

Feature Review

Chomsky's Theory of Language:
Some Recent ObservationsSheldon Rosenberg
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The story of psycholinguistics in the second half of the 20th century is to a large extent the story of the evolution of Chomsky's theory of grammar (Chomsky, 1957, 1965, 1980, 1986). At each stage in this evolution, Chomsky's views regarding linguistic knowledge or competence have had a profound impact on research and theory in psycholinguistics as they concern both language acquisition in children and language performance in mature language users. The foundation of Chomsky's theory is the view that there is a strong, innate, specifically linguistic component in language acquisition (henceforth, the *innateness view*) that acts to constrain the language acquisition process and the form of the resulting language. In the present article, I review three books that offer data relevant to this view (Poizner, Klima, & Bellugi; Radford; Yamada) and one that applies Chomsky's recent description of mature linguistic competence to the study of language deficits in adults (Grodzinsky). First, however, I provide some background.

According to Chomsky (see also the reviews in Cook, 1988; Haegeman, 1991; Rosenberg & Abbeduto, in press; and Sells, 1985), the initial state of a child's linguistic knowledge (i.e., prior to the onset of language acquisition) is captured by the notion of *universal grammar*, which consists of an innate system of principles and parameters (of variation across languages) that guide the acquisition of any human language. One of the principles is the projection principle. For example, certain aspects of the meaning of the word *hit* require an entity to do the hitting and an object to serve as the recipient of the action. The minimal syntactic (sentential) structure for expressing such a semantic relation would have to consist of a verb and two nouns, one to serve as subject and one as direct object. Thus, the semantic restrictions of

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LIST OF BOOKS

What the Hands Reveal About the Brain, by Howard Poizner, Edward S. Klima, and Ursula Bellugi. Cambridge, MA: MIT Press, 1987. 236 pp. Cloth, \$30.00; paper, \$10.95.

Laura: A Case for the Modularity of Language, by Jeni E. Yamada. Cambridge, MA: MIT Press, 1990. 169 pp. \$27.50.

Syntactic Theory and the Acquisition of English Syntax: The Nature of Early Child Grammars of English, by Andrew Radford. Oxford: Blackwell, 1990. 311 pp. Cloth, \$54.95; paper, \$18.95.

Theoretical Perspectives on Language Deficits, by Yosef Grodzinsky. Cambridge, MA: MIT Press, 1990. 192 pp. \$25.00.

a lexical item project onto the syntactic structure of a sentence. In the innateness view, prior knowledge of such a principle is expected to facilitate the process of mapping word meanings onto abstract syntactic structures during language acquisition and to establish a close bond between semantic and syntactic decisions in subsequent language processing.

An example of parametric variation is the head parameter, which determines whether the head of a phrase (e.g., the preposition in a prepositional phrase) will appear on the left side of the phrase, as in English, or on the right side of the phrase, as in Japanese. Exposure to English leads the child to set the head parameter on the left (e.g., *in Japan*), whereas exposure to Japanese leads him or her to set it on the right (e.g., *Japan in*).

However, knowledge of language involves more than a core grammar made possible by the operation of the innate principles and parameters. Some features of a language are the result of historical accident. Moreover, in the broadest sense, knowledge of language in the language user consists of three indepen-

(Rosenberg, continued on p. 16)

Theory and Reality
in PsycholinguisticsValerie F. Reyna
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For each generation of scholars, there is an area that captures the collective imagination, that displays a surge of productivity, and that projects its influence across disciplinary boundaries. Theoretical linguistics was such an area, its influence in psychology having peaked in the 1970s. Since those days, both theoretical linguistics and psycholinguistics have become differentiated into a variety of specializations. Among these, language acquisition (Radford) and clinical neuropsychology and neurolinguistics (Grodzinsky; Poizner, Klima, & Bellugi; Yamada) are treated in this review. Is it inevitable that such subdisciplines will have little to say to the broader community of psychological science—or even to one another?

Of course, as fields become highly specialized, there is a natural tendency for interest in them to become confined to the cognoscenti. That temptation should be resisted in the case of research on language. Despite the field's fragmentation, language remains a phenomenon that lies at the core of psychology.¹ This fact has long been recognized by linguists: As Grodzinsky points out, "Generative linguists have traditionally considered their field to be a branch of psychology" (p. 110).

