Chapter IV

Language Structure, Linguistic Capacities and the Evolution of Generative Grammar from Formalism to Biologism

In this chapter, we examine in what sense, if any, theories of language structure and grammars of particular languages may be considered to be theories of human linguistic capacities and abilities, and we chart the succession of approaches made by generative grammar in attempting to privide instances of such theories. The interest accruing to this issue stems not from any desire to stipulate disciplinary boundaries between linguistics and psychology (or biology), but rather lies in bringing to the forefront of discussion the metatheoretical goals and assumptions -- especially in regard to the 'autonomy' view of a purely formal and non-semantically based syntax -- motivating the widely-held belief that linguistic theory is perforce a branch or subdicipline of theoretical (cognitive) psychology. [1]

We begin by considering some general problems with ascribing capacities and abilities, and with the conceptual difficulties encountered in speaking of theories of capacities and abilities and of the relation of these to behavior under one or another description. Although the relation between a capacity and its manifestation or exercise in behavior remains problematic, we conclude that only well-articulated theories of capacities and abilities --e.g., as (hierarchically) organized behavior -- warrant systematic ascriptions. In respect to the linguistic

[1] For example, Chomsky speaks of an approach to the study of language which is "a branch of theoretical psychology" whose "goal is to exhibit and clarify the mental capacities that make it possible for a human to learn and use a language" (1972a:114).

ability of speakers of a language to 'recognize' certain word sequences as well-formed and not others, an ability extending to arbitrarily many word sequences that a given speaker has not previously encountered, the proposal is examined (§2) that a grammar systematically and compositionally characterizing 'all and only' the intuitively well-formed word sequences of a language can be taken as a theory or rational reconstruction of this ability. Here we find that illicit analogies with the theory of formal systems have resulted in a misleading emphasis on the notion of a "rule of grammar". Taken as applying to entire classes of lexical items of some traditional (Noun, Verb, etc.) category, this notion appears to have but limited validity, due to a completely general lack of uniform applicability of syntactic generalizations ('rules') to different lexical items within the specified category. This finding underwrites the view that sentence structure, as articulated by an empirically adequate grammar capable of accounting for the range of occurrences (distribution) of each lexical item, may ~~be comprised~~ consist of a structure of word dependences; the elaboration of this proposal is deferred until Chapter 5 §§ 1 and 3, and Chapter 6. Finally, in §3, we trace how the evolution of generative grammar, under the impetus of developing an explanatory account of child language acquisition, has in effect abandoned its prior concern to provide a rule-governed description of 'all and only' the sentences of a language. With this development, the claim that linguistic theory is to be subsumed by cognitive psychology or the neurobiology of the future no longer rests upon the

generative methodological goal, but upon certain inchoate views as to the central notions of 'language' and 'grammar', supported by a so-called 'plausibility argument' concerning the character of empirical data available to the child language learner. In pursuit of "explanatory adequacy", generative grammar has been led to formulate its claims about innateness on the basis of a limited range of linguistic data (so-called "sample facts") accumulated in various languages. However, when we examine representative arguments in support of innateness claims concerning data in even one language, we find that the posited innate constraint supposedly governing the form of the described data is, as shown by a concerted effort to determine the domain of the constraint by delineating its coverage, at best a restriction which pertains only to the "sample data" under consideration. And, as several examples of this argumentation show, despite the concern to forward explanatory hypotheses, questions may be raised as to whether the posited restriction in fact correctly describes even the selected data; pending such demonstration, the too-ready interpretion of "sample facts" as evidencing the presence of an innate constraint on grammatical form is simply not credible. It may be concluded that generative grammar has not presented a persuasive case for its conception of the disciplinary standing of linguistic theory and the nature of language structure.

4.1 On Ascriptions of Capacities. Before addressing the question as to what kind of theories are or can be theories of linguistic capacities, we would do well to first direct our attention to an explication of the term 'capacity' and to an overview of some traditional problems associated with capacity ascriptions. Given the tendency of some recent writers in the philosophy of psychology to use the terms 'capacity' and 'disposition' interchangeably,[1] it seems useful initially to draw some systematic distinctions. To be sure, there are presystematic distinctions which might be drawn on the grounds of common usage. For example, it seems distinctly odd to say that sugar has the capacity to dissolve in water; after all, we will hardly allow the implicature that, although having such capacity, sugar might 'decide' not to dissolve in water. Still, since it is perfectly admissible to say water has the capacity to dissolve sugar and salt but not oil, it is not immediately obvious how to separate the two cases. Other examples of what may pass as ordinary usage are somewhat more perspicuous for the purpose of presystematic clarification. One can, e.g., say that the president has the capacity to act as head of state; less readily, that he is disposed to do so. Similarly, that a photosynthesizing plant is disposed to orient its growth towards the direction of a light-source rather than that it has the capacity for directional growth. Such differences in usage might, more systematically, be accounted for [by way of reference] to the (problematic) analysis of dispositional statements as supporting

[1] E.g., Cummins (1983:28 fn.1). This is understandable since the case Cummins offers for "functional interpretive analysis" as a legitimate scheme of psychological explanation turns on the distinction between flow-chart programs and subsumption under causal laws. The flow-chart model, making no reference to laws or lawlike statements, thus encourages the reversion to the traditional, pre-Carnapian synonomy of these terms.

counterfactual or hypothetical conditionals. For our purposes, however, it may suffice to adopt a distinction available in the literature.[1] To say that A has the capacity (or ability[2]) to x is to say that there are conditions (which need not be specified) under which A does x (A x's). Whereas to say that A is disposed to x is to require both that the relevant conditions be specified and to affirm a lawlike statement that, when these conditions obtain, A x's (does a bit of x-ing). Chomsky's aversion to dispositional accounts of language[3] are, of course, rooted in his scepticism

1 Danto and Morgenbesser (1957:502) cited by Chomsky (1980a:255 fn.4). Cf. Hempel (1965:457 ff).

2 Chomsky has used these terms synonomously in the past in referring to what a grammar, as a competence theory, describes. More recently, with the postulation of modular 'competences', he has sought to sever the association with the term 'ability'; see § 4.3 below.

3 The notable protagonist here, certainly, is Quine; see Chomsky (1967a:10), (1969b:267-8)and(1969c:315-6). This has not dissuaded Chomsky, following Leibniz in this regard, from employing the term 'disposition' to characterize rationalism's view of a priori knowledge with which he is in general agreement: "...rationalist speculation has assumed that the general form of a system of knowledge is fixed in advance as a disposition of the mind ..." (1965a:51). As he points out at some length in polemics with Quine, the dispute over dispositional accounts of language concerns not the term 'disposition' but rather Quine's definition of language as a system of dispositions to respond, where this may be construed as involving such notions as the probability of the occurrence of a particular utterance in a given situation (1969b) and (1975c:194-204). However, Chomsky is clearly uneasy with the term 'disposition' as referring to the principles of Universal Grammar. These are not to be construed as dispositions to speak in ways that are in accordance with them, but as " 'disposition(s)' (if one insists on this term) to acquire a certain competence (i.e., a certain cognitive structure, a grammar, knowledge of language)" (1975c:222). The hesitance to use the term 'disposition' to refer to what is otherwise held to be "the child's innate predisposition to learn a language"(1965a:25) is in line with the familiar analysis of dispositions as involving reference to lawlike statements, which are admittedly not part of the account Chomsky offers. Hence, it would seem, the preference for the term 'capacity'.

also parallel use of term 'habit' by Hockett & others?

(expressed most forcefully in his well-known review of Skinner's Verbal Behavior) that any such statements (or any such statements which are 'interesting') can be discovered which will lawfully associate particular stimulus conditions of the appropriate, physicalistic kind with particular verbal utterances.[1]

Whereas on the analysis just presented, to say that a person has a particular capacity or ability is to say no more than that there are (unspecified) conditions under which he exhibits the relevant behavior. In particular, it is not to say that if these conditions do obtain, then the relevant behavior will be manifested. And this means that A's x-ing behavior may only be a sufficient but not a necessary condition for the ascription to A of a capacity or ability to x. Under this construal, therefore, capacity and ability ascriptions are non-disconfirmable (Danto and Morgenbesser, op.cit).

At this point we can see the epistemological problems that capacity ascriptions in general occasion in view of the difficulties which confront the legitimate demand that some evidential warrant be provided for such ascriptions. These are, in the main, two. On the one hand, in the admission that behavior may provide even a sufficient warrant for capacity ascriptions, we run into the immediate difficulty (as Locke was well aware, see p.207 fn 1 below) that since we can posit the existence of capacities, indeed innate capacities, for everything we actually do succeed in doing, the notion of capacity holds little interest. For

[1] However, if 'engaging in the normal use of language' is unpacked, as it often is in discussions of "linguistic creativity", e.g.,Chomsky (1972a:11-13) as involving the speaker's ability to produce "appropriate" and "coherent" utterances, it would seem that these latter notions, if not a mere façon de parler,do or may involve reference to lawlike statements. In general, it is incumbent upon any account of "linguistic creativity" to specify in what "appropriateness" consists (a point stressed to me by M. Gottfried), an obligation not met in generative grammar (§ 4.3 below. We address this issue under the head of 'discourse' and 'sublanguage' in chapter 6.

if A does a bit of x-ing, A is certainly capable of x-ing and, a fortiori A may be said to have the capacity to x. [1] There would thus appear to be as many capacity ascriptions to A as there are descriptions of A's behavior, leaving the notion of capacity an idle wheel, turning nothing. And, on the other hand, how can capacities be significantly ascribed if it is maintained that there need be no evidential warrant sought in the behavior of one said to possess the capacity? Do we not then run the risk of subscribing to a doctrine of occult powers of mind?

With regard to difficulties of the former kind, it appears requisite -- in order to salvage significance for capacity ascriptions -- to stipulate that only certain aspects or instances of behavior 'count' as the exercise of the capacity in question, i.e., may be held as warrant sufficient for one to be said to possess the capacity. We can immediately appeal here to 'clear' cases: Jan's bicycling and Jean's pastry-making. But what about John, sitting at his desk? Which aspects of his behavior 'count' as the exercise of capacities, and which ~~ones?~~ capacities do they exemplify? Short of a 'natural kinds' taxonomy of behavior, which prima facie simply begs the question at issue, it seems most unlikely that behavior can be partitioned into what is and what is not the exercise of particular capacities. But need it be assumed that the exercise of a capacity be manifested as observable behavior at all? [2] For if it is not even intuitively clear what may count

[1] Cf. the Scholastic principle Ab esse ad posse valet consequentia, cited by Geach (1957:15).

[2] Cf. Leibniz (1765:52):"This is how ideas and truths are innate in us -- as inclinations, dispositions, tendencies, or natural potentialities, and not as actions; although these potentialities are always accompanied by certain actions, often insensible ones, which correspond to them."

as the exercise of a capacity and whether this must satisfy the evidential criterion of being observable behavior, then it is certainly not apparent how capacity ascriptions can be legitimated by reference to their exercise. Of course, the term 'observable behavior' is a loaded one, for its meaning is contingent upon a decision as to the character of permissible language employed in descriptions of behavior, a decision laden with assumptions about the correctness of particular theories or points of view.[1]

At this point we seem caught in a quandary. Although admittedly non-disconfirmable, we have sought to elucidate how capacity ascriptions can nonetheless retain significance, i.e., be non-trivially true or false. Certainly there are capacities whose ascription is warranted by behavioral manifestations which can non-problematically be taken as their exercise. But not all behavioral manifestations can serve as license for capacity ascriptions, if these are to be made non-trivially. However, in the effort to be more specific about which behavioral aspects ~~are accountable~~ for as the exercise of particular capacities, we have run into the difficulty that decisions about the very language used to describe behavior already implicate the capacities whose exercise the so-characterized behavior is stipulated to be. And this seems uncomfortably circular.

[1] Cf. Taylor (1964:96):"...the assumption that a certain language is the data language is precisely the assumption that one theory must necessarily be the correct one,..."; a point made in the context of a discussion of the severe inadequacies of behaviorist and operationalist strictures which do not allow a distinction between "directed behavior" (or "action"), due to the attendant implication of hidden 'inner' causes, and "mere movement".

Confronted with an inability, at this point, to clarify the precise nature of the evidential relation between a capacity and its exercise, can we not therefore urge that no such relation is, in fact, required and that a capacity may be considered distinct from its exercise? Philosophers of staunchly empiricist persuasion, self-consciously following more Newtonian percept than Newtonian practice,[1] have traditionally sought to debunk appeals to such 'occult forces' as unactualized powers, capacities and dispositions. Hume, whose scepticism in its strongest form most strongly expressed questioned that we can have any legitimate notion of 'power' at all,[2] nonetheless also argued that that such a distinction is altogether without foundation:

> the distinction, which we sometimes make, betwixt a power and the exercise of it, is simply frivolous, and...neither man nor any other being ought ever to be thought possesst of any ability, unless it be exerted and put into action. [3]

1 Cf. Heilbron (1982), esp. 38-47; Heimann and McGuire (1971); Koyré (1965:Appendix C, 149-171).

2 Treatise, Bk.I, Pt. III, Sect.XIV (Selby-Bigge ed.,p. 161) "All ideas are deriv'd from, and represent impressions. We never have any impression, that contains any power or efficacy. We never therefore have any idea of power."

3 ibid., Bk. II, Pt.I, Sect. X, p. 311. To be sure, Hume goes on to qualify his remarks somewhat for "the philosphy of our passions": "But tho' this be true in a just and philosophical way of thinking, 'tis certain it is not the philosophy of our passions" (op. cit.) For our passions are indeed influenced by considerations of powers that are never exercised (e.g. our fears stemming from others' power to do us harm, a miser's delight in the power his riches represent). Yet these anticipations are only "illusions of the fancy" unless rooted in the consideration that "power always has reference to its exercise, either actual or probable, and that we consider a person as endow'd with any ability when we find from past experience, that it is probable, or at least possible he may exert it"(313). The illicit distinction would therefore seem to lie between a power and its probable exercise.

But just as, according to Locke, a man may be said to have the capacity of assenting to many truths which he will die in ignorance, so it may be held that capacity ascriptions can be true or false even if the capacities are never exercised. Thus it can be maintained that A may be truly said to have the capacity to climb Mt. Everest although in fact he never does so (because, for that matter, he never leaves Kansas). In general, however, we usually have good reasons for being suspicious of proclaimed abilities and capacities for which we otherwise (pace Hume) lack what may be considered as sufficient confirming evidence. For clearly we would not normally say that John knows French or John has the ability to converse in French unless there is some warrant in John's linguistic practices to which we or some reliable informant are privy. Similarly to say that John can solve this problem, if sincerely affirmed, is usually taken to imply that the speaker has some knowledge of John's problem-solving facility, ultimately based upon reports or observations of proficient problem-solving by John. And this brings us to another point: capacities are typically specified by their exercise, by an account of what it is that the possession of a capacity enables an

[1] In view of the discussion above, Hume's empirical strictures tying capacity attributions to the evidential conditions of their (probable) exercise contrast somewhat with Locke's remarks (Essay, Bk.I,Ch.I,Sect. 5) which suggest that there is no distinction gained between the notion of capacity and that of innate capacity, since we may be said to have innate capacities for whatever we in fact do (see Atherton (1983:232-3). But Locke's point is not the evidentiary requirement for capacity ascriptions, but rather that any capacities can be explained coherently in terms of innate ideas and principles. As Atherton points out, this is not to deny that anyexplanations of capacities need be given.

agent to do.[1] How then can it be maintained that some capacities can be correctly ascribed in the absence of a demonstration or even a probable demonstration of their exercise?

It may be interjected here that we have overlooked the qualifying term 'normal' which is often predicated of certain ascribed capacities: in saying that A has normal abilities and capacities, we are saying only that A is capable of acting in a manner which conforms to or is consonant with what is non-problematically held to be within the province of normal human behavior and accomplishment. And, since we are here alluding to capacities which are characteristically human, we are really referring to second-order capacities.[2] Thus to say of a newborn infant (if normal, etc.) that it has the capacity ("power") to 'acquire' a language (i.e., to speak and understand a language) is to make an assertion concerning a species-wide capacity rather than about the potentialities of this particular baby. It is an assertion rooted not in any observations of the behavior of this child (other than those leading to the conclusion that it is a normal child) or indeed of any particular child but only in the fact that humans are characteristically language users. There is, naturally, an equivocation in the term 'characteristically'. Humans characteristically do many things (and have many abilities) which are not 'characteristically human': walking, using tools, counting (?), tying their shoes (?), etc. And there is a famous controversy whether the use of language is a species-specific

[1] Cf. Baker and Hacker (1984:323).

