model also used in appraisals of Chomsky's method. The present study provides strong evidence that such recent models of scientific rationality provide more adequate frameworks than the older falsificationist model for an account of Chomsky's method.

The last issue to be considered in this section is to what extent Chomsky uses rhetorical tricks to persuade others to accept his theories, and in particular, specific theory choices made by him. This question was raised in § 2.3.5 above, in a comparison of Laudan's and Newton-Smith's views on theory appraisal with those of Feysabend.

It was argued in § 6.5 above that the making of the markedness claims by Chomsky (1981d) in connection with the GB governor binding theory can be regarded as a mere rhetorical trick. Chomsky's (1982a:110) comments on these markedness claims provide textual evidence for the correctness of this analysis of Chomsky's (1981d) markedness claims. These markedness claims thus provide evidence that Chomsky does in fact make use of rhetorical tricks in the presentation of this theory choices.

Although there is no textual evidence from Chomsky's work to support this, it is not unreasonable to assume that the account of the developmental history of binding theory presented in chapters 3 - 6 provides further evidence of the use of rhetorical tricks by Chomsky. Consider again the following two problems threatening

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the minimal rationality of Chomsky's methodological beliefs:

(1) The connection which Chomsky makes between simplicity and unifiedness as properties of linguistic theory and simplicity and unifiedness as properties of neural structure/the brain conflicts with his views on the "distance" between the abstract level of the characterization provided by linguistic theory and the concrete level of neural structure/the brain - see (82b), and (ii) Chomsky's failure to specify what specific non-linguistic theories are involved in determining the naturalness of linguistic principles, makes it unreasonable to actually use a principle of theory appraisal based on the notion 'naturalness as principles of mental computation' in appraising linguistic theories - see (82c). In view of the problems created by Chomsky's claims about the link between simplicity and unifiedness as properties of linguistic theories and simplicity and unifiedness as properties of neural structure/the brain, and his claims about the naturalness of linguistic principles as principles of mental computation, it is reasonable to ask whether these claims should not also be regarded as mere rhetorical tricks. These claims could then be analysed as misleading claims made by Chomsky about the merit of his theory. Such claims would then qualify as propaganda, in the second sense of the term distinguished by Feyerabend (1978:214) - see § 2.3.5 above.

One could, of course, only speculate about Chomsky's motives for making such propagandistic claims. A plausible explanation would be a desire on his part to persuade others to take his linguistic theories seriously as theories of the mind, theories which must not only fit in with other abstract theories of components of the mind, but theories which would ultimately have to fit in with theories of neuro-physiology. Such an explanation is plausible in view of Chomsky's view of linguistics as part of the study of the mind, a study which should proceed in essentially the same manner as the study of physical organs.

Given that Chomsky does in fact make use of rhetorical tricks,

the obvious/ . . .

the obvious question arises how this fact affects his rationality. In particular, does the use of such rhetorical tricks imply that theory appraisal within Chomsky's linguistics - or at least the theory choices in connection with which Chomsky made the propagandistic claims - is irrational? In § 2.3.5 reference was made to two opposing views on the relation between the rationality of science and the use of rhetorical tricks. On the one hand, there is the simplistic view held by Feyerabend, who regards the use of rhetorical tricks within science as providing evidence that science is not completely rational. On the other hand, Finocchiaro and Newton-Smith deny that the use of rhetorical tricks within science necessarily provides evidence for the irrationality of science. Finocchiaro (1980:1981) claims that rhetorical factors are allogical, and should be appraised by their own criteria. Newton-Smith links the use of rhetorical tricks with the merit of the theory involved, where this merit is determined in terms of the usual criteria of theory appraisal. According to Newton-Smith (1981:141), the crucial question when one is dealing with the use of rhetorical tricks is whether "a rational case" can be constructed. If it can be shown that a theory choice in connection with which propagandistic claims were made is the best choice in terms of the (non-propagandistic) principles of theory appraisal used within that domain of inquiry, then the use of such propagandistic claims does not adversely affect the rationality of the choice. Within the context of the present study, it is of interest to determine whether a rational case can be constructed in those instances where Chomsky used rhetorical tricks.

Consider first Chomsky's (1981d) markedness claims made in connection with the GB governor binding theory, which constitutes a clear case in which Chomsky used rhetorical tricks. In the choice between the OB binding theory and the GB governor binding theory there was conflict among some of Chomsky's principles of theory appraisal. In particular, the principle of the preservation of empirical success (55) conflicted with the principles

of greater/ . . .

of greater simplicity (17) and (18) and the principle of greater deductive depth (21). It was argued in § 7.2.4 above that within Chomsky's linguistics there are no precise rules on the basis of which such conflicts can be resolved. In particular, it is not the case that the principle of the preservation of empirical success (55) must always overrule all other principles of theory appraisal. In choosing the GB governor binding theory over the OB binding theory, Chomsky thus did not violate any rule, even though the GB governor binding theory fails to preserve the empirical success of the OB binding theory. Chomsky's choice of the GB governor binding theory was based on non-rule governed judgment. Further developments in the theory - specifically, the development of the GB SUBJECT binding theory as a variant of the GB governor binding theory - indicate that this non-rule governed choice was indeed "the best choice". A rational case, in Newton-Smith's sense, can thus be constructed for the change from the OB binding theory to the GB governor binding theory.

Consider now Chomsky's claims about the naturalness of certain linguistic principles, claims which may also amount to mere rhetorical tricks. Chomsky made use of the principle of theory appraisal based on naturalness - see (27) above - to justify only two of the theory choices outlined in chapters 3 - 6. First, he claimed that the 1973-Conditions, including the SSC, the TSC, and the Subjacency Condition, are more natural than Ross' island conditions - see (26a). Second, he claimed that the Empty Category Principle is more natural than the \* $[that \bar{t}]$  filter, which it replaced - see (26b). Both these choices are independently justified in terms of at least one principle of theory appraisal employed by Chomsky, namely the principle of increased deductive depth (21). For both these choices a rational case, in Newton-Smith's sense, can thus be reconstructed. The fact that the consideration of naturalness had no real effect on any of the theory choices made by Chomsky, provides some support for the view that claims about the naturalness of specific linguistic principles amount to mere rhetorical tricks.

In the case of Chomsky's claims about the link between simplicity and unifiedness as properties of linguistic theory and simplicity and unifiedness as properties of neural structure/the brain, the rationality of the actual choices made by Chomsky on the basis of considerations of greater simplicity and unifiedness also remains unaffected. Such a link is by no means a necessary condition for the use of the relevant considerations in theory appraisal.