In this review, I briefly explore the events leading to fragmentation and alienation from the larger psychological

(Reyna, continued on p. 19)

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1. For reasons of space, I have omitted many complexities in the history of the relationship between psychology and linguistics, but it should be noted that not all trends have supported fragmentation. The advent of cognitive science, for example, has increased interdisciplinary interactions, and much of that work is related to language. Such interactions have also created some assimilation of methods. However, I concentrate here on typical examples of psychological and linguistic methods.

1978); the nature of the relationship between nonlinguistic cognitive and language development in language- and intellectually impaired persons with mental retardation (Cromer, 1991); the species specificity of language (Petitto, 1988); how aspects of language might be acquired in the absence of negative feedback concerning errors (Pinker, 1988); universals in language structure (Hawkins, 1988); the question of why human language has the characteristics (e.g., structure dependence) it displays (Chomsky, 1986); individual differences in the language acquisition process (Bates, Bretherton, & Snyder, 1988); the role of parental input in first-language acquisition (Snow, 1986); the role of universal grammar in second-language acquisition (Flynn, 1987); and the question of a critical period in language acquisition (Johnson & Newport, 1989).

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(Reyna, continued from p. 15)

community, as well as the potential benefits of renewed interaction. I argue that although the question of psychological relevance can be answered affirmatively, real rapprochement between psychology and language-related subdisciplines awaits the resolution of fundamental methodological issues. These issues do not have to do with which kinds of data are "correct," as some scholars have claimed, but with the use of data to illustrate theories rather than test them. The main conclusion that emerges from this review is that, despite elegant formalisms, theories of language will continue to be regarded as unscientific (in the sense that this term is used in psychol-

ogy) so long as there are disagreements about whether such theories should be constrained primarily by data or by a priori assumptions.

BACKGROUND

In 1957, Chomsky's *Syntactic Structures* began a revolution in linguistics in which not only did hitherto disreputable mentalism become respectable, but complex layers of unobserved structure were used to explain linguistic phenomena. The catechism established in those early years, and echoed today in Grodzinsky, Poizner et al., Radford, and Yamada, consisted of arguments that were primarily logical rather than empirical. For instance, it was said that language acquisition could not be attributed to general principles of learning or cognition. However, this claim was not substantially grounded in empirical disconfirmation, or even in the shortcomings of specific learning theories, but, instead, in the logical argument that the infinitely creative aspects of language use (e.g., the ability to produce and comprehend novel combinations of words) could not be accounted for, in principle, by any theory of learning.

The idea that language (i.e., syntax) could not be learned led to the proposal of biological determinism, from which ensued a search for universal principles governing the structures of all languages. In this view, language acquisition (e.g., Radford) and language loss (e.g., Grodzinsky) eventually became the setting and unsetting, respectively, of parameters in an inherited generic grammar. The universality thesis was founded, in part, on a presumed gulf between linguistic inputs to children and their knowledge of language. Detailed evidence about actual input was not considered crucial. The supposed fact that language learners were exposed to indeterminate inputs was obvious; demonstrating the obvious might be nice, but it was hardly necessary. In general, experiments with large numbers of subjects were not required. Instead, informant judgments of well-formedness, for example, were evidence for a universal grammar of possible human languages that existed at a level of abstraction removed from language use. Thus, after the Chomskyan revolution, linguistics was

no longer concerned with behavioral reality but with the "mental reality underlying actual behavior" (Chomsky, 1965, p. 4).

The linguistic revolution was initially well received by psychologists, although the subsequent history of that relationship has been described as "bizarre" (Marshall, in Grodzinsky, p. ix). Early transformational grammars consisted of two levels of representation, deep and surface structure, and transformations mapped one level onto the other (Chomsky, 1965). Psycholinguists focused on testing the derivational theory of complexity (that the number of transformations intervening between deep and surface structure indicated psychological complexity) with disappointing results. The competence-performance distinction, between the underlying structure of language and how that structure is implemented in language use, proved to be elusive. Meanwhile, linguistic theories were undergoing fundamental changes.