[2] As Broad (1925:437) recognized, this is a typical jumping-off point for mentalism: "Mental substances seem to start mainly with powers to acquire other more determinate powers. A baby does not have the power to talk or to reason, but it has the power to acquire these powers if proper stimuli are applied."

property of humans alone. There would seem to be reason to view with some suspicion the interest which may accrue to such traditional anthropocentric concerns as determining what is and what is not part of 'essential' human nature, what is and what is not an exclusively human preogative.[1] But there is another query which is of greater import for the moment: if some (in particular, second-order) capacity ascriptions are licensed simply on grounds that the capacities in question are characteristically human, then they appear to be merely definitional of what it is to be human. Does this not fall under the opprobrium of being virtus dormitiva explanation?

For it may well appear that such attributions of capacities, like those of dispositions, powers, faculties and the like, are prima facie subject to Molière's parody of Aristotelian explanation: to explain A's x-ing (or how A can x) by remarking that A, being human, has the capacity to x (or, a second-order capacity to acquire the capacity to x) seems as idle and question-begging as accounting for the fact that an iron bar attracts nearby iron filings because it is disposed to do

1 Cf. the remarks of the eminent neuroscientist Geschwind (1984): " I have the feeling that somehow the last bastion of uniqueness of the human is, in the minds of some, his possession of language, so that one finds the events of the first part of Genesis being revived among distinguished scholars. Yet we must be able to face the fact that perhaps this last fortress of human uniqueness may also fall. Perhaps we should be cautious about assuming that language will take the place of the soul in keeping us in a special position, different from that of the beasts of the field"(38).

so. [1] Ascriptions of capacities, whether characteristically human or not, are like dispositional predicates in that they do not suffice for explanation; rather they constitute a task for explanation. But because capacities differ from dispositions precisely in the fact that the latter are stipulated to require analysis involving a lawlike (and hopefully true) statement, capacity predications differ from dispositional ones in the manner of their insufficiency. To see this, it may be helpful to first briefly consider some remarks about the insufficiency of dispositional explanations.

Broad contended, some 60 years ago, [2] that dispositional predicates in the physical sciences only acquired their license in the form of empirically confirmed theories of the microstructural character and properties of the objects of which dispositions are predicated. In this context he noted that it was the association of physical dispositions (e.g., "is magnetic") with microstructural properties that occasioned the demise of medieval "faculty physics". [3] Percipiently, he further

[1] Mill (1833-6:996) provides a particularly clear formulation of the objection: "(I)t rests with the believers in an entity per se bearing that name, to produce some proof of its existence. Until they do so, their opinion can only be held to be a lingering remenant of the Scholastic doctrine of occult causes; the very absurdity, in fact, which is so happily ridiculed by Molière, when he makes one of his pedantic physicians account for the fact that "l'opium endormit" by the maxim "parcequ'il a une vertu soporifique"."

[2] Broad (1925), chapter 10 "Traces and Dispositions".

[3] ibid., 434: "It is characteristic of modern science as contrasted with medieval science to correlate causal properties with minute spatial or spatio-temporal structure, and not to take them as ultimate facts."

observed that as long as psychological dispositions analogously lacked such specifications and associations to theories of internal structures, the prospects for advancing beyond a "faculty psychology" remained rather dim, an assertion which is tantamount to a ringing denial of the explanatory value of straightforwardly functional explanations.

However, Broad's strictures upon the kind of explanation that can legitimately serve as accounting for, and justifying, dispositional predicates may be considered too severe. According to a more recently advanced "placeholder" view of dispositions,[1] not every dispositional predicate need be associated with a microstructural theory as a license (a base or basis) for its ascription to an object; what may be considered the license or basis of a disposition is a matter of what is entertained at a particular time to be a satisfactory and empirically warranted theory which addresses agreed-upon explanatory goals.[2] It may even be maintained, on this view, that dispositional ascriptions can function

[1] Levi and Morgenbesser (1964); see also Levi (1980:237-44). As placeholders, dispositional predicates are in a fundamental respect like ceteris paribus clauses (1964:229) in that explanations containing them are extendable,i.e., asking for further explanation is legitimate. However, unlike ceteris paribus clauses, a particular dispositional predicate may entail a commitment to the kind of predicates (e.g., physical, chemical, biological) which can replace it in each of the laws in which it occurs.

[2] Ibid., 230: "The basis (if any) of a disposition is the set of conditions which are specified by that description which we are entitled on theoretic and empirical grounds to substitute for the disposition predicate." Cf. Levi (1980:238):"What qualifies as a fully acceptable explanation depends on the state of inquiry and the program for explanation to which the investigators are committed." Recent attempts to flesh out more fully what has been called the "pragmatics" of explanation are made by Garfinkel (1981) and van Fraassen (1980), chapter 5.

in the conduct of inquiry without provision of a basis, whether this be specified by a microstructural theory or no, as long as their provisional and ad tempore character is not obscured. Accordingly, dispositional predicates can be classified on the grounds of their relation to a basis: those (e.g., "is magnetic") for which a legitimate base has been found (termed "mystery and problem solving"), those lacking legitimate bases for which it is yet alleged that such exist ("mystery-making"), and those both lacking legitimate bases and the claim for them ("problem-raising").

The intent of the placeholder view of dispositional predicates is to expressly recognize that theories containing them can and do play a role in scientific inquiry, even when no further explanation is immediately forthcoming for the regular connection of events in which the disposition is held to be manifested. [1] Can this liberality be extended to non-presystematic talk of capacities? What may be the basis or alleged basis for capacities? What sort of theory can be a theory of a capacity? How are capacities to be accounted for?

It seems obvious that a theory which is explanatory of a capacity can be advanced only upon provision of a specification of that capacity, i.e., upon a reasonably convincing description of the explanandum. [2]

[1] We assume here, following Carnap (cf. Levi (1980:238)), the construal of dispositionality as compulsive or invariable; i.e., that glass is brittle (disposed to break) is analyzed as entailing that, if dropped from height h onto a hard surface, glass will break. Fortunately, this is not really invariably true.

[2] Cf. Cummins (1983:52-3):"(U)ntil recently no one had any serious idea -- i.e., no scientifically workable idea -- how to describe cognitive capacities; hence no one had any serious idea what the explanandum was. An obvious example of this is the capacity to learn a language; before Chomsky, no one knew how to describe what was learned with the kind of detail and precision that makes explanatory theory a serious possibility." Just "what is learned" and how it is to be described will be considered in §4.3 below.

But we have seen that real difficulties emerge in the requirement that a capacity be specified by its exercise if this be a requirement that the exercise be stipulated to be the observable behavior or aspects of the observable behavior of the organism. For there is a problem of determining which aspects of behavior are to be accounted as due to which capacities. And there is an overriding difficulty encountered in even trying to describe the behavior of an organism, a description which is not a given but involves the choice of a data language which implicates highly theory-internal and metatheoretical assumptions. [1] But if it is supposed that the exercise of a capacity can be taken as consisting not in the myriad events of behavior, however described, but in the demonstrable existence of recurrent patterns or structures which, in part, can be seen to characterize behavior (e.g., by having behavioral correlates) then the specification of these patterns and structures may provide a basis for the capacity in question. A theory of a capacity is not, therefore, an account of behavior itself, which would seem to require the identification of lawlike regularities between events or, more properly, between kinds of events (not all of which, of course, need be observable), but an account of these recurrent patterns and structures in terms of which (a certain form of) behavior may, grossly considered, be said to be organized. This conclusion, due to the abstract level of its formulation, can hardly be said to be very satisfactory or informative: What, for example is meant by "grossly considered" and

[1] Wilkes (1974:277):"one goal of psychological research is surely the discovery of the most perspicuous and fruitful way of describing behavior -- carving up the behavioral flux -- this is not a given, but a reputable aim for psychological theory."

by "organized"?

If we allow that the theoretical representation of an ability or capacity is not eo ipso intended as a complete specification of that behavior which, at least in part, is accounted its exercise, nonetheless there must be some facets or aspects of the behavior that comprise, or can be represented as comprising, the exercise of the capacity. Furthermore, these aspects must be representable as structural properties in the two-fold sense of determining a recurrence or pattern of events -- the prerequisite for any theoretical representation -- and that they be non-incidental, i.e., that they form a structural 'core' around which any more complete characterization of the relevant behavior must be developed. To say that the behavior is thus 'organized' is then to apparently adopt one of the following: (1) that a certain behavioral repertoire can be represented as in accord with the theory and that this accordance extends to new manifestations of behavior, i.e., not simply to those upon whose basis the theory was originally formulated; or (2) that not only does this accordance obtain but that the theoretical representation of the capacity accounts for the form of the behavioral manifestation in the strong sense that it characterizes (some of) the 'inner' events and structures that have played a role in the actual production of that behavior. Of course, for this statement to have any content, the locutions "characterize", "inner events and structures" and "played a role in " require further elucidation. But it is at least to make an assertion that the characterization

given the product of the exercise of the capacity, be taken as pertaining to, if not actually as being a theory of, the processes and structures that have produced or been involved in the production of this behavior. And this is to make a claim, traditionally associated with teleological or purposive explanations of behavior, that any explanation of the order or structure determinable in behavior -- the product of the exercise of a capacity -- is not to be separated from, or regarded as satisfactory without, an explanation of how this order comes about, granted that it does not come about 'merely' as the result of the theoretician's organization of the data of behavior. [1] The purposive character of this explanatory strategy need not be taken as imputing vital or non-material entelechies to an organism; indeed, the inner processes to which the biological mechanist refers are intended as analogous to those of humanly produced machines. [2]

[1] Cf. Taylor's (1964:17) formulation of purposive explanation: "The claim is that animate beings are special in that the order visible in their behaviour must itself enter into an explanation of how this order comes about."

[2] Broad's criticism of biological mechanism (1925:92) is instructive: "The Biological Mechanist points to the analogy between organisms and artificial machines and asks us to believe on this ground that organisms are machines. To this we answered that matter has no natural tendency to arrange itself in the form of machines (i.e., of teleological systems whose characteristic behaviour is mechanistically explicable); and that therefore, if organisms be of the nature of machines, there is no reason to suppose that they could have arisen spontaneously and without design." It should be pointed out that by "mechanistically explicable" Broad meant in terms of laws governing one level or kind of thing. One may quibble with the expression "artificial machines"; cf. Polanyi's (1968) perceptive comments on the anthropocentric character of machine analogies of natural processes.

However, there are theories of capacities formulated in a similarly purposive fashion that are intended as making claims independent of claims about particular biological mechanisms or metaphysical claims about materialism, viz., those proposed as functional analyses which utilize flow-chart programming analogies and a heuristic of assembly line production. On this view what makes something a part of a larger assemblage is the functionally specified role it plays in the input-output analysis of the larger assemblage. Thus a contemporary paradigm for capacity explanations according to functionalism is the analytical segmentation of a complex capacity C into components $c_1,\ldots,c_n$ such that the programmed (a term which in this context is ambigous between 'sequentially specified' and actually implemented as a computer program) manifestation of the c_i results in or amounts to a manifestation of C.[1] Are these programming analogy theories also to be considered as theories 'about' the inner processes and structures held to be 'responsible for' the form of behavioral manifestations?

Both critics and defenders of functional analysis have argued that descriptions of functional organization are, or should be considered, quite distinct from descriptions of whatever it is that physically 'realizes' the functionally specified states of an organism or a machine. In fact, it is precisely this point upon which rests the envisaged fruitfulness of computer or Turing machine analogies

[1] Cummins (1975).

of mental functioning.[1] Yet a recent proponent of functional analysis explanation in psychology has deemed it necessary to caution against the apparently widespread tendency to assimilate, or through the incautious employment of ambiguous terminology to conflate, functional analysis with explanation through subsumption under causal laws, a development which has resulted in "internal manuals" accounts of psychological explanation. Thus it is supposed (e.g., Fodor (1968b))that an internal representation of a program causes events to take the course specified in the program.[2] An adequate examination of these issues requires a discussion extending beyond the scope of this chapter; moreover, it is highly unlikely that these issues can be greatly clarified, touching as they do on the problematic relation between the character of structural and functional explanations, without entering upon a discussion of particular theories proposed as explanatory of particular capacities. For differing assessments of this relation may well predetermine what will be admissible as an explanatory theory.[3] In turn, a conception of the character of psycholog-

[1] E.g., Putnam (1960); for Putnam's subsequent repudiation of Turing machine functionalism, see his (1973a) and (1973b).

[2] Cummins (1983) the proponent, argues against this view that a program can be executed by a system though the program is not represented, in the system nor anywhere else (47-51). See also the criticism of the 'internal representation' view in Stabler (1983).

[3] Cf. Morgenbesser (1969:471):"...we must distinguish between the task of the programmer and explainer. The programmer may be concerned with his flow charts and may not care about the structure of possible mechanisms that realize his program; the explainer must be concerned with the actual entities whose behavior he wants to explain; a physiologist cannot dismiss our flesh and bones as of no interest to him."

ical explanation bears upon the subordinate point of whether theories of (human) capacities need be considered psychological theories at all.

However, in view of the presently unavailable neurophysiological explanations of mental functioning,[1] it may be not too much to say that the impetus for the abstract ("mental") character of functional theories is sure to continue, regardless of the various ways in which such theories can be interpreted. To be sure, another impetus for the continuing vogue of functional analysis can be located in the methodologically more precise program of computer simulation of human capacities.[2] Here, we may recall, the earliest workers expressly disavowed that any claims as to the structure of biological or neurological mechanisms followed from functional ("information processing level") explanations. This is clearly stated by the developers of one of the first 'successful' simulation programs:

> Problem-solving -- at the information processing level at which we have described it -- has nothing specifically "neural" about it, but can be performed by a wide class of mechanisms including both the human brain and digital computers. We do not believe that this functional equivalence implies any structural equivalence at a more minute anatomical level (e.g., equivalence of neurons with circuits). Discovering what neural mechanisms realize these information processing functions in the human brain is a task for another level of theory construction.[3]

It should also be recalled that the goal of such work was, and is,

[1] For some recent optimism on this score by a leading neuroscientist, see Changeux (1985).

[2] Cf. Kosslyn (1980:467-8):"One of the oft-cited uses of actually programming the computer, as opposed to merely conceiving the flowcharts or the like, is that the computer helps one discover the actual consequences of some claim and helps one discover and study complex interactions among separate components."

[3] Newell, Shaw and Simon (1958:163).

actual simulation of certain human behavior, a goal originally articulated in the enthusiastically strong terms posed by Turing's (1950) condition for machines to be intelligent. [1] Despite this unrealistic element, the claims of computer simulation researchers to be seeking a de facto functional equivalence are testable if not really empirical claims: either the specified program performs in the prescribed manner or it does not, [2] and this requirement for implementation remains as "the heart of the approach" in much of contemporary cognitive science as a check on complex theoretical accounts "positing interactions among numerous components". [3]

[1] Cf. Feigenbaum and Feldman (1963:273): "The goal of the researcher is to find an ordered sequence of these basic processes which when provided with suitable information will produce behavior indistinguishable from the behavior produced by human beings when they are provided with comparable information."

[2] Cf. Newell, Shaw and Simon, ibid., 165-6: "The heart of the approach is describing the behavior of a system by a well-specified program, ...Once the program has been specified we proceed exactly as we do with traditional mathematical systems. We attempt to deduce general properties of the system from the program(...); we compare the behavior predicted from the program (...)with the actual behavior observed ...; we modify the program(...) when modification is required to fit the facts."

[3] Kosslyn, ibid.,136-7:"There are at least five reasons for constructing a computer model of mental functioning. First, it forces one to be explicit; hand-waving maketh not a program run. Second, it helps one to consider processes in terms of a system of interacting "functional capacities"....Third, it allows one to know whether one's ideas are sufficient to account for the data. If the program runs as executed, it is a kind of "sufficiency proof"....Finally, the simulation helps one realize that the theory makes certain predictions; this is its deductive function. ...Given a complex theory positing interactions among numerous components, it is not obvious what are the predictions of the theory. Actually running the simulation sometimes produces unexpected results."

