

## CHAPTER 2

# *Rabbit-Pots and Supernovas: On the Relevance of Psychological Data to Linguistic Theory*

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A number of philosophers remonstrate against the 'psychologizing' of linguistics: in particular, Jerrold Katz (Katz 1984), Scott Soames (Soames 1984), and Michael Devitt and Kim Sterelny (Devitt and Sterelny 1987, 1989). All of them have argued against the view, due to Chomsky, that linguistics ought properly to be conceived as a branch of psychology. On the Chomskian model, linguistic theories aim at describing the internalized grammars of native speakers of particular languages, in a way that reveals universal principles of grammatical structure native present in all human beings. Katz, Soames, and Devitt and Sterelny all contend that this conception is based on a conflation of two theoretical domains—an abstract domain of linguis-

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tic structure, and a psychological domain of *knowledge* of that structure. The result of this conflation, they all argue, is a linguistics inappropriately constrained by psychological data. In what follows, I shall refer to the sort of linguistics deployed by these philosophers as 'psychologized linguistics', and I shall call the alternative, which they endorse, 'pure linguistics'.<sup>1</sup>

The arguments of the critics of psychologized linguistics fall into roughly two categories: 'conceptual' arguments, intended to show that the psychologizing of linguistics amounts to a change in theoretical domain, and thus to a 'change of subject'; and empirical arguments, which purport to show that the kinds of appeal made to psychological data by proponents of psychologized linguistics have yielded results that are empirically inadequate by uncontroversial theoretical standards. Both kinds of argument need answering, but in this paper I am going to focus on a representative argument of the first type, developed by Soames. An analysis of his discussion as to whether psychology and linguistics are (in Soames's terms) 'conceptually distinct' will reveal that advocates of pure and psychologized linguistics are divided by deep metatheoretical disagreements about the objectives of linguistic theory which in turn are based in deep disagreements about the nature and purpose of empirical enquiry in general.

Indulge me in a rehearsal of some recent history. Pre-Chomskian linguistics was a largely descriptive field of enquiry. Linguists like Bloomfield and Harris aimed primarily at the systematization of facts about human languages; the metatheory for this endeavour was thoroughly instrumentalist, so that the only accepted empirical constraint on linguistic theories was the observational adequacy of their predictions.<sup>2</sup> Chomsky's alternative was a linguistics constrained by an explanatory, rather than a descriptive, goal:

<sup>1</sup> It should be noted that there is substantial disagreement among the critics of psychologized linguistics about the nature and proper methodology of pure linguistics. In particular, there is no consensus among the critics about whether linguistics should be regarded as an empirical discipline. Soames believes that it is an empirical discipline, concerned with the analysis of an empirical phenomenon. Katz, on the other hand, contends that languages are abstract objects, and that linguistics as a science is therefore on a par with mathematics. My arguments in this paper are mainly addressed to Soames, and thus they presume that linguistics is an empirical science. Although Katz makes arguments very similar to those of Soames that I am about to discuss, his arguments will be less vulnerable to my criticisms than Soames's, precisely because Katz is unwilling to treat linguistics as an empirical science.

<sup>2</sup> This is uncontroversial, even among advocates of pure linguistics. See Katz (1985b).

figure out how human children master their languages in the conditions in which they do.<sup>3</sup>

Chomsky's revision of the theoretical objectives of linguistics is acknowledged to have resulted in a wholesale transformation of the field. But it is important to see exactly how and why this transformation was effected. Chomsky's different understanding of the goals of linguistic theory did not automatically entail a psychologizing of the field, nor the abandonment of a descriptive, instrumentalist take on linguistic theory. What transformed linguistics was not the question Chomsky asked, but rather the way he answered it.

The going line on language acquisition in the time of structuralism had been behaviourist: language learning was simply an aspect of general learning, and thus, by current theories, a matter of behavioural conditioning. This line, had it worked, would have justified treating linguistics in just the way the structuralists had treated it, as a systematic description of the distributional patterns of sounds and ink-marks.

But Chomsky had compelling arguments for the empirical inadequacy of the behaviourist account of language acquisition (Chomsky 1959). The alternative he advocated involved the rehabilitation of two dishonoured philosophical views, mentalism and nativism. Chomsky argued, first, that language acquisition was essentially a computational process by which a child constructed a grammar for her language, and second, that the child's ability to construct an adequate grammar on the basis of the data available to her depended upon the existence of powerful native constraints on the set of hypotheses she could entertain.