In Newton-Smith's view about the use of rhetorical tricks, then, Chomsky's use of rhetorical tricks does not adversely affect the rationality of his theory appraisals. Note that Laudan has not stated his position on the use of rhetorical tricks. In principle his model of scientific rationality is compatible with some version of the weak position on the use of rhetorical tricks held by Finocchiaro and Newton-Smith. That is, Laudan's model of scientific rationality does not in principle rule out all use of rhetorical tricks in science.

#### 7.5 Other recent accounts of theory appraisal in Chomsky's linguistics

The question was raised in chapter 1 above whether the present inquiry into the rationality of Chomsky's linguistics is justified, given Chomsky's extensive metascientific comments on scientific rationality, theory appraisal, the best method of inquiry, and related matters. It was then pointed out that it cannot be taken for granted that Chomsky's metascientific comments accurately reflect his actual method. Against the background of the account of theory appraisal within Chomsky's linguistics presented in § 7.2, it is now possible to consider to what extent Chomsky's metascientific comments do provide an accurate and complete reflection of his actual method of theory appraisal.

In § 7.2 above a detailed account of theory appraisal within Chomsky's linguistics is presented. The main source of evidence for the various claims incorporated in this account is the actual

theory choices made by Chomsky during the developmental history of binding theory, as described in chapters 3 - 6 above. Chomsky's metascientific comments provide additional textual evidence for several of these claims, a fact reflected by the frequent references made above to comments by Chomsky on, for example, his strategy of setting aside counterexamples, the importance of deductive depth and unification in theory appraisal, the role of considerations of simplicity in theory appraisal. In so far as Chomsky's metascientific comments provide additional textual support for claims initially justified on the basis of Chomsky's actual choices, it must be concluded that Chomsky's metascientific comments do accurately reflect his method of theory appraisal.

However, the crucial question to be considered here is in what respects Chomsky's metascientific comments fail to provide an accurate and complete reflection of his method of theory appraisal. One way to approach this question is to ask whether it would be possible to construct an account of theory appraisal within Chomsky's linguistics identical to the account presented in § 7.2 solely on the basis of Chomsky's metascientific comments about theory appraisal. There is good reason to doubt whether this task can be accomplished successfully. In fact, at several points during the discussions above it was noted that Chomsky's metascientific comments do not provide a complete and accurate account of his method. Let us briefly consider a few of the respects in which Chomsky's metascientific comments fail to provide a complete and accurate account of his method of theory appraisal.

A first problem facing any attempt to construct a complete and accurate account of theory appraisal within Chomsky's linguistics solely on the basis of Chomsky's metascientific comments, is that Chomsky does not explicitly comment on all aspects of his method of theory appraisal. Complete reliance on Chomsky's metascientific comments would thus lead to the construction of an

account that would be incomplete, and possibly even wrong. For instance, it was argued in § 7.2.3.6 above that the developmental history of binding theory provides evidence that the principle of the preservation of empirical success (55) guides Chomsky's theory choices. Specifically, this principle was found to strongly influence decisions to introduce special devices to explain negative evidence, instead of adopting a tolerant attitude towards such negative evidence. Yet in Chomsky's metascientific comments on the appraisal of linguistic theories no reference is made to the fact that the preservation of the empirical success of earlier versions is an important factor in decisions to adopt a tolerant attitude towards negative evidence.

Chomsky's recent comments on the appropriate reaction to negative evidence threatening linguistic theory provide an interesting case where his metascientific comments provide an incomplete account of his method of theory appraisal. In his recent comments on the issue of negative evidence which threatens linguistic theory, Chomsky emphasized the appropriateness, and in fact the necessity, of setting such negative evidence aside. If one were to focus exclusively on these recent comments by Chomsky, one would erroneously get the impression that negative evidence, and falsifications in particular, at present plays an insignificant role in Chomsky's work on linguistic theory. The developmental history of binding theory shows just how wrong this impression would be. The fact is that a large number of the theory changes discussed in chapters 3 - 6 were aimed at explaining negative evidence threatening binding theory. Chomsky's recent metascientific comments are thus incomplete, in that he does not spell out how prominent a role negative evidence still plays in his work on linguistic theory.

When one considers the context in which Chomsky made his recent comments on the handling of negative evidence threatening lin-

guistic/ . . .

guistic theory, it becomes possible to understand his emphasis on the appropriateness of setting aside such negative evidence. Chomsky is arguing against those linguists (and other scholars who, in his view, attach too much weight to counterevidence threatening a theory. He (1979a:188) explicitly refers to "methodologists" who assert "that a counterexample serves to refute a theory and shows that it must be abandoned".

Chomsky's recent comments on the appropriateness of setting aside negative evidence threatening a linguistic theory apparently to some extent contrast with his earlier views on the importance of counterevidence for the improvement of linguistic theory. Consider in this connection Chomsky and Halle's (1968:ix) argument for presenting "a hypothesis concerning general linguistic theory in "very explicit terms". They claim that "only such precise and explicit formulation can lead to the discovery of serious inadequacies and to an understanding of how they can be remedied". If one were to focus exclusively on Chomsky's metascientific comments on the handling of counterevidence, one would get the impression that his views on the importance of counterevidence have changed fairly dramatically. Closer examination of Chomsky's actual work on linguistic theory reveals that, even at the time when he emphasized the importance of counterevidence, he in some cases adopted a tolerant attitude to counterevidence, and protected his theory in various ways from such counterevidence.<sup>79)</sup> Similarly, as indicated above, at present the existence of counterevidence for a theory is still regarded as an indication of an inadequacy of the theory, and frequently attempts are made to overcome the inadequacy.

Note that neither Chomsky's earlier statements about the importance of counterevidence, nor his recent claims about the appropriateness of setting aside such counterevidence, are strictly speaking false. The problem is rather that since Chomsky at different times focusses on different aspects of the handling

of counterevidence, his metascientific comments on their own cannot provide a complete and accurate account of his handling of such evidence, nor of possible changes in his handling of such negative evidence. To construct such an account, Chomsky's actual handling of counterevidence must also be examined.

