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Problem Solving in Science and the Competence Approach to Theorizing in Linguistics

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I. INTRODUCTION

The goals of this paper are to identify (in Section II) some general features of problem solving strategies in science, to discuss (in Section III) how Chomsky has employed two particularly popular discovery strategies in science, and to show (in Section IV) how these strategies inform Chomskyan linguistics. In Section IV I will discuss (1) how their employment in linguistics manifests features of scientific problem solving outlined in Section II and (2) how an analysis in terms of those features suggests a natural account for many controversial aspects of Chomsky's methods. I will specifically examine how this analysis bears on Chomsky's claims concerning competence theories' centrality in psychology and their psychological reality.

Although no methodological considerations bar Chomsky from tinkering with our notion of psychology, competence theories *must* eventually offer some fruitful *empirical* connections with psycholinguistics and that part of mainstream cognitive psychology concerned with language study to justify that tinkering. After twenty years of research such connections have proven rare enough to raise important doubts about the justification of Chomsky's claims for the place of competence theory in cognitive psychology (and for the psychological reality of the mechanism it describes). Although I will argue (*contra many of Chomsky's critics*) that his methods are unexceptionable, still (*contra Chomsky*) his theories have not proven sufficiently robust empirically to justify his strong claims concerning their position in cognitive psychology.

II. SOME GENERAL FEATURES OF PROBLEM SOLVING STRATEGIES IN SCIENCE

Even after the erosion of the firm distinction between justification and discovery, philosophers have explored little of that side of the resulting

continuum more clearly concerned with discovery. Scientific discovery is a species of scientific problem solving, and the available evidence indicates that the differences between the latter and everyday problem solving are only matters of degree. Thus, an analysis of scientific pursuits can capitalize on the wealth of literature in cognitive psychology on human problem solving (some of which directly concerns problem solving *in science*[1]).

If there is no logic of discovery, then no algorithms can effectively guide scientific research. This does not, however, preclude systematic analysis of these issues. I will outline four general features of problem solving strategies. They exhaust neither problem solving strategies generally nor scientific ones particularly, nor are they logically independent. Still, if scientific problem solving does not differ in kind from the ordinary varieties, the following framework should suggest some grounds for the analysis of scientific pursuits. Although it is both general and brief, it will help in the examination of linguistic theory that follows.

(1) Scientific problem solving typically involves *problem transformations*. For example, theoretical accomplishments often come at the expense of cherished facts, when the pioneering theoritician denies the significance of certain phenomena that earlier theories addressed. Kuhn argues that this is most prominent early on and during revolutionary change, but scientists regularly modify the form, conditions, and range of problems. Each approach selects and emphasizes different variables — altering problems, sometimes inconspicuously but often substantially. Collectively, minor modifications can constitute a considerable change. In extreme cases, it may be more accurate to say that the strategy does not even address the problem that motivated it, but another that only resembles the first. These alterations always aim to increase a problem's manageability, often by increasing its resemblance to better understood problems (e.g., in psychology, treating cognitive functioning as if it were computer functioning). Philosophers have talked about such transformations in terms of "idealizations", but they are not confined to theoretical pursuits.

(2) Problem solving in science attends to cost/benefit considerations, acknowledging limits on problem solving resources. Often scientists willingly settle for *cheap* shortcuts. Complex problems can require expenditures that may exceed the available material or computational resources. Such predicaments often provoke technological developments (such as the new generation of supercomputers), but an economical alternative is to use heuristic strategies to simplify the problem at hand. "The expected time, effort, and computational complexity of producing a solution with a heuristic procedure is appreciably less (often by many orders of magnitude for a complex problem). . ."[2] The resulting gains in efficiency compensate for losses in accuracy.