Naturally, simulation is (pace Goodman) always simulation in some respect, and not in all; we can speak of functional equivalence only if there is agreement that the target of the simulation, the behavior itself, has been adequately described. [1] And, since the behavior of contemporary machines is for the most part far from indistinguishable from that of humans (the exception being in the most highly specialized domains where it may, in fact, improve on 'normal' human abilities and performances), simulation too must be of only certain characteristics or properties of human behavior.

In this regard, the recognition by early 'cognitivists' such as Tolman, e.g., (1948), and Lashley (1951) that certain kinds of behavior should be conceived and described as hierarchically organized has not only been of fundamental interest to artificial intelligence but has also, through the seminal work of Miller, Galanter and Pribram (1960), resulted in the introduction of programming analogies into the discussion of a wide range of psychological topics, [2] including, we might add, the character of psychological explanation. In this work, notably, the view is promoted that a "complete description" of behavior should have the properties of a set of

[1] Cf. Miller, Galanter and Pribram (1960:47):"A machine cannot be expected to simulate something that has never been described -- it can be held responsible only for those aspects of behavior that an observer has recorded. No simulation is complete and no simulation preserves all the characteristics of behavior."

[2] See Boden (1979).

instructions adequate to have produced the behavior.[1] Moreover, the requirement on description is not intended merely as a functional characterization or heuristic for functional explanation; the theorist's articulation of the hierarchical organization of behavior is defined as representing a plan (or "Plan") present in the organism:

> A Plan is any hierarchical process in the organism that can control the order in which a sequence of operations is to be performed (16).

Miller, Galanter and Pribram assert not merely that an organism has (or "internally represents") a sequential and hierarchical series of "instructions" which control or guide its actions, but also that the theoretical representation of this behavior (its "complete description") is a representation of the internal instructions which have "generated" it:

> (W)e regard a computer program that simulates certain features of an organism's behavior as a theory about the organismic Plan that generated the behavior (cp.cit).

The use of the term 'generates' to refer, apparently, to internal control instructions whose operation causally produces, or is causally involved in the production of, is not fortuitious. For the substance of their proposal clearly resides in what sort of characterization can be provided for the hierarchical organization of behavior. On this

[1] "Any complete description of behavior should be adequate to serve as a set of instructions, that is, it should have the characteristics of a plan that could guide the action described (16)."

point, Miller et al are helpfully explicit:

> The traditional method of parsing a sentence is the prototype of the kind of behavioral description we demand. Noam Chomsky, in chapter 4 of his monograph Syntactic Structures (...) provides a formal representation of this kind of description, which linguists refer to as "constituent analysis" (14,fn 8).
>
> (W)e shall select the work of a single linguist and follow it slavishly. Our selection is based upon the fact that this linguist seems to agree so well with our own ideas about how human behavior in general, not merely in speech, is organized. (The agreement is not accidental, since many of our own ideas were stimulated by his example.) The linguist whose ideas we shall exploit is Noam Chomsky, and the ideas are presented summarily in his monograph, Syntactic Structures. From considerations of grammar and syntax we hope to be able to gather some impression of how complicated the planning device must be in order to generate grammatical sentences. This result should provide a sort of lower bound for the complexity of the human planning equipment in general, for nonverbal as well as verbal planning (144).

The "lower bound" of "complexity of the human planning equipment" is therefore to be provided by the model of a grammar which is required to provide an explicit specification of all and only the well-formed word sequences of a language, thereby "generating" them as grammatical sentences. Since this is the only sense of "generation" in Syntactic Structures,[1] it would seem that some further argument is required to establish that the structure sufficing to characterize a fundamental aspect of linguistic behavior -- the ability of speakers of a language to recognize word sequences as well-formed, including many to which they have not been previously exposed -- has some bearing upon the character or complexity of the

[1] See §4.2 below.

ambiguity of "generate" a frequent confusion

"planning device" whose existence and operation is presumed to be inferable from linguistic data of the grammarian. For the proposal which takes the constituent analysis of sentences as a set of instructions for "generating" certain features of linguistic behavior appears to amount to the collapse of the distinction between description in terms of conformity-to-rule and some other kind of account ('explanation'?) in terms of rule-guidance. Without this further argument, we might well wonder if nothing more than connations of 'process' occasioned by the term 'generate' are involved, as in fact is revealed in claims pertaining to the "psychological reality" of grammatical rules.

It is only in his review of Skinner's Verbal Behavior that Chomsky expressly (i.e., in published form) adopts this perspective in linguistic theory. Here it is proposed that a generative grammar, as an explicit specification of the grammatical sentences of a language, comprises an explanatory theory, not merely of "intuitions of linguistic form" but, as "internalized" by the language user, of his ability to use and understand a language. And subsequently it is argued that a theory of language structure, or "universal grammar", by determining the form of this "internalized" grammar, comprises a theory explanatory of a chld's innate capacity to acquire an ability to use and understand his native language. Before examining the justification for adopting such a goal for linguistic theory in §4.3, we first turn our attention to the task of elucidating the sense or senses in which a grammar, as above, may be considered to be a theory of a linguistic ability.

4.2 The Theoretical Representation of Linguistic Abilities. Our ruminations in the previous section suggest that talk of capacities and abilities, if intended as having more than casual or non-systematic import, should be required to have a basis or license in particular theories specifying wherein the capacity or ability, in terms of its actual or potential exercise, consists. Although this conclusion is surely a commonplace and hardly to be considered worthy of interest, genuine difficulties emerge as soon as we do inquire into the character of these theoretical representations and their interpretation. It is surely also a commonplace to require that, unavoidable conditions of idealization aside, the capacity so represented presents a reasonably approximate model or 'rational reconstruction' of what our pre-systematic assessment tells us is the character and nature of the capacity. There are corresponding difficulties here. It may, for example, be all too easy to leap from an appreciation that our theoretical representations are idealizations to the surmise that we are instead theorizing about idealized capacities, held by agents who are not flesh and blood, but abstractions, and that actual capacities and abilities are somehow degenerate or 'noise'-laden instances of these awesome powers.[1] Additional problems are encountered due to unclarities and a lack of consensus about how the terms 'model' and 'rational reconstruction' are

[1] Thus a famous passage (Chomsky (1965a:3) holds that:"Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of language in actual performance."

to be understood, and how the constraint of "reasonable approximation" is to be observed. Once again we are treading the ground of the meta-level issues concerning the character of explanations of human capacities and whether these need be considered psychological.

Various interpretations have thus accrued to the contention that a grammar is a theory of certain linguistic abilities. We observed above [1] that e.g., Hockett, among structural linguists, suggested that the linguist's task of writing a grammatical description which can, on the basis of an observed corpus, predict 'new' [2] sentences that are acceptable to native speakers of the language is "operationally parallel" to the child's task of 'predicting' or synthesizing 'new' utterances acceptable to members of his speech community. In Chapter 3 it was shown how Chomsky in his (1955a) refashions the parallel in beginning with the observation that a speaker's ability to produce utterances new both to himself and to other speakers is "a fundamental fact of linguistic behavior". To the question of whether "it is possible to reconstruct this ability within linguistic theory", Chomsky conjectures that "an account of this process of generation or projection" can be given "within the limits of distributional analysis" by which is meant "in terms of

[1] Chapter 2 §5.

[2] It is often overlooked that to refer to the speaker's production of 'new' utterances presupposes some criterion or set of criteria according to which utterance tokens are assigned to utterance types, i.e., the identification of repetitions. This is, of course, the task of a grammar; the "parallel" if systematically considered, is somewhat circular.

the structural characteristics of observed utterances" (IV,113-114). Now the status of the (linguistic) theoretical account or reconstruction of the speaker's ability to produce new utterances requires, for its evaluation, a fuller depiction of the nature of this ability. This is provided as follows:

> We might restate this ability, somewhat figuratively, by saying that in learning a language, the native speaker has done much more than merely absorb a large set of sentences which he can now reproduce. He has also abstracted from this set of sentences, somehow, and learned a certain structural pattern to which these sentences conform. And he can add new elements to his linguistic stock by constructing new sentences conforming to this structural pattern (IV,113).

It is this structural pattern, conformity to which distinguishes new sentences, which manifests the speaker's ability and which it is the assigned task of linguistic theory to reconstruct. The point to be raised here, surely, is not whether speakers have productive linguistic abilities, i.e., whether their utterances do conform to a structural pattern, but rather: What is the character of these productive abilities? And this question can only be answered in the context of another: What is the character of the theoretical representation of this structural pattern?

We noted in Chapter 3 that Chomsky's LSLT (=1955a), à propos the Quinean attack on the "theory of meaning", put forward a view that linguistic theory is a theory of linguistic form, or, more correctly, of "intuitions of linguistic form" which have nothing

to do with meaning. These intuitions provide evidence of the conformity of utterances to structural pattern. Their non-semantic character was held to be demonstrated, paradigmatically, by the existence of sentences such as Colorless green ideas sleep furiously whose well-formedness is operationally attestable, but which are "thoroughly meaningless and non-significant" (I-37). Correspondingly, and in accord with Quine's dicta regarding the irredeemable obscurity of the concepts of the theory of meaning, the reconstruction of the native speaker's "intuitive sense of grammaticalness" is to take place in a theory of grammatical structure from which notions of meaning are expressly excluded. The goal of linguistic theory is to provide a theory of the speaker's linguistic intuition, a goal which may not be realizable in purely formal terms, but for which no convincing evidence exists that semantic notions can be of any assistance (I-39). Two "adequacy criteria" governed the reconstruction of this ability within linguistic theory: (1) the sentences generated or projected by the grammar beyond those in the observed corpus must be in conformity with the acceptability judgements of native speakers of the language ("external adequacy"), and (2) a linguistic theory or general theory of language structure must provide a mechanical procedure to select, on grounds of simplicity of notation, a highest-valued grammar from among those satisfying the first criterion ("internal adequacy").

This program provided a quite definite proposal as to the character of a specifically circumscribed linguistic ability of speakers of a language, ~~that which~~ (the ability that enables them to 'recognize' certain 'new' strings of elements (word sequences) as well-formed and others as ill-formed; viz., that this ability is purely non-semantic in nature [1] and, as such, is adequately reconstructed by a linguistic theory utilizing or relying upon no semantic notions. However, since it is assumed that this ability extends to arbitrarily many 'new' word sequences, the ability can be idealized as encompassing all the (denumerably) infinite sentences of a language.[2] In this way, a natural avenue is opened to adopt the view -- borrowing concepts from the theory of formal systems [3] -- that a grammar, as a "device" or "set of rules"

[1] Hence, as we noted in Chapter 3, the claim is that well-formedness does not depend in any way upon the properties of lexical items (i.e., words), which are surely semantic.

[2] That natural languages are infinite sets of sentences is an idealization which follows from the consideration that there is no longest well-formed sentence (in principle); see Chapter 2, p.96 fn 1.

[3] As is well-known, during this period Chomsky pursued an active interest in investigations of various formal systems as candidates for the "device" adequate to generate all and only the grammatical sentences of a natural language, focusing in particular upon the character of the rules such a device must possess if adequate to describe a natural language; see his (1956) and (1959b) and Chomsky and Miller (1958). It has often been said (following in this regard remarks in Chomsky (1965a:60-61)) that finite state (Markov) sources and "simple" (i.e., context-free) phrase structure grammars are in principle descriptively inadequate for natural language, i.e., that natural languages lie outside the weak generative capacity of such theories (on "weak" and "strong" generative capacity, see below). But careful perusal of the cited arguments show that the crucial consideration adduced in each case is based upon a mathematical error (Postal, Bar Hillel) or upon simplicity (Chomsky); see Daly (1974), Chapters 3 and 4. However, the significance for empirical linguistic theories of all results pertaining to generative capacity, and indeed of algebraic linguistics in general, is currently denied in generative grammar, for reasons discussed in §4.3.

which explicitly characterizes the (infinite) set of sentences of a language in a finite manner, [1] provides a model and indeed an explanation for this ability which is a "fundamental aspect of linguistic behavior":

> Any grammar of a language will project the finite and somewhat accidental corpus of observed sentences to a set (presumably infinite) of grammatical utterances. In this regard, a grammar mirrors the behavior of the speaker who, on the basis of a finite and accidental experience with language, can produce or understand an indefinite number of new sentences. Indeed, any explication of the notion "grammatical in L" (i.e., any characterization of "grammatical in L" in terms of "observable in L") can be thought of as offering an explanation for this fundamental aspect of linguistic behavior (1957a:15).

[1] (1957a:13):"The fundamental aim in the linguistic analysis of a language L is to separate the grammatical sequences which are the sentences of L from the ungrammatical sequences which are not sentences of L and to study the structure of the grammatical sequences. The grammar of L will thus be a device that generates all of the grammatical sequences of L and none of the ungrammatical ones."

Cf. (1956:105):"The grammar of a language can be viewed as a theory of the structure of this language. ...A properly formulated grammar should determine unambiguously the set of grammatical sentences." (106):"By a grammar of the language L we mean a device of some sort that produces all of the strings that are sentences of L and only these."

(1958:125):"A grammar of a language should at least be expected to offer a characterization of the set of objects that are sentences of this language, i.e., to enable its user to construct a list or enumeration of these utterances." (152):"A grammar of L is a device which enumerates the sentences in such a way that a structural description can be mechanically derived for each sentence. ...The structural description should, if the grammar is at all adequate, provide a basis for explaining how sentences are used and understood." (156):"The goal of a grammar is to characterize all the utterances of the language."

Chomsky and Miller (1958:92):"A grammar is a set of rules -- preferably a finite set, if we expect finite automata to learn them -- that specify the grammatical strings of symbols."

(1961:7):"A grammar...is essentially a theory of the sentences of a language; it specifies this set (or generates it, to use a technical term which has become familiar in this connection) and assigns to each generated sentence a structural description:"

What manner of explanation is this? An answer here requires that we look more closely at what is meant in saying that a grammar "generates" a language. Here a distinction is recognized between an effective procedure that merely enumerates the word sequences (or strings of symbols) that are the sentences of the language and the specification of what is termed the "structural descriptions" of these enumerated sentences.

> We learn nothing about a natural language from the fact that its sentences can be effectively displayed, i.e., that they constitute a recursively enumerable set. The reason for this is clear. Along with a specification of the class F of grammars, a theory of language must also indicate how, in general, relevant structural information can be obtained for a particular sentence generated by a particular grammar. That is, the theory must specify a class Σ of "structural descriptions" and a functional ϕ such that given $f \in F$ and x in the range of f, $\phi(f,x) \in \Sigma$ is a structural description of x (with respect to the grammar f) giving certain information which will facilitate and serve as the basis for an account of how x is used and understood by speakers of the language whose grammar is f; i.e., which will indicate whether x is ambiguous, to what other sentences it is structurally similar, etc. These empirical conditions that lead us to characterize F in one way or another are of critical importance. (1959b:138)

> It is, incidently, important to recognize that a grammar of a language that succeeds in enumerating the sentences will (although it is far from easy to obtain even this result) nevertheless be of quite limited interest unless the underlying principles of construction are such as to provide a useful structural description. (ibid., fn 3)

To recursively enumerate the sentences of a language is simply to provide an effective, i.e., algorithmically formulable, listing of which word sequences are sentences of the language. A grammar

which succeeded in this task might be thought to constitute a "formal explanation" of the speaker's "intuition of grammaticalness" by explicitly displaying all the products of the exercise of this ability, past, present and future. In much the same way, it could be maintained that the Dedekind-Peano axioms comprise a formal explanation of the human ability -- call it "arithmetical ability" -- to segment and group objects in a purely quantitative way: all such arrangements can be explicitly reconstructed or generated in a theory characterized by these axioms. Each instance of a 'correct' exercise of the respective ability would be represented by appearing on a list whose members were determined by an effective procedure, thus the ability as a whole could be said to be "reconstructed" by such a procedure. But there is an immediate and glaring disanalogy: We can reconstruct indefinitely many summing activities by deriving these from the specified axioms in a prescribed fashion, but what procedure or set of operations suffices in the case of language? And this is to ask: Which word sequences <u>are</u> sentences?