Chomsky's recent comments on the adoption of the "Galilean style" of inquiry within linguistics is another area in which exclusive reliance on Chomsky's metascientific comments would make a complete and accurate account of Chomsky's method impossible. For instance, Chomsky fails to indicate at what stage he himself started to work within the Galilean style. In so far as it can be determined what the "Galilean style" is, there is every indication that Chomsky started working within this style of inquiry some time before he explicitly referred to this style of inquiry. Chomsky's comments on the "Galilean style" also do not make clear that the adoption of this style does not rule out the use of other styles of inquiry, such as a style of inquiry which leads to the establishment of empirical generalizations.

It is also not clear from Chomsky's metascientific comments exactly what the crucial properties are of the "Galilean style" of inquiry in Chomsky's view. Botha (1982a) outlines in detail the problems involved in determining the precise content of Chomsky's notion 'the Galilean style', and in fitting in his comments on this style with his actual practice. Also, it was noted in § 7.3 above that Chomsky's metascientific comments on the role of metatheoretic simplicity in the appraisal of linguistic theories leave many crucial questions about his views on this issue unanswered. In this respect, too, an examination of Chomsky's practice is a necessary precondition for a complete and accurate account of his method.

It was suggested in § 7.4 above that Chomsky employs rhetorical tricks at the level of metascientific comment. Thus, it was

suggested that Chomsky's claims about the link between simplicity and unifiedness as properties of linguistic theory and simplicity and unifiedness of neural structure/the brain, and his claims about the naturalness of linguistic principles are mere rhetorical tricks. In these cases Chomsky's metascientific comments thus also fail to accurately reflect his method.

In sum, then, Chomsky's metascientific comments do not provide a complete and accurate account of his method of theory appraisal. A detailed examination of Chomsky's actual practice of theory appraisal is necessary for the construction of Chomsky's method. Note that I am not claiming that Chomsky's metascientific comments provide a completely inaccurate account of his method of theory appraisal. The use made above of textual evidence from Chomsky's work to support claims about his method indicate that in several instances Chomsky's metascientific comments do accurately reflect his method. I argue rather for a weaker claim, namely, that Chomsky's metascientific comments do not on their own provide a complete and accurate account of his method of theory appraisal. This conclusion about Chomsky's metascientific comments indicates that studies such as the present one - that is, studies which attempt to reconstruct aspects of Chomsky's method on the basis of a detailed examination of Chomsky's actual practice - are indeed justified.

A brief look at two other recent attempts to reconstruct Chomsky's method of theory appraisal provides further justification for the approach adopted in the present study. The two accounts examined below are by Lightfoot (1982) and Cook (1982).

Lightfoot is a Chomskyan linguist who closely identifies himself with Chomsky's ideas. In his recent book, *The language lottery: Toward a biology of grammars* (1982), Lightfoot attempts to describe the research program of generative grammar, and, more specifically, Chomskyan generative grammar. Lightfoot's (1982: xi) aim is to describe the work of Chomskyan linguistics "accu-

rately/ . . .

rately and faithfully", in order to make this work accessible to a non-specialist audience of non-Chomskyan linguists, biologists, ethologists, psychologists, and anthropologists. Lightfoot thus placed himself under an obligation to provide an accurate account of Chomskyan linguistics, including its method, a topic that he addresses in chapter 5.

Lightfoot (1982:xii) regards Chomsky as the dominant figure within Chomskyan generative grammar. In accordance with this view, Chomsky's own work on linguistic theories occupies a special position in (Lightfoot 1982). Lightfoot (1982:212) explicitly states that his book is based on "the work of Chomsky". It is then reasonable to assume that Lightfoot's claims should apply to Chomsky's own work.

Lightfoot devotes the whole of chapter 5 to the method of Chomskyan generative grammar. In the last section - with the title "Improving the hypothesis" - Lightfoot addresses the issue which forms the topic of the present study, namely the factors which guide theory choice. Lightfoot's (1982:98-99) account of the factors which guide theory choice, that is, the criteria of theory appraisal, may be summarized as follows.

- (i) The crucial factor in science, and also in linguistics, is depth of explanation, and not coverage of data.
- (ii) The depth of explanation provided by a linguistic theory is determined by three factors: (1) coverage of data, (2) criteria of simplicity and elegance, (3) insight provided into the acquisition of grammars.
- (iii) Criterion (3), namely insight provided into the acquisition of grammars, is of primary importance.
- (iv) The citing of a counterexample is to show that a theory is rated lower by criterion (1) and "has lower empirical force by that single criterion".

- (v) A theory rated lower by criterion (1) may be more highly valued by criterion (3), and so be preferred overall.
- (vi) In practice theories are refuted or revised by offering a theory more highly rated overall by the three weighted criteria.

Lightfoot's account of the criteria which guide theory appraisal in Chomskyan linguistics is obviously incomplete and lacking in detail. On one point this account is apparently also incoherent. On the one hand, Lightfoot (1982:98) contrasts depth of explanation with coverage of data, when he states that "the crucial factor in science is depth of explanation, not coverage of data". On the other hand, he (1982:99) identifies coverage of data as a factor which (in conjunction with other factors) determines explanatory depth.

The most serious shortcoming of Lightfoot's account of theory appraisal within Chomsky's linguistics is that it provides an inaccurate account of the criteria on which Chomsky's recent theory choices are based. Lightfoot assigns the greatest weight to the third criterion. And it is correct that success in explaining language acquisition is the most fundamental criterion in terms of which the merit of UG is judged. However, Lightfoot fails to point out that this third criterion did not play a role in the choices which Chomsky recently made among different versions of his theory. For instance, since 1978 this criterion played no role in Chomsky's choices among different versions of binding theory.

Lightfoot's failure to accurately characterize the role of the third criterion in Chomsky's recent theory choices is aggravated by the fact that he (1982:15) characterizes arguments from the "deficiency" of the stimulus - arguments which directly bear on the third criterion as representing "the basic line of reasoning" used by Chomskyan linguists. One would then expect that arguments from the "deficiency" of the stimulus would play the major role

in the justification of specific general-linguistic hypotheses proposed by Chomsky in his recent works. In fact, as the analyses presented above of recent theory choices by Chomsky indicate, Chomsky rarely used an argument from the deficiency of the stimulus to justify the choice of a specific version of his theory. For instance, in (Chomsky 1981a) - which Lightfoot (1982:212) regards as "the best and most comprehensive discussion of more technical aspects, the substance of current theories" - the argument from the "deficiency" of the stimulus is not once used to justify a specific choice, even though some of the theory changes proposed by Chomsky (1981a) are fundamental changes.<sup>80)</sup>

Lightfoot not only provides an inaccurate account of the role of his third criterion in the appraisal of current versions of Chomsky's linguistic theory. He also fails to accurately identify the factors on which actual choices were based. Note that in Lightfoot's account of theory appraisal no mention is made of the important criterion of deductive depth, and the related notion of unifiedness. It was argued in § 7.4 above that the notion of deductive depth is crucial to an understanding of Chomsky's recent work on linguistic theory. The criterion of deductive depth was used by Chomsky to justify several fundamental changes in binding theory, including the introduction of the OB binding theory, the replacement of the OB binding theory by the GB governor binding theory, and the replacement of the GB governor binding theory by the GB SUBJECT binding theory. It was briefly noted above that the criterion of deductive depth also played a role in the development of other components of Chomsky's current theory, including the Subjacency Condition (which forms part of bounding theory). Because of his failure to cite deductive depth as a factor in the appraisal of linguistic theories, Lightfoot cannot explain the majority of the fundamental changes Chomsky recently introduced into his linguistic theory.