(3) Such simplifying strategies offer no guarantees. Problem solving strate-

gies in science (as well as theories, experiments, and technology that emit therefrom) are *conditionally reliable.* Although the interdependence of theoretical development and the technology of observation and measurement seem to insure the accuracy of a dominant theory in a mature science, the "approximative character"[3] of most theories (and of the computations of their test predictions) is noncontroversial. Increased accuracy is typically balanced against theoretical unity, explanatory range, and computational costs (eventually small gains in accuracy can become quite expensive). Not only do these strategies not guarantee accurate solutions, they need not guarantee any. This is a function of their economy. Seeking short cuts (even in familiar territory) can lead to dead ends. At scientific frontiers (where *no one* is at home) even the distinction between short and long cuts may break down. There, short cuts amount to adopting previously successful strategies hoping that familiarity with the strategy will compensate for ignorance of the new domain. Kepler's preoccupation with the five regular solids for modeling the architecture of the solar system is an apt illustration. The general point consists in the simple fact that in scientific problem solving previously successful strategies sometimes work in new domains, but sometimes they do not.

(4) These deficiencies reflect a less obvious consequence of these strategies' thrift. Emphasizing one factor, where saliency is a function of inexpensive access by way of statistical significance or computational efficiency (or mere physical availability with practical problems), must come at the expense of other factors when resources are finite. This constitutes *bias.* Emphasis is inherently discriminatory. Every item emphasized is another item ignored. Where strategies fail can be partially anticipated, viz., where, because of extraordinary circumstances, underemphasized variables become salient or where fundamental systemic changes occur. Such failures are predictable, because the biases are systematic. During a theory's history scientists discover where it fails and, therefore, information about the biases it inherits from the discovery strategies that generated it. Independent testing, i.e., exploring the consequences of theories beyond the domains they were originally designed to address, reveals these biases. The direction of this research is a function of experimental, technological, and theoretical developments in the science at hand and in related sciences. As such limitations accumulate and persist, they serve to delimit the theory's range of effective application. Scientists learn how to manipulate the context to make the theory fail. We know not only when Newtonian equations cease to adequately approximate phenomena, but why they so cease. The frontiers of a theory's range of application are of particular interest, because they define problems for future research and important constraints on any adequate successor.

To summarize, then, if most problem solving strategies in science are

biased, conditionally reliable, and potentially misleading (in their problem transformations), then they must have some compensating advantages! There are at least two. They are less expensive and they simplify and clarify the problems scientists face. Some theories (e.g., in mechanics) seem to offer alogorithms for certain explanatory and predictive problems. In the course of research, though, even these theories disclose both their own limitations and those of the discovery strategies that generated them. However, if the changes in successor theories are not severe, an earlier theory may persist as a *calculating heuristic.*[4] It is not that scientists *never* utilize alogorithms, but they are not prominent in most scientific problem solving.

III. DISCOVERY STRATEGIES AND LINGUISTIC THEORY

The remainder of this paper examines the discovery strategies that inform Chomsky's linguistic theories. Many critics offer *principled* complaints about Chomsky's use of these strategies.[5] If linguistics is an empirical science, though, these objections are ill-founded — as Chomsky maintains. A priori complaints about common discovery strategies are ineffective, *if the resulting theories prove empirically fruitful*, but on the other hand, a strategy's popularity guarantees nothing about the empirical adequacy of those theories.

The competence–performance distinction is central to Chomsky's project. It contrasts the *use* of language with the *cognitive structure* (or tacit knowledge of a grammar[6]) that informs that ability. Competence concerns the internally represented grammar of some particular natural language *and* more general principles ("universal grammar" or "UG") that constrain particular grammars. The *move* from behaviour ("performance" in one of its senses[7]) to knowledge and the *idealized* account of that knowledge (as competence) are the transformations which undergird Chomsky's programme and distinguish it from its predecessors and competitors. These transformations result from Chomsky's use of two ordinary strategies in science, viz., a level shift and an idealization.

A. Level Shift

The move from behaviour to knowledge permits Chomsky to ignore the overwhelming diversity and complexity of language use. He offers a theory of linguistic competence, concentrating on what language users must *know* (in some sense) to use language as they do. The focus shifts from behaviour to the psychological structures which constrain it.[8] This purchases a theoretically enhanced descriptive vocabulary which differs substantially from that of other approaches. As Chomsky claims, "this level of abstraction is appropriate in that by moving to it, we are able to discover and formulate explanatory

principles of some significance, principles that should, furthermore, help guide the search for mechanisms."[9] Specifying mechanisms is Chomsky's goal; the novelty of his proposal in linguistics concerns the level at which he describes them. This is an example of a class of problem solving strategies in science that involve a shift from one level of analysis to another.