It is this explanatory goal—that of accounting for acquisition—that seems to me to have been lost in much of the criticism of psychologized linguistics. The critics seem to feel that the Chomskian turn has resulted in a change

<sup>3</sup> See Chomsky (1977). It is admittedly a bit anachronistic to suggest that Chomsky's motivations for abandoning structuralism were identical to the justifications he would now give for the generative approach. Katz points out that Chomsky originally defended structuralism's positivistic ontology and methodology, and began to have doubts only as a result of his inability to get a minimally adequate grammar for Hebrew working within structuralist constraints. But the fact that Chomsky's reconstrual of the ontology of linguistics was motivated by a need for more abstractness in the grammar than structuralism allowed does not mean that abstractness *per se* is a virtue, as Katz's discussion sometimes suggests.

of subject matter for linguistics, or even that it has led to the demise of an entire area of study—that what used to be the science of *language* has been replaced by a specialization within the science of the mind. My picture of the development of linguistics is rather one of the refinement of the theoretical goals of the science of language—a movement from a relatively young and unconstrained descriptive project to a mature science devoted to the explanation of central linguistic phenomena, and creative in finding empirical purchase for its theoretical claims. In what follows, I shall try to show how Soames's neglect of the explanatory impetus behind the Chomskian revolution has distorted his general views on how the domain of a theoretical field is determined.

## 1 The 'Conceptual Distinctness' of Linguistics and Psychology

As Soames characterizes it, his goal is to demonstrate that linguistics and psychology are 'conceptually distinct', which is to say that 'they are concerned with different domains, make different claims, and are established by different means' (Soames 1984: 155). Soames's argument for this segregation of linguistics from psychology is essentially a priori, and proceeds from the observation that the subject matter of the two fields is ostensibly different. Specifically, Soames contends that a neutral, pre-theoretic survey of the domain of linguistic theory is enough to establish that linguistics is distinct from 'psychological theories of the mental states and processes underlying language acquisition and mastery' (Soames 1984: 156).

Now on the face of it, such a strategy seems flawed in its very conception—one simply cannot derive substantive conclusions about the methodology and database of an empirical field of study just from a characterization of the field's domain. The reason is that a specification of the phenomena of interest—no matter how accurate or well motivated—cannot by itself tell us what kinds of evidence we may need to appeal to in constructing an *account* of those phenomena. This can only be determined empirically, because in science, a discipline's methods and databases depend upon the empirical connections in which its subject phenomena participate, some of which may be known, and some of which are yet to be discovered. Knowing what a language is does not equip us to say what facts about the world we

shall need to exploit in order to account for the linguistic phenomena that interest us.

Verification is holistic: a suitable chain of inference could bring any fact to bear on any empirical issue.<sup>4</sup> It is thus impossible to rule out anything as a potential source of data for an empirical theory. We do not know a priori where evidence about languages may be found because we do not know a priori what empirical connections languages participate in, nor can we fully anticipate the ways clever experimenters and theorists may contrive to exploit those connections, once discovered, to generate testable predictions.

It is standard and valorous scientific practice to achieve testability for a particular hypothesis by conjoining it in a creative way with well-confirmed theories in other domains. This is a particularly prudent practice in areas where it is impractical, for one reason or another, to obtain 'direct' evidence. Particle physics, evolutionary biology, and cognitive psychology are, in this respect, all in the same boat. In any of these fields, researchers would be hamstrung if they were to follow (what appears to be) Soames's advice, viz. mind your own business.

But surely Soames agrees with all of this—that verification is holistic, and that empirical disciplines borrow readily from other empirical disciplines. So why does he think that linguistics is any different? Why does he think that a psychological linguist's appeal to psychological data is any different from an evolutionary biologist's appeal to geological data? Why does he think that a linguist who talks about memory limitations or order of acquisition has literally *changed the subject*?

To answer these questions, let us look at his argument in detail. Here is my reconstruction of the central line in 'Linguistics and Psychology' (Soames 1984):

(1) Linguistics is the science of language.

Therefore,

- (2) Linguistics ought to be constrained only by facts that are directly relevant to the linguistically significant properties of sentences.
- (3) Facts about the psychological states and processes underlying linguistics

<sup>4</sup> Jerry Fodor emphasizes this point in Fodor (1985).

tic behaviour are not directly relevant to the linguistically significant properties of sentences.

Therefore,

- (4) Facts about the psychological states and processes underlying linguistic behaviour should not constrain linguistic theory.

Therefore,

- (5) Linguistic theories are conceptually distinct from psychological theories. QED.

The crucial step in this argument, it seems to me, is the move from (1) to (2), and this move turns on the notions of 'linguistic significance' and 'direct relevance'.<sup>5</sup> We thus need to see, first, what Soames takes to be the criterion of 'linguistic significance', and second, what evidential relation he has in mind when he speaks of data that either are or are not 'directly relevant' to a given body of fact.

Let us start with 'linguistic significance'. Not just any criterion will do here—what Soames needs is a criterion of linguistic significance that is both intuitively plausible and non-trivial, and at the same time substantive enough to rule out certain kinds of things without begging the crucial question. And in fact, Soames thinks that he has such a criterion: he says that the linguistically significant properties of linguistic phenomena are those properties of utterance and inscription types that are *essential* to the individuation of the language to which they belong—those 'characteristics which define languages and serve to identify or distinguish them'. More precisely, the linguistically significant properties and relations holding of and among sentence types are those which 'constitute individually sufficient and disjunctively necessary conditions for individuating the languages (or dialects) of different speakers' (Soames 1984: 159).