Recall that Lightfoot placed himself under an obligation to provide an "accurate" and "faithful" description of the work of Chomskyan linguists, of whom Chomsky himself is the most important.

Given Lightfoot's failure to accurately identify the factors which actually guided Chomsky's recent choices, it must then be concluded that as far as theory appraisal is concerned, Lightfoot has failed to accurately and faithfully describe the work of Chomskyan linguistics.

The fact that Chomsky's metascientific comments do not provide a complete and accurate account of his method has an important, if rather obvious, consequence for anyone who is interested in determining precisely what a certain aspect of Chomsky's method comprises. While Chomsky's metascientific comments can provide many clues to his method, a specification of any aspect of his method must be based on an analysis of Chomsky's actual practice. The dangers inherent in relying too heavily on Chomsky's metascientific comments when attempting to characterize his method, can be illustrated with the aid of Dean Elton Cook's (1981) doctoral thesis, "Chomsky: Towards a rationalist philosophy of science". Cook's (1981:5) aim is to investigate "in considerable detail the methodological issues that are implicit in Chomsky's thought". He (1981:6) approached Chomsky's work "from the point of view provided by the philosophy of science".

Cook's method is, in essence, to try to explicate a great number of meta-comments by Chomsky on different aspects of linguistics. He then tries to show that Chomsky's views on the nature of linguistic inquiry "fit in" with certain general views on scientific inquiry proposed within the philosophy of science. Note incidentally that Cook is very sympathetic toward Chomsky's linguistics, and he in fact devotes a great deal of time to rebut certain criticisms levelled at the fundamental assumptions of Chomsky's linguistics. One of Cook's (1982:304) final conclusions is that his explication of Chomsky's views on the nature of science and linguistics "allows his program to be presented in a form that is philosophically more plausible than has originally been perceived by critical commentators".

An extensive/ . . .

An extensive review of Cook's various claims about Chomsky's linguistics falls outside the scope of the present study. Instead I will only try to make plausible the claim that Cook's lack of attention to Chomsky's actual practice led him to give an incorrect account of the appraisal of specific linguistic hypotheses by Chomsky.

In chapter III of his thesis Cook (1982:113) argues that Chomsky's view on the nature of scientific theories "falls neatly into a view of the structure of scientific theories developed by Imre Lakatos". In the course of his attempt to relate Chomsky's views on the nature of theories to those of Lakatos, Cook makes certain claims about the appraisal of specific linguistic hypotheses, such as those contained in binding theory. For instance, he (1981:108) states that such hypotheses are "accepted or rejected according to the normal canons of empirical investigation". According to Cook (1982:102), these canons amount to "verification or falsification of the hypothesis by a comparison with the observational data". Cook specifically emphasizes the rejection of specific hypotheses in the face of disconfirming evidence. Consider in this connection his (1982:104; 127; 128) comments on the falsification, and consequent rejection, of specific linguistic hypotheses.

Cook acknowledges that Chomsky in some cases adopts a tolerant attitude towards potential counterevidence. But, according to Cook, this attitude does not apply to specific hypotheses. Instead, it applies only to very general hypotheses of Chomsky's linguistics, such as Chomsky's rationalist hypothesis of language acquisition. The distinction which Cook draws between the falsification of specific linguistic hypotheses and general hypotheses of Chomsky's linguistics comes out clearly in Cook's (1981:126ff) summary of his comparison of Lakatos' views on the structure of theories with Chomsky's views on the issue.

What Cook fails to note, is how frequently Chomsky sets

aside counterevidence threatening specific linguistic hypotheses proposed by him. The analysis of the development of binding theory presented above shows that Cook's claims about the role of potential counterevidence in the development and appraisal of specific linguistic hypotheses are wrong. The main reason for the incorrectness of Cook's account is that he paid almost no attention to what Chomsky actually did when he appraised specific hypotheses, but relied almost exclusively on Chomsky's metascientific comments.<sup>81)</sup> If Cook had looked closely at the developmental history of binding theory, for instance, he would have noticed that the appraisal of specific linguistic hypotheses is by no means a simple matter of "verification of falsification of the hypothesis by a comparison with the observational data". Given the negative conclusion reached above that Chomsky's metascientific comments do not on their own provide an accurate and complete account of his method, it is not surprising that Cook's account of Chomsky's method is inaccurate.

Apart from the issue of the role which potential counterevidence plays in the appraisal of specific linguistic hypotheses, there is a second respect in which Cook's account of the appraisal of specific linguistic hypotheses is incorrect: Cook makes no provision for the role which so-called conceptual considerations play in the appraisal of specific linguistic hypotheses within Chomsky's linguistics. This is surprising since in several recent works by Chomsky that were available to Cook Chomsky did explicitly comment on the relevant issue. Again, closer attention by Cook to the factors which actually guided the development of the various components of Chomsky's linguistic theory would have enabled him to construct a more accurate account of theory appraisal in Chomsky's linguistics.

The inadequacy of both Lightfoot's (1982) and Cook's (1982) accounts of Chomsky's method of theory appraisal further highlights the need to base any account of this method on a detailed examination of the actual practice adopted within Chomsky's linguistics.