Transformational grammar was judged to be too powerful, and the search for constraints to reduce its power was undertaken. Less of the explanatory burden was placed on language-specific transformations, and more was placed on the lexicon and on universal constraints. For instance, the familiar transformations of movement, insertion, and deletion were eliminated until only one transformation (Move-alpha) that could move anything anywhere remained. Other principles were invoked to filter out ungrammatical strings. Among these was the subadjacency condition, which evolved from Ross's (1967) well-known island constraint. Conceived to be a universal principle, subadjacency requires that moved constituents cannot cross more than one node (in a sentence's tree structure) of a particular grammatical category, although those categories vary from language to language.

The resulting government and binding theory (Chomsky, 1981) consisted of several subsidiary theories, including X-bar theory (a theory of permissible sentence structures), binding theory (a theory of the relations between constituents that co-refer), and trace theory (a theory of constituents that are phonologically unexpressed yet structurally represented). Although trace theory, in partic-

ular, is replete with interesting psychological implications, the government and binding approach has not inspired psycholinguistic experimentation to the same extent as its predecessors. The rapid and complex changes in linguistic theory had created a perception among some psychologists that the basic assumptions of linguistics were in flux. This perception naturally encouraged the belief that research should not be dependent on theories that might be discarded imminently—in effect, that psycholinguistics should move forward without linguistics. Although firm stands have been taken on both sides of this issue (see Table 1), the presence of linguistic theory is significant in much of the work before us. Is a reconvergence between linguistics and psychology desirable?

PHENOMENA

As the quotations in Table 1 illustrate, opinion is strongly divided when it comes to the usefulness of linguistics to other disciplines, such as psychology.

Although it is possible to make such categorical claims, or to argue in principle, usefulness has a pragmatic aspect that can be captured only by examining results. Grodzinsky and Radford, and, to a lesser extent, Poizner et al. and Yamada, provide cases in point. The latter group is generally unconcerned with the particulars of modern linguistic theories. In Poizner et al. and Yamada, for example, global principles of the early catechism (e.g., biological determinism) are assumed, and case histories are presented as illustrations. In contrast, the former group links specific phenomena to current principles, but despite commendable efforts, the complex assumptions of linguistic theory entirely overwhelm the data. Both groups simply cast their theoretical nets over existing data sets, rather than systematically varying antecedents and examining consequences. To illustrate such reasoning, a precis of some phenomena that are covered, and their explanations, follows.

Radford applies Chomsky's X-bar theory (1986; with modifications based

Table 1. *Contradictory views on interdisciplinary interaction*

Positive views	Negative views
<p>"Talking about linguistic activities cannot be done without referring to grammar" (Grodzinsky, p. 8)</p>	<p>"The present theory . . . can, indeed, be considered as a psycholinguistics without linguistics" (Johnson-Laird, 1977, p. 76)</p>
<p>"It is no longer even halfway plausible to imagine that 'external' evidence from aphasiology should be excluded from the data base of grammatical theory, the inverse error to the psychologists' dismissal of 'internal' evidence [from linguistics]" (Marshall, in Grodzinsky, p. xii)</p>	<p>"The notion that we need to know what language is before we ask where it is has little to justify it" (Geschwind, 1983, p. 62)</p>
<p>"A theory of language acquisition should posit mechanisms consistent with what we know about neurology and biology" (Yamada, p. 115)</p>	<p>"Many findings in biology and neurology are relevant to language acquisition, though the application of such findings has been piecemeal since Lenneberg's (1967) landmark effort" (Pinker, 1990, p. 364)</p>
<p>"Important insights about the structure, processing, and acquisition of the linguistic signal can be gained by looking at aphasic syndromes" (Grodzinsky, p. 16)</p>	<p>"It is a sad truth that remarkably little has been learned about the psychology of language processes in normals from over a hundred years of aphasia study" (Fodor, Bever, & Garrett, 1974, p. xiv)</p>

on more recent work by Abney, 1987) to the study of syntax acquisition in the young child. Within X-bar theory, there are four lexical categories (nouns, verbs, prepositions, and adjectives) and three functional categories of interest (determiners; complementizers; and so-called I constituents, such as inflected forms [e.g., *walked*]). Drawing largely on archival data of children's spontaneous utterances, Radford proposes a developmental ordering in the child's grammar from precategorical, the one-word stage when categorization has not taken place, to lexical, characterized by the acquisition of lexical categories, followed finally by the acquisition of functional categories.