Thus, a property of a sentence type counts as linguistically significant if and only if a difference with respect to that property would be enough to make us count two sets of sentence types as distinct languages. Soames cites the following as examples of linguistically significant properties and relations: grammaticality, ambiguity, synonymy, entailment, analyticity, and

<sup>5</sup> These are Soames's terms; see Soames (1984), 159.

contradiction; and the following as examples of non-linguistically significant properties and relations: degree of ease with which a construction can be processed, and relative time at which a construction is acquired. He offers the following thought-experiment in defence of this categorization (this is a close paraphrase):

Imagine two linguistic communities, the Xers and the Yers, and suppose that we wish to discover whether the language spoken by the Xers is different from or the same as the language spoken by the Yers. If we were to discover that there was a certain class of sentences which the Xers accepted as grammatical, but which the Yers rejected, we would conclude that the X-language was indeed distinct from the Y-language (similarly for variation in judgements about synonymy, entailment, and ambiguity).

But now suppose that there were no differences between Xers' and Yers' judgements regarding grammaticality, synonymy, entailment, or ambiguity, but that there were systematic differences between the Xers and Yers of the following sorts, concerning classes of constructions A and B: (i) Xers acquired As before Bs, while Yers acquired Bs before As; or (ii) Xers process As faster than Bs, but Yers process Bs faster than As; or (iii) Xers make characteristically more errors with respect to As than Yers make. If these were the nature of the differences between the linguistic behaviour of Xers and Yers, then we would *not* conclude that the X-language was different from the Y-language. (Soames 1984: 159)

One thing to notice right away is that Soames is advocating an *a priori* methodological methodology for discovering the essential properties of something he regards as an *empirical* phenomenon: he maintains that we can identify the linguistically significant properties by consulting our intuitions about the kinds of difference two bodies of linguistic phenomena would need to display in order for us to judge them to be distinct languages. This strategy should cause eyebrows to rise—we know this method would not serve us well in discerning the essential properties of water. The reason it would not is that in advance of any *theory* about water, we do not know which of its superficial characteristics reflect essential properties and which are mere accidents: does water *have* to be colourless, or is there (genuinely) blue water somewhere? The process of answering this question is inseparable from the process of finding out what water *is*, and either answer to the first question must be tentative, pending further progress on the second.

But these misgivings concern Soames's method for identifying the lin-

gustically significant properties of linguistic phenomena, and have nothing to do with the *criterion* of linguistic significance itself. Let us therefore suppose that, by whatever means, Soames has correctly isolated the properties of languages that satisfy that criterion—let us suppose that grammaticality etc. are the properties that are genuinely individuating of languages. Will it then follow that linguistics need only account for data that are 'directly relevant' to the attribution of those properties and relations?

Here is where things get really interesting (in my opinion). I said earlier that the holistic character of verification entails that one cannot tell *a priori* what data might be relevant to a given field of enquiry, and thus that no *a priori* considerations could show that psychological data are irrelevant to linguistic theorizing. But Soames is not claiming *that*—he does not say that psychological data are *always* or *necessarily* irrelevant to linguistic theorizing. What he in fact says is that '*once the linguistically significant facts have been accounted for*, there is no need for the theory to be concerned with psycholinguistic data . . . ' (Soames 1984: 160, my emphasis). The qualification is crucial—while Soames readily concedes that 'data which are not themselves linguistically significant often provide evidence for attributions of properties and relations which are', he still avers that: 'even when [such data] bear on [attributions of linguistically significant properties], they need not be predicted by linguistic theories, so long as the theories correctly account for all linguistically significant facts' (Soames 1984: 160). He thus acknowledges that linguistically insignificant data can be relevant to the construction of theories of the facts that interest us, but stigmatizes their contribution, saying that 'the relevance of such psycholinguistic data to theories in linguistics is limited to this indirect role' (Soames 1984: 160).

It is not immediately clear what Soames has in mind—how can data 'bear on' theories without constraining them? Surely data 'bear on' a theory when the occurrence of those data is entailed by the theory in conjunction with background assumptions, or when those data can be conjoined with the theory to achieve greater explanatory or predictive power. In any such case, the theory would seem to be 'constrained' by the data at least to the following extent: a theory that predicts or conforms to data that 'bear on' it in one of the above senses is to be preferred, *ceteris paribus*, to one that does not.

What Soames's remarks suggest is that he thinks that there are, in gen-

eral, 'direct' and 'indirect' ways in which data can support theory, and that only data that bear 'directly' are ones that the theory must answer to— for the rest, the theory's ability to predict them is gratuitous. I presume that he is not here making tacit appeal to a discredited observation/theory distinction (observations providing 'direct' support, and theoretically mediated inferences providing only 'indirect' support). Rather, he seems to be alluding to some measure of *typical* distance. The idea, perhaps, is that bits of information very far removed from the domain in question may by some convoluted inferential route, provide evidence for some hypothesis concerning that domain, without theorizing about the domain becoming constrained to predict every bit of such potentially useful information.