Footnotes to chapter 7

1. Cf. the discussion of Newton-Smith's (1981:4) views on the structure of a model of rationality in § 2.2 above.
2. Cf. § 2.2 above for an explication of the notion 'minimal rational account'/'minirat account'.
3. Cf. chapter 1 above for the relation between generative grammar and Chomsky's linguistics.
4. In his (1980a:104-109), Chomsky makes it quite clear that he does equate truth with psychological reality. It is only in his response to Harman (1980) that, for the first time, he admits that "there is a question of physical (or psychological) reality apart from truth in a domain".
5. The formulation of this qualification, as well as the next one, is to a large extent based on (Newton-Smith 1981:223).
6. Popper is one of the most prominent proponents of this view. Cf. in this connection, for example, Popper 1968, 1972. Cf. also Newton-Smith 1981 for a more recent proposal that science should be seen as aiming at theories with an increasing degree of verisimilitude, a proposal in which an attempt is made to overcome the problems threatening, for instance, Popper's notion. Cf. § 2.3.3.1 above for some discussion of Newton-Smith's views on verisimilitude.
7. For two recent discussions of the problems surrounding especially Popper's notion of increased verisimilitude, cf. Newton-Smith 1981:Chapters 3 and 8, and Laudan 1977:125-127. Cf. also the references cited there to earlier discussions of verisimilitude, and the problem which led to the incorporation of this notion in an account of the scientific enterprise. Note that Laudan and Newton-Smith take opposing positions on the issues involved. Laudan, given the problems

surrounding the idea that present theories are closer approximations to the truth, attempts to provide an account of the scientific enterprise that does not involve an appeal to truth. Newton-Smith, on the other hand, argues that an appeal to truth is necessary for an adequate account of the scientific enterprise, and attempts to solve the problems surrounding the notion of increased verisimilitude. Cf. in this connection the discussion in § 2.3.4.2 above.

8. For a more complete representation of the relations among the initial and final states of the language faculty, UG, a grammar, the products of language use, cf. Botha 1981:437. Note that Botha's schema does not include the general assumptions about the nature of the language faculty. "UG" in (7) corresponds to "general-linguistic theory" in Botha's schema, "final state of the language faculty" in (7) corresponds to Botha's "linguistic competence", and "initial state of the language faculty" in (7) corresponds to Botha's "language acquisition device".
9. As explained in § 3.2.5, this is an *indirect* contribution.
10. Cf. §§ 3.2.3 and 3.2.5 for a more detailed discussion of Chomsky's view of the fundamental empirical problem of linguistics, and his views on the solution for this problem.
11. Cf. also Kaplan 1964:312-314 for an explication of the link between a correspondence view of truth and the "fit" between a theory and the facts.
12. Note that several of the changes listed in (13) and (14) actually involve changes both at the level of UG and at the level of specific grammars. For instance, the proposal about structure-building rules - see (14h) - entails that UG must be modified to permit such rules. The classification of each change as a change either at the level of UG

or the level of specific grammars is in fact a convenient simplification of the matter. This simplification does not affect the main point, namely, that Chomsky frequently makes changes to UG, or to specific grammars, or to both, in order to increase the explanatory and predictive success of UG with respect to such grammars. The conclusions drawn in the rest of § 7.2 also remain unaffected.

Note also that some of Chomsky's proposals for changes listed in (13) and (14) are highly tentative. Cf., for example, (13h, i, u). The significance of the tentative nature of some of the proposals listed in (13) and (14) will be considered in § 7.2.3.

13. Cf. also the discussion in § 7.2.3.2 below for this apparent conflict between empirical success and simplicity.
14. Cf. the formulation of the principles of evidential comprehensiveness and of evidential independence presented in § 3.2.4 above.
15. Cf. the brief discussion of the link between theoretical success and empirical success in § 2.3.4.1 above.
16. Cf. the discussion in § 2.3.2.3 above of Laudan's view that relations which are weaker than logical inconsistency - including mutual implausibility - can give rise to conceptual problems for theories.
17. Cf. Newton-Smith 1981:228 for a discussion of the idea that the physical world is unified, and its consequences for the appraisal of the relations among physical theories from different domains.
18. See also Chomsky's own account of the role which the attempt to reconcile restricted formal power with descriptive adequacy has played in the developmental history of

his linguistic theory. Cf. in this connection, for example, Chomsky 1978a:13ff.; 1981b:36ff.

19. Consider also the following remarks by Chomsky (1965:53) on how the correctness of the conflicting claims embodied in empiricist and rationalist views on language acquisition can be determined. (The italics are mine.)

*"When such contrasting views are clearly formulated, we may ask, as an empirical question, which (if either) is correct. There is no a priori way to settle this issue. Where empiricist and rationalist views have been presented with sufficient care so that the question of correctness can be seriously raised, it cannot, for example, be maintained that in any clear sense one is 'simpler' than the other in terms of its potential physical realization, and even if this could be shown, one way or the other, it would have no bearing on what is completely a factual issue. This factual question can be approached in several ways. In particular, restricting ourselves now to the question of language acquisition, we must bear in mind that any concrete empiricist proposal does impose certain conditions on the form of the grammars that can result from application of its inductive principles to primary data. We may therefore ask whether the grammars that these principles can provide, in principle, are at all close to those which we in fact discover when we investigate real languages. The same question can be asked about a concrete rationalist proposal. This has, in the past, proved to be a useful way to subject such hypotheses to one sort of empirical test." (Footnote 31 is omitted.)*

Chomsky (1980a:48) also regards the explanatory success of theories as the test, when he contrasts the view that knowledge of language can be characterized in terms of mental states and structures with the view that knowledge of language must be characterized in terms of dispositions.

*"The choice between these alternatives cannot be settled by a priori argument, but only by trying to refine each of them to the point where we can ask how they fare as theories that explain some significant range of facts; for example, that certain sentences do or do not mean such-and-such and that we know this to be the case."*

20. Chomsky's (1980a:64-65) discussion of the question of what should be included in representations of meaning illustrates his approach to the general problem of determining the domain of a specific phenomenon.

" . . . in the matter of stress and presupposition, there is reason to believe that the rules fall within grammatical competence, so that the properties appear in the representations of meaning it provides. Were it to be shown that these matters do not bear on logical inference but only, say, on conversational implicature, we would then conclude that representations of meaning generated by rules of grammar provide materials for conversational implicature, not that they must exclude these elements. And if some attribute of a sentence that enters into logical inference turns out not to be provided by the best theory of grammar that we can devise, we will conclude that this is not an element of the representations of meaning provided by grammatical competence. Proceeding in this way, we will try to identify just what it is that we have loosely been calling 'representations of meaning', much in the way that we will try to determine the properties of linguistic representations of sound. The fact that the conclusions may not conform to some a priori scheme or satisfy some specific need such as codifying inference is, plainly, irrelevant to this empirical inquiry. I am assuming, in short, that we are trying to answer a difficult empirical question, only partially clear, which can become more precise only in the course of finding some answers to it: namely, what are the real components of mental states."

