

ROBERT N. McCAULEY

THE NOT SO HAPPY STORY OF THE MARRIAGE
OF LINGUISTICS AND PSYCHOLOGY
OR
WHY LINGUISTICS HAS DISCOURAGED
PSYCHOLOGY'S RECENT ADVANCES

Apparently, it is as rare in inquiry as it is in life for two admirers to rekindle a dying relationship. Linguistics and psychology have twice failed to sustain a lasting romance. Certainly, each shares some of the responsibility for these failures. In the first case, Arthur Blumenthal has discussed (here and elsewhere: see Blumenthal, 1970, 1975) how (academic) generations of empiricistic linguists and psychologists in the first half of this century permitted serious misrepresentation of Wundt's views generally, and suppression of his program of psycholinguistic research in particular. In the more recent liason, however, Arthur Reber argues (see also Reber, 1973) that it is primarily linguistics that has discouraged psychology's advances. Presumably, linguistics is particularly culpable, since *it* initiated the courtship this second time around. Chomsky, after all, has repeatedly asserted that he would "try to develop the study of linguistic structure as a chapter of human psychology" (Chomsky, 1972, p. 66).

It is where Wundt's and Chomsky's views converge that is most central to understanding the emergence of psycholinguistics (the first time, the second time, or *any* time). I will briefly address those points of convergence first, ultimately suggesting that they constitute important contributions if not to the theoretical progress, then surely to the methodological progress of the relevant sciences. Then I will return to discuss at greater length some of the peculiar details of this second affair between linguistics and psychology, offering in the process a *partial* justification for contemporary linguistics' wanton ways.

It is, first of all, certain negative claims on which Wundt and Chomsky concur. Both have directed devastating criticism against the associationism which has generally prevailed in the philosophy, psychology, and linguistics of the last hundred years. Both have rejected claims that association, analogy, general learning strategies, inductive inference, or any other such process can suffice to account for either

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linguistic competence or performance, and both have recognized the *theoretical* sterility of any such approach. Chomsky and Wundt look to our mastery of grammars and the character of the grammars we have mastered for support. The grammatical principles necessary to account for the diversity of sentences which native speakers readily comprehend and produce involve abstract relationships sufficiently removed from any physical properties of tokens to eviscerate whatever plausibility claims might accrue to the notion that the child acquires these principles inductively. No account of sentence frames in terms of the surface features of sentences will accommodate the syntactic facts (at least as they are evinced in the intuitions of native speakers). No history of associations can explain either the availability of the indefinitely large class of sentences we can process or the facility with which we, in fact, process them. The cumulative effect of these objections is overwhelming, and it is only our theoretically impoverished imaginations that have permitted us to rest content with some species of associationism (Blumenthal, 1970, pp. 22–31; Chomsky, 1959, 1965, pp. 16–17).

These considerations collectively motivate the “rationalist” alternatives that Wundt and Chomsky have developed. Each denies that the surface forms of sentences have any necessary isomorphic relationship with their underlying linguistic representations. Yet, both assert that those abstract representations are necessary for the explanation of both our linguistic ability and its products. Consequently, Wundt and Chomsky theorize about corresponding psychological processes and representations to account for that ability.

Although (at least some versions of) their theories manifest *more* than a rare, isolated coincidence of substantive detail, the foregoing exhausts most of what I take to be *most* significant about their positions. I will return at the end to what I see as the source of this significance. Now, though, I wish to turn to an important point about which Wundt and Chomsky do not agree, viz., the extent to which our linguistic abilities are of a piece with our other cognitive accomplishments and, derivatively, the extent to which common principles apply to both.

For Wundt, Blumenthal reports, “the particular form that spoken sentences take in all languages is a result . . . of universal characteristics of human mental processes” (p. 317, see also Blumenthal, 1970, p. 12), but as Blumenthal also accurately notes “Chomsky . . . does not

make language so subservient to psychological principles as did Wundt" (p. 318). Prima facie, this reluctance on Chomsky's part seems a bit inconsistent with his claim (cited earlier) to the effect that linguistics will prove to be a sub-discipline of psychology, or with his claims that the distinction between the two fields seems "quite senseless" (Chomsky, 1980, p. 202) and that linguistic competence constitutes a "central domain of human psychology" (Chomsky, 1980, p. 204). Such comments have led many of Chomsky's critics to think that he not only wishes to resurrect the relationship between linguistics and psychology, but that he would have them happily married at the earliest possible moment. In addition, they have assumed that such claims commit Chomsky to the further Wundtian position that any general psychological principles that we might discover should constrain the form of linguistic theory, at least in principle. This, however, is a move that Chomsky has consistently resisted. In the remainder of this paper, I wish to examine the ways in which Chomsky is and is not justified in opposing the psychological imperialism that *he* seems to have invited.