## 2 The Lesson of the Rabbit-Pots

If this is what Soames has in mind, then here is a case that may serve to illustrate: the story begins seventy years ago, when an archaeological team from the University of Minnesota discovered a cache of over 800 ceramic pieces at Indian ruins near Silver City, New Mexico. About 200 of the pieces were 'narrative' bowls, some with drawings depicting activities like hunting and fishing; others illustrating stories or chronicling events.

Many of the pieces, including a particular bowl of special interest, were decorated with stylized drawings of a rabbit. These 'rabbit-pots' were especially interesting to Dr R. Robert Robbins, an astronomer at the University of Texas at Austin, and his associate, Dr Russell B. Westmoreland, an archaeologist. Robbins and Westmoreland contend that these pots have *astronomical* significance. It seems that when the Mimbrès Indians (the potters) looked at the moon, they saw on its surface not the image of a man, as many North Americans see, but rather that of a rabbit, so that the drawings of rabbits found on the pots were either depictions of, or symbolic references to, the moon.

Now the bowl that Robbins and Westmoreland found particularly interesting (Figure 2.1) differed from the other rabbit-pots in the following detail: just below the moon-rabbit's back foot there is a small, filled-in circle surrounded by twenty-three radiating lines. Robbins and Westmoreland are convinced that this 'sunburst' is in fact a record of the Mimbrès tribe's observation of the supernova that created the Crab Nebula. Astronomers

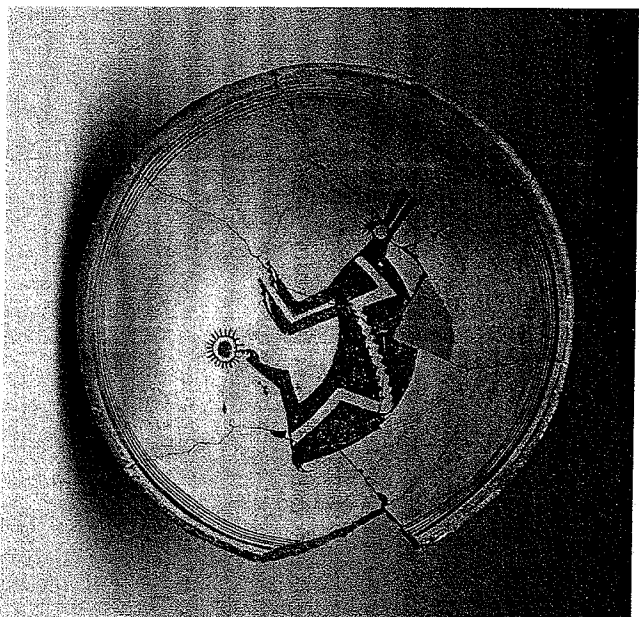


FIG. 2.1. Mimbres Painted Bowl

Collection Frederick R. Weisman Art Museum at the University of Minnesota, Minneapolis; Transfer, Anthropology Department, University of Minnesota

had previously dated the occurrence of the supernova from documents left by astrologers to the emperor of China, which told of the sudden appearance of a bright object in the sky on 5 July 1054, visible in daylight for twenty-three days. Carbon dating indicates that the Mimbres rabbit-pot was produced in the middle of the eleventh century, placing it at the time of the supernova, and the number of rays extending from the object at the rabbit's foot—twenty-three [I'm not making this up!—corresponds to the number of days the supernova was visible, according to the Chinese documents. Dr Robbins says of the pot that it is 'the most certain record of the supernova that has ever been discovered outside China and Japan' (Wilford 1990).

Now perhaps this is the sort of situation Soames has in mind: it seems not unreasonable to describe this case as one in which a certain bit of evidence bears on, but does not constrain, a hypothesis. After all, one might contend, it is fine and good if archaeology turns up some pots depicting an astronomical occurrence—they certainly can be marshalled in support of

a theory that asserts such an occurrence. But at the same time, we would not want to insist that a theory of astronomy *must* make predictions about ancient pots—that is just not astronomy's job. A theory that thoroughly explained celestial phenomena, but was utterly silent on the subject of eleventh-century American pottery, would be quite satisfactory.

There is something that seems right here—we do not in fact expect theories to be able to predict every single datum that might conceivably bear on their truth or falsity, and yet there are other data that we regard as absolutely central, data that must be accounted for by any adequate theory of that domain. But this innocent observation must not mislead us—there is nothing here that is going to help Soames establish that linguistic theories are constrained only by linguistically significant data.