The notion of a level of analysis assumes that science has a roughly hierarchical structure which mirrors one in the world. Corresponding to each level of analysis is a level of organization, where entities of roughly the same size causally interact to form what are, for most explanatory purposes, relatively closed functional systems. Usually, how finely or coarsely levels are differentiated depends on the problem at hand. However imprecise criteria may be, they are not arbitrary and for most theories we have few problems ascertaining the governing level of analysis.

The altitude of a level of analysis is directly proportional to the complexity of the systems with which it deals and inversely proportional to the size of its domain. A higher level of analysis describes smaller ranges of events in increasingly organized physical systems. Biological analyses describe more complex systems than the chemical analyses that undergird them do, but the events with which biology deals are only a subset of those chemistry discusses. Thus, biology is the higher level. The higher in this arrangement we go, the less well componential and structural considerations distinguish levels. In its upper reaches functional analyses prevail since they are better suited for the analysis of complex systems.[10]

Considerations motivating level shifts include computational complexity, conceptual and theoretical resourcefulness, technological and metrical development, and the familiarity and robustness of entities, concepts, and principles. Concerns about resourcefulness come in two varieties. Sometimes the goal is to enlist a powerful theory at some level; sometimes it is to escape one. Scientists undertake interlevel moves in search of explanatory power. Thus most level shifts go *down* to capitalize on the maturity and mathematical rigor that lower level theories afford. They are, in short, reductionistic or, more accurately, microreductionistic.[11]

Level shifts,though, need not always go down. Theoretical resources are not always unambiguously distributed between levels.[12] Usually, mature theories strictly constrain the problem solving approaches they endorse. One less popular, but no less legitimate, strategy is to move *up* to what is generally a less ossified level to gain[13] conceptual space in which to theorize anew, i.e., to gain theoretical freedom. Crucially, these interlevel moves should be *primarily* construed as a means of either acquiring new problem solving resources or avoiding problem solving burdens and not primarily (as many philosophers are wont to do) as reductionistic ploys whose goal is ontological economizing. This holds *even* in those contexts where hypothetical identities seem to clearly offer grounds for drawing ontological conclusions. Philosopher's preoc-

cupations with reduction in such interlevel (*in contrast to intralevel*) contexts, however, are thoroughly misplaced.[14]

Chomsky's upward shift from his empiricist, structuralist, and behaviourist predecessors' approaches is not unique. The shift to studying populations aided development of the unified, synthetic theory of evolution. In both the biological and linguistic cases, it is not obvious precisely what systems the theories address, *if* systems are taken either to be or to be made up of discrete, localizable objects with distinct functional roles. Predictably, componential approaches have offered little help in these contexts, since it is unclear what would even count as components (as the levels and units of selection controversies demonstrate). As Sober argues[15], the barriers to analysing the competence–performance distinction in terms of levels of analysis are no greater than and quite similar to those in the biological case (where researchers have employed such analyses without hesitation). When bottom-up strategies yield little progress, upward level shifts permit a top-down approach, focusing more easily on the *processes* which distinguish the system.

The value of a level shift, its direction notwithstanding, resides in the type of problem transformation it achieves. This procedure translates an apparently intractable problem at one level of analysis into a more tractable problem at another, even if the new form of the problem must be created just for the occasion. This amounts to little more than trading one set of theories and their biases for another in hope of gaining new insights.

B. Idealization

Using idealizations is doing science in what Chomsky calls the Galilean fashion. His approach involves at least three: that UG is "common to all human beings", that language learning is "as if it were instantaneous", and that competence theory deals with "an ideal speaker–listener, in a completely homogeneous speech community . . . unaffected . . . by grammatically irrelevant conditions . . . in applying his knowledge of the language in actual performance."[16] All, and especially the last, have proven controversial.

Scientists use idealizations to abstract significant features of the phenomena — ignoring those taken to be causally insignificant. This enables them to incorporate insights about a system without engaging in the expensive procedures of methodical observation and testing. A ceteris paribus clause summarizes the import (or rather the lack of it) of the eliminated variables.