The arguments briefly outlined above against associationist approaches to language have collectively come to be known as the argument from the poverty of the stimulus. In Chomsky's hands, the aim of this argument is not exclusively destructive. He employs the argument constructively as well. He argues that it offers evidence for "specific theories of language universals" (Chomsky, 1980, p. 68) in the form of an inference to the best explanation. In reflecting on the extreme subtlety of grammatical judgments which any native speaker is capable of making, Chomsky asserts "the argument from poverty of the stimulus leaves us no reasonable alternative but to suppose that these properties [of our grammatical judgments] are somehow determined in universal grammar, as part of the genotype" (Chomsky, 1980, p. 66). The assumption is that since the information available in the environment is so scanty, we must have reasonably powerful mechanisms by which we construct systems of knowledge on the basis of impoverished input. Chomsky and his followers have, thus, propounded a view of the mechanism behind linguistic processing (at least the perceptual side of that processing) which is what Jerry Fodor has called a module (Fodor, 1983).¹

Modular mechanisms and the principles according to which they operate are, among other things, thoroughly endogenous with respect

to their general structure, domain specific, informationally encapsulated, and cognitively impenetrable. (On this final notion see Pylyshyn, 1980.) This is to say that their structure and functioning are innately constrained, that they are task specific, and that they operate, for the most part, in relative isolation (as Reber puts it), generally impervious to attempts at cognitive intrusion. An important consequence of this account (for our purposes) is that Chomsky, Fodor et al. need not concede that linguistic theory will be constrained in any interesting way by otherwise general psychological principles, even if we might ever discover any (the appearances of various assertions by Chomsky to the contrary notwithstanding: for example, Chomsky states that "the structure of the grammar internalized by the learner may be, to a presently quite unexpected degree, a reflection of the general character of his learning capacity..." [Blumenthal, 1970, p. 208]). Of course, what Chomsky always seems to mean by such comments is that principles quite like those which linguists (will) discover for universal grammar will populate our future theories of cognitive functioning generally. Thus, Chomsky not only resists psychological imperialism; he counterattacks with a linguistic imperialism of his own.

From the poverty of linguistic stimuli, it certainly does follow that associationism will prove inadequate to account for our linguistic facility. The impoverished stimuli of this domain, however, are not unique. *All stimuli are impoverished!*² Philosophers of science, at least since Popper, have recognized that *all* statements are theoretical statements, i.e., that we can make sense neither of a pure observation statement nor of a pure observation (Popper, 1965, especially pp. 42-48, 386-88). What follows is that in *every* domain we must search for increasingly fruitful theories, but, contra Chomsky, nothing specific follows about the content of those theories.

The consequences of an encapsulated language module are the source of most of the barriers to a harmonious relationship between the two disciplines that Reber notes in his paper. They balkanize psychology, because they insulate theories of the language organ from other psychological research. These consequences, in conjunction with the narrow domain of linguistic theory (viz., syntax), seem to generate a peculiar dilemma for those anxious to see linguistics and psychology consummate their recent reunion. For, certainly, if something like the modularity thesis is true (the first horn), then, contra

Chomsky, it would seem to undermine any substantial pretensions about the study of human language arriving at principles that will offer direct insight into cognition generally, since it would establish linguistic *perception*, anyway, as a highly specialized, functionally isolated, and essentially unique cognitive process. On the other hand, if the modularity claims are false (the second horn), then it is still difficult to see what guidance formal theories of syntax (to the extent that they embody modular assumptions) can offer cognitive psychology either theoretically or experimentally as it tackles other areas of cognition. So, it seems that whether modularity is true or false, theories of linguistic competence seem unlikely to offer much direction to either cognitive psychology or even the psychology of language, at least in the short run.