Notice that although drawings on eleventh-century pots do not 'constrain' astronomical theories in one sense, there is another sense of 'constrain' in which they do. On the one hand, I doubt that anyone would want to make it a condition of adequacy for a theory of celestial phenomena that it can account for rabbit-pots, whereas most people would be willing to say in advance that an astronomical theory ought to have something to say about the orbit of the moon. But on the other hand, the rabbit-pots do constrain astronomical theories in the sense that is relevant to the debate about linguistics: other things beings equal, we shall prefer the astronomical theory that *can* account for the rabbit-pots over the one that *cannot*. Even more to the point, a *dispute* about the astronomical facts could in principle be decided by appeal to the rabbit-pots: if theory  $T_1$  says that a supernova *did* occur in the eleventh century, and theory  $T_2$  says that it did not, then the discovery of the rabbit-pots counts in favour of  $T_1$ .

Now of course, two astronomical theories are not likely to disagree just on the occurrence of a supernova. Supernovas are a big-time, gauche kind of thing, and tend to leave traces of themselves all over the place, so it is very unlikely that the whole matter would boil down to there being or not being rabbit-pots. But the idea that astronomy can be indifferent to the existence or non-existence of such things is, I think, a kind of perceptual illusion—we boggle at the sheer number of mediating assumptions necessary to take us from the hypothesis that the supernova occurred to the prediction that rabbit-pots will be found. The feeling that astronomy should not be expected to account for whatever archaeologists happen to dig up stems

partly from our inability to imagine—in advance of its happening—a route that takes us from supernovas to rabbit-pots.

### 3 The A Posteriori Nature of Nomological Connections

But maybe this is a misdiagnosis—maybe our reaction to the rabbit-pots case is not simply due to the fact that there is a long inferential chain from data to theory. Maybe the reason the rabbit-pot evidence seems to bear only 'indirectly' on astronomical theory is rather that the connection between the evidence—some old pottery with pictures on it—and the hypothesis—that a certain star became a supernova at a particular time—seems to be utterly contingent. There seems to be no *lawlike* connection between the two: it is perfectly consistent with the laws of astronomy generally, and the laws governing supernovas in particular, that human beings never even existed, much less that any of them ever set out to record heavenly events on clay pots.<sup>6</sup> The connection between supernovas and rabbit-pots would appear to differ in this regard from the connection between, for example, the phases of the moon and the timing of the tides, since the latter are nomologically tied to the central phenomena of the theory's domain. But this is not going to do Soames any good either. Let us quickly recap. Soames wants to argue, ultimately, that linguistics and psychology are 'conceptually distinct', by which he means that they 'are concerned with different domains, make different claims, and are established by different means' (Soames 1984: 155). He intends to show this by showing that there is a body of evidence to which psychology must answer, but towards which linguistics can be indifferent. The reason why linguistics is not constrained to explain or predict this evidence is supposed to be that such evidence is not *directly relevant* to *linguistically significant* properties of sentence types. We have been assuming,

<sup>6</sup> Actually, I am not sure that this point should be conceded. I am sympathetic to the view that whenever one event explains another, there is some sort of nomological connection between the two. Suppose that instead of finding rabbit-pots, archaeologists had uncovered pages of incredibly detailed photographs. I imagine that people would not feel that the connection was quite as contingent in this case, but there is no essential difference between this and the case of the pots. Or, at any rate, so I am prepared to argue. But it would take me too far afield to make this argument, and I do not want to rest anything on it.

with Soames, that we have an adequate criterion of linguistic significance, and that we have in fact identified several properties and relations that satisfy this criterion. The issue we have been exploring is whether or not there is any acceptable interpretation of 'direct relevance' that will yield the conclusion Soames is after.

The current suggestion is that the only data that are 'directly relevant' to the linguistically significant properties are data that derive from phenomena that are *nomologically connected* to the linguistically significant phenomena. But now we can see at a glance two reasons why such a suggestion will be of no help to Soames: first, on this interpretation of 'direct relevance', it is going to come out false that theories are only constrained by data that are directly relevant to the 'significant' phenomena in that domain; and secondly, we shall not be able, in any case, to tell which data will or will not be directly relevant to the theoretically significant properties just by looking at them. The game is over once it is conceded that *we cannot know in advance which phenomena are nomologically connected to which other phenomena*.

We can make both points at once by considering the case of palaeontology: the study of ancient things. Palaeontologists wish, among other things, to achieve the correct biological classification of the animal fossils they discover. Now while the notion of 'species' is somewhat controversial, we probably have a good enough consensus on what makes two organisms conspecific to be able to effect at least a partial division of properties according to their 'palaeontological significance'. Let us, at any rate, for the sake of clarity, presume the notion of 'biological' as opposed to 'taxonomic' species (Futuyma 1986: 219), according to which two individual organisms are assigned to distinct species only if they belong to populations that are reproductively isolated from each other. On this view of species, morphological differences between two animals are neither necessary nor sufficient for assigning them to different species.