In this regard it may be recalled that the linguist's task is considered to be that of "producing a device of some sort (called a grammar) for generating all and only the sentences of a language, which we have assumed were somehow given in advance"(1957a:85). Such assumption seems clearly equivalent to an assumption about the character of the linguistic ability manifested as "intuitions of grammaticalness", namely, that the (ideally infinite) exercise of this ability determines

a target set which it is the task of a grammar, if it is explanatory in the required fashion, to reconstruct or generate.[1] Obviously, such an assumption engages a rather stringent idealization away from the character of the abilities of actual speakers of a language, for it is extremely doubtful that speaker intuitions do circumscribe any well-defined aggregate.[2] But the fundamental point here concerns not merely the legitimate scope of idealizations of language users' abilities; to do so is to lose sight of the essential empirical control governing the conception of a grammar as a theory of the sentences of a language. 'Grammaticalness' or 'sentence' or 'well-formedness' are not well-defined by intuitions, but by grammars. And the test of any such theory is that, given a 'recognized' sentence (i.e., an intuitively well-formed word sequence) of the language, the grammar either provides a structural (i.e., compositional) characterization of this sentence, thus 'accepting' it as a grammatical sentence, or it does not. And, although it may be admitted that the 'recognition' ability extends to indefinitely many 'new' word sequences, the empirical or constructive meaning of saying that a grammar generates "all and only the sentences of a language" (thereby reconstructing this ability) can only be: given any word sequence recognizable as well-formed (or occurring in a text or discourse), the grammar generates this sequence by producing one or more[3] explicit

[1] Cf. (1957a:13):"(S)uppose that we assume intuitive knowledge of the grammatical sentences of English, and ask what sort of grammar will be able to do the job of producing these in some effective and illuminating way."

[2] See below.

[3] Allowing for "ambiguous" sentences; however, see p.249 fn 1 below.

structural descriptions for it.[1] We should not be deceived, by illicit analogies with the theory of formal systems, into thinking that the expression "all and only the sentences of the language" has any other determinable meaning. Chomsky is entirely correct in holding that the conception of a grammar which merely enumerates the word sequences that are the sentences of a language (termed "weak generation" in Chomsky (1965a:60)) without providing any details about how the words are 'in construction with' each other (termed "strong generation", op.cit.) is of no interest at all to empirical linguistics.[2] From these remarks it may be concluded that, since the notion of "well-formed sentence" as precisely defined by a grammar is an empirical one, i.e., is adequate to the extent that it describes all recognizably well-formed word sequences and only these, there is a warranted sense in which it may be said that a grammar, as a theory of the sentences of a language, explains or reconstructs a speaker's abilities manifested in his "intuitions of grammaticalness". As it stands, however, this formulation calls for immediate qualification: different speakers (or the same speaker at

[1] The character of the structural characterization of a sentence will crucially depend upon the domain of the operations which specify the elements of the sentence; thus, the same sentence may be differently structured according to whether the domain is accounted as that of the language as a whole or that of a discourse or sublanguage, where operations are constrained by additional restrictions. See below.

[2] Cf. (1966b:48-9):"The fact that a grammar generates a language is hardly of any interest. What is important is that it should do so in such a way as to assign to each sentence the correct deep and surface structure, and, beyond that, that it succeeds in this task of strong generation in an internally motivated way." See also (1965a:61).

different times or in different contexts) may give differing reports as to the well-formedness of the same word sequence. Thus it is indeed questionable that the linguistic ability is representable as an effective procedure, that the exercise of the ability specifies a well-defined set, except under a strong idealization. Clearly, what a grammar (of sentences) seeks, in reconstructing this ability, is a formulation of the difference between what Husserl called Unsinn and Widersinn,[1] between 'impossible' combinations -- "mere heaps of words" such as king but or like and, impress adverse forever instead egregious -- and those which are not 'impossible' although including combinations which are unfamiliar, or of low likelihood, or are semantically deviant: #Which of the nuclear trombones supports spotted ordinals?, Any nationalization harmonizes cement in bloody green implication, etc. However, appeals to intuition, if it be required that these be manifested in elicited speakers' judgements,[2] may not yield definitive results: are I didn't hope in a moment or Who impugns their calibrator? well-formed? Notoriously, context plays a large role in whether a particular word combination is 'recognized' as well-formed; standard examples are 'journalese' sentences, such as Kissinger conjectures poached [3] and Mets farm Sisk,[4] or sentences from repair manuals, e.g.,

[1] Husserl (1928:326 ff)

[2] Or in observations of how speakers relate various sentences; see Hiż (1985).

[3] McCawley (1976:236) suggests this sentence as occurring in the context of a Cabinet discussion on the topic of how President Ford liked his eggs.

[4] New York Post, May 6, 1985

Check fuel system full or Bleed fittings brake assembly.[1] Before the widespread usage of long distance telephone communication, such sentences as OK your suggestion and Depart on Tuesday might have been immediately recognizable as occurring in a telegram; today they might be acceptable only to someone with experience of intra-office memos. Man-machine "interfacing" raises new possibilities for altering what speakers 'recognize' as well-formed and, in this regard, changing the domains of co-occurrence for certain words. Is a grammar to be required to account for these? There may be, in addition, sentences which are used, and thus 'recognized' as well-formed (perhaps by only some speakers), but which we should hardly want the grammar to 'accept', e.g., Twenty dollars was cost by the book or Max took a shower and a pink handkerchief. Moreover, is a grammar to be responsible for metaphor, or for slang, or for dialectal peculiarities? Considerations such as these -- as well as the fact that every language contains expressions, such as Hello, which are not describable in terms of the apparatus set up for the bulk of the sentences (and are therefore "grammatically petrified"[2]) -- provide the basis for understanding Sapir's famous cautionary

[1] Cf. Lehrberger (1982) 1981? 1982 not in bibl

[2] Harris (1968:197)

maxim that "all grammars leak" (1921:38). It would seem then that the demand that a grammar characterize all and only the recognizably well-formed word sequences is an impossibly strong one in two respects: (1) there is no precise meaning which can be given to "all and only the recognizably well-formed sequences" due to variations among speakers and among contexts, and (2) that a particular word sequence is recognizably well-formed by some speaker of the language should not be considered a sufficient reason to require that the grammar of the language as a whole should generate it. And this is just to say that 'grammaticality' (in the sense of explicit compositional well-formedness) is a notion which is only specified by a grammar, and as such, is distinct from the acceptablity judgements which are the elicited reports of a speaker's "intuitions of grammaticalness". But recognition of this conceptual point does not detract from two related conditions of what might be termed 'empirical adequacy': (1) any grammar which did not generate the vast bulk of intuitively recognized sentences [1] could not be considered empirically adequate; and (2) it is furthermore only when 'grammaticality' is well-defined that such a test is at all possible. Grammars which meet these two conditions might well be considered candidates for a formal explanation or reconstruction of a speaker's ability to 'recognize' and produce 'new' sentences.

[1] Or intonationally recognized; see Lieberman (1984:98-9). On the intonational criterion of sentencehood, see Hoenigswald (1960:1). Harris (1968:36-40) shows that the sentence boundaries within a discourse can be distinguished by a recurrent stochastic process on words.

Are there, or can there be, empirically adequate grammars in this sense and, if so, what kind of grammars are these? In order to see what is involved in asking such a question, recall that to say a grammar generates a sentence means that the grammar structurally or compositionally derives the sentence, reconstructing its intuitive well-formedness in terms of the elements and operations of the grammar and hence showing how the sentence is used and understood. What sort of structures, elements and operations are these? A standard assumption in generative grammar since at least Chomsky (1965a) has been that an abstract level of underlying structure (termed "deep structure")[1] is required in order to adequately represent the intuitive differences which English speakers recognize between superficially similar sentences, such as (a) John is easy to please and (b) John is eager to please. Although (a) and (b) are similar in "surface structure" (i.e., both may be represented by the same phrase structure labelled bracketing), this state of affairs does not reflect the 'knowledge' English speakers have that (a) may be transformed into (a') It is easy to please John whereas (b) does not

[1] These examples are discussed in Chomsky (1960:532), (1964:34), but the term "deep structure" appears only in (1965a), defined as "determing the semantic interpretation" of a sentence (16) and in a discussion of examples of this kind in a remark about "how unrevealing surface structures may be as to underlying deep structure"(24). There may be some cause for disagreement with Chomsky's assessments of the transformations distinguishing easy and eager; see e.g., his reference (1977b:131 fn 48) to "the fact that we have "an easy man to please" but not "an eager man to please"." I find John is an eager man to please, i.e., 'John is a sycophant' perfectly natural.

transform to (b') *It is eager to please John.[1] Such examples have been used to institutionalize a view that an adequate representation of sentence structure, i.e., reflecting the intuitions of the speaker about how the sentence is to be understood, must be given in a form that is more abstract than a relation among words (i.e., adjunction and replacement) such as co-occurrence or the dependence relations familiar from categorial grammars. Such an assumption has never been demonstrated[2] and, to the contrary, results have been obtained showing that, in principle, the number of levels of hierarchical structure required for the compositional description of sentences is two,[3] i.e., of ordering relations among words, such as in the string adjunct grammar of Harris (1962) or in the grammar of partially ordered word dependences of Harris (1982). This means, of course, that the 'knowledge' that speakers have which purportedly requires a "deep structure"[4] repre-

[1] The * indicates a word sequence that does not constitute a possible English sentence. Of course, this S is acceptable w/ non-epithetic *it*, which is not a transform of (6).

[2] See p.186 ff above.

[3] Joshi (1972); Joshi, Kosaraju, and Yamada (1972a) and (1972b).

[4] The character and claims made on behalf of "deep structure" have changed over the years, but this need not unduly detain us here. All that is essential for the present discussion is that "deep structures" are viewed as comprising structures generated by a system of categories (a "categorial component") into which lexical items possessing "inherent properties" are "inserted" and on which transformations are defined; see Chomsky (1981a: 5, 18, 92 ff).

sentation is, in fact, representable as consisting in words and and relations defined on words. The argument for an underlying level of abstract structure should not be confused with the fact that similar sentences can be grammatically described as entering into the domains of different transformations, a completely separate matter. There is, in other words, no necessity of defining transformations or other grammatical operations and elements on abstract structures (see e.g., Harris (1982) and Chapter 5§3 below, for details).

While there is no necessity ~~for~~ to posit ~~of~~ structure (or levels of representation) more abstract than relations among words in accounting for the recognizably well-formed sentences of a natural language (say, English), it may be argued that grammatical theories incorporating such "deep" structures may be preferable in other ways, for instance on grounds of simplicity, or elegance in the sense that they evidence, or should be required to evidence, properties of "deductive depth" that are desirable in theories which purport to provide accounts of child language acquisition.[1] But this must remain a moot issue, it would seem, in view of the fact that actually constructing an empirically adequate grammar (in the sense, as modified, above) incorporating structures of this kind remains an "as yet unaccomplished job" which "poses a serious intellectual challenge".[2]

[1] Cf. Chomsky (1981c:9):"The ideal is to reach the point where we can literally deduce a particular human grammar by setting parameters of universal grammar in one or another of the permissible ways."

[2] Higginbotham (1982:147). We will return to the theme of the relation of "descriptive" to "explanatory" adequacy below in §4.3.

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There are, on the other hand, rather strong indications that a grammar positing "deep structures" or abstract levels of structure into which lexical items are inserted faces insuperable difficulties in accounting for the range of distribution of lexical items, i.e., the observable occurrence of lexical items (words) in recognizably well-formed (attested) sentences, and thus meeting the condition of empirical adequacy. In the most ambitious attempt to construct a grammar of this kind for a single language (French), which would provide "a coverage comparable to that of traditional grammars" such as those of Jespersen (1909-1949) and Poutsma (1904-1929) for English, Gross and his co-workers [1] found that for a "significant portion of French" -- a corpus containing more than 12,000 lexical items and constituting a classification of simple predicates (s'agir, apparaître, s'avérer, y avoir avantage, etc. [2]) -- no two lexical items have identical syntactic properties, i.e., share the same range of environments of co-occurrence. This finding makes clear the reasons for the difficulties encountered in studying particular transformational rules:

> each time we introduced a new example, the rule had to be applied in a way different from that used in all previously studied cases. Variations were minor most of the time: prepositions could appear or not, a special tense or mood was involved, etc. (Gross (1979:861))

As a result, their attempt to write a transformational generative

[1] Gross (1975) and (1979) and the references cited there.

[2] The data are represented in the form of a 12,000 by 600 binary (occurs in this position, does not occur in this position) matrix.

grammar of this corpus failed. The significance of the mammoth investigation of Gross et al. lies, at base, in the demonstration of the fundamental empirical inadequacy of the notion of a 'rule of grammar', insofar as such rules are considered operations applying 'blindly' to purely formally defined structures and having validity for whole classes of lexical items, despite their different selectional restrictions. By the same token, it is a demonstration that there are no lexical classes having semantic properties which precisely correlate with a formally specified rule. [1] The specification of the possible word sequences or combinations occurring as sentences cannot be provided a priori by the assumption that lexical items have inherent properties; the determination of what semantical (or other) properties lexical items have is consequent upon a prior investigation of the range of co-occurrence of each lexical item and a principled accounting for this range of co-occurrence. [2]

[1] Compare Gross (1975:231 fn 22):"A la limite, las disparition d'une règle peut s'interpréter comme las disparition de la classe d'éléments lexicaux à laquelle la règle s'appliquait, ou, d'un manière plus générale, comme la disparition d'une propriété sémantique de la classes, qui était corrélée à la propriété syntaxique définie par la règle." This conclusion had been anticipated by Hiż (1961:49) in the early years of transformational grammar. Some linguists have questioned Gross' analysis of the linguistic data and hence his conclusions are controversial, but, given that the findings of Gross et al. are empirically supported, it is apparent that these results bear upon any generative theory (whether transformational or not) which attempts to account for the observable restrictions on word co-occurrences.

[2] Thus avoiding what Bloomfield (1933:204) termed "class-cleavage", that is, allowing occurrences of the 'same word' in ostensibly different grammatical positions to be differently classified, and thus to remain unexplained. For example, expect occurs in sentences

It seems an obvious point that whatever grammars can be constructed which do succeed in accounting for the distribution of large numbers of lexical items, ~~i.e., what~~ in traditional grammars, this was termed "coverage", will not readily be thought to be "internalized" by the speaker-hearer in the sense of the remarks of Chomsky, cited at the end of 4.1 above. That is to say, the compositional account -- if any such can be provided by the linguist -- of the recognizably well-formed word sequences not only represents the linguist's cleverness in stating a non-ad hoc system of elements and operations that is adequate in this respect; but by this fact (it may be said) an empirically adequate grammar also formally (i.e., explicitly) reconstructs the linguistic ability manifested as "intuitions of well-formedness" and as well says a good deal about the semantics of the language. [1] Success in these efforts does not mean that the linguist is merely rearranging the data of his corpus in one or another manner, or that his theory is merely an artifact of its data. [2] To the contrary, it can be maintained that the linguist's grammar is 'real' to the extent that it explicitly shows how patterns characterizing use of language in a particular speech community

merely

(continued from previous page)

like I expect John (an N V N structure) and I expect John to arrive (which is N V N S), where expect has a clausal complement. We could simply say that there are two words expect in English, each with its own inherent features, or word word expect with some Boolean disjunction of features. But either of these 'options' is obviously ad hoc, overlooking that in many occurrences these sentences 'say the same'. See further in Chapter 5 for general details of how the former occurrence may be transformationally derived from the latter. It is apparently often forgotten that distributional statements constitute evidence (see Ch. 2 §3) that particular lexical items have certain semantic properties; see Hoenigswald (1965)on this point.

[1] Including "intuitions of meaning" inasmuch as these are based upon an informant's experiential knowledge of 'normal' range of occurrence.

[2] Which Gross has shown, in the case of grammars, to be a necessary sin.

patterns which are 'real' in that they are determinably 'recognized' by these speakers, are compositionally, i.e. p(structurally)explicable. There is, in other words, no basis for a distinction between "mere coverage of facts" and "insightful coverage" as was once demanded[1] since the "facts" to be "covered" are indeed "insightful" in virtue of being the constraints of well-formedness and co-occurrence, of relations between sentences and the like, which are observed in how speakers actually use their language. In the sequel to this chapter we will consider further the grammatical metatheory (and its evolution) motivating this demand for "insightful" coverage.