Fodor is quite willing to embrace the first claim; however, he thinks that the *truth* of the modularity thesis portends an even *more* discouraging future for cognitive research. As he construes it, linguistic theory offers a formal model for one of the human mind's six input systems, but it offers us few, if any, clues about the specific nature of more central cognitive processes. Indeed, Fodor speculates that we will never say much of a systematic character about these central processes, because of what he calls the isotropic and Quinean character of the contents on which they operate. This is to say that "the level of acceptance of any belief is sensitive to the level of acceptance of any other and to global properties of the field of beliefs taken collectively" (Fodor, 1983, p. 110). Not only does the modularity thesis generally insulate linguistic theory from results in other areas of cognitive psychology, Fodor contends that very little of a tractable character remains for cognitive psychology to be about (Fodor, 1983, p. 119). Theories of syntax are doable, precisely because Chomsky's competence approach to linguistic theorizing so effectively compartmentalizes their object of study. Chomsky's approach isolates syntactic theory not only from other cognitive considerations (such as memory, processing capacity, imagery, etc.) but from other *linguistic* factors as well (semantics in particular). Fodor's position *expands* Chomsky's imperialist ploy by declaring that linguistic theory (and his modularity approach) properly discusses all about cognition that we will ever be able to discuss.

It is not just the results of research in experimental psychology from which modularity claims insulate linguistic theory. They also seem to

immunize these theories from the experimental *methods* of that research as well. Wundt *seems* to offer something of a precedent on this count. He "preferred dialectical over experimental methods in the study of . . . concept formation and language" (Blumenthal, 1970, p. 15), because he despaired of the feasibility of experimentation in these areas (see Blumenthal, 1975, p. 1082).³ It is important to note, however, that Chomsky's grounds are different.

In fact, Chomsky has rather consistently ignored the experimental work not only of cognitive psychology generally but even of the psycholinguistics that his theoretical work has inspired. As Reber states, "no theoretical model of language has ever been rejected because it failed to account for the data from a psychological study of language" (p. 13). Hence, ironically, psycholinguistics emerged in the past two decades without much meaningful support from linguists.

The reluctance of linguists to accommodate the findings or appropriate the methods of experimental psychology rests on two grounds, neither of which they discuss much – especially in print. The first, which I will discuss immediately, is not justified. The second, which I will discuss ultimately, is justified, though it is unclear whether it alone fully warrants linguists' standoffishness on this count. Because linguistic theory is a highly idealized account of linguistic competence (see Chomsky, 1965, p. 3; see also McCauley, 1986), and because linguistic competence "is not realized in any direct or simple way in behavior" (Chomsky, 1972, p. 4), and because linguistic analysis is an encapsulated process, many linguists have validly concluded that the object of Chomsky's theory is isolated from considerations concerning our other cognitive faculties. But in addition, some seem to have invalidly concluded, at least tacitly, that the methods and techniques with which we test theories about those other cognitive faculties will therefore prove to be unnecessary at least, and perhaps even inappropriate, in testing linguistic theory. (See the discussion in Paivio, 1975. Also, I should note that Fodor is *not* guilty on this count.)

Clearly, Chomsky has worded his comments on these issues carefully. At one of the few points where he discusses the status of such experimental work, he asserts that "the linguist . . . the experimental psychologist, and the neurophysiologist are engaged in a common enterprise, and each should exploit as fully as possible the insights derived from all approaches . . .". He justifies his caution regarding

psycho-linguistic⁴ findings, however, on the grounds that "what is actually known or even plausibly surmised about these matters is limited and generally still quite remote from the questions that arise in the theoretical study of language". However, he then describes certain corroborating results as "suggestive" and argues that *they* "should be seriously considered" (Chomsky, 1975, p. 37).

As Reber emphasizes, however, linguists' reluctance to accommodate experimental findings has not interfered with their willingness to alter their theories repeatedly over the past two decades. Such inconstancy on the part of linguists has frustrated psychologists anxious to "exploit as fully as possible the insights derived from all approaches". Linguistics is a fickle date. Over the years, psychology had faithfully brought its experimental findings to bear on linguistic theory, only to find that linguistics had stood it up and gone off in some new theoretical direction – in Chomsky's case, from *Syntactic Structures*, to the standard theory, to the extended standard theory, to trace theory, to its government and bondage. But if not in response to psychology's experimental findings, then in response to what has linguistics proven so transformational?

In fact, these changes, without exception, have been in response to developments *internal* to linguistics. The point is that psychologists (and some linguists) have *seriously overestimated* the range, power, and adequacy of current linguistic theories. *No* proposed grammar for any particular natural language has achieved descriptive adequacy. Consequently, linguistic theories about principles of universal grammar (and derivatively about the innate structure of the language organ) have stood and fallen on rather partial linguistic grounds. The primary thrust of this claim is not critical. All theories, as Chomsky has forcefully noted (Chomsky, 1980, pp. 16–24), are crucially underdetermined – that is precisely the source of their interest. The point is that linguists have not needed to worry too much about accommodating psycho-linguistic data so long as so much theoretically unaccountable linguistic data abounds.