Applying Soames's criterion to the field of palaeontology, then, we might argue that phylogenetic history is 'palaeontologically significant', but that geographical location and morphological similarity are not. That is, if two organisms share the appropriate aspects of phylogenetic history, we would count them as conspecifics, whether or not they are (or were) found in different parts of the world, and whether or not they display what otherwise might appear to be significant morphological differences. Neither location

nor morphology *per se* can serve to individuate species; location and morphology must *bear on* phylogenetic history in order to do that. (Take, for example, human beings: because of our relatively recent descent from common ancestors, and because of the lack of effective reproductive isolators between human populations, we are all people counted as members of the same species, wherever we live, and whatever we look like.)

Now of these two palaeontologically insignificant properties, it could be argued that one is, and that one is not, *nomologically* connected to phylogenetic history.<sup>7</sup> Geographical placement is a largely contingent affair—although there are some lawful constraints on the kinds of genotype that will lead to viable fertile offspring in various environments, limiting the *de facto* geographical range of certain genomes, a good deal about where an animal ends up living is a matter of chance, and some organisms that evolve in one location need only a trick of fate to turn up and flourish in a different one. Thus, on the current interpretation, facts about geographical placement will not be ‘directly relevant’ to palaeontologically significant properties.

Suppose it so—does it follow that palaeontology is not constrained by facts about the geographical distribution of animal kinds? Clearly not. It is on precisely such facts that palaeontologists must rely in the early stages of phylogenetic theorizing: the physical location of a fossil might well provide crucial evidence about the phylogenetically significant properties.<sup>8</sup> As a result, the finding of a fossil at the ‘wrong’ place might easily count against assigning it to the species suggested by its morphology.

The reason that the location of the find can be evidence for the species of the fossil it discloses is that there is an *empirically regular* connection between the area in which something lived and the kind of thing it is. Even if not a law, it may be a *fact*, and a discoverable one, that certain animals will never be found at certain geological depths or in the company of certain other kinds of animal or in particular geographical locations. And even though

<sup>7</sup> Remember that I am allowing the possibility of making such a division sensible only for the sake of argument. Rejecting such a division only strengthens my case against Soames.

<sup>8</sup> Conversely, palaeontological findings can occasion changes in geological theory, as happened when discrepancies between the order of fossil placement in a geological formation and the order predicted by the extant fossil record prompted (or supported) a theory of geological folding. See Kitcher (1982).

these accidental facts about the distribution of species in time and space are not conceptually criterial for being a member of a particular species, they may none the less *predict* species membership quite reliably, *just because of the way the world happened to be*.

Analogously, a systematic difference in the order of acquisition of two sets of syntactic constructions might well predict deep underlying grammatical differences in the languages of two speaker groups, despite the contingency of the connection between order of acquisition *per se* and linguistically significant properties. It could happen that, in the absence of complete grammatical descriptions of the languages from which two fragments are available, information about the order in which certain constructions are acquired could be used reliably to predict the existence of hitherto unremarked differences in the ‘linguistically significant’ properties of the language.

This possibility seems especially salient if one keeps firmly in mind the fact that the grammars of humanly natural languages *are* acquirable, under conditions of casual learning. If two groups of speakers showed *systematic* differences in the order of acquisition of certain constructions, that fact would itself require explanation. One possible explanation would be a significant difference in the linguistic environments of the two different speaker groups during the relevant stage of development. But the existence of such differences would itself undercut the view that the two groups were speaking the same language—that is, that the languages of the two groups were identical with respect to the linguistically significant properties.

More moderately, and more realistically, it could be that differences in acquisition corresponding to differences in linguistic environment served to signal the early stages of the kind of grammatical reanalysis involved in linguistic change. David Lightfoot points out that while parents and children usually share linguistic environments, there are circumstances in which the environments of a later generation can come to differ from those in which the members of the parent generation acquired their language, to such an extent that the children develop a different grammar from their parents’. Lightfoot believes that something of this sort might explain the switch from SOV word order in Old English to the SVO order that is present in modern English (Lightfoot 1982: 149–58).

There are, Lightfoot observes, factors that cause languages with SOV

order to 'leaf' sentences with surface SVO order: for example, (1) perceptual and memory limitations that make 'heavy' centre-embeddings difficult to process will sometimes cause displacement of sentential NPs in object place to post-verb position, and (2) stylistic innovations for rhetorical effect often have the form of 'violations' of normal word order. If such forms become sufficiently common, there may be enough SVO sentences in a language learner's environment to trigger a grammar that has SVO as an underlying phrase-structure rule—yielding a reanalysis of the language spoken by the adults.