[1] Chomsky (1960:549):"What we want in a grammar is not mere coverage of facts, but insightful coverage, something much more difficult to define or attain." Cf. (1964:53):"Comprehensiveness of coverage does not seem to me to be a serious or significant goal in the present stage of linguistic science. Gross coverage of data can be achieved in many ways, by grammars of many different forms. Consequently, we learn little about the nature of linguistic structure from the study of grammars that merely accomplish this." To the contrary, we maintain, a great deal can be learned about the nature of linguistic structure from the study of grammars that cannot "merely accomplish" "gross (a prescient pun?) coverage". As we showed in Chapter 3, the denigration of "mere coverage", while ostensibly sounding the theme of 'explanatory' versus 'descriptive' theories, in fact stems from initial metatheoretical assumptions that: (a) linguistic theories must be purely formal (i.e., non-semantic) in character and therefore linguistic descriptions (provided by corresponding grammars of "formally autonomous"syntax) can be achieved in many ways; (b) as a result, some metacriterion is required to 'select' from among the various possible formal descriptions which are assumed to be empirically adequate. On this latter point recall Weyl's (1949:61) summary of Brouwer's criticism of formalism: "The question why he sets up just these rules must remain unanswered by the consistent formalist. He will have to refer us to philosophy, psychology, or anthropology, so Brouwer thinks, in order to justify his "lustgevoel van echtheitsovertuiging" (translated "consciousness of legitimacy" in Brouwer (1913:84)-TR) and his belief that the chosen axiom system is more suitable than any other to be projected onto the world of experience."

4.3 In Search of "Explanatory Adequacy". ~~The provision of~~ a grammar which structurally characterizes 'all and only' the intuitively well-formed word sequences, in virtue of which it may be considered a formal (i.e., explicit) reconstruction of this linguistic ability, is not ~~the provision of~~ a model of lingusitic behavior, a point Chomsky drew attention to in his review of Skinner:

> The behavior of the speaker, hearer, and learner of language constitutes, of course, the actual data for any study of language. The construction of a grammar which enumerates sentences in such a way that a meaningful structural description can be determined for each sentence does not in itself provide an account of this actual behavior. It merely characterizes abstractly the ability of one who has mastered the language to distinguish sentences from non-sentences, to understand new sentences (in part), to note certain ambiguities, etc. (1959a:576)

It is no accident that this point should be made in a review of a book entitled Verbal Behavior, whose purport was indeed to account for the occurrence of particular utterances in particular situations, [1] a conceit Chomsky was certainly right to upbraid as a scientistic pipe-dream, at least in the form presented by Skinner. On the other hand, there is some latent equivocation in the assertion that a grammar does not provide an account of the "actual behavior" of the speaker, hearer, or learner (and as well in the assertion that this behavior "constitutes the actual data for any study of language" [2]). Is it not the task of a grammar to account

[1] See Chapter 2 §6.

[2] Cf., for example, the differing formulations in Chomsky (1958):"The empirical data I want to explain are the native speaker's intuitions." (158), and "The data are sentences. Utterances of the language." (175).

for the occurrence of a particular sentence in a particular situation (qua Skinner) or it it not the task of a grammar to structurally characterize actual utterances which occur in the context of a discourse? [1] Is any sound or noise made through the mouth to count as part of the "actual data" of linguistic behavior? Is a grammar to be relieved of the responsibility of accounting for mispronumciations, of interruptions, of discontined utterances, of imitative noises, clicks, whistles, and the like?

It is evident that some distinctions must be recognized in the assertaion that a grammar is not "in itself" an account of the behavior of the language user. We may begin by noting that since language occurs as discourse,[1] i.e., as connected utterances, there is a clear sense in which a grammar of sentences is an idealization: it characterizes particular utterances (word sequences) "atomistically", [2] i.e., without regard to restrictions and dependences which cross sentential boundaries. [3] Thus, the same sentence, described from the point of view of the language as

[1] Cf. Harris (1952a:315):"Language does not occur in stray words or sentences but in connected discourse -- from a one word utterance to a ten-volume work, from a monolog to a Union Square argument."

[2] The term "sentence atomism" is used by Hiż in roughly this sense.

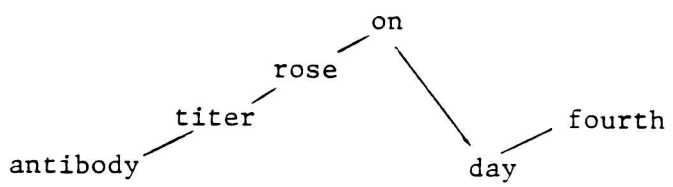
[3] The most familiar (but far from the easiest to describe) cases are referential relations whose resolution requires trans-sentential domains; e.g., First, Max and his brother went to the same school. Later, Ted transferred to Fordham.

a whole, that is, as a sequence whose well-formedness is compositionally reconstructed by the sentence-defining elements and operations of a grammar, and (2) where described from the point of view of whatever similarities it may share with other sentences occurring in the same discourse (or set of discourses, as in a sublanguage -- see further in Chapter 6) may be provided with different structural characterizations, any of which can be considered 'correct' vis-a-vis the respective domains over which the elements and operations are defined. [1] How much structure can warrantedly be posited

[1] For instance the sentence The antibody titer rose on the fourth day (as may occur in a text of cellular immunology) can be represented in various ways by different sentence grammars: as having the phrase structure

($_S$ ($_{NP}$ the antibody titer) ($_{VP}$ ($_V$ rose) ($_{PP}$ ($_P$ on)($_{NP}$ the fourth day))))

or a partially-ordered word dependence structure, representable as a semi-lattice



As occurring in a text in a sublanguage of cellular immunology, the sentence might be reconstructed, showing its similarities with other sublanguage sentences, as: On the fourth day following the reinjection of viral antigen into the footpad of rabbits of the same strain, the titer of antibody rose in the lymph follicles, which can be represented as a instance of the word class sequence GJB : AVT, with : = on the fourth day following; G = viral antigen; J = (re)injection; B = rabbits of the same strain; A = antibody; V = titer present in; T = the lymph follicles. See Chapter 6 for details.

in sentences taken, under the idealization, as occurring in a null context (or, perhaps, the metalinguistic context of grammatical discussion)? The answer, clearly, is just the structure sufficient to characterize the speaker's intuition that the sentence is well-formed: the restrictions on word combinations that suffice to rule out (in Husserl's term) Unsinn. Word combinations satisfying these constraints must be considered sentences, and therefore, 'sayable' no matter how unlikely they might be, or whether or not they express a coherent meaning. Any further assignment of structure is based upon a priori notions of meaning or interpretation which themselves must be justified. Whereas sentences occurring in a discourse (even a two-sentence one, e.g., under a conjuction) exhibit the property that speakers (or, more generally, users) of the discourse-language 'recognize' additional (that is to say, beyond well-formedness) restrictions on word combinations. Just as it is not the case that any two sentences may be joined under a conjuction, e.g., ∄ Manifestation is a relation of a whole and its parts and it was all characteristically Teutonic, and, critically examined, not very tactful; but tact was never Wagner's strong suit when trying to convince the world that its only hope of salvation lay

in hitching itself to the German chariot,[1] so in particular discourses and uses of language, certain word combinations which must be accounted sentences of the containing language (e.g., English) will be 'recognized' by users as not belonging to the restricted language, even though all the individual words may so belong. E.g., in the sublanguage of cellular immunology Lymphocytes produce renal adipose tissue is not a possible sentence, not merely because it is false which is unimportant[2] here, but because its utterance would immediately identify its producer as not a member of the restricted speech community, or because its occurrence in a text would be recognized as not forming part of the text.[3] Grammars of the language as a whole (i.e., which specify sentencehood) do not represent structures (restrictions on word combinations) which result from the occurrence of sentences in discourse and sublanguage.[4] So in the sense that they do not structurally characterize the actual occurrence of sentences in their discourse context, they are not models of linguistic behavior (or usage). But neither sentence grammars or any other grammars are models of behavior in that they are not accountable for all the sounds emitted from the mouth of a speaker[5] nor are they accountable

[1] '∄' indicates the sequence is unacceptable for all speakers; sources: J.M.E. McTaggart, The Nature of Existence, I, (1921), p. 121 and E. Newman, The Life of Richard Wagner, IV, (1946), p. 314.

[2] Science languages are, presumably, replete with false sentences though not with ones for which there is no evidential warrant.

[3] As in Z. Harris (forthcoming), "Introduction".

[4] These terms will be discussed in Chapter 6.

[5] The sounds of interest to the grammarian are those which display invariance under repetition, see Chapter 5 §3.

for the occurrence of particular utterances under given stimulus conditions.

The intent of these remarks has been to distinguish the idealization under which all sentence grammars are gathered -- that structure can be assigned to sentences taken as independent of context of occurrence -- from the seemingly obvious disclaimer (which is not an idealization) that such a grammar is not an account of (verbal) behavior. To be sure, the idealizing assumption of a sentence grammar is not completely innocuous: there is both far more 'recognizable' structure in sentences taken as occurring in discourse and, in some cases, far less -- a situation which may lead to pseudoproblems. [1]

This view of the 'not-a-model-of-behavior' matter would seem to be in accord with certain of Chomsky's early writings, e.g.,

> It is first of all clear that the formalized grammar, regarded as a predictive theory, is an idealization in at least two respects; first, in that it considers formal structure independently of use; and second, in that the items it generates will not be the utterances of which actual discourse is composed, but rather they will be what the untutored native speaker knows to be well-formed sentences. (1960:531)

But, on the other hand, under the supposition that a generative grammar has been "internalized" by the speaker-hearer (see the concluding part of §4.1 above) such a grammar "must be regarded

[1] Thus it is said that a (sentence) grammar is to account for the intuitions of native speakers that certain sentences are "ambiguous", but the sentences provided as illustrating this claim, e.g., They are flying planes, He heard the shooting of the hunters, John decided on the boat, etc.,are typically only "ambiguous" in a null context of occurrence, i.e., as grammatical examples. The fact that speakers can parse these sentences in different ways should be accountable for in such a grammar, but the examples are better described as "homonymous" rather than "ambiguous".

as a component in the behavior of the speaker and listener":

> It is not easy to accept the view that a child is capable of constructing an extremely complex mechanism for generating a set of sentences, some of which he has heard, or that an adult can instantaneously determine whether (and if so, how) a particular item is generated by this mechanism, which has many of the properties of an abstract deductive theory. Yet this appears to be a fair description of the performance of the speaker, listener, and learner. If this is correct, we can predict that a direct attempt to account for the actual behavior of speaker, listener, and learner, not based on a prior understanding of the structure of grammars, will achieve very limited success. The grammar must be regarded as a component in the behavior of the speaker and listener....(1959a:577)

Of course, the operative assumption behind regarding a generative grammar as "internalized" or "as a component in behavior" is that it is empirically adequate -- that it actually does characterize the (de-contextualized) intuitively well-formed word sequences without ad hoc adjustments.[1]

In his writings throughout the 1960's, Chomsky often returns to the theme that a generative grammar is not to be considered as a model of the speaker or hearer;[2] now the distinction between the abstract characterization of linguistic abilities and the behavior in which such abilities are manifested is designated as that between "competence" and "performance", a conceptual distinction which is

[1] Cf. Katz and Fodor (1964:484):"The justification which permits the grammarian to study sentences in abstraction from the settings in which they have occurred or might occur is simply that the fluent speaker is able to construct and recognize syntactically well-formed sentences without recourse to information about settings, and this ability is what a grammar undertakes to reconstruct."

[2] E.g., Chomsky and Miller (1963:272); Chomsky (1965a:9), (1967a:435-6). Much of this discussion was tied up with, and in response to, the matter of the "psychological reality" of grammatical rules and structures.

hailed as a sine qua non for the study of human cognition.[1]

"Competence" is defined as "the speaker-hearer's knowledge of his language" and the competence of an "ideal speaker-hearer" is the object of grammatical description:

> A grammar of a language purports to be a description of the ideal speaker-hearer's intrinsic competence. (1965a:4)

"Performance" refers to "the actual use of language in concrete situations", presumably in discourse or ~~other~~ non-linguistically describable settings of occurrence. But "performance" also refers to other "gramamtically irrelevant conditions" such as "memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic)".[2] The concept of "performance" thus

[1] Chomsky (1968:78):"I think that if we contemplate the classical problem of psychology, that of accounting for human knowledge, we cannot avoid being struck by the enormous disparity between knowledge and experience....In principle, the theory of learning should deal with this problem; but in fact it bypasses the problem....The problem cannot even be formulated in any sensible way until we develop the concept of competence, alongside the concepts of learning and behavior, and apply this concept in some domain." Metaphors of depth seem to be essential to the characterization of competence, as are rationalist assumptions about the explanation of complex phenomena. Thus Chomsky (1965a:4):"...linguistic theory is mentalistic, since it is concerned with discovering a mental reality underlying actual behavior." and Pylyshyn (1972:31):"Basic to the notion of competence is the belief that what is behind such intuitions is best characterized as a set of implicit rules or a procedure. ...This view of cognitive competence is fundamentally a rationalist position. It claims that underlying all cognitive activity is a more perfect system than that displayed by the record of behavior itself..." Fodor (1983:2) speaks of "orthodox mentalist doctrine", the view that "Behavior is organized, but the organization of behavior is merely derivative; the structure of behavior stands to mental structure as an effect stands to its cause." As we shall see, the siren call of a "more perfect underlying reality" leads even further than the methodological abstraction of "competence".

[2] (1965a:3).

conflates under the rubric of "the actual use of language in concrete situations" the notion of discourse together with "various psychological factors" which may be involved in verbal performance.[1] This results in a de jure identification of 'language structure' with what is describable within the domain of "competence". What comprises this domain of "knowledge of language"?

We have seen that the intent of a sentence grammar is to reconstruct the ability of speakers to recognize 'new' word sequences as well-formed, the condition of such reconstruction being that the grammar provide a compositional account of recognizably well-formed sequences, showing how the words are 'in construction with' one another. But "knowledge of language" involves far more than this:

> It seems clear that we must regard linguistic competence -- knowledge of language -- as an abstract system underlying behavior, a system constituted by rules that interact to determine the form and intrinsic meaning of a potentially infinite number of sentences. (1968:71)

> At the systematic level, competence is expressed by a generative grammar that recursively enumerates structural descriptions of sentences, each with its phonetic, syntactic and semantic aspects. (ibid., 185-6)

> When we try to characterize the state of mind of a person who knows a language, taking account of his ability to use and understand an indefinite range of sentences, each with its phonetic form and meaning potential determined in a specific way, we are led..., specifically, to the construction of a generative grammar, a system of rules and principles that establishes a certain sound-meaning relationship. (1969c:314)

> ...the technical term "competence" refers to the ability of the idealized speaker-hearer to associate sounds and meanings strictly in accordance with the rules of his language. (1967a:328)

[1] Cf., Katz (1972:25):"...the study of performance assumes the contribution of competence and directs its attention to the manner in which the contributions of various psychological factors, e.g., memory limitations, attention shifts, distraction, brain damage, errors -- interplay with linguistic factors to produce natural speech, with all its characteristic distortions and irregularities."

That is to say, the ability of a speaker to use and understand his language was now seen to require representation by a grammar with a semantic component in addition to the previously assumed "level" (now "component") of "phonological representation":

> ...we stress again that knowledge of a language involves the implicit ability to understand indefinitely many sentences. Hence, a generative grammar must be a system of rules that can iterate to generate an indefinitely large number of structures. This system of rules can be analyzed into the three major components of a generative grammar: the syntactic, phonological, and semantic components. (1965a:15-6)

Depending upon what is to be understood by the notion of a "semantic representation" generated by a "semantic component",[1] this new requirement could be viewed as placing a rather onerous burden on the grammarian, e.g., by requiring that a linguistic description should "specify all the information about the sentences that a speaker utilizes to produce and understand them".[2] However this may be, it is unclear how seriously Chomsky ever entertained the the idea of incorporating a "universal semantics" into the model of a generative grammar[3] and, in any event, talk of a "semantic

[1] About which there is an enormous literature; see e.g., Katz (1972) and McCawley (1973a) and the references cited therein.

[2] Katz (1966:123):"the rules of a linguistic description must not only be capable of producing an infinite list of formal objects, but the formal objects on the list must be the sentences of the language under study (!!-TR) and the list must exclude any string in the vocabulary of the language that is not a sentence of the language. Furthermore, these rules must somehow specify all the information about the sentences that a speaker utilizes to produce and understand them."