So, for instance, the young child appears to "project" nouns into noun phrases, resulting in noun-plus-complement structures such as *bottle juice*. But, what evidence is there that this utterance is a phrasal unit consistent with X-bar theory? Radford points out that children at about this age can also recursively stack adjectives in front of the nominals they modify, as in *big heavy book* or *nice little chicken*. According to X-bar theory, *chicken* is an N-bar constituent that is expanded to another N-bar constituent, *little chicken*, which, recursively, expands to *nice little chicken*. As evidence for this analysis, Radford suggests that children seem to refer back to the whole sequence (i.e., *nice little chicken*) using the proform *one*, as in *Nice little chicken! Stroke that one*. Such evidence is equivocal, as Radford admits: *One* might refer to *little chicken* or simply *chicken* (or even refer directly to the chicken in a deictic fashion), in which case the recursive X-bar structure would not be implicated.

The complementary claim, that children initially lack a functional categorical system, is supported by observations of consistent omissions in children's speech, for instance, of determiners: An adult says, "Look at the hippo," but the child's response omits *the*, as in "Hippo" or "Where hippo?" Universal grammar supplies an overarching rationale for why lexical categories are acquired before functional ones. Specifically, we should expect children first to acquire those aspects of language that are universal and later to acquire parameterized aspects of language, those aspects that require linguistic experience in order to

set parameters at appropriate values. Significantly, there is little variation among languages in lexical category systems, but great variation in functional systems. Japanese, for instance, appears to lack the determiner system that children learning English initially omit. Accordingly, articles, part of a determiner system, do not appear in either Japanese or in early child speech. Thus, Radford argues, because it is universal, much of the lexical category system is innate, whereas the functional system develops mainly as a result of exposure to the idiosyncrasies of particular languages.

Grodzinsky also uses the parametric approach of government and binding theory (with qualifications) to analyze agrammatism in Broca's aphasia and links his account to language acquisition. (*Agrammatism* is not precisely defined, other than as referring to language deficits that clinically diagnosed agrammatics exhibit.) While acknowledging that function words occur far less often in aphasic speech than in normal speech, Grodzinsky notes that both omissions and substitutions for functional elements (e.g., inflections) occur. So, as in children's speech, a common feature in aphasia is lack of tense or agreement inflections on verbs. Grodzinsky explains this by arguing that functional terminals (those sentence constituents that dominate nothing syntactically) are deleted from surface structure in agrammatism. However, agrammatic speech is constrained by lexical, though not necessarily syntactic, well-formedness. Thus, if deletion of a terminal element (say, an inflection such as *-ed*) would result in a nonword, there is no omission. Instead, some allowable word is substituted—one that may be aberrant in tense or agreement.

Grodzinsky also notes that agrammatics have difficulty with sentences that are transformationally derived from deep structure (i.e., that involve movement from object position, leaving, for normal speakers, a trace in object position). For agrammatics, such traces also appear to be deleted from surface structure. This analysis explains why passives (e.g., *The girl was pushed [trace] by the boy*), for example, are difficult to interpret. Because the trace indicating that *the girl* is the object of *pushed* has been deleted, default principles assign such roles as

agent to nouns. Sometimes this assignment is correct, and comprehension performance is above chance. In other cases, such as the passive example here, default assignment leads to uninterpretable anomalies, such as two agents (the girl and the boy) in the passive sentence, resulting in guessing on comprehension tests.

Grodzinsky's characterization of aphasic grammar is that it is larger, or more inclusive, than normal grammar. Where normal grammar specifies agreement, for example, agrammatism is indeterminate. As the prior discussion of Radford suggests, the child's early grammar can be similarly described as underspecified. Thus, Grodzinsky contends, "Aphasia is a mirror image of development" (p. 145): Whereas language learning is the fixing of parameters, aphasia is the loss of parametric values. Grodzinsky acknowledges, however, that such a regression hypothesis has not met key empirical tests.

Historically, the study of language learning and loss has had special status in linguistics because these processes relate to the issue of biological determinism. Both Yamada's study of a cognitively impaired (but linguistically fluent) young woman and Poizner et al.'s investigation of aphasia in deaf signers address the issue of the biology of language. Poizner et al., for example, present evidence from six aphasic signers to the effect that sound is not crucial in the development of hemispheric specialization for language. Despite the reliance on spatial manipulations in sign language, right-hemisphere damage did not disrupt signing, just as it does not disrupt spoken language. An interesting dissociation was observed between linguistic and nonlinguistic uses of space, however. Although left-hemisphere damage disrupted the use of space for representing syntactic relations, other spatial processing was left relatively intact. Right-hemisphere damage produced complementary deficits; patients were unable to convey the correct spatial locations of objects in drawings, but could use space to represent syntactic relations. Such results are then used to argue that aspects of language are autonomous from other cognitive systems, a key corollary of biological determinism.