Such a reanalysis could have been evidenced by changes in the manner of acquisition (although this is not the evidence that Lightfoot in fact appeals to). While 'the language' of both parent and child would contain both SOV and SVO surface structures, it is most likely that the parent would not have produced SVO sentences until after or at about the time that he or she was beginning to produce sentential complements. On the other hand, we would expect that the child, for whom SVO order is the default, would produce sentences displaying that order *initially*, even before displaying any competence with respect to sentential NPs.<sup>9</sup>

The alert reader will notice that the above example does not really meet specifications: it is not an example of acquisitional data prompting revisions in the theory of grammar—on the contrary, it is a case in which acquisitional differences are posited in order to account for already known grammatical differences. But this situation is hard to avoid—the fact is that linguists are <sup>9</sup> This example serves to make a couple of other points. First, it illustrates the role that processing information—in this case, independently confirmed claims about memory limitations and perceptual capabilities—can play in the development of an elegant grammatical analysis—in this case, explaining the occurrence of SVO sentences in a language that otherwise appears to be SOV. Second, the case serves to raise doubts about the perspicuity of the notion of 'language' itself: does Soames have any clear intuitions about whether or not the parent and child languages should be counted the same? If the grammatical analyses are correct, then they differ in grammatical (and hence linguistically significant) properties. But by the same token, the parental and child languages are perfectly mutually intelligible, and may even be very close in output, differing extensionally only to the degree that might be expected in the corpora of two different speakers with different rhetorical styles. From a Chomskian perspective, the question of whether we have one or two languages is utterly without interest, since from that perspective we are interested in what the internalized grammar is, and in what it reveals about universal cognitive structure. From Soames's perspective, it is a question of central importance that must receive a principled answer.

rarely, if ever, faced with a situation like that of Soames's imaginary linguist, in which they possess refined data about acquisition and processing, but at the same time have so small and fragmentary a corpus that there is serious doubt about whether two bits of text or speech should be counted as part of the same language or not.

None the less, despite its disanalogy with Soames's thought-experiment, the above case still serves to make the needed point, viz. that facts that are only contingently linked to the linguistically significant properties of a language can still constrain theorizing in that domain. Thus, either the notion of 'direct relevance' of data has no interesting bearing on questions about the relevance of a particular body of data to a particular theoretical domain, or we ought to reject the principle that only data with properties lawfully connected to linguistically significant properties are 'directly relevant' to linguistic theorizing. That said, I would like to conclude by pointing out that even if we were to accept this principle, we would not obtain the conclusion that Soames wants.

If we were to accept the principle, then the whole issue about the relevance of psychological data to linguistic theory would turn on the facts about what actual nomological connections exist. Two things immediately follow that undercut Soames's position: first, because it cannot be determined a priori that two phenomena are *not* lawfully connected, no conclusions can be drawn about the relation between psychological data and linguistic phenomena without looking at the facts, and at currently available theories. Second, when one *does* look at the facts, and at currently available theories, it begins to look *very* likely that there are connections between psychological phenomena and linguistic phenomena that are every bit as lawful as the connection between genotype and morphology.

Imagine someone arguing in the following way: genotype is not paleontologically significant because if two organisms were found to have the same genotype, but morphologies so different that they could not interbreed, we would not count them as members of the same species. The proper response to such an argument, I take it, is to say that if such a case were ever found, it would disprove a great deal of biological theory, but that we can therefore have the same degree of confidence that such a scenario will never come to pass as we have in the relevant portions of biological theory. That is, our reasons for thinking that no case of this sort will be forthcoming are

precisely our reasons for thinking that a particular *account* of morphological variation is true—namely, the account that posits a lawlike connection between genotype and aspects of the phenotype.

Thus, it is no good for Soames to point to the *logical* possibility that two speakers could produce utterances conforming to exactly the same grammar, while differing non-trivially with respect to acquisition or processing. The thought-experiment should not yield results. Until we know what nomological connections there may be between the psychological facts and the linguistic facts, we are not in a position to say what factors can *in fact* vary independently of what other factors, and the mere logical point that the description of the case is coherent is quite irrelevant to the matter of what data can be brought to bear on what theories.

#### 4 Connecting Psychology and Linguistics

What are the connections, then, between processing and acquisitional facts, on the one hand, and linguistic facts on the other? The first thing that must be noted is that Soames tends to caricature the ways in which psychological linguists actually do make use of psychological data. Soames seems to suggest, for example, that a psychological linguist is saddled with the view that *any* difference in the details of processing or acquisition must be theoretically relevant, so that the psychological linguist is not allowed any freedom to idealize. Soames does not burden the abstract linguist with what would seem to be the corresponding principle—that *any* difference between two speakers with respect to the linguistically significant properties is enough to count their languages as distinct.<sup>10</sup>

But in fact, linguists of a Chomskian bent have always emphasized the dangers of trying to derive quick predictions about production or comprehension—'performance'—from descriptions of grammatical knowledge—'competence'. They have, furthermore, emphasized the importance of keeping an open mind about the classification of phenomena, abjuring a priori rulings about whether a fact is syntactic or semantic or pragmatic, or

<sup>10</sup> Soames gives us no indication, for example, of how we are supposed to tell whether a sentence like 'He's a good man, Moe is' is to be counted as part of English or not, given that people who regard themselves as English-speakers are divided on its grammaticality. Is one disagreement enough? Are some speakers simply *wrong* in their grammaticality judgements? Which ones? More on this below.

something to be accounted for by the grammar or by a theory of perception or attention or memory. In sum, Chomskians have a very complicated story to tell about how processing and acquisitional data bear on theories of grammar.