Chomsky's footnote 24 is omitted from the remarks quoted above.

21. The importance of depth of understanding versus comprehensiveness of coverage of data is also stressed in, for example, (Chomsky 1977b:21; 1978a:10, 25-26; 1979a:72, 106-108; 1982a:82).
22. In addition to the references cited below, see also, for example, (Chomsky 1979a:54-57) on the importance of abstraction and idealization in this connection.

23. Cf. Chomsky 1980a:40-46, 89-90 for discussion of the modular structure of the mind, and Chomsky 1980a:59, 60-61, 64 for discussion of the modular structure of the language faculty.
24. Cf. § 3.3.5 above for more details on the idealization of sentence grammar.
25. Cf. § 4.3 above for more details on the idealization of core grammar.
26. Cf. also the following remarks by Chomsky (1981a:6):

"Ultimately, one hopes of course that it will be possible to subject proposals concerning UG to a much broader test so as to determine both their validity and their range of parametric variation, insofar as they are valid. Since these proposals concern properties of grammars - apart from empirical generalizations, which should be regarded as facts to be explained rather than part of a system of explanatory principles of UG - it is possible to put them to the test only to the extent that we have grammatical descriptions that are reasonably compelling in some domain, a point of logic that some find distasteful, so the literature indicates."

27. Note that Lightfoot (1979:73) refers to Pullum (1975) as a work in which "unanalyzed phenomena" are cited as "alleged counterexamples to a theoretical claim".
28. Cf. in this connection Botha's (1981:408) distinction between indifference and reasoned apathy as forms of reaction to criticism levelled at a theory. Indifference implies rejecting the criticism as inappropriate, irrelevant, or inaccurate. Reasoned apathy implies that the scientist takes no direct steps to remedy the defects of the criticized theory, but provisionally retains it in an unrevised form. The similarities between reasoned apathy and epistemological tolerance should be obvious.

29. The following exposition draws heavily on Botha's (1982a) analysis of Chomsky's conception of the "Galilean style of inquiry".
30. Cf. §§ 2.3.3.5 and 2.3.4.7 above for an exposition of the notion 'non-rule governed judgment', and for Newton-Smith's (1981) claims on the role which such judgment plays in theory appraisal. The issues of non-rule governed judgment in Chomsky's linguistics will be considered in more detail in § 7.2.4 below.
31. These examples were presented above as 6.(87) and 6.(85), respectively.
32. Note that as regards these cases the SSC makes the same predictions as the OB binding condition which corresponds to the SSC, namely the Opacity Condition.
33. Cf. § 6.3 for details of the differences between the OB and GB governor binding theories.
34. Cf. § 6.5 for a more detailed exposition of Chomsky's markedness claims.
35. This question will be considered in § 7.3 below.
36. Cf. § 2.4 above for an exposition of this style of inquiry. Cf. also the discussion in § 7.2.4 below.
37. Consider in this connection also the discussions in, for example, Chomsky 1976b: 47; 1979a:187; 1981a:6.
38. Cf. also § 3.2.4 above for an explication of this connection.
39. In an earlier work, Chomsky (1976b: 47) expresses a similar view, while discussing cases in which it is implausible to

assume that speakers have been taught a principle *P*, or that they acquired *P* by induction from experience:

"Note that in such cases as these we may plausibly postulate that *P* is a property of universal grammar on the basis of investigation of a single language. There is no paradox here. The argument rests on the alleged fact that something is known without relevant experience so that knowledge must be attributed to the language faculty itself, a faculty common to the species. Deep analysis of a single language may provide the most effective means of discovering nontrivial properties of universal grammar."

40. Chomsky's view that language universals can be successfully studied on the basis of data from a single language, is of course not generally accepted. The work done by Greenberg (for example, (Greenberg 1963 a, b), and the Stanford group (Greenberg *et al.* 1978)) is based on the assumption\* that insight into linguistic universals can only be obtained through the analysis of a wide variety of languages.
41. The developmental history of binding theory thus provides support for Coopmans' (1981) claims about the role of data from languages other than English in the development of Chomsky's linguistic theory. In his review of (Comrie 1981) - a work which is critical of Chomsky's approach towards the study of linguistic universals - Coopmans cites numerous references to show that data from languages other than English did in fact influence the development of Chomsky's linguistic theory.
42. Cf. also the references cited in (Coopmans 1981).
43. Cf. also Chomsky's (1975a:33) comments on the corroboration of the hypothesis that the principle of structure-dependence is not learned, but forms "part of the conditions for language learning".

44. Cf. Chomsky 1981b:55 for further detail on this parameter.
45. Cf. § 2.2 above for a more detailed exposition of Newton-Smith's views on minimalist accounts of beliefs.
46. Note that the adequacy of Laudan's and Newton-Smith's models of scientific rationality will also be critically appraised in § 7.4 on the basis of the findings of this study on Chomsky's rationality.
47. Note that this distinction actually comprises two separate distinctions: a distinction between core and periphery, and a distinction between unmarked and marked rules and constructions. To a very large extent, these distinctions fall together. That is, what is in the core is unmarked, and what is in the periphery is marked. However, it must be kept in mind that the two distinctions do not coincide completely. For instance, Chomsky (1981d:127) mentions the possibility that within the core grammar itself certain choices of parameters may be marked relative to other choices. For the purposes of the present discussion the focus will be exclusively on those cases where the distinction between core and periphery and the distinction between unmarked and marked rules and constructions fall together. The fact that considerations of markedness also enter into core grammar only provides further support for the arguments in the text concerning the problematic status of the distinction between unmarked core and marked periphery.
48. The other three fundamental principles of Chomskyan generative grammar distinguished by Botha (1981:433) are (i) the principle of ontological realism, (ii) the principle of phenomenological rationalism, and (iii) the principle of methodological generality.
49. Cf. chapter 1 above for the distinction made in the present study/ . . .

study between *Chomskyan* linguistics and *Chomsky's* linguistics.

