There’s no reality like psychological reality

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There are some terms whose mere introduction into a discourse is almost guaranteed to bring rational discussion to an abrupt halt. One such is ‘political correctness’: as soon as we try to ascertain whether some position is or is not politically correct, the entire matter is sent reeling into a dark pit from which there is no exit. As with ‘the present king of France’, the term incorporates a false presupposition which can undermine any statement it is part of, as long as it is permitted to lurk undetected.

The expression ‘psychological reality’ is of this kind. I assume that the grammars posited on the basis of linguistic evidence ought to be taken as models of the competence of native speaker-hearers, i.e. as ‘real’ properties of the speaker-hearer. Ah, I can hear some ask, but are they psychologically real? This seems to be a new question, a question of great moment, though the modifier ‘psychologically’ adds nothing to the characterization of the situation. It does misleadingly suggest that there is some other sort of reality to which linguistic constructs can aspire.

Among those who believe that linguistic reality is different from psychological reality there appear to be hard-liners and moderates. The hard-liners maintain that psychological reality is all there is, and since ordinary linguistics does not address it, linguistic analyses can be dismissed as the products of history, coincidence, or delusion. The moderates appear to be more sympathetic. They profess to admire the elegance, ingenuity, and sheer imagination of linguistic analyses. They may read such things with pleasure while taking a break from the hard work at the lab. Moderates grant us our linguistic reality, as long as we make no claims about psychology. But what kind of reality is linguistic reality apart from psychological reality? The moderate view can be further explored in the form of an analogy in the domain of the study of clouds. One person (the ‘psychologist’) has an analysis in terms of air pressure, condensation, and so on. This analysis might be a bit prosaic, but it has meteorological reality. Another analyst (the ‘linguist’) sees in the clouds shapes of horses and castles, and fashions narratives which may be admired for their imagination and coherence, but which do not attain to meteorological reality, or, come to think of, to any other kind of reality. At least with the hard-liners we know where we stand.

This being so, I would like to propose a simple rule of thumb: nothing good can ever come from talking about psychological reality. I offer as proof two well-meaning attempts to discuss this issue. The origin of the phrase is usually traced to the famous paper ‘La Réalité Psychologique des Phonèmes’ by Edward Sapir (1933). In this article, Sapir offered a number of cases where a linguistic analysis posits a certain system of phonemes. In each case, he illustrates how the phonemic analysis sheds light on some otherwise puzzling behaviour of native speakers: one spells different sounds the same way, another hears two identical sounds as different, etc. I think that what Sapir meant to say was: here is some evidence (the synchronic patterning of sounds) that the phoneme is real, and now here is more evidence (external to the synchronic pattern) for the same conclusion. Chomsky (1980) suggests that Sapir’s argument has been consistently misinterpreted as follows: here is evidence from synchronic sound patterns for a linguistic analysis, and now here is perceptual evidence that this analysis is psychologically real. According to Chomsky, Sapir’s argument, so (mis)understood, paved the way for the subsequent practice of dividing evidence into two types: evidence which does
not bear on psychological reality—synchronic and diachronic patterns, i.e. conventional linguistics—and evidence which does—evidence from acquisition, aphasia, speech errors, or any experiment which requires special apparatus or clothes, or consent forms. It is unlikely we would draw such distinctions in the study of the cognitive abilities of any other species. Could Sapir have foreseen and averted this historical misunderstanding, writing at a time when the term ‘psychological’ was itself so offensive in North America that he published the paper in France? In hindsight, it would have helped us more if he had used the title ‘The Psychological Reality of Phonemes’ for his article on sound patterns in language, which uses only conventional linguistic evidence. Be that as it may, the end result is that no good has come from introducing the term ‘psychological reality’.

As a second proof, consider the essay ‘Psychological Reality of Grammars’ by Robert Matthews (1991). Like Sapir, Matthews sets out to defend the claim that the grammars made available by linguistic theory are (psychologically) real. He ends his defence, however, by concluding that although the function specified by a grammar is intended to be psychologically real, the ‘linguistic constructs that figure in the grammar’ are not intended to be psychologically real. It should be noted that these linguistic constructs include the ‘rules, representations, and the computations that figure in syntactic derivations’. To be fair, Matthews is concerned with the general question of what it means for a grammar to be psychologically real, supposing that we grant that it is psychologically real. He does not distinguish between research which leads to psychological reality and that which does not. Nevertheless, if a defence of the psychological reality of the grammars proposed by linguists ends with the conclusion that their rules, representations, and derivations are not psychologically real, then we have again proved that nothing good can ever come from talking about psychological reality.

So why talk about it now? Because, with rare exceptions, the only time we hear about psychological reality is when it is being asserted that this or that aspect of linguistic theory is not psychologically real. Moreover, many people implicitly accept the view that there are some types of research which bear more on psychological reality than others. It would be natural to think that something called psycholinguistics would be necessarily closer to psychological reality than theoretical linguistics (especially given the popular understanding of ‘theory’ as ‘wild speculation’); similarly, seekers after psychological reality might be more drawn to a journal called Brain and Language than to one called Natural Language and Linguistic Theory. But is there really a fundamental difference in the kind of research assigned to these categories that bears on their relevance to (psychological) reality?

By way of investigating this matter empirically, let us consider two recent studies of Polish syntax. The first is a study in Brain and Language by Jarema, Kądzielawa, and Waite (1987) of active/passive sentence comprehension by a Polish-speaking agrammatic aphasic. They found that their subject did quite well in interpreting both active and passive sentences with one word order (Agent-Verb-Theme in the active, Theme-Aux-Verb-by-Agent in the passive), but performed at about chance level when word order was inverted, even though such sentences are grammatical in Polish. They suggest that this evidence supports the idea that there is a basic word order in Polish, and that thematic roles are preferably assigned to canonical positions. They go on to contrast the performance of their subject with that of English speakers with similar aphasia, who have been reported to be less successful in interpreting English passives. They attribute this result to morphological differences between Polish and English verbal forms. Since their evidence is drawn from aphasia, there would be little dispute that their analysis bears on psychological reality.

Compare now the recent study by Borsely and Rivero (1994) in Natural Language and Linguistic Theory on clitic auxiliaries in Polish. Like the previous study, they adduce evidence that, although word order in Polish appears to be quite free, there is a basic order from which others are derived by movement rules of various kinds. They argue that Polish participles incorporate with their auxiliaries by movement of the participle to the head of AuxP; apparently identical sentences in
Bulgarian, Czech, Serbo-Croatian, and Slovak have different structures, derived by long head movement of the participle into the complementizer. They show that this hypothesis accounts for a number of systematic differences between Polish and the other Slavic languages.

Since this evidence is drawn not from the performance of a single aphasic but from the daily behaviour of millions of ordinary Poles, the analysis presumably does not bear on psychological reality, by standard criteria. If Poles were dolphins, the irrationality of this distinction would be apparent to almost everyone.

References