Chomsky's guilt on this point does not extend quite so far as some of his critics contend. Without a doubt some of his claims about the prospects of linguistic theory have remained unfulfilled hopes. Also, he has not always been completely forthcoming about the fact that it is the inadequacy and incompleteness of *linguistic theory* that is (to a great extent) responsible for its remoteness from research in experi-

mental psychology (see Chomsky, 1975, pp. 36–38 and Chomsky, 1980, Chapter 5). Similarly, Chomsky's insistence that bringing as yet unexplored linguistic considerations to bear on a grammatical hypothesis constitutes an "independent empirical test" (Chomsky, 1980, p. 199) is a bit misleading, at least in the context of such discussions, and finally, as we have seen, he is not wholly innocent of confirmation bias (see Tweney et al., 1980).

To his credit though, Chomsky has never surrendered his claims for the value of construing linguistics as a part of psychology. He has consistently affirmed the value of ultimately integrating the two disciplines. The fates of their particular formulations notwithstanding, Chomsky's and Wundt's most important insight is their recognition and insistence that considerable empirical progress in linguistics and the psychology of language ultimately depends upon their successful marriage and their procreation of the appropriate interfield theory in psycholinguistics (see Darden and Maull, 1977). The issue on which Chomsky *has*, perhaps, misled concerns whether we have sufficient grounds for anticipating both a successful union and its offspring sooner rather than later.

The short term inconclusiveness of this relationship has so discouraged some that they have recommended a Las Vegas divorce. Katz in our own time (Katz, 1981) and Delbrück in Wundt's (see Blumenthal, 1970, pp. 39–40) have argued for a view of linguistics in which it is completely independent of psychology. On Katz's view natural languages are abstract objects which we not only can study independently of a theory of language users, but which, in fact, *are* thoroughly independent of language users. For Katz mathematics offers the proper analogue for linguistic theorizing, not psychology. This approach is functionally equivalent to pushing the modularity view to its absolute limit. As Blumenthal notes (p. 8) this strategy buys the linguist complete theoretical freedom but at the price of most, if not all, of linguistics' empirical interest. Of course, if some future, more nearly adequate linguistic theory does begin to stand up to considerable psycho-linguistic testing, then Katz's position will suffer the same fate as Kant's arguments for the a priori truth of classical mechanics.

In contrast, others, such as Reber, foresee something more like a legal separation – at least between psychology and any "Chomsky-inspired" linguistics. In the space which remains, I will argue for a

slightly more sanguine view of both the history and the future of this relationship.

Reber indicates that Chomsky-inspired psycholinguists are an increasingly endangered species, and he asks why recent psycholinguistics has lacked staying power. He (correctly) answers that this is primarily due to the failure of Chomsky's theory to meet the empirical expectations it (perhaps inadvertently) engendered among psychologists. One specific factor contributing to this failure is what Reber regards as linguistics' preoccupation with theory, at the expense of data.

Although the apparent power of Chomsky's theories was an important contributing factor in instigating the romance, Reber is quite right in claiming that it was also influential in producing the recent estrangement (p. 10), precisely because appearances can be deceiving. That failure, though, has in no way undermined the importance of Chomsky's larger vision of linguistics entering the family of the psychological sciences. Chomsky's inability to bring that vision to fruition is a function of the incompleteness and inadequacy of both the specific grammars and the larger linguistic theories that he (and others) have so far proposed. It is *not* a function of their preference for theory over data (as Reber puts it). The lack of staying power of Chomsky-inspired psycholinguistics is due to the absence of a linguistic theory capable of withstanding the critical heat *within linguistics*.

Another way of putting this point is to ask not about psycholinguistics' lack of staying power, but rather about that initial fascination psychologists had with Chomsky's earlier work. As Reber states, theory has not been the long suit of twentieth century psychology, but contrary to Reber, *no* science can bootstrap itself into respectability on the basis of collecting data alone (see Popper, 1965, p. 46). Psychology so willingly embraced linguistics in our time, and linguistics so willingly embraced psychology in Wundt's, because in each case they wished to exploit the theoretical treasures in the other's dowry. They married, so to speak, for money. Chomsky has repeatedly attacked the absence of theoretical and explanatory interest in recent behaviorism (see Chomsky, 1959; Blumenthal, 1970, p. 197), and in its place he has proposed bold, new theories for one domain of psychological investigation. Until recently, as Reber testifies, theories have been something of which psychology has been in desperate need!

It is, as Reber shows, the theoretical flux in linguistics that has discouraged psychologists' advances. Psychologists have not broken off the courtship because their own field has suddenly inherited more powerful theoretical alternatives. Psycholinguistics will reemerge (and perhaps again fail) precisely when someone, anyone, even Chomsky, hits upon a more satisfactory linguistic theory.

