

REPLY

Language Is More Than Its Parts: A Reply to Bird, Howard, and Franklin (2001)

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Bird, Howard, and Franklin (2000) proposed a model that purports to account parsimoniously for two kinds of deficits often observed in patients with brain damage: semantic category-specific deficits (i.e., problems accessing knowledge about animals, artifacts, or other categories of objects) and grammatical class-specific deficits (problems accessing lexical representations of nouns or verbs). In a commentary on this model, which we called the extended sensory/functional theory (ESFT; Shapiro & Caramazza, 2001), we argued that the assumptions upon which it is based are not empirically credible and that even if they were, the model would not be adequate to account for all cases of putative grammatical category-specific deficits.

The proponents of the ESFT have now responded to our criticism by defending the core assumptions of the model and by questioning our explanation of cases we judged to be problematic for it (Bird, Howard, & Franklin, 2001). In our view, their response is flawed in two crucial respects. First, some of the empirical claims made in defense of the ESFT are either misleading or plainly inaccurate. Second, and more importantly, we believe that the strictures that Bird and colleagues would impose on the collection and interpretation of data have the practical effect of redefining the topic of inquiry to avoid the question that is of greatest interest—namely, how spoken language is produced.

We will first deal briefly with the inaccuracies in Bird and colleagues' response, after which we will return to the broader theoretical issue at stake.

To begin with, the arguments that Bird and colleagues make in defense of the sensory/functional theory, on which the ESFT is based, are substantially irrelevant. First they assert that even “small” differences in the ratios of sensory to nonsensory features attributed by normal subjects to living and nonliving things are “worth noting.” We contend that even if these differences were statistically reliable, they would not be “worth noting” because they are not large enough to support the kinds of apparently categorical distinctions that they are supposed to underlie.¹ In other words,

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¹ Apropos of this issue, Bird et al. suggest that at least one empirically derived set of feature ratios (Hodges et al., 1996) shows little difference between nonliving and living items because musical instruments are included as nonliving, though “it has been suggested that musical instruments tend to be

damage to sensory features might give rise to an apparent category-specific deficit for living things *only if* the difference in the amount of sensory information associated with living and nonliving things were large enough to distinguish between the two categories on that basis; if the difference were not sufficiently large, any damage severe enough to impair knowledge of living things would also substantially impair knowledge of nonliving things, so that nothing resembling a category-specific deficit could arise. Bird and colleagues point to small differences in feature ratios between living things and nonliving things without any evident appreciation for this basic requirement of the theory.

The authors then go on to speculate that, in studies where subjects were asked to list features pertaining to living and nonliving things (which also failed to produce differences in feature ratios), respondents may have omitted sensory features of animals because they are not distinctive. That is, because most animals have eyes and a mouth, naming these features would not distinguish an elephant from other animals. This may be true (although it is pure speculation), but its significance is unclear. Several categories of nonliving things also have common and nondistinctive sensory features: most tools have handles, for example, and are made of metal. In any case, it is incumbent upon the supporters of sensory/functional models to demonstrate experimentally that intercorrelation of features is relevant to understanding the lack of an observed difference in normal subjects' feature generation for living and nonliving items. At press time, they have not done so.

Nor do we observe that patients with category-specific deficits tend to lose access to knowledge about particular kinds of semantic features along with categories of objects. A patient like JJ (Hillis & Caramazza, 1991), with a putative category-specific deficit for nonliving things, appears to have substantially preserved access to all sorts of features of animals—including sensory, functional, and encyclopedic features, distinctive features, and supposedly nondistinctive features—and damaged access to these features for nonliving things. Compare his definitions of a mouse and of celery:

A small little animal with a pointed nose, pointed ears, and a little snout; about one inch high, or one and a quarter inches. It doesn't have much value, except that it can be eaten by animals. Cats chase them. It eats whatever it can steal in people's houses, even in my house. They move rather quickly, climb up on things, and can stand on two feet.

Very white bodied meat . . . soft . . . easy to eat.

Bird and colleagues claim that JJ's performance "is suggestive of relative sparing of . . . semantic representations which might pertain to sensorimotor information," yet they offer *no specific account* of what kind of semantic representations might be involved (and how the value and eating habits of a mouse, for example, constitute "sensorimotor information"). In the absence of such a hypothesis it is difficult to take this sort of speculation seriously. Indeed, the majority of well-studied cases (and not merely a "few," as Bird and colleagues suggest) show no association between sensory features and deficits for living things or between nonsensory features and deficits for nonliving things.

From all this it would seem that Bird and colleagues have not successfully defended the core tenets of the sensory/functional hypothesis, as they claim to have done. Nor, as we shall see, have they resolved the problems posed for the ESFT by several patients discussed in our earlier comments, who present either modality-specific grammatical class deficits (SJD in Caramazza & Hillis, 1991; EBA in Hillis & Cara-

impaired" along with living things. This suggested link has not been corroborated by recent investigations and therefore should be regarded as having little bearing on the question at stake (Barbarotto, Capitani, & Laiacona, 2001).

mazza, 1995) or deficits that appear to be explicitly grammatical, and not semantic, in nature (JR in Shapiro, Shelton, & Caramazza, 2000).

Indeed, Bird and colleagues go so far as to allege that SJD's pattern of performance is incompatible with a modality-specific explanation because she presented with deficits in both written verb production (Caramazza & Hillis, 1991) and in reading verbs (Badecker & Caramazza, 1991). The logic behind this argument is somewhat mysterious. For one thing, SJD's deficit in reading verbs was restricted to morphologically regular inflected forms; such a specific impairment may not be pertinent to the overall sparing of verbs relative to nouns in production. Even if the two deficits are related, this would be consistent with the proposal that SJD has difficulty in accessing orthographic forms of verbs.