That scientists regularly employ idealizations is no news. However, that idealizations display the properties of problem solving strategies outlined above, perhaps, is. For example, idealizations neither insure success nor offer guarantees. Their transformations of problems can be so severe that they can virtually guarantee the resulting theories' failures![17] An idealization's prob-

lem transformation clarifies because it simplifies. If a complex problem persistently resists solution, one strategy to get the research programme back on track is to work on a simpler, idealized problem that resembles the former in crucial respects. These respects are not decided a priori, but are implicit in the theory and stand or fall with its success. What matters in mechanics are bodies' masses and momenta, not their ages or colours. "Extraneous" variables are ignored. Their values' resemblance to the real world is presumed unimportant (so the universe is frictionless and bodies are point masses).

IV. THE COMPETENCE APPROACH TO THEORIZING: PROSPECTS AND PROBLEMS

This section discusses, first, what the competence approach to theorizing buys and, then, it examines the price. The first half discusses its advantages and comments upon unsatisfactory objections. The second, however, considers an important respect in which the resulting theories have yet to follow through.

A. Prospects

Chomsky's focus on an *idealized* speaker–listener in an *idealized* speech community (whose learning is instantaneous) frees his theorizing from addressing the diversity and complexity of actual linguistic behaviour.[18] The *level shift* underlies his claim that linguistic theory accounts for that idealized speaker–listener's *knowledge* of a grammar, liberating it from biological (and behaviourist) concerns and placing it squarely within the cognitive.[19] Consequently, *as Chomsky has consistently argued, linguistics is part of psychology* and as such this sort of insulation from physical concerns is neither unique (see note 13) nor intrinsically problematic. This shift to the psychological level of analysis involves a theoretical and explanatory leap (across an apparent ontological divide) to environs where theories do not seem to submit readily to microreduction.[20] The shift is fairly described as upward for two reasons. First, it is in the opposite direction of shifts from the psychological to the physical (e.g., the neurophysiological), which are clearly downward, i.e., reductionistic. Also, although bodily states are not *parts* of cognitive states, they are constitutive of them in some sense.

A formal theory of an idealized speaker–listener's grammar helps to characterize the parameters of universal features of human language which define its biological basis and the initial state of linguistic "knowledge". In conjunction with the "idealization" that "UG is a species characteristic, common to all humans"[21], this approach (compared to its predecessors) faces a simpler theoretical task in certain respects. The competence approach restricts the range of phenomena to be explained and the number of variables to be considered. Redefining linguistics as the study of *knowledge* of a grammar enabled linguists to trade in a set of apparently disparate empirical

generalizations about aspects of behaviour for a unified, formal, quasi-algorithmic theory. Its *psychological focus* motivates Chomsky's claim that a developed theory of competemce will render our pretheoretic notion of language "a derivative and perhaps not very interesting concept." On Chomsky's view linguistics is not even concerned with language study in one sense. He is interested in "internalized language" (I-language) which is a "structure in the mind", not "externalized language" (E-language) which is "a collection (or system) of actions or behaviours." Chomsky says that "the E-language that was the object of study in most of traditional or structuralist grammar or behavioural psychology is now regarded as an epiphenomenon at best."[22]

Central to competence theory's psychological project is the controversial claim that competence is knowledge.[23] Critics argue that it is not *knowing that*, not exactly *knowing how*, not conscious generally, and not a matter of justifying beliefs. Chomsky, however, says that the issue is terminological and has offered the more neutral "cognize" to replace "know." He stresses that competence is a cognitive structure which must be represented in the mind to account for language use. (Whether the proposed grammars describe that structure accurately is another issue.) Fodor has argued[24] that if conscious, linguistic processing would be as intelligent as any use of knowledge.

Chomsky and Fodor correctly hold that nothing *directly empirical* is at stake in extending the concept 'knowledge.' This move offers problem solving dividends and reinforces the claim that competence theories reveal features of cognitive structures. The aim is to bring such unconscious, cognitive systems (like linguistic competence) into the psychological fold. Science has proceeded before where critics (due to conceptual or methodological scruples) have feared to tread. Chomsky willingly expands our concept of knowledge, because it offers a wealth of problem solving opportunities and it transforms the problems at issue in some particularly fruitful ways.