50. The principle of restricted formal power also instantiates epistemological empiricism, in so far as the need for restricted formal power is justified on the basis of the facts of language acquisition.
51. The assumption that markedness claims must be tested and justified, is also (explicitly or implicitly) made in (Chomsky 1978a:13, 1981d:127, 141; 1982a:110).
52. Cf. also Chomsky's (1978a:13) brief reference to data on child language and creole languages in connection with the justification of claims about the core, the periphery, and markedness. Cf. also Chomsky's (1981d:141) suggestion that markedness claims make predictions about language acquisition.
53. Cf. § 2.3.4.8 above for Laudan's and Newton-Smith's views on the use of *ad hoc* protective devices.
54. Consider for instance the various contributions to (Belletti *et al.* 1981).
55. Van Riemsdijk (1978:260ff) also emphasizes the role of a theory of markedness in accounting for cross-linguistic variation.
56. A precondition for progress in this area is the development of adequate auxiliary or bridge theories. The task of such bridge theories is to relate the linguistic competence of speakers - as described by linguistic theories - to the external linguistic objects or phenomena from which external linguistic evidence is derived. Cf. Botha 1981:32f for some discussion of the nature of bridge theories, and of current problems with the development of such theories.

57. Cf. § 2.3.3.3.2 above for a brief explication of Newton-Smith's (1981:227) views on fertility as a factor in theory appraisal.
58. The Preface to (Sober 1975), and the references cited there, give some indication of the variety of approaches adopted towards the role which simplicity plays in scientific theories.
59. Cf. Sober 1975:chapter 3 for such an analysis of one feature of Chomsky's linguistics, namely the role of an evaluation measure in phonology. Sober tries to show how the latter notion fits in within his general analysis of metatheoretical simplicity.
60. In addition to the references cited below, cf. also the reaction by John Morton (1980) and David Rosenthal (1980) to (Chomsky 1980c).
61. Cf. for example, Rudner 1961:11, Hesse 1974:185 for references to works in which Galileo and Einstein expressed such beliefs. Note, incidentally, that there is also some difference of opinion on the exact views held by Galileo about simplicity in science. Consider in this connection, for example, Finocchiaro's (1980:248ff) criticism of Clavelin's analysis of Galileo's claims about simplicity. The same is true for Einstein's views, as indicated by Newton-Smith's (1981:230) claim about the relative complexity of general relativity. Cf. also Bunge's (1961:141) account of the factors which played a role in the acceptance of Einstein's gravitation theory. Bunge claims that, in spite of Einstein's own statements about the value of simplicity, considerations of simplicity have not played a prominent role in the choice of Einstein's theory. Bunge points out that the *complexity* of this theory has in fact been regarded as a problem by some, and has motivated the

search for theories "which are syntactically, epistemologically, and pragmatically simpler".

62. In addition to the references cited below, cf. for example also Rudner 1961:111 and Bunge 1961:121 for criticism of this thesis.
63. Cf. Popper 1968:chapters 6, 7, and 10.
64. Cf., for example, Rudner 1961:110-111 and Hesse 1974:223ff for alternative classificatory schemas for the various types of metatheoretical simplicity. By selecting Bunge's classification for use in the text, I do not wish to claim that his is the best. His schema is used for illustrative purposes only. The only point I wish to make in the text is that the notion of metatheoretical simplicity is sufficiently complex that some clarification of Chomsky's views is required.
65. Cf. Bunge 1961:129ff for a discussion of how the various types of simplicity relate to the various other properties of theories used in theory appraisal.
66. Consider in this connection also Goodman's (1961:151) claim that "brevity" is not a reliable test of general simplicity. According to him, "since we can always by a calculated selection of vocabulary, translate any hypothesis into one of minimal length, the simplicity of the vocabulary must also be appraised".
67. In § 7.4 below I return to the question of whether Chomsky's views on the role of simplicity in theory appraisal have really changed.

68. Cf. § 6.3.3 above for a discussion of the problems in question.
69. Cf. § 2.3.3.2 above for a brief explication of Newton-Smith's views that the distinction between the observational and the theoretical is a matter of degree.
70. Cf. the references cited in footnotes 44 - 48 for such criticism.
71. Cf. § 7.2.2.3 above for details on the general principles which underlie the various conceptual problems.
72. Note that Laudan has challenged some of the criticisms levelled at him on the grounds that his model requires reference to truth. For instance, Laudan (1982) rejects Sarkar's (1981) criticism that Laudan's notion of a problem solution inevitably requires reference to truth, and that Laudan's opposition to inconsistent theories only makes sense on the assumption that the scientist aspires to the truth. Laudan's counterclaim is that his notion of a problem solution and his opposition to inconsistent theories require that theories have truth values, a point he has never denied. Consider in this connection the discussion below on the distinction between semantical instrumentalism and epistemological instrumentalism. The criticism regarding the ontological component of a research tradition requires a stronger assumption than that theories have truth values, and to my knowledge Laudan has not answered the criticism regarding the role of an ontological component in his model.
73. For some recent remarks by Chomsky on the realist interpretation of linguistic theories, cf., for example, Chomsky 1981a:18, 23-24, 106-107, 189-192; and his (1980d) "Author's response" to (Harman 1980). Cf. Botha 1981: §§ 6.3.2.3.4 and 6.4.2.2 for a brief overview of the realistic status of linguistic theories. Cf. also Katy 1981 for discussion of the ontological status of linguistic theories.

74. One of the examples of theoretic intuitions provided by Botha (1976), is Chomsky and Halle's (1968:31) judgment that a grammar containing both of the two formulations below clearly misses a generalization, "for the obvious similarity between the two cases is not expressed":
- (i)  $V \rightarrow [\bar{1} \text{ stress}] / \underline{X} \text{ \_\_\_\_\_\_ } C_o(W) + \text{affix}$
- (ii)  $V \rightarrow [\bar{1} \text{ stress}] / \underline{X} \text{ \_\_\_\_\_\_ } C_o(W)$
75. Cf. § 2.4 above for some background on this style of inquiry.
76. Botha 1982a:13 claims that epistemological tolerance is also manifested in Chomsky's (1970) study of English nominalizations.
77. As noted in § 3.2.2, this condition was a direct predecessor of the SSC and TSC.
78. Newmeyer's (1980:24-25) exposition of Chomsky's (1957) argumentation against phrase structure grammars, and in favour of transformational grammars, highlights the role which considerations of simplicity played in this argumentation.
79. Cf., for example, Botha 1971, 1976 for evidence on Chomsky's early reactions to counterevidence. Cf. also the references cited in this connection in § 7.4 above.
80. This criticism of Lightfoot's account is also raised by Botha (1984). Botha (1984) also criticizes Lightfoot's account on issues which do not directly relate to the issues dealt with in the present study.
81. Cook's interpretation of Chomsky's metascientific comments can also in some instances be questioned. For example, in

his explication of the following passage from (Chomsky 1979a:188) Cook accepts that Chomsky is specifically referring to very general hypotheses, or hypothesis-types. I find no evidence in this passage, or in the discussion of which it forms part, for this view.