Bird and colleagues also fail to provide any convincing explanation of how the ESFT might accommodate the pattern of performance displayed by patient EBA (Hillis & Caramazza, 1995). They dismiss the challenge that patient EBA presents to their theory by claiming that a strong imageability effect can explain her poor performance in lexical decision tasks for verbs relative to nouns, while damage to sensory features, or a disconnection between sensory features and spoken output mechanisms, accounts for her deficit in naming concrete nouns. This account relies on the tacit assumption that lexical decision enjoins access to semantics—a postulate with no obvious motivation. They also fail to mention that EBA made almost no errors in spoken and written word-to-picture matching, tasks that almost certainly *do* require access to semantics. EBA's spared comprehension belies the notion that her knowledge of sensory semantic features is damaged.

Finally, Bird and colleagues take exception to our description of patient JR (Shapiro et al., 2000), who is said to have a specific deficit in processing lexical representations as nouns. They criticize specifically our characterization of his performance on a number of tasks that they interpret as reflecting "normal" imageability or concreteness effects. We are indeed open to the suggestion that JR's comprehension is sensitive to such variables as imageability and concreteness, as is (evidently) his spoken production of nouns, and we say as much in Shapiro et al. (2000).

What is at issue is whether these variables are *sufficient* to account for JR's pattern of performance. Bird and colleagues agree that they are *not* sufficient to explain his difficulty using nouns in sentence contexts, but conclude based on this that JR's difficulties using nouns in sentence contexts were attributable to "some kind of syntactic impairment unconnected" with his difficulties producing nouns in isolation. This solution strikes us as inelegant and moreover inadequate in that it fails to account for particularities in the data, such as why JR found it harder to produce inflected forms of nouns and pseudo-nouns than verbs and pseudo-verbs. Bird and colleagues are correct in noting that JR's production of verb morphology is not perfect, but it is still significantly better than his production of noun morphology, a fact for which they have no explanation.² Indeed, the emphasis they place on JR's relatively minor problems with verb morphology seems to reflect a misguided notion that category-specific deficits must be categorically discrete.

More to the point, the claim that "the only reliable data are those of the naming assessments" merely serves to exclude from consideration precisely the data that

² The suggestion that JR has difficulty in comprehending morphology (Bird et al., 2001, attributed to Shapiro) is unfounded. The reference is to a statement that JR made mistakes on a grammaticality judgment task which included sentences with errors in subject-verb or quantifier-noun agreement. The source of JR's problems with this task (i.e., whether it reflected problems with noun morphology, morphology in general, or simply inattention or a deficit in working memory) was impossible to determine and therefore the task was not reported in Shapiro et al. (2000).

are problematic for the ESFT. Our account holds that JR's access to grammatical information about nouns is impaired and predicts that the manifestations of this impairment should be subtler (if still evident to some degree) in tasks in which access to grammatical information is not crucial, such as single-word naming. There seems to be no disagreement that this is precisely JR's pattern of performance.

At the other extreme, Bird and colleagues are not convinced that patient JJ, whom we discussed earlier in the context of semantic category-specific deficits, did not also have a deficit in verb production: "one would like to see evidence for the lack of any verb deficit," they write, "but this is not provided." We are not sure precisely what evidence is wanting. It is more than clear from JJ's fluent and grammatical spontaneous speech that he retains the ability to use verbs in various syntactic contexts, including even passive constructions, to say nothing of his excellent command of prepositions, pronouns, auxiliaries, and other low imageability functional elements. Surely it is strange to insist that a patient who can produce pragmatically appropriate and semantically meaningful verbs spontaneously in the context of sentences may still have an effective verb impairment if he is unable, say, to read a list of abstract words.

The dispute over how to interpret the deficits exhibited by JR and JJ gets to the heart of the current debate. It seems to us that Bird and colleagues miss a critical point when they insist that an impairment "truly limited to the grammatical class of verbs . . . should maintain outside sentence contexts [sic]." Our discussion of grammatical categories in Shapiro et al. (2000) is worth repeating here:

It is important to stress . . . that a term like 'noun feature,' used in an expository manner, should not be taken to indicate a physical node or a unitary representation, but rather a package of closely related computational processes that apply exclusively to a certain syntactic category of speech elements—to wit, nouns. In naming tasks, which require little morphological or syntactic manipulation of the target word, these processes will be engaged minimally, though not necessarily undetectably. On the other hand, 'propositional' tasks like sentence formulation and inflection will place greater demands on the mechanisms in question. In general, utilising a word in its syntactic context enjoins access to 'class features' or grammatical properties specified in its lexical representation.

Bird and colleagues' statement effectively dismisses the relevance of data obtained in propositional naming tasks—in other words, in precisely those tasks for which grammatical class information is likely to be most relevant and the potential importance of this information in lexical access least obscured by confounding variables like frequency, imageability, and length (Griffin & Bock, 2000).

But this is not merely a question of methodology. Language, in the sense in which we ordinarily use the term, is propositional in nature: words are normally produced as elements embedded in phrases and sentences, not as freestanding units. The study of language must therefore include the study of words in their syntactic context. Though we do, of course, employ tasks which require that words be produced in isolation, we do so in hopes that they may shed light on some aspect of a larger process and not because we are interested in isolated naming as such. (Even the proverbial drunk searching for his wallet in the light of a streetlight does so only because it is easier to see in the light than in the dark, not because he believes that his wallet cannot be found elsewhere.)

It follows that any enterprise which ignores or categorically denies the relevance of syntactic operations is not the study of language, but something else, perhaps the study of vocabulary or some other branch of semiotics. For those of us interested in language, however, the ESFT has little to offer, except possibly a cautionary tale on the dangers of narrowly focusing on a single variable.

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