Construing competence as knowledge buys a new body of data to explain, viz., native speakers' linguistic intuitions which are manifestations of their tacit grammatical knowledge. They constitute a device for ascertaining facts about I-language. This intuitive evidence has some obvious assets. It can be multi-dimensional — providing evidence about the acceptability of strings, their constituent structures, and semantic considerations such as ambiguity and synonymy. Also, speakers' intuitions are not univocal and, thus, can require some theoretically inspired selection: "since performance . . . obviously involves many factors apart from competence, one cannot accept as an absolute principle that the speaker's judgments will give an accurate account of his knowledge."[25] Where intuitions conflict or are unclear, the theory adjudicates. Finally, this evidence is about as inexpensive as it could be, since it is *intuitively* available!

The transformations employed yield other startling economies. Ideally, data from only one informant (often the linguist) and only one language[26] (often the linguist's) are necessary to test hypotheses about particular grammars *and (at least, in principle) UG*. Although examining other informants and languages is not forbidden, it is also not required.[27]

B. Problems

A problem solving strategy's strengths can also be the source of some of its weaknesses. The means by which it renders a problem manageable can be the devices that render it ineffective in other contexts. Its long term success directly relates to the number and prominence of such problematic contexts and the character of its failures within them. The competence approach offers the advantages but also incurs the responsibilities and risks of exploration in relatively uncharted territory. The price comes in the expectations it engenders regarding the resulting theory's ability to defend its psychological aspirations on independent grounds and to contribute to the rest of psychology. *This, however, turns on the theories' contributions to empirical research in psychology* and *not* on critics' aprioristic objections.

Chomsky rejects the possibility of a developed theory of linguistic behaviour, but he has also repeatedly suggested[28] that some representation of a grammar makes an effective contribution to *actual* linguistic *processing* in real, not-so-homogeneous speech communities. Hence, a competence theory should presumably *contribute* to theories of linguistic processing and suggest and pass psycholinguistic tests. Such claims of Chomsky's as "study of performance relies *essentially* on advances in understanding of competence" have encouraged such expectations, and the rapid rise of a Chomsky-inspired psycholinguistics in the late 1960s reveals how seriously experimental psychologists took Chomsky's claims.[29] So far, unfortunately, competence theories have not been especially effective with these tasks. This is due to the biases inherent in the competence approach and a further empirical assumption that Chomsky makes (concerning modularity) which has only added to the barriers involved.

Like all empirical problem solving strategies the competence approach embodies some crucial biases. Focusing exclusively on the formally characterizable dimensions of a language and, according to the theory, that part of the mind concerned with the same involves ignoring other variables that affect linguistic processing. In actual linguistic processing it is *not* obvious that these factors that the competence approach ignores (such as computational limitations, pragmatic and conversational considerations, mistakes, and irregularities) are irrelevant noise, and the first of these, at least, concerns cognitive mechanisms with which competence surely shares a functional interface. So, for example, generative grammars allow for indefinitely long

sentences (via syntactic means they deem legitimate) which sheer memory limitations preclude in actual language use. They also approve sentences with degrees of syntactic complexity that exceed our computational limits in other ways as well.[30] The obvious fear is that this approach's idealizations depart so far from the conditions of actual linguistic processing that they undermine any larger explanatory suggestiveness which might be claimed in behalf of the theories that result.

Chomsky's modularity claims, which stress that the principles involved in linguistic processing are task specific and that the relevant cognitive system is relatively isolated both functionally and ontogenetically, would not seem to encourage researchers either. The "advances in understanding of competence" that Chomsky awaits certainly *might* suggest constraints on the other cognitive mechanisms involved in the production and comprehension of utterances. Still, Chomsky has consistently minimized the suggestion that the character of these other cognitive mechanisms might constrain competence theories in any significant way. The desired theoretical integration requires accounts (of both competence and those other cognitive mechanisms) which are more commensurable.[31] The aim here is not to attribute fault, but rather simply to point out this failure of integration. Since it is Chomsky who lays claim to some of psychology's turf, it seems that the burden of the evidence is on him.