[3] Thus, within a few pages of the same book, the revised 1972 edition of Chomsky (1968), we find somewhat opposing perspectives:"Let us turn now to the study of underlying competence, and consider the general problem of how a sound-meaning pairing might be established. As a preliminary to this investigation of universal grammar, we must ask how sounds and meanings are to be represented. Since we are interested in human languages in general, such systems of representation must be independent of any particular language. We must, in other words,

component" soon disappeared from theoretical discussions to be replaced by a new level of "logical form". [1] This development, apparently, traces Chomsky's increasingly strong conviction that matters of "fact" and "belief" and indeed "knowledge of the world" enter so intimately into meaning that the notion of a "semantic representation" based upon "universal semantics" is not a tenable one.[2] So pronounced has been this volte face, it has been suggested recently "that

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develop a universal phonetics and a universal semantics that delimit, respectively, the set of possible signals and the set of possible semantic representations for any human language." (120); whereas less sanguine expectations are expressed just six pages earlier:"In fact, the notion "representation of meaning" or "semantic representation" is itself highly controversial. It is not clear at all that it is possible to distinguish sharply between the contribution of grammar to the determination of meaning, and the contribution of so-called "pragmatic considerations," questions of fact and belief and context of utterance"(114).

[1] Cf. (1977a:5):"A grammar...assigns to each sentence (in particular) a structural description consisting of a representation on each of a set of linguistic levels; specifically, on the level of phonetics, phonology, words, morphemes, higher level syntax, and what I will call here "logical form" (LF). I use the latter term to refer to those aspects of semantic representation that are strictly determined by grammar, abstracted from other cognitive systems."

[2] E.g., (1969a:67):"Thus one might argue that nonlinguistic beliefs, intentions of the speaker, and other factors enter into the interpretation of utterances in so intimate -- and perhaps so fluctuating and indefinite -- a fashion that it is hopeless to attempt to represent independently the "purely grammatical" component of meaning,...". The same point is made in a stronger vein in (1979b:142):"Why, then, raise a question about the possibility of a universal semantics, which would provide an exact representation of the full meaning of each lexical item, and the meaning of expressions in which these items appear? There are, I believe, good reasons for being skeptical about such a program. It seems that other cognitive systems -- in particular, our system of beliefs concerning things in the world and their behavior -- play an essential part in our judgements of meaning and reference, in an extremely intricate manner, and it is not at all clear that much will remain if we try to separate the purely linguistic components.... I doubt that one can separate semantic representation from beliefs and knowledge about the world."

the study of word meaning is not, properly speaking, part of the study of language at all, but rather concerns other cognitive systems which are connected in part to language through some sort of 'labelling'"(1979c:37), which might be accounted a rather unusual position for a linguistic theory to adopt. Unfortunately, further details of this intriguing proposal have not been forthcoming.

But whatever the levels of linguistic representation provided by a generative grammar in its characterization of "knowledge of language" or, more properly, "competence" -- the idealized speaker's ability to produce and understand indefinitely many sentences, determining a sound-meaning relation for each of these -- two metatheoretical (i.e., as stipulated in the theory of language structure) "criteria of adequacy" have been proposed. On the one hand, the linguist's grammar is "descriptively adequate" if it "corresponds to linguistic fact", i.e., correctly characterizes the linguistic intuition of the speaker of the language.[1] On the other hand, "on a much deeper and hence much more rarely attainable level" a grammar is to be justified according to the extent to which it is "explanatorily adequate"; as in LSLT, this is a level of (theory) "internal justification": a grammar

[1] "Descriptive adequacy" is the correlate of "external adequacy" in LSLT: "On one level (that of descriptive adequacy), the grammar is justified to the extent that it correctly describes its object, namely the linguistic intuition -- the tacit competence -- of the native speaker. In this sense, the grammar is justified on external grounds. on grounds of correspondence to linguistic fact"(1965a:26-7). However, the nature of linguistic facts has been somewhat altered; recall that in LSLT, external adequacy required that the sentences predicted by the grammar be acceptable to native speakers; in (1965a) "Acceptability is a concept that belongs to the study of performance, whereas grammaticalness belongs to the study of competence"(11).

which is "descriptively adequate" and is highest valued by an evaluation procedure belonging to the general linguistic theory, is thereby justified on the level of "explanatory adequacy".[1] As such, a grammar which is "explanatorily adequate" is addressing the problem of accounting for language acquisition, "an account of the specific innate abilities that make this achievement possible".[2] Although these adequacy criteria are conceptually separate, Chomsky frequently cautions that in practice they are inseparable, and that even "descriptive adequacy" cannot be achieved without concern for the development of an explanatory theory, a theory of universal grammar.[3] In (1965a) and until very recently with the onset of

[1] (1965a:27)

[2] op.cit.; Cf., "To acquire language, a child must devise a hypothesis compatible with the presented data -- he must select from the store of potential grammars a specific one that is appropriate to the data available to him. ...and all concrete attempts to formulate an empirically adequate linguistic theory certainly leave ample room for mutually inconsistent grammars, all compatible with primary data of any conceivable sort. All such theories therefore require supplementation by an evaluation measure if language acquisition is to be accounted for and selection of specific grammars is to be justified; and I shall continue to assume tentatively, as heretofore, that this is an empirical fact about the innate human faculté de langage and consequently about general linguistic theory as well"(ibid., 36-7). As we saw in Chapter 3, the requirement that theory selection be a matter of evaluation measures stems from the metaphilosophical requirement that the theories in question be based on purely non-semantic primitives. Notice however, that selection is from such theories as are empirically adequate, i.e., as are compatible with "primary data". It is simply assumed therefore that the empirical adequacy condition can be easily satisfied by such theories. Discounting the bogeyman of "a mere listing" of the data of a corpus which should satisfy no one's criterion of empirical adequacy, this does not appear -- in the light of the diversity Gross has shown to exist in language, as well as in the failure of generative grammar to come up with anything like a grammar with a systematic approach to significant coverage in a single language -- to be a justifiable assumption.

[3] (1965a:41),(1968:27-8), (1970:428-9).

"modular theories" of grammar (see below), this level of "theory-internal" justification (thus hypotheses pertaining to language acquisition) was conceived in the manner of LSLT, i.e., as a formal (i.e., mechanical) evaluation measure ranking theories in terms of notational simplicity, and was thus subject to the criticism that the imposition of formal methods of theory selection on linguistic theory is sufficiently unlike anything else in science that it may be genuinely regarded as "bifurcationist" in the sense in which Chomsky, rightly or wrongly, indicts Quine's thesis of "indeterminacy of translation".[1] However, under "modularity" such criticism is anachronistic.[2] But whether via evaluation measures or by other means, the explanatory task of linguistic theory to account for language acquisition by children is served only insofar as this theory succeeds in "restricting the class of possible grammars" which are compatible with the available data.[3]

[1] (1980a:16-22). See Chapter 3.

[2] In the "modular theory" presented in Chomsky (1981a) and (1982), general rule-writing systems have been dispensed with and, a fortiori, also evaluation metrics defined on these systems. See the (favorable) review of these two works by Williams (1984), who observes: "the intent of the theory, that general rule-writing systems are dispensed with, is...quite clear. There can therefore be no such activity as 'writing the rules of a language' in the traditional transformational sense; rather there is only 'fixing the parameters of universal grammar (UG) in the appropriate way for a particular language.'" Williams also suggests a reason for abandoning selection via evaluation measures: "so far as I know, no interesting details of the evalution measure have been forthcoming since the initial proposals of ASPECTS and of SOUND PATTERN OF ENGLISH (Chomsky and Halle 1968). The failure of this 'formal' avenue to an explanatory theory laid the ground for the more 'substantive' modular theories of current work"(402).

[3] The exact formulation varies; see (1965a:61), (1973:81), (1976:164), Chomsky and Lasnik (1977:427). Chomsky (1979a:1) speaks of restricting "the class of attainable grammars", presumably indicating a proper subset of "possible" ones.

Regarding the task of "explanatory adequacy", i.e., "restricting the class of possible grammars", it has been widely believed [1] that the results of Peters and Ritchie (1973) showed that the theory presented in Chomsky (1965a) placed virtually no constraints on the form of possible grammars, and thus abjectly failed to address the central concern of providing an account of child language acquisition. For their main result demonstrated that an ASPECTS-type grammar (suitably formalized) had the weak generative capacity of an unrestricted re-writing system, i.e., was able to characterize membership in any recursively enumerable set, and thus was no more restrictive than a given Turing machine. [2] But does this finding really have any relevance for empirical linguistic theories? To be sure, Peters and Ritchie themselves assume the significance of their result for empirical linguistics in proposing an amendment to the ASPECTS formalism which ensures the generation of only recursive sets. [3] This is done, they

[1] Cf. Newmeyer (1980:175):"But around 1970, the task of power reduction took on a new urgency. Studies by Stanley Peters and Robert Ritchie (...) demonstrated that the situation was far worse than imagined. Put simply, Peters and Ritchie proved that the weak generative capacity of a transformational grammar was that of an unrestricted rewriting system (Turing machine). What this meant was that transformational rules were so unconstrained that transformational grammar as formulated then made no claim at all about any human language except that its sentences could be generated by some set of rules." See also Kimball (1973:50) for a similar statement.

[2] This is due to the fact that transformations are permitted in the ASPECTS theory to iterate (cycle) indefinitely.

[3] By defining a (primitive recursive) exponential function of the length of a sentence which bounds the cycling of transformations.

note,, tc "justify the intuition of virtually all linguists that natural languages are recursive"(82). In the light of the issues raised in the previous section, it may be doubted that such a consensus in fact exists among linguists, although in the heady early days of the application of formal systems to natural languages this may have obtained among certain formally-inspired linguists and onlookers.[1] But we have already seen that any results pertaining to weak generative capacity (i.e., to strings of symbols) have no bearing on the central matter of linguistic theory, the structure of natural languages.[2] And, whereas Chomsky in his (1965a) allows that "it seems that, when the the theory of transformational grammar is properly formulated, any such grammar must meet formal conditions that restrict it to the enumeration of recursive sets" (208, fn 37), he also contends that questions of generative capacity are not "necessarily" in correspondence with "what is probably the empirically most significant dimension of increasing power of linguistic theory", viz., "the scattering in value of grammars compatible with fixed data" (62) by which is meant, apparently, how many possible grammars must be assessed by an evaluation measure.[3] It may be

[1] See the citations in §4.2 above and Putnam (1961). A recent appraisal is Matthews (1979) who concludes "there are no compelling theoretical reasons for requiring that transformational grammars enumerate only recursive sets"(209).

[2] The same is true of so-called "strong generative capacity" insofar as this as viewed merely as the generation of "tree structures".

[3] (1965a:62):"Along this empirically significant dimension, we should like to accept the least "powerful" theory that is empirically adequate." Cf. Chomsky and Lasnik (1977:427):"To attain explanatory adequacy the theory T must be sufficiently restricted so that relatively few grammars are available, given a reasonable amount of experience E, to be submitted to evaluation; otherwise, the burden on the evaluation procedures is intolerable."

indeed questioned, therefore, that the Peters and Ritchie result indicates that an ASPECTS-type grammar is "explanatorily inadequate" in the special sense given to this expression. But there can be no question that this result or any result derived from the mathematical theory of formal languages will scarcely be pertinent to a linguistic theory for which "it may turn out that grammars do not generate languages at all" (1980a:122). But before considering this possibility and what meaning may be given to the demand to "restrict the class of possible grammars" in the context of such a theory, it is first necessary to examine further the relation between the criteria of "descriptive" and "explanatory" adequacy and the antagonism spawned by these competing concerns.

The claim that a grammar is "internally represented" and "involved in the use and understanding of sentences" by a speaker of a language certainly can be made -- if at all -- only of grammars that are, as Chomsky prefers, "descriptively adequate". That is, it is obvious that only such a grammar as actually succeeds in correctly describing the speaker's intuitions of well-formedness, of relations between sentence constituents and relations between sentences, etc., can be considered as guiding or somehow determining the manner in which a language user understands his language. Furthermore, it would seem to be equally obvious that the question of how such a "system of knowledge" is acquired can logically only follow the demonstration that a particular candidate grammar is "descriptively adequate" over a non-

trivial range of data. In other words,

> We can certainly raise the question of acquisition of knowledge only where we have a reasonably convincing characterization of what has been learned. (Chomsky (1971:26)) [1]

But given the requirement that a linguistic theory provide an account of the acquisition of a speaker's "knowledge of language", there is engendered a "tension" between the twin pursuits of attaining "descriptive" and "explanatory adequacy":

> To attain explanatory adequacy it is in general necessary to restrict the class of possible grammars, whereas the pursuit of descriptive adequacy often seems to require elaborating the mechanisms available and thus extending the class of possible grammars. (Chomsky and Lasnik(1977: 427)) [2]

In the light of the difficulties encountered in attaining empirically adequate coverage comprising sizable numbers of lexical items which we have outlined above, we can well understand the pressure to "elaborate" the descriptive "mechanisms available" in order to accomodate the sought-for correspondence between grammatical "rule" [3] and "linguistic fact", i.e, the intuition of the language user.

[1] Cf. Chomsky (1964:113):"(T)he construction of a model of acquisition (whether a model of learning or a linguistic procedure for discovery of grammars) cannot be seriously undertaken without a clear understanding of the nature of the descriptively adequate grammars that it must provide as output, on the basis of primary linguistic data."

[2] See also Chomsky (1983a:163):"It is the tension between these two tasks that makes the field intellectually interesting, in my view."

[3] Here recalling Peirce (1931:606):"The most generic possible sense of 'rule' is 'a general formula applicable to particular cases'."

How has this problem been addressed within generative grammar? The answer here depends upon how the relation between the central notions of 'grammar' and 'language' is conceived. According to the formal systems view of this relation, i.e., that a grammar generates all and only the sentences of a language (a position not explicitly stated in LSLT as in SYNTACTIC STRUCTURES[1] and at least tacitly in subsequent work up until the middle 1970's), a research program is pursued to restrict "the expressive power of rules" thus reducing "the class of accessible grammars". [2]

[1] As Chomsky has recently observed (1984:11 fn 14):"(T)he earliest publications on generative grammar were presented in a framework suggested by certain topics in automata theory (e.g., my SYNTACTIC STRUCTURES, actually course notes for an undergraduate course at MIT and hence presented from a point of view related to this (sic) interests of these students). Specifically linguistic work, such as my LOGICAL STRUCTURE OF LINGUISTIC THEORY (1955-6; published in part in 1975), was not publishable at the time. In the latter, considerations of weak generative capacity, finite automata and the like were completely absent, and the emphasis was on I-language ("internalized language" -TR; see below), though the term was not used." On the remark that LSLT "was not publishable at the time" see Murray (1980), esp. 77-8.

[2] Chomsky (1977a:19). This would seem to indicate that the restriction on the class of grammars envisaged results from constraining the generative capacity of this class, i.e., the character (e.g., Kleene (regular), context-free, -sensitive, etc.) of the language generated. See the careful discussion in Kimball (1973), chapter 4, and the statement (62):"The more limited the generative capacity of the class of grammars available as potential grammars for human languages, the closer to an explanation of child language acquisition the linguist has come. Adding new mechanisms to grammar which increase the generative capacity of the resulting class of grammars is an overall loss in explanatory power of universal grammar, and each such addition must be justified by empirical considerations." As we have seen, Chomsky has not held such a straightforward view of the bearing of generative capacity upon the explanatory task of linguistic theory, although his language surely sometimes suggests just such a relation. See footnote 16 (1977a:19) which observes that Peters and Ritchie "have shown how transformational grammars with cyclic rules can be restricted to generation of recursive languages, in some quite natural ways" and then notes that "the crucial issue is not the recursiveness of generable languages but restriction of the class of accessible grammars."