Yamada focuses on language learning, especially on the disparity between

linguistic and nonlinguistic attainments in a young retarded woman (Laura). That disparity is interpreted as evidence against the position that cognitive abilities "are at least necessary for language learning" (p. 2). The time lag between the emergence of abilities in linguistic and cognitive domains similarly suggests, so Yamada claims, that these abilities are not governed by some common third factor. For example, unlike other hyperlinguistic subjects, Laura has a poor short-term memory span, yet is able to construct complex hierarchical structures linguistically. She also shows other dissociations between language and cognition, for example, performing poorly on nonlinguistic tests of categorization, rule abduction, and hierarchical organization. This facility for language learning, in the absence of putative cognitive processes, is said to favor the innateness, as well as the uniqueness, of language.

This brief review enables us to consider the consequences of invoking linguistic theories, or at least some of their central theses, in psychological research. In the work of Radford and Grodzinsky, on the one hand, linguistic theory is used to organize differing observations around common underlying constructs. Thus, what is learned or lost linguistically is described in theoretical terms. In the case-oriented studies of Yamada and Poizner et al., on the other hand, the focus is on boundary conditions for theories: the possibility of language without (much) cognition in the case of Laura, the dissociation of linguistic and nonlinguistic uses of space in the case of aphasic signers, and so on. Thus, Radford and Grodzinsky essentially use linguistics to explain psychology (e.g., language learning, behavior after brain damage), the others explore the relationship between language and cognition, and all of them commingle linguistics and psychology. As in many interdisciplinary endeavors, however, methodological standards differ by discipline. Thus, the scientific value of these efforts, and, by implication, whether linguistic research is useful to psychology generally, depends on methodological assumptions in the broad sense.

OF TRUTH AND METHOD

The methodological disagreements between psychology and linguistics are

usually attributed to differences in opinion about the legitimacy of certain kinds of data, such as grammaticality judgments. I do not make that argument here. I argue, instead, that linguistics has taken mentalism to such an extreme that the modern product is as different from earlier mentalism as mentalism was from behaviorism. In making this argument, I am not proposing a return to behaviorism, but to the common ground that was briefly shared by psychology and linguistics during their most productive period of collaboration. The basal premise of mentalism—that explanation can refer legitimately to unobservables—is today widely accepted in psychology. But psychologists have not surrendered the belief that explanations must be constrained by observables. It is precisely this belief that currently separates psychology from linguistics.

As I indicated, there are disputes over the types of data that constitute legitimate evidence, such as intuitions about grammaticality versus reaction times. Chomsky (1978), for example, has argued that intuitions are genuine psychological data, whereas Grodzinsky points up the indeterminacy of reaction times; both of these arguments have some merit (but see Townsend, 1990). Although such discussions sometimes confuse important features of the experimental method (with correlational methods, for instance; see Radford, p. 9), indictments of particular methods miss more basic underlying disagreements about the theory-data interface. More specifically, in linguistics, data are used mainly as illustrations of broader theoretical principles, whereas in psychology, data serve a hypothesis-testing function of differentiating among competing explanations.

Such disagreements are highlighted in Radford's discussion of observational data. Because such data are equivocal (e.g., factors are not varied orthogonally; the contexts in which data are gathered differ) and usually of insufficient quantity, "any analysis of child syntax based on naturalistic speech samples is bound to be grossly *underdetermined* by the data" (p. 66). Much the same can be said of grammaticality judgments. For example, the fact that *Mary said that Bill was hit by John* is grammatical, but *Bill was said that was hit by John by Mary* is ungrammatical, does

not fix the proper theoretical interpretation. Nevertheless, a great deal of theoretical ingenuity has been expended on data that are known to be incapable of discriminating theoretical claims. As noted earlier, linguists have already acknowledged that their data do not carry sufficient information to discriminate complex hypotheses. Linguists recognize the problem—the need for constraints—but they favor constraints that are imposed a priori (i.e., before the facts are known). The psychologist's response to such a dilemma, of course, would be to gather more and better facts (see Altmann, 1990, for an elegant progression of experiments in which exactly this is done).²