The results of the psycholinguistic research which *has* been done are equivocal *at best*. Also, although Chomsky's theory *has* undergone numerous changes over the years, there is no evidence that *any* have been in response to uncongenial (or congenial) psycholinguistic findings. It is not that such considerations are or should be definitive. The evidence is mixed and what to make of much of it is not obvious. Nor is Chomsky clearly wrong to ignore this research — as he has, for the most part, done — in his various reformulations of his theory of competence.[32] Instead, the point is that competence theories have generally failed to connect with the rest of mainstream (cognitive) psychology, and when they do seem to connect (Chomsky's imperviousness notwithstanding) the empirical results have been neither particularly encouraging nor sufficient to support Chomsky's optimistic prognostications concerning their importance for that field.

It is not especially clear *what is at stake empirically* for psychologists, then, when Chomsky insists that linguistics is an integral (or even *a*) part of their field. (Chomsky himself has recently conceded that there are grounds for fearing that competence theory's "evidential base is too narrow to carry conviction"[33]). Additional intuitive evidence is unlikely either, generally, to produce the requisite progress or, specifically, to clarify the connection between theories of competence and the rest of psychology. Chomsky's claim that such evidence constitutes "independent empirical tests" of competence theories is not helpful *here*, since it is precisely for the explanation of such

phenomena that the theory was designed in the first place. Such tests speak to the theory's coherence, but indirectly, at best, to its ability to contribute to theories of linguistic processing, to theories in cognitive psychology generally, or to claims about the psychological reality of proposed grammars.

In conjunction with the modularity assumptions, then, the strategies which inspire competence theories tend to isolate (*but also to insulate*) them from work in related sciences. The level shift removes them (like theories in psychology generally) from research in biological science. The idealized domain removes them from important factors affecting language processing. The hypothesized functional and ontogenetic isolation of the system, the claims for the task specificity of its principles, and Chomsky's historic reluctance[34] to attend to the experimental results in psycholinguistics tend to remove them from work in other areas of psychology. Although these considerations do not *necessarily* vitiate the theories' psychological pretensions, they have generally done so *in fact*, since they insure (1) that the impact *on* competence theories of research in other areas of psychology is negligible, (2) that the impact *of* competence theories on research in other areas of psychology is negligible[35], and (3) that competence theories are not generally constrained by any of our knowledge about performance, cognition, or biology. Unfortunately, the isolation seems inevitably to accompany the insulation.

The insulation is important, though, because, so far, competence theories have failed to achieve even descriptive adequacy. (Chomsky acknowledges this.) Competence theories have yet to satisfactorily account for many phenomena in their own highly idealized domains, let alone suggest specifically how they bear on problems arising in less idealized settings or how they might meet intertheoretic or broader empirical responsibilities! Unfortunately, it seems that the cost of the insulation is the isolation and that the cost of the isolation is a sharp reduction in the number of sources of possible empirical support for the theories' strong psychological claims. The testimony of the intuitive evidence alone is insufficient, since it is the proximal goal of the theory to organize that. Without some other source of *independent* support, these psychological presumptions remain just that. Chomsky's confidence notwithstanding, few grounds presently exist for expecting competence theories to play a pivotal role in the progress of *psychological* research on language.

Of course, these worries are groundless, if linguistics is a strictly formal pursuit seeking to achieve *descriptive adequacy* only.[36] It is, however, *Chomsky* who has (1) argued that the study of competence is a subdiscipline of psychology, (2) sought to establish that study as an *independently testable* empirical science, (3) insisted that "study of performance relies *essentially* on advances in understanding of competence," and (4) held, against views like Katz's that *explanatory adequacy* is the goal of linguistics and, thus, that the *psychological reality* of linguistic competence is a viable topic for empirical (which presumably includes experimental) research. It is not unreasonable in

light of (1)–(4) to expect linguistic theory to offer some near-term empirical payoff in the psychology of language, and Chomsky himself has certainly *seemed* to anticipate[37] just that. Whatever his intentions, Chomsky's comments about his position *should* have engendered such expectations, and progress on these fronts is imperative, if Chomsky's claims about the import of competence theory for psychology are to wash.

Still, *these are questions of fact, not principle.* These comments ought not be taken as condemning the discovery strategies which inform competence theories, but rather as making some suggestions about their limits. Unexceptional in other fields, they collectively constitute a potentially suggestive discovery procedure in psychology, and thus, per se, *they* ought not to be the focus of attacks on Chomsky's position.