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But at least as early as 1975 the view is entertained that the innate faculté de langage might not suffice to determine a grammar:

> I have been assuming that UG suffices to determine particular grammars (where, again, a grammar is a system of rules and principles that generates an infinite class of sentences with their formal and semantic properties). But this might not be the case. It is a coherent and perhaps correct proposal that the language faculty constructs a grammar only in conjunction with other faculties of mind. If so, the language faculty itself provides only an abstract framework, an idealization that does not suffice to determine a grammar. (1975b:41)

This more abstract framework, whereby the language faculty takes its place among "the system of mental faculties in a fixed way",[1] subsequently termed "modularity of mind" or less tolerantly, perhaps, "Massachusetts modularism",[2] has entailed rather drastic changes in not only the model of a generative grammar but also a conceptual revision of the central notions of 'grammar' and 'language' and an increasing preoccupation with explanatory concerns, a "shift in focus" which, it seems warranted to say, has led to the de facto abandonment of the criterion of "descriptive adequacy" altogether and an increasing reliance on a plausibility argument, the so-called

[1] "The theory of UG remains as a component of the theory of mind, but as an abstraction. Note that this conclusion, if correct, does not imply that the language faculty does not exist as an autonomous component of mental structure. Rather the position we are now considering postulates that this faculty does exist, with a physical realization yet to be discovered, and places it within the system of mental faculties in a fixed way." (ibid.,42-3)

[2] Flanagan (1983:200 ff.)

"argument from the poverty of the stimulus".

The increased abstractness of the modular conception is seen initially in the proposal that theorizing about mind is to be conducted at the more abstract level of "cognitive" or "mental structures" rather than of "first-order capacities to act":

> I want to consider mind(...) as an innate capacity to form cognitive structures, not first-order capacities to act. The cognitive structures attained enter into our first-order capacities to act, but should not be identified with them. Thus it does not seem to me quite accurate to take "knowledge of English" to be a capacity or ability though it enters into the capacity or ability exercised in language use. (1975c:23) [1]

The issue of relevance in the new distinction is ~~to~~ that it allow ~~that~~ a fully developed "cognitive structure" ~~may~~ to exist without there being a capacity to use this structure. According to Chomsky, one may attribute ~~ion of~~ "knowledge of language" ~~can be made~~ to persons, who, for one reason or another, do not evidence this knowledge through linguistic behavior. Such might be the case of someone who has taken a vow of silence, or -- to use the example Chomsky provides -- a person who has suffered cerebral trauma, leaving the "language centers" unaffected but "prevent(ing) their use in speech, comprehension, or let us suppose, even in thought" (1980a:51). A distinction of this kind is, apparently, intended to short circuit the evidentiary premiss that "behavior provides a criterion for the possession of knowledge," a principle to which those who would analyze "knowledge of language" as "a capacity or ability

[1] Cf. (1980a:4):"I would like to think of linguistics as that part of psychology that focuses its attention on one specific cognitive domain and one faculty of mind, the language faculty. Psychology, in the sense of this discussion, is concerned, at the very least, with human capacities to act and interpret experience, and with the mental structures that underlie these capacities and their exercise; and more deeply, with the second-order capacity to construct these mental structures, and the structures that underlie these second-order capacities."

to do something" are, according to Chomsky, apt to be "misled".[1] Thus, in obvious reference to his prior use of "competence" to refer to linguistic abilities in just this now proscribed sense, Chomsky maintains that "the term (i.e., "competence" - TR) is misleading in that it suggests 'ability' -- an association I would like to sever." [2] In place of "competence" in the old ability-conjuring sense, Chomsky proposes two conceptual clarifications. First, A's knowledge of a language (to know a language) is to be analyzed as A's being "in a certain mental state" where this means that A possesses "a certain mental structure consisting of a system of rules and principles that generate and relate mental representations of various types." [3] In addition, the term 'competence' is now relativized by the assumptions of modularity, ~~i.e.,~~ to a model of mind comprising interacting modular (autonomous) faculties. As a result, there is now "grammatical competence" [4] and "pragmatic competence"

[1] (1980a:48)

[2] (1980a:59). Cf., Higginbotham (1982:144):"In retrospect Chomsky's terminology (in ASPECTS) seems to have been unfortunate. The term 'competence' suggests that the possessor of competence possesses a skill of some sort and 'performance' correlatively suggests a domain of actual behavior that falls short in various respects of being ideally 'competent'. Both suggestions are misleading."

[3] (1980a:48). Thus grammars do not generate sentences or structural descriptions but "mental representations of various types".

[4] By which is meant "the cognitive state that encompasses all those aspects of form and meaning and their relation, including underlying structures that enter into that relation, which are properly assigned to the specific subsystem of the human mind that relates representations of form and meaning. A bit misleadingly perhaps, I will continue to call this subsystem "the language faculty"(ibid.,59) I do not pretend to understand what is meant by a "subsystem of the human mind that relates representations" nor what is meant by saying (1981a:34) that "the brain uses notations" such as"quantifier-variable rather than quantifier-free".

where this latter "underlies the ability to use" the former [1] and is possibly also to be characterized as a 'cognitive state'. [2]

The re-analysis of "competence" ("knowledge of language") in terms of "cognitive states attained" rather than "first-order abilities" etc., underscores a rather radical "conceptual shift" [3] regarding the fundamental notions of 'language' and 'grammar'. Just how far Chomsky has come from the original program of generative grammar of LSLT or even of ASPECTS can be seen in the distinction now drawn between "externalized" and "internalized language". For Chomsky now maintains that it "makes no sense" to think of a natural language as one does of arithmetic, that is, as "the set of well-formed sentences...given in terms of some external criterion, whereas 'grammar' is some characterization of this infinite set of objects".[4] To the contrary a "language" is "an epiphenomenon" [5] and the question, as to what a language is, is "not, as it stands, a question of science at all".[6] Even demarcating what is, or is not, an utterance or

[1] "Pragmatic competence underlies the ability to use such knowledge (i.e., "grammatical competence" - TR) along with the conceptual system to achieve certain ends or purposes." (1980a:59) Another (more exhaustive?) inventory is given in (1981a:18):"It is reasonable to suppose that the representations PF and LF stand at the interface of grammatical competence, one mentally represented system, and other systems: the conceptual system, systems of belief, of pragmatic competence, of speech production and analysis, and so on."

[2] (1980a:59):"It might be that pragmatic competence is characterized by a certain system of constitutive rules represented in the mind...."

[3] (1983a) and (1984).

[4] (1983a:159).

[5] (1981b:5).

[6] (1979b:32).

sentence of a language is not to be thought an interesting pursuit.[1] This is a remarkable development for a program which began by declaring that "the goal of a grammar is to characterize all the utterances of the language" (Cf.,the programmatic statements cited in §4.2 above and in Chapter 3).

The terminology "externalized language" is intended to emphasize a distinction from the "internalized grammar" by which it is "determined" and which "constitutes the knowledge attained".[2]

> A generative grammar is not a set of statements about externalized objects selected in some manner; rather it purports to depict what one knows when one knows a language....(1983a:156)

Accordingly, in referring to the domain of phenomena that a grammar seeks to characterize, Chomsky suggests the concept "knowledge of grammar" rather than "knowledge of language".[3] The difference is one between statements pertaining to a "definite real-world object, situated in space-time and entering into causal relations", statements which in virtue of being "about steady states attained or the initial state (assumed fixed for the species)" are "true or false" and those pertaining to the "externalized language" which "have a status that is much less clear, since there is no corresponding real-world object".[4]

[1] (1983a:156):"how one chooses to draw its boundaries is not a very significant question."

[2] op. cit.

[3] Ibid.,157. However, in a later paper (1984) Chomsky distinguishes "E-language" as above and "I-language" which refers to "a state of the language faculty" (8), and continues to speak of "knowledge of language" in the sense of "I-language".

[4] Ibid., 156-7.

It would appear that what is being propounded here is something akin to physicalism; depending upon how one understands "situated in space-time and entering into causal relations" statements about armies, religions and bank accounts share this "much less clear status". But the "shift in focus" is instead perhaps better characterized by what I shall call 'biologism', given both Chomsky's own preference for disciplinary affiliation,

> With this shift of focus, linguistics becomes in principle part of biology. ...It should sooner or later disappear as a discipline as new kinds of data become available, remaining distinct only in that its concern is a particular faculty of mind, ultimately, the human brain: its initial state and its various attainable mature states (1983a:157)

and the otherwise encountered resistance to reduction 'all the way down' elsewhere in cognitive psychology.[1] What is clear, however, is that this shift of focus is "a shift towards realism:

> I-languages are things in the world in particular mind/brains, while E-languages are not; theories of I-languages are on a par with scientific theories in other domains, while theories of E-languages, if sensible at all, have some different and more obscure status. Linguistics....will be incorporated within the natural sciences insofar as mechanisms are discovered which have the properties revealed in these more abstract studies (1984:10).

The claim that (biological) mechanisms "have the properties revealed in these more abstract (i.e., linguistic) studies" is at least initially

[1] As, for instance, Fodor (1978), esp. 171-2: "if neurological representations specify those properties of states of the central nervous system in virtue of which they constitute formulae belonging to a code, then the descriptions such states receive in sciences still more basic than neurology almost certainly do not. ...The more reason we have for thinking that neurology might substantively reduce psychology, the less reason we have for thinking that physics might substantively reduce neurology." This resistance can also be observed, perhaps, in the use of the neologism "mind/brain".

troubling without further clarification;[1] however, although our concern in the remainder of this chapter is that of scrutinizing the "properties revealed" in the "more abstract" investigations of linguistic theory. We can, however, nonetheless point out that talk of statements about "real-world objects" which are "true or false" and therefore "on a par with scientific theories in other domains" ostensibly conflates a long-standing question about the justification of formal grammars (which we showed in Chapter 3 was the original impetus for generative grammar) with a metaphysical position about the status and interpretation of scientific theories. For in maintaining that claims made on behalf of linguistic theories (and grammars of particular languages formulated in their terms) are, in principle,[2] to be justified by the determination that the underlying (biological) mechanisms do in fact "have the properties revealed in these more abstract studies", Chomsky is importing the issue of realism into the realm of the validation of empirical theories of language, a distinct and arguably independent concern.[3] And, despite the derogation of attested (observed) sentences to the status of "externalized language", the grammarian

strange transition

[1] For example, how is a "biological mechanism" to be identified as determining, say, the "abstract" property that play with "is (virtually) a symmetrical predicate" (see below)?

[2] "(L)inguistics should soomer or later disappear as a discipline as new kinds of data become available" op. cit.

[3] See, e.g., the discussion of this point in Fine (1986).

(for the foreseeable future, at least) has no alternative to assessing his predictive generalizations according to whether they correctly describe such data. Moreover, since "as a discipline, linguistics is defined by its attention to a certain kind of data -- for example, informant judgements", [1] and since, presumably, speakers can render "informant judgements" only about observed test sentences, which certainly belong to the "externalized language", the test of any proposed structural principle requires determining if the principle is obeyed in other sentences which the grammatical theory indicates to be similar in a relevant respect. If no such generality can be demonstrated, the grammarian is hardly warranted in saying more than that this or that test sentence can be described by such-and-such a structural principle; however, the precise domain of validity of the principle remains to be established.[2]

If the methodology for linguistic theory, schematically construed here, seems suspiciously like advocating an extension of coverage for principles which purport to characterize the structure of a particular language, it is because we know of no other manner of empirically assessing particular grammatical proposals. And surely any claims forwarded as to a native speaker's "knowledge" of a proposed grammatical principle can only be

[1] Chomsky (1983a:157).

[2] It (should) go without saying that this caution is all the more appropriate where the principle is alleged to be innately determined, within the parametric spectrum of an innate constraint.

subsequent to an investigation of this kind. Failing this, we shall be quite secure in the belief that any "knowledge" evidenced in an informant's intuitions regarding a sample sentence remains a matter of unformulated, perhaps other, structural principles and a knowledge of the occurring lexical items, based on the informant's experience of how they are used. This methodology does not appear congenial to the distinction sought between "externalized language" and "real-world objects situated in space-time" etc. Furthermore (as we shall see, immediately below) the highly theory- and assumption-internal character of linguistic theories, if taken or conceived as biological theories about hypothetical genetic or neural structures and mechanisms which stand in only a remote and very indirect relation to observable linguistic evidence, appears contingent upon something like the conflation, indicated above, of metaphysical views about the nature of scientific theories and the more pragmatic issue of justifying particuar grammatical principles. It is only the latter problem for which a solution is attainable by present-day linguistic theory with its reliance on linguistic evidence; the claim that 'something else', other kinds of evidence, perhaps, is required to really validate the theoretical constructions of linguistics would seem to be simply the expression of a negative opinion about the scientific standing of present-day linguistics, a disparagement about what linguistics at present is able to do.

Precisely because generative grammar deigns to characterize "Universal Grammar", violations of the proposed constraints and the extent of their application should be in the forefront of concern. Instead, the methodological situation seems to be as follows: if it can be shown that a restriction is observed in some sentence or paradigm of "sample facts", then, if it can be shown that this restriction "follows from" some more abstract principle for which there may be similar evidence on other grounds, then this principle is held to "restrict the class of possible/accessible/attainable grammars" and an "explanatory hypothesis" is put forward for the existence of the restrictions observed in the data set. Since such an abstract principle could not possibly have been "learned" by "inductive generalization" from the "primary linguistic data" to which the child acquiring a language is exposed, 'inference to the best explanation' reasoning offers the conclusion that this abstract principle is part of the initial (i.e., genetically endowed) state of the child.[1] In just this way, generative grammar seeks to acquire the highly prized "deductive depth" which enables a few principles admitting of parametric variation to account for the enormous diversity of languages, and which, as a whole, characterizes

[1] Cf. Chomsky (1980c:54-5):"The evidence bearing of the hypothesis attributing rules of grammar to the mind is that sample facts are explained on the assumption that the postulated rules are part of the structure of (the) A(ttained) S(tate) and are used in computations eventuating in such behavior as judgements about form and meaning. The evidence with regard to UG is that properties of states attained are explained on the assumption that the principles are as postulated in (the) I(nitial) S(tate)."

theories of the mature sciences, which theorize in the "Galilean style". [1] But as a result of its highly selective posture towards what constitutes linguistic data, it may be questioned whether it is pursuing a goal which is empirically constrained. Thorny decisions are involved as to whether the "sample facts" chosen bear upon the significant ("real-world") object, other faculties of mind, or are otherwise due to some idiosyncratic historical or cultural accretion:

> (W)e have little a priori insight into the demarcation of relevant facts -- that is, into the question of which phenomena bear specifically on the structure of the language faculty in its initial or mature state as distinct from other faculties of mind or external factors that interact with grammar (in the broadest sense) to produce the data directly presented to the investigator. (1980b:2)

> (E)ach actual language will incorporate a large periphery of borrowings, historical residues, inventions and so on, that we can hardly expect to -- and indeed would not want to -- incorporate within a principled theory of UG. ... What a particular person has inside his head is some kind of artefact resulting from the interplay of many idiosyncratic factors, as contrasted with the more significant reality of UG (an element of shared biological endowment) and core grammar (one of the systems devised by fixing the parameters of UG). (1979a:3-4)

Still, it is debatable whether the "sample facts" appealed to are in fact univocally described in the postulated manner and thus whether the principles hypothesized are underwritten by judgements quite as striking and distinctive as claimed (and as they need

[1] Chomsky (1980a:8-10,218-19). Of course, appeals to Galileo are hardly novel in psychology, but then Galileo means different things to different psychologies. For instance, Hull (1937) sees in Galileo the progenitor of the postulational method in empirical investigation, whereas Lewin (1931), (1936) sees the major contribution as the downfall of Aristotelian modes of explanation.

be to clearly warrant, it would seem, their nativist pedigree). Several, arguably representative, cases of argumentation selected from the recent literature are examined below in demonstration of this point. In the first it can be shown that the posited grammatical principle allegedly determined by innate constraints is rather an artifact of the restricted selectional data considered; in the second, a choice made between two competing theoretical proposals is seen to be based largely, if not entirely, on highly theory-internal assumptions, with observable consequences that are disputable.

The first example is taken from a discussion [1] which focuses on the relation between the metatheoretical criteria of descriptive and explanatory adequacy. The cited data are as follows (we use the enumeration of the text for convenience):

(3) Mary bought a dog to play with

which has the relevant structural description

(4) $\underline{\text{Mary}}_1$ bought $(_{NP}$ a dog$)_2$ $(_{\bar{S}}$ for $(_S$ NP_1 to play with $NP_2))$

where the noun phrases in the embedded sentence are assigned an interpretation according to the indicated indexing by a "rule of construal". Thus (3) has the interpretation

(7) Mary bought a dog for Mary to play with the dog

and not

(8) Mary bought a dog for the dog to play with Mary.