"'Methodologists' sometimes assert that a counter-example serves to refute a theory and shows that it must be abandoned. Such an injunction finds little support in the practice of the advanced sciences, as is well known, virtually a truism, in the history of science. The willingness to put aside the counter-examples to a theory with some degree of explanatory force, a theory that provides a degree of insight, and to take them up again at a higher level of understanding, is quite simply the path of rationality. In fact, it constitutes the precondition for significant progress in any nontrivial field of research."

## Chapter 8

### SUMMARY AND CONCLUSIONS

The main aim of the present study has been to provide answers to the following two related questions.

- (1) a. Is theory appraisal within Chomsky's linguistics rational? That is, are the choices made by Chomsky among different versions of his linguistic theory rational choices?
- b. If so, wherein does this rationality lie?

In terms of the framework adopted in § 2.2 above, there is no single, simple answer to the question in (1a). In (2) below four conclusions reached above about the rationality of theory appraisal in Chomsky's linguistics are presented. These conclusions jointly provide an answer to the question in (1a).

- (2) a. Theory appraisal within Chomsky's linguistics is rational in that, given the choice between two versions of his theory, Chomsky chooses that version which is "best" in terms of his goal and his principles of theory appraisal. That is, Chomsky's actual choices are minimally rational.
- b. Chomsky's methodological beliefs - that is, his beliefs about the goal of linguistic science and the principles of theory appraisal - are not in all respects minimally rational. That is, as regards the whole of these beliefs there are doubts about the reasonableness of his simultaneously holding all the beliefs.
- c. When analyzed within the framework of the model of scientific rationality proposed by Newton-Smith (1981), theory appraisal within Chomsky's linguistics is maxi-

mally rational. That is, Chomsky's linguistics has the same goal as that specified in Newton-Smith's model and employs the same principles of theory appraisal - with the possible exception of Chomsky's use of considerations of simplicity in theory appraisal.

- d. When analyzed within the framework of the model of scientific rationality proposed by Laudan (1977), theory appraisal within Chomsky's linguistics is not maximally rational. Since Laudan's model has been criticized on independent grounds, it can be concluded that the conflicts between Chomsky's rationality and Laudan's model of scientific rationality reflect shortcomings of Laudan's model.

The question of what constitutes rationality in Chomsky's linguistics also has no simple answer. In essence, rationality consists in choosing the best available version of linguistic theory, where the "best" version is the one most likely to bring one nearer to truth, the ultimate aim of linguistic inquiry. In actual fact, the choice between two versions of the general theory of language is fairly complicated. In (3) below the main conclusions reached above about theory appraisal, and specifically the choice between two versions of UG, are summarized.

- (3) a. Given a choice between two versions,  $T_x$  and  $T_{x+1}$ , of linguistic theory, choose that version which is the "best", where the relative merits of  $T_x$  and  $T_{x+1}$  are determined by one or more of the following factors: (i) restricted formal power, (ii) explanatory and predictive success with respect to specific grammars (that is, descriptive adequacy), (iii) metatheoretical simplicity in the sense of nonredundancy, (iv) metatheoretical simplicity in the sense of a limited number of stipulations, (v) unifiedness, in the sense of generality, (vi) deductive depth, (vii) naturalness as principles of mental computation, (viii) absence of

inconsistencies/ . . .

inconsistencies, (ix) degree of compatibility with the autonomy thesis.

- b. The choice between two versions of linguistic theory is not completely rule-governed. Non-rule governed judgment enters into the handling of conflicts among the various specific principles of theory appraisal, and into the application of some of these principles.
- c. Empirical success is not only a determinant of the merit of some version of the theory of UG. It also provides a test for the correctness of employing specific conceptual factors in the appraisal of linguistic theory.

Theory appraisal within Chomsky's linguistics is further complicated by the fact that Chomsky employs rhetorical tricks in cases where he made rational theory choices.

The main contribution of the present study towards an understanding of Chomsky's linguistics has two aspects. On the one hand, the present study provides a detailed account of the complexity of recent theory appraisals within Chomsky's linguistics. It is not only the case that a great variety of empirical and conceptual factors determine the relative merit of different versions of linguistic theory, but it is also the case that theory appraisals, and consequently theory choices, within Chomsky's linguistics are not completely governed by precise rules. That is, there is no complete algorithm for theory choice. Theory choices within Chomsky's linguistics are to some extent based on non-rule governed judgment. On the other hand, the present study shows that within the context of current theorizing on scientific rationality, theory appraisal within Chomsky's linguistics can be regarded as rational. Specifically, theory appraisal within Chomsky's linguistics fits in very well with the model of scientific rationality proposed by Newton-Smith (1981). To the extent that Newton-Smith's model provides an adequate ac-

count of theory appraisal within the natural sciences, this study indicates that, as regards theory appraisal, Chomsky approaches the study of language in the manner of the natural sciences. The present study also highlights the need to take into account recent work done within the philosophy of science when an attempt is made to appraise certain aspects of Chomsky's method. For instance, Popperian or Lakatosian falsificationism can no longer be regarded as providing an adequate framework for the appraisal of aspects of Chomsky's method.

Viewed from the perspective of the study of scientific rationality in general, the present study has highlighted some of the differences between two recent models of scientific rationality, namely, that of Laudan (1977) and that of Newton-Smith (1981). It was shown that Laudan's model conflicts with Chomsky's rationality on several points on which Laudan's model have been criticized on independent grounds. The present study thus provides additional evidence for questioning the correctness of certain of Laudan's claims. At the same time, the present study showed that theory appraisal within Chomsky's linguistics is similar to the account of theory appraisal contained in Newton-Smith's (1981) model. The present study thus provides some support for Newton-Smith's model.

It must be borne in mind that the present study focussed almost exclusively on highly specific choices made by Chomsky in connection with his own linguistic theory. For a more general account of theory appraisal within Chomsky's linguistics, one would also have to examine the choices made by Chomsky between his version of linguistic theory and alternative theories, and the choice of his particular approach to the study of language in preference to other, alternative approaches. As evidenced by the argumentation in (Chomsky 1980a, 1980c), the latter choice is still regarded as an important issue by Chomsky.

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