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NOTES

[1]See, for example, R. Tweney, M. Doherty, and C. Mynatt (eds.), *On Scientific Thinking* (New York: Columbia University Press, 1981).

[2]W. Wimsatt, "Robustness, Reliability, and Overdetermination," in M. Brewer and B. Collins (eds.), *Scientific Inquiry and the Social Sciences* (San Francisco: Jossey-Bass, 1981), p. 153.

[3]L. Laudan, *Progress and its Problems* (Berkeley: University of California Press, 1977), pp. 22–24.

[4]See R. McCauley, "Intertheoretic Relations and the Future of Psychology," *Philosophy of Science* 53 (1986): [in press] for more on heuristics of discovery and calculation.

[5]Probably the most extended attack appears in B. Derwing, *Transformational Grammar as a Theory of Language Acquisition* (Cambridge: Cambridge University Press, 1973). More recently, see R. Harris, "Theoretical Ideas," *Times Literary Supplement*, October 14, 1983.

[6]N. Chomsky, *Aspects of a Theory of Syntax* (Cambridge: MIT Press, 1965), p. 27 and *Reflections on Language* (New York: Pantheon, 1975),pp. 160–69.

[7]See p. 22 of F. D'Agostino, *Chomsky's System of Ideas* (Oxford: Oxford University Press, 1986).

[8]In chapters 1 and 2 of N. Chomsky, *Knowledge of Language* (New York: Praeger, 1986), Chomsky refers to his approach to linguistic theorizing as involving "a shift of focus" from "behaviour and its products to the system of knowledge that enters into behaviour . . ." (p. 28).

[9]*Op. cit.*, Chomsky 1986, p.221.

[10]R. McCauley and E. Lawson, "Functionalism Reconsidered," *History of Religions* 23 (1984): 372–81.

[11]I talk of reduction and microreduction only to identify the intertheoretic contexts at issue — *not* because I endorse the microreductionists' conclusions.

[12]As between neurophysiology and cognitive psychology (M. Allen, "Models of Hemispheric Specialization," *Psychological Bulletin* 93 [1983]: 73–104).

[13] . . . or to maintain it (by resisting downward shifts). M. Posner (*Chronometric Explorations of Mind*, Hillsdale, New Jersey: Erlbaum, 1978, p. 29) warns against closely identifying psychological and physiological processing: it "leaves psychologists without any independent definitions to explore new levels or to determine the organization of levels (within psychology). This is particularly critical . . . where physiologists have not obtained data . . . Psychological studies could then aid in understanding the type of mechanisms that might be required at a given level. This approach is of particular value if psychological findings are to be useful in guiding and directing neurophysiological research in such areas as language and attention."

[14]See N. Maull, "Unifying Science Without Reduction," *Studies in History and Philosophy of Science* 8 (1977): 143–62, R. McCauley, "Hypothetical Identities and Ontological Economizing: Comments on Causey's Program for the Unity of Science," *Philosophy of Science* 48 (1981): 218–27, and McCauley 1986.

[15]E. Sober, "Language and Psychological Reality: Some Reflections on Chomsky's *Rules and Representations*," *Linguistics and Philosophy* 3 (1980): 395–405.

[16]References (in order) are Chomsky 1986, pp. 18 and 52 and 1965, p. 3.

[17]Wade has shown that in virtue of certain idealizing assumptions all previous models of group selection *inevitably* failed. These models greatly reduced research costs. but also insured either the undetectability or impossibility of group selection. In his experiments where these (and certain other) idealizing assumptions were relaxed or discarded, Wade found group selection to be a robust phenomenon. (M. Wade, "A Critical Review of the Models of Group Selection," *Quarterly Review of Biology* 53 (1978): 101–14 and "An Experimental Study of Group Selection," *Evolution* 31 (1977): 13–53.)

[18]Chomsky claims (*Language and Mind* [New York: Harcourt Brace Jovanovich, Inc., 1972], p. 111) that the idealization ". . . expresses the working hypothesis that we can proceed with the study of 'knowledge of language' . . . in abstraction from the problems of how language is used. The working hypothesis is justified by the success that is achieved when it is adopted."