[1] Chomsky (1981d:36-7).

The interest of this example, for Chomsky, is that, despite the fact that play with "is (virtually) a symmetrical predicate", i.e., having the meaning 'x plays with y iff y plays with x', native speakers of English nonetheless understand (3) to mean (7) and not (8), which is "a very sharp but quite remarkable judgement".[1] According to Chomsky,

> there are two facts that call for explanation....One is the fact that (3) means (7) and not (8). The second is the fact that speakers of English somehow know that this is the case. Returning to the descriptively adequate grammar that we have assumed to provide rules sufficient to generate (3) with the structural description (4), but not with the indices reversed in the embedded sentence, we may say that this grammar provides an explanation, at a certain level, for the first of these facts,... The form of the explanation is straightforward. Attributing the descriptively adequate grammar and principles for interpretation of indices to the speaker-hearer, we conclude, ...correctly in this case, that (3) will mean (7) rather than (8). The theory that attributes to the speaker-hearer the descriptively adequate grammar and accompanying principles of interpretation therefore qualifies as an explanatory theory, at a certain level(37).

As we have seen above, however, "descriptive adequacy" is not to be considered without relation to the concept of "explanatory adequacy":

> But the descriptively adequate grammar provides no answer to the second and deeper question: How does the child come to know that the facts are as specified in the descriptively adequate grammar?(37)... The explanation offered by a theory of U(niversal) G(rammar) that meets the level of descriptive adequacy is illuminating to the extent that properties of UG rather than properties of the course of experience, determine the elements of the steady state attained (38).

Since this case for explanatory principles (the posited rule of construal) rides on the assumption that play with "is (virtually) a

[1] (1981d:36); in discussion of this example in (1980a:178), Chomsky observes that "the two possibilities (of interpretation) are virtually if not completely synonymous, yet we understand (36 = 3) to be associated with one interpretation though not the other." Here, Chomsky suggests a rather different "minimum distance principle" as the relevant principle which applies to determine this judgement. Our argument below extends to this account as well.

symmetrical predicate" it may not be unfair to observe that our "course of experience" (determining, presumably, the elements of the steady state of "knowledge of language" that we have attained) tells us that many occurrences of play with are with NP arguments that are not symmetrical in the required sense. For instance, Boys play with guns is hardly equivalent (or synonymous) to Guns play with boys because of the ironical nuance; He is playing with fire (whether literally or metaphorically interpretated) is not equivalent to Fire is playing with him; so also, He is only playing with me (as said by a boxer between rounds) is not equivalent to I am only playing with him, etc. The range of NP arguments of play with does not support the statement that play with "is (virtually) a symmetrical predicate".

But it seems other selectional properties are involved as well in the analysis of this data. Consider Mary brought a friend to play with (since Mary bought a friend to play with, although perfectly interpretable as violating the proposed rule of construal, seems primarily ironical): in this case one can certainly maintain that play with is symmetrical with respect to its argument NPs likewise violating the proposed rule of construal. Similarly, Mary needs a friend to play with, Mary desired a friend to play with, are also symmetrically interpretable; to stipulate an asymmetrical interpretation appears purely arbitrary. Even more clearly, consider

Mary sold a dog to play with. Buy and sell are clearly related semantically, and this relation shows up in the fact that we hardly can interpret this sentence as Mary sold a dog for Mary to play with the dog. It is clear that the asymmetry of interpretation posited by the "rule of construal" for play with is really only an artifact of the selectional properties of the two NP arguments as well as of the verb in the primary sentence. Despite the avowed concern to push on to "explanatory adequacy" from "descriptive adequacy", it is apparent that the analysis of the "sample facts" proposed here is descriptively inadequate, ~~from which analysis~~ and that no "explanatory" hypotheses may legitimately be generated from it.

A second case provides an example of how, in pursuit of "explanatory adequacy", a vastly increased latitude is alloted to idealization -- elsewhere referred to as the "Galilean style" of theoretical inquiry -- allowing highly theory-internal considerations to base a choice between competing proposals concerning very little empirical data. Looked at less theory-internally, the choice appears rather to involve primarily ad hoc and incidental factors. In a recent work introducing "government and binding (GB) theory", [1] Chomsky

[1] (1979a). Chomsky cites this theory as changing the direction of research in generative grammar: "(I)f something like this GB theory turns out to be more nearly correct, as I rather suspect given its more principled character, then it follows that a certain range of evidence that has been quite central in the development of theory in some recent work, in particular my own, is in fact not central but rather represents a category of marked phenomena of English and in part a few other languages (2)." Chomsky (1981a) is, to date, the major presentation of this theory.

appeals to the conception of "core grammar and markedness" to situate the explanatory goals of generative grammar. The linguist's task of providing a "highly structured theory of UG based on a number of fundamental principles that sharply restrict the class of attainable grammars and narrowly constrain their form" (1) is to proceed by determining how the "parameters" of the principles of UG are "fixed by experience". Since the parameters may be embedded in a theory of UG "that is sufficiently rich in deductive structure", fixing the parameters in one way or another allows a mechanism which can account for the great diversity of languages. A core grammar of a language such as English is then a theory which specifies how the parameters of UG are fixed by the experience of a child raised in an English speech community. This determines a particular grammar which, it is supposed, "generates a specific language" (3). But this language is rather different from what is usually meant by the term:

> (I)t is hardly to be expected that what are actually called 'languages' or 'dialects' or even 'ideolects' will conform precisely or perhaps even very closely to the systems generated by fixing the parameters of UG, the systems that I will call "core grammars". This could happen only under idealizing conditions that are never realized in fact in the real world of hetereogeneous speech communities. (3)

In a passage we have already cited, it is noted that the languages generated by "core grammars" are to be considered free of "borrowings, historical residues, inventions and so on".

, the theory of "on Binding" (1980b), or
oB Theory, and the Theory of Lectures on
Govt. & Binding (1981d), or GB theory

The data we shall consider pertain to the choice of particular theoretical proposals within this guiding framework of "core grammar and markedness". In terminology suggestive of notions familiar from the logical analysis of language, an anaphoric expression (i.e., an expression entering into coreference relations with other terms -- for instance each other in the men like each other) is said to be "bound" by its antecedent, the binding being indicated by coindexing at the level of linguistic representation called Logical Form (LF). In the case of each other, the following "interpretive principle" is assumed (e.g. (1981d:62)):

NP_i...each other ... means (roughly) ...each of NP_i ...the other NP_i

Thus (a) the men like each other has the interpretation 'each of the men likes the other men'. On the other hand, an element that is not bound, i.e., assigned indexing at LF, is "free". Thus in

(b) $They_i$ believe (each $other_i$ to be intelligent)

each other is bound as indicated to they outside the embedded clause of its occurrence, but free within this clause. Such distinctions are used to account for unacceptable sentences like

(c) $They_i$ believe (each $other_i$ are intelligent)

where the embedded clause is tensed (i.e., is not infinitival). Accordingly, the tensed clause is said to be an "opaque domain": no anaphoric expression can be free in it even if bound by another element outside this domain (as in (c)). It is the task of the grammarian to formulate the conditions

of UG, such as opacity conditions, governing elements such as each other.

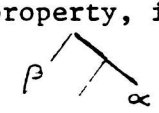
According to ~~the theory of "On Binding"~~ (OB theory~~; (1980b))~~, there are precisely two opaque domains: the subject of a tensed sentence (as in (c) above) and the c-command domain of the subject of any category.[1] This latter condition accounts for the deviance of sentences like

(d) They$_i$ saw ($_{NP}$ John's pictures of each other$_i$).

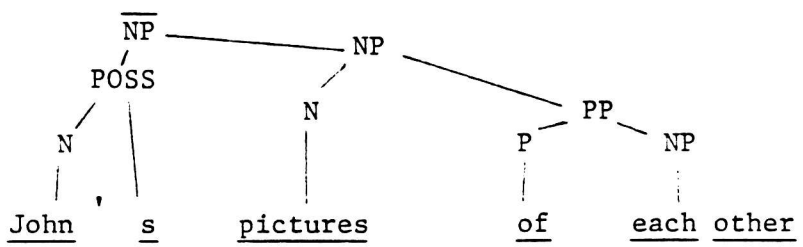
Here opacity prevents any interpretation since each other is in the c-command domain of the subject John of the category NP, and each other is free in the domain NP, although bound by they outside this domain.

1 (1980b:10) gives the following Command condition, regarded as as general property of coindexing rules: "an antecedent must c-command its anaphor, where β is said to c-command α if β does not contain α (and therefore $\beta \neq \alpha$) and α is dominated by the first branching category dominating β; then α is in the domain of β."

To illustrate this property, in the following tree β c-commands α:



The opacity condition (1980b:13) states: "If α is in the domain of the subject of β, β minimal, then α cannot be free in β." The minimality condition is irrelevant to our example. For the example (d) above, this is illustrated by:



In GB theory, there are two binding conditions relevant to our discussion. One is for pronouns:

(e) if NP is pronominal, it is free in its governing category[1],

and one is for anaphoric expressions like each other,

(f) if NP is an anaphor, it is bound in its governing category.

Now the OB and GB theories make different predictions where the governing category is NP. The significant cases are (using Chomsky's enumeration):

(18) John$_i$ read ($_{NP}$his$_i$ books)

and

(21) They$_i$ read ($_{NP}$ each other's$_i$ books)

Under the OB theory, the position of his in (18) should be transparent since it is in neither of the opaque domains, i.e., neither the domain of tense nor the subject of the governing category NP. Accordingly, OB theory predicts that his should be disjoint in reference to John just as him is in John saw him. But clearly his and John can be coreferential. In GB theory, under binding condition (e) above, the facts are otherwise:

> the correct facts are predicted. Thus in (18), the governing category for his is the NP in which it received Possessive Case -- the exact mechanisms still have to be made explicit but the point is obvious. Therefore by Principle (B) (= e) of the binding theory, his must be free in this category, as it is in (18). But it can be coindexed with John or any NP outside of the NP in which it appears, as required. (19)

[1] (1979a:12):"We say that α governs β if α minimally c-commands β (α a lexical category or Tense); that is, α c-commands β and there is no γ c-commanded by α and c-commanding β but not α. Finally, α is the governing category for β if it is the minimal category in which β is governed (where α = NP or S)."

In (21) the two theories also make opposing predictions. In this case it is GB theory which, via the binding condition (f), incorrectly predicts that (21) is ungrammatical since each other is free in its governing category (= NP) whereas the OB system correctly permits each other to be co-indexed with they. Now by appeal to the notion of core grammar and markedness, Chomsky maintains that what a theory incorrectly predicts -- either as to grammaticality or as to a permissible interpretation of coreference relations -- is "marked" according to that theory, i.e., is part of "a large periphery of borrowings, historical residues, inventions, and so on" (3), and so lies outside the structures of UG as specified by the principles of that theory. Thus the OB and GB theories differ in their properties concerning markedness. How is one to assess the difference between them? According to Chomsky:

> In these cases, it seems to me reasonable to conclude that the predictions of the GB system are in fact correct, as contrasted with those of the OB system. Thus (18) is surely the normal case in the languages of the world whereas such structures as (21) appear to be rare....
>
> Suppose that we accept these conclusions: thus let us tentatively accept the GB system that has been sketched here and take (21) to be a marked structure, thus supposing it to be rare and specifically learned in English on the basis of explicit data that indicates that somehow the conditions of core grammar are to be relaxed. We could predict, then, that a child learning English would take (21) to be ungrammatical. Note that this is an assumption with specific empirical content, though the obvious experiment to test it cannot be carried out for ethical reasons. (20-1)

It may come as a surprise that a "core grammar" should be required to generate such structures as (18) John$_i$ read (his$_i$ books) with his referring indifferently to John or to some other person, structures that are "surely the normal case in the languages of the world", but not structures such as (21) They$_i$ read (each other's$_i$ books), because these are "rare and specifically learned in English on the basis of explicit data that indicates (sic) that the conditions of core grammar are to be relaxed". Of course, it should first be recalled how the conception of a grammar has changed under the aegis of "core grammar" and "markedness", from that of demarcating the well-formed sequences of words to that of characterizing certain strings as grammatical through a stipulation that only certain co-reference possibilities obtain. [1] One may also query the methodological decision that restricts structural investigations of reference to intrasentential domains (despite the facts of discourse and text reference [2]); to be sure, the principles of "UG" are nearly always forwarded as proposals constraining the form of sentence grammars. The increasing prominence accorded to the explanatory concern to isolate the "phenomena which bear specifically on the structure of the language faculty in its initial or mature state" [3] in effect means that the linguist's grammar is no longer held to be

[1] The import of reference (conceived as a relation between words and things) in recent generative theorizing thus markedly contrasts with the scepticism Chomsky expressed concerning the linguistic significance of "the theory of reference" in LSLT; see Chapter 3 §31 above and the references cited there.

[2] The notion of co-reference must be distinguished from the purely linguistic notion of cross-reference ; see Gottfried (1986) for an analysis of cross referential relations in discourse.

[3] Chomsky (1980b:2).

descriptive of the linguistic intuition of the native speaker; presumably, linguistic intuition does not suffice to distinguish among (18) and (21) ~~as~~ do the competing proposals for "core grammar", although, it goes without saying, some ~~unformulated~~ criterion is surely operative in the assessment that (18) is "surely the normal case in the languages of the world".

Even granting the highly theory-internal character of this argumentation pertaining to the principles of "core grammar", it is not prima facie obvious that formulating algorithms correctly describing co-reference possibilities, possibilities artificially bounded by intrasentential contexts, should advance any hypothesis as to "the structure of the language faculty", the supposed biological endowment for language. More importantly, how is the claim to be understood that "data" such as (18) and (21) provide the basis of a choice between competing theoretical construals regarding the nature of the "innate schematism" of the language faculty? Such a basis scarcely seems sufficient and the resulting choice can, at best, only be maintained provisionally, pending a much more systematic treatment of relevant data. As it is, the mere fact that a single principle of co-reference encompassing both (18) and (21) has yet to be formulated appears to be the primary reason for declaring that one is "(core)grammatical" and the other is "marked", i.e., an "artefact resulting from the interplay of many idiosyncratic functions, as contrasted with the more significant reality of UG (...) and core grammar...". For although

we are assured that this choice between competing theoretical proposals is one with "specific empirical content", what possible empirical control -- on the grounds of evidence presented in this argument -- prevents the sceptic from simply reversing the decision concerning (18) and (21)? References to "ethical reasons" which preclude "the obvious experiment to test" this decision are not ~~exactly~~ likely to satisfy the sceptic's doubt that an empirical proposal has been advanced. From the argumentation surveyed here, it appears to be a reasonable policy to be wary of claims that theory-internal decisions of this kind are proposals with definite empirical consequences.

From an initial endeavor originating in an attempt to characterize a particular linguistic capacity of speakers of a language by generating 'all and only' the well-formed word sequences of the language, generative grammar has emerged in recent years as entirely preoccupied ~~in~~ with efforts to isolate and identify -- using linguistic evidence, i.e., sentences and the judgements of native speakers concerning these -- aspects of language structure that are allegedly due to a highly specific genetic endowment of the human species. That such a program can make headway at all on the basis of linguistic evidence is, of course, moot, pending the demonstration of confirmed universals of structure obtaining in widely diverse ~~and~~ many different languages. Organology metaphors aside,[1] it is open

[1] E.g., Chomsky (1981e:6):"It seems to me not unlikely that much of our knowledge of the nature and behavior of objects in our physical environment is rooted in principles of mental structure....These "mental organs" -- which need not, of course, be isolable in a particular neural region -- develop in a specific way on the basis of our biological endowment and provide the basis for substantial parts of our knowledge."

to question whether the required sharp conceptual divide between the categories of 'biological' and 'historical'/'cultural'/'learned' can withstand the criticism of contemporary evolutionary theory.[1] But, above all, despite the appearance of results produced by many workers and an impressive array of terminologies and formal notations, a closer scrutiny of the methods, goals, and assumptions of generative grammar renders a rather severe and negative assessment: that, as presently conceived and practiced, generative grammar can hardly succeed in its goal of formulating universals of language thereby accounting for child language acquisition, and that it has not produced, under its governing assumptions and methods, an empirically accountable theory of language structure.

[1] See, e.g., Lewontin (1978) and (1981), and Gould and Lewontin (1979).