The upshot of these policies on theoretical constraints is that facts serve very different functions in linguistics and psychology. In linguistics, isolated facts can carry a heavy explanatory burden. A single spontaneous utterance can nullify a linguistic theory (Radford, throughout), and rare observations can be the crux of theoretical confirmation (e.g., Grodzinsky, p. 41). In psychology, in contrast, statistically rare observations are usually interpreted as error. For this reason, the exception that is pivotal in linguistics can be considered trivial in psychology. Moreover, because an error theory is generally not assumed in linguistics, individual observations are expected to reflect theoretical principles in an all-or-none fashion. Radford, for instance, criticizes experiments because their results are "probabilistic." In psychology, however, any observation might be the result of error, and variability in small samples has little theoretical importance. A single utterance is a psychological datum, but it cannot, by itself, support a complex theory.

The difference between disciplines, then, boils down to the extent to which

2. An approach to this dilemma that is fast becoming popular is to support theories by invoking internal considerations as well as desultory data. Although it might be supposed that this strategy would provide greater constraints on theories, it has done just the opposite—namely, it has made these theories unaccountable on either internal or empirical grounds. Unfortunately, internal criteria tend to be applied when empirical ones cannot be satisfied, and vice versa.

theories are induced from data, as opposed to data being deduced from theories. The latter applies to linguistics; as Grodzinsky says, "A theory is said to explain a set of facts if they are deducible from it" (p. 16). Rather than observe all languages and infer from their commonalities that there is an underlying universal grammar, a universal grammar is postulated, and data are treated as its instantiations. The data are but one small set of realizations of the universal grammar, and, consequently, the theory's complexity necessarily exceeds that of the data. In psychology, however, theories should have fewer degrees of freedom than the data they seek to explain. Otherwise, there is no way to know if they are true (because theories with more degrees of freedom than the data can always be made to be consistent with them). By this standard, it is impossible to judge whether modern linguistic theories are externally valid (i.e., whether they have anything to do with reality), which makes psychologists reluctant to embrace them.

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In sum, the view of psychology from linguistics is of complex data (reaction times, recall rates, slips of the tongue, etc.) and simplistic theories. As Wasow (1990) remarked, psychological "theories of language use are not nearly so well developed as theories of grammar" (p. 197). The view of linguistics from psychology is of complex theories precariously balanced on small amounts of narrow evidence (mostly spontaneous utterances and grammaticality judgments). The very elaboration and detail

that Wasow regards as a virtue in linguistic theories is seen as a vice by psychologists because of the lack of empirical justification.

Thus, the basic disagreements between linguistics and psychology center not on the form of data so much as on how evidence is used to support theories. Spontaneous utterances, for example, are psychological data, and they can serve a useful role in suggesting hypotheses that are then systematically tested. Again, mine is not an argument for a behaviorist counterrevolution. In psychology, as in physics, theories can and should incorporate constructs that are never literally represented in tangible behavior. The issue is the complexity of those constructs, and whether their consistency with behavior can be evaluated.

CONCLUSIONS: WHAT I DID NOT SAY

I have claimed that linguistics as a field is relevant to psychology, but that linguistic research must conform to certain methodological ground rules to be useful to psychologists. My argument is not that linguistic data, such as spontaneous utterances and grammaticality judgments, are not empirical. I am also not criticizing linguists for failing to use the experimental method. Although experimental evidence is particularly well suited for testing theories, other kinds of data will suffice for this purpose. So long as sufficient data are gathered under a strategy of converging operations, theories can be tested. Finally, I do not claim that linguists never attempt to falsify their theories.

The real dispute between disciplines involves the nature of the falsification procedures. Once their a priori assumptions are granted, it is possible to falsify (some) linguistic theories. It is well known, however, that, without these powerful a priori assumptions, the data alone do not provide a test of the theories. A priori assumptions make it possible to take single observations seriously in linguistics—precisely because they do

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not carry the main burden of theory testing. Thus, although some assumptions are necessary in any theoretical enterprise, linguistic theories are constrained primarily by unproven assumptions, as opposed to data. For psychologists, therefore, the ultimate concern is that linguistic theory might bear no relation to behavioral reality—that it is just an abstract game with symbols.

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