[19]Z. Pylyshyn, "Competence and Psychological Reality," *American Psychologist* 27 (1972): 548, claims that competence theory ". . . is about 'mental representation'." It is *not* that competence theorists are not physicalists. but only that physical findings need not constrain their theorizing in any significant way. See Chomsky 1986, pp. 36–37 and 253–54.

[20]J. Fodor, *Psychological Explanation* (New York: Random House, 1968), pp. 111–12 and *Representations* (Cambridge: MIT Press, 1981), pp. 127–74.

[21]*Op. cit.*, Chomsky 1986, p. 18.

[22]See N. Chomsky, *Rules and Representations* (New York: Columbia University Press, 1980), p. 90 and Chomsky 1986, chapter 2 and pp. 147–60.

[23]See, for example, D. Cooper, *Knowledge of Language* (London: Prism Press, 1975) and C. McGinn, Review of *Rules and Representations*, *Journal of Philosophy*, (1981) 78: 288–98.

[24]*Op. cit.*, Fodor 1968, p. 87.

[25]*Op. cit.*, Chomsky, 1972, p. 187. Also see Chomsky 1965, p. 24, Chomsky 1986, p. 36, F. Newmeyer. *Linguistic Theory in America* (New York: Academic Press, 1980), pp. 20–21, Pylyshyn 1972, p. 547 and Pylyshyn, "The Role of Competence Theories in Cognitive Psychology," *Journal of Psycholinguistic Research* 2 (1973): 30.

[26]Recently, Chomsky has avoided (but not renounced) this claim. See Chomsky 1986, p. 37 and his *Lectures on Government and Binding* (Dordrecht: Foris Publications, 1982), p. 6.

[27]Whether Chomsky's approach justifies his pessimism about theories of E-language, the major alternative (entertained in S. Soames, "Linguistics and Psychology," *Linguistics and Philosophy* 7 [1984]: 155–79 and embraced in J. Katz, *Language and Other Abstract Objects* (Totowa, New Jersey: Rowman and Littlefield, 1981]), which affirms their centrality, is less satisfactory. First, it disregards competence theories' successes at systematizing linguistic intuitions. Although (contra Chomsky 1986, p. 37) such data are not privileged, *nor are they irrelevant*, since they suffice ". . . to

confirm and refute proposed theories." (Chomsky 1986, p. 260) (The value of evidence turns on factors like its novelty, accessibility, quantity, and precision, and the theory's original aim and the immediate problems.) This data and its systematization should be of interest to psychology. Moreover, this view underestimates the potential contribution of these problem transformations to theorizing about the cognitive mechanisms behind performance. Whether linguists' original problems (Chomsky 1986, chapter 2 and D'Agostino 1986, chapter1) required them (and their economies) to get a theory flying is not the point. What matters is that they *did* and some theory is better than none.

[28]*Op. cit.*, Chomsky 1980, pp. 204 and 226, for example.

[29]*Op. cit.*, Chomsky 1980, emphasis mine. A. Reber, "The Rise and (Surprisingly Rapid) Fall of Psycholinguistics," *Synthese* (forthcoming) discusses the steady increase in citations to Chomsky in psychological papers through 1973 (*and their corresponding decline ever since*).

[30]*Op. cit.*, Newmeyer 1980, p. 219 and Chomsky 1980, p. 225 and 1986, p. 25.

[31]*Op. cit.*, Chomsky 1982, p. 18.

[32]The major reasons he has concern competence theories' inability to handle problems in their own domain. See below p. 309 and R. McCauley, "The Not So Happy Story of the Marriage of Linguistics and Psychology or How Linguistics Has Discouraged Psychology's Recent Advances," *Synthese* (forthcoming).

[33]*Op. cit.*, Chomsky 1986, p. 260.

[34]*Op. cit.*, Chomsky 1975, pp. 36–38.

[35]This is Reber's finding (see note 29) in analysing citations since 1973.

[36]*Op. cit.*, Chomsky 1965, chapter 1 and D'Agostino 1986, pp. 6–7.

[37]*Op. cit.*, Chomsky 1980, pp. 25 and 225–26.