Having registered that caveat, we still have to show that theorizing in this vein vindicates the claim that there are nomological connections between psychological properties and linguistic ones. I shall comment here only on the relevance of facts about acquisition. Lydia White, although generally very cautious about the applicability of acquisitional data to issues in theory of syntax, has none the less offered several examples of ways in which grammatical facts may be linked to, and thus generate predictions about, acquisitional facts (White 1981: 257–69). I shall conclude with a brief summary of one of these examples.

It is well known that children are not provided with much in the way of *negative* data—parents and care-givers tend not to correct utterances that are ungrammatical from the adult point of view, and on the few occasions when they do issue corrections, the children pay little attention. This raises the question of how children do eventually come to learn of certain forms that they are ungrammatical. They cannot be relying on trial and error, because they cannot count on being told when they are in error. And obviously they cannot infer from the mere non-occurrence of the form in the primary linguistic data that it is ungrammatical, since there are an infinite number of grammatical forms that they will not see (or have not yet seen). This is one facet of the poverty-of-the-stimulus argument: how do kids acquire rules that proscribe certain forms?

Consider the proposed transformation of Dative Movement. This is supposed to move an NP that is the object of the preposition 'to' into indirect object position. Dative Movement, applied to:

(1) We sent the book to George.

yields:

(2) We sent George the book.

If such a rule were part of the grammar, and if the child had to infer it from positive instances, then how would a child, on hearing a sentence like:

(3) We reported the accident to the police,  
ever learn that:

(4) \*We reported the police the accident.<sup>11</sup>  
is ungrammatical?

This situation has suggested to some linguists that it should be a constraint on the kinds of rule that can compose a humanly natural grammar that they be acquirable on the basis of positive evidence alone. C. L. Baker, for example, proposes what he calls an 'inductive requirement' on grammars: 'If a linguist proposes a grammar G for some language, then G meets the "inductive requirement" if it is accompanied by a set of hypotheses about human cognitive capacities from which G can be deduced, given primary linguistic data' (Baker 1979, cited in White 1981: 267). Baker proposes a different analysis of data (1)-(4) that satisfies the inductive requirement. He argues that the grammar base-generates dative constructions (V NP NP) alongside sentences with prepositional phrases (V NP PP), and that verbs are subcategorized for either or both constructions. The child's task is considerably simplified—she simply waits for positive evidence that a particular verb allows the dative construction before producing a sentence with that verb in that context.

I note in passing another interesting feature of this example: Baker's hypothesis effectively involves a reassignment of a phenomenon from the category 'grammatical' to the category 'lexical'. Now if we reflect on the fact that, whatever else is true of languages, human children have to acquire them, we should find this sort of move a very good one. Given that children are not built to acquire specific languages, but rather need to be able to pick up the local lingo, would it not be extremely sensible for nature to have worked it out so that *grammar* was more or less universal, and only the *lexicon* showed local variation? We know that languages vary with respect to the words that compose them, and so we know that children *have* to cope with variety of that sort. Wouldn't it be a good deal if *all* linguistic variety could somehow be shown to consist in or derive from the lexical variety?

<sup>11</sup> The \* symbol before a construction is the conventional way among transformational linguists, of indicating that native speakers judge the sentence to be unacceptable.

This is indeed the sort of thinking that has been informing a good deal of syntactic theory in the last decade and a half.

Moral: linguistics is hard. It is considerably more than a semester's work to figure out the grammar of a language—even of one's own idiolect. Linguists, like all other scientists, are going to take whatever help they can get. The fact that languages are things that human beings can and do acquire under certain circumstances, and things that human beings can understand and use, given the cognitive resources at our disposal, is, essentially speaking, of *huge* importance. On pain of being laughed at, philosophers should stop trying to tell linguists to ignore it.

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## CHAPTER 3

# Is Linguistics a Branch of Psychology?

Stephen Laurence

### 1 Introduction

According to Noam Chomsky's well-known and influential account, linguistics is properly conceived of as a branch of cognitive psychology. Linguistics studies one aspect of the mind, namely our 'competence' or knowledge of the natural language we speak. This view is widely endorsed by linguists, but has encountered considerable and sustained resistance among philosophers. Though the issue has been much debated, it seems far from settled. It would certainly be interesting if it could be shown, for example, that the Chomskian approach was fundamentally in error (perhaps embodying some deep confusion), or that linguistics could not be thought of as a scientific discipline, or that some different conception of linguistics was manifestly superior. However, the kinds of considerations philosophers have raised do not in fact show anything of this sort. Nearly all of the arguments purporting to raise particular problems for linguistics generalize in fairly immediate ways to other sciences. But, whereas the arguments may initially seem plausible when directed particularly against linguistics, I think that it becomes clear when they are considered in the light of these other sciences that they are based on rather dubious general principles. Accordingly, my strategy will in large part be to draw analogies to other sciences, and see how the arguments directed against linguistics play out there.

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