The value of theories (even clearly false ones) is insufficiently appreciated. They direct scientific research, because they structure our expectations. It is the frustration of those expectations which defines science's problems. All theories, and especially new ones, are crucially underspecified. That immaturity is simultaneously the source of their vulnerability *and their potential* – both of which scientists explore in the course of empirical research.

Theories demonstrate their promise by solving problems. Solutions further specify theories which establish new consequences, which generate new expectations, which suggest experimental tests, which will yield imperfect results, which will define new problems whose solution will, in turn, require further theoretical specification. Repeated successes in this cycle of scientific discovery contribute to both the stability of a theory and its command over a particular domain. The direction of the extension of successful scientific theories depends upon a number of factors, including their general coherence with (and, ideally, reinforcement of) other theories in their conceptual vicinity. This, of course, is the engine that integrates scientific disciplines.

Theoretical stability is particularly important in this process. Successful theories embody numerous substantive, ontological, and methodological commitments which are the materials and the tools of integrative tests. Although scientists regularly borrow problem-solving strategies from other fields (see McCauley, 1986), pushing for a full scale integration of two scientific disciplines with plenty of staying power (for example, in molecular genetics or in biochemistry) can only occur when *at least* one (and usually both) has a stable, mature theory (see Maull, 1977). Such theories guide research in the new discipline by constraining the conceptual latitude in theorizing and by commending certain specific means for experimental tests.

Relatively speaking, both psychology and linguistics are theoretically immature. They are like two early adolescents – teeming with energy and promise, but insufficiently equipped on a number of counts

to go steady with one another, let alone to take on the responsibility of producing and guiding offspring as challenging as psycholinguistics. However, as Larry Laudan has observed, "...noting... a logical inconsistency or a relation of non-reinforcement between two theories need *not* force scientists to abandon one, or the other, or both" (Laudan, 1977, p. 56).

The modularity thesis has served as the first ground on which some linguists have relied to insulate their theories. I have argued that this is illicit. We have now arrived at a second ground, which is somewhat more sound, to justify the relative imperviousness of linguistic theoreticians to the results of psychological experimentation. At this point in the theoretical development of linguistics, it is a disguised blessing that most of our psycholinguistic knowledge (and our psycholinguistic knowledge) is "still quite remote from the questions that arise in the theoretical study of language". Linguistics and psychology both need as much conceptual space as they can get to develop more adequate theories (see McCauley, 1986). It is precisely because Chomsky-inspired linguists have remained at rather abstract, formal levels that they can tinker with their theories so freely on the basis of intuitive evidence. Although in the process they temporarily sacrifice some scope and reduce the testability of their formulations (see Tweney et al., 1980 for arguments in favor of confirmation bias at certain early stages of theorizing in science), the resulting freedom to speculate, unconstrained by possible inter-theoretic prospects, is crucial to the development of a syntactic theory mature enough to handle the major empirical problems in its own domain, at least. It is only *after* reasonably stable theories appear in either the psychology of language, linguistics, or both that we should hold out any hope of a psycholinguistics with any staying power.

NOTES

¹ Fodor correctly states (1983, p. 3) that his claims about modularity differ in certain respects from Chomsky's comments about the "language organ". The differences that in fact exist between their views, however, have no bearing on the arguments I wish to make here. Those differences, on nearly every count, reflect the additional remoteness of Chomsky's claims (in comparison to Fodor's).

² Though as J. J. Gibson has argued with respect to visual perception and as Arthur Reber has argued with respect to linguistic perception, the stimuli may not be as destitute as many have suspected (see Gibson, 1979; Reber, in progress). The force of

their arguments, as I see it, is not that perceptual systems do not structure their inputs, but rather that they need not do so to nearly the degree that various competing theorists have thought and that the structuring they do is not necessarily as complex as various competing theorists have thought.

³ It should be noted in passing that Wundt never displayed such reluctance about experimentation generally: "... as for experimental tests of ... phenomena or hypotheses, he sought the same standards of objectivity that we seek today" (Blumenthal, 1979, p. 550). Rather, as noted above, Wundt was simply pessimistic about the chances of doing such rigorous experimentation in this area.

⁴ I here adopt Reber's (1973) distinction between psycho-linguistics (roughly equivalent to the psychology of language generally) and psycholinguistics (an area of experimental research in psychology directly inspired by linguistic theory).

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Department of Philosophy
Emory University
Atlanta, GA 30322
U.S.A.