Some Limitations to the Psychological Orientation, in Semantic Theory*

Richmond H. Thomason

Intelligent Systems Program
University of Pittsburgh
Pittsburgh, PA 15260
U.S.A.

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Comments Welcome

Notes on this manuscript. This paper was begun in the mid 1980's – I will try to find out exactly when sometime. The last time that I worked at all on the text was in 1988. This is essentially the 1988 draft, with some minor formatting changes. I have never published the paper because it seems to me to need more thought.

1. Historical Perspective[1]

Gottlob Frege was strongly opposed to attempts to provide a psychological foundation for mathematics, and hence to attempts to provide such a foundation for logic. The influence of his views was powerful, and is still potent among philosophers.

There is no substitute for constructive achievement: probably the most important factor in this success story was Frege's formalization of logic and mathematics. But he urges a variety of other arguments in the *Grundlagen*, Frege [3]. Here is a list of what seem to me to be the most convincing of these arguments. All of these take the form of what seemed to Frege to be absurd consequences of psychologism in mathematics.

1. In proving the Pythagorean theorem we would have to allow for the phosphorus content of the human brain. [P. V1]

2. We all have different ideas of numbers, so there would be a separate number two for each person. [P. 37]

3. But in another way there would be too few numbers. We wouldn't be able to get infinitely many of them. [Pp. 37-8.]

4. Sense perception would be the foundation of mathematics; but objectivity requires reason. [P. 38.]

5. It would be strange if the most exact of the sciences needed support from psychology, which is still feeling its way none too surely. [P. 38.]

These arguments are all outdated. None of them applies to a modern cognitive psychology that (a) distinguishes competence from performance, and (b) accounts for competence using mathematical techniques suited to the study of effective processes and the modeling of artificial intelligence.

As for Point 1: the phosphorus content of the brain would presumably be a mere performance factor. As for Point 2: well, maybe it's a bit tricky to say exactly what ideas are, but you can easily imagine two different physical realizations of the same Turing machine. If you can define the number two relative to the machine—say, as '0' followed by rwo strokes—then these two realizations wouldn't disintegrate the number two as Frtege imagines. Even if you had quite different realizations on the level of competence of the same number, the very fact that they *are* realizations of the same number would render them functionally equivalent in some way. As for Point 3: we allow our ideal machines to have infinitely expandable memory. This is one of the chief respects in which they are ideal. So they can think about any number, though perhaps they can't (in some sense) think about all numbers at once. As for Point 4: reason is just what a cognitive theory of this sort is good at handling. And finally, as for Point 5: there is no problem on this score. The theories we are talking about are perfectly exact.

[1]These remarks are not historical in any scholarly sense; but they may provide some useful conceptual background.

It is largely a speculative question, I believe, whether or not Frege would disagree with this modern psychologism. But I do not want to discuss either this topic, or the foundations of mathematics. As far as I know, psychologism is not at present much of an issue in the philosophy of mathematics.

But it certainly is a live issue in semantics. For the purposes of this paper I want to include under semantics any theory that (a) tries to account for the meanings of the phrases (including the sentences) of reasonably rich languages, and (b) subscribes to an ideal of formal exactitude. The first clause rules out a good deal of work devoted to word meaning by anthropologists, linguists, and psychologists—work on semantic fields, for example. The second clause rules out those, like Otto Jespersen and Ogden and Richards, whose account of meaning does not pretend to be other than informal and discursive. It is meant to include people like Noam Chomsky, Jerry Fodor, Ray Jackendoff, Jerrold Katz, and George Lakoff, who (I believe) do subscribe to the ideal, though an exact formulation of the theories they endorse, for a suitably rich language, is not to be found (as far as I know) in the literature.

Two traditions are alive at present in what remains of semantics given these restrictions: logical and cognitive. The cognitive approach is pursued mainly by people who have worked in phonology or syntax, or in artificial intelligence, and who regard these–and semantics as well–as branches of cognitive psychgology. The other approach, oc course, belongs to logicians, and to a group of linguists who were influenced by Richard Montague's work.

In my introduction to Montague [13] I said that Montague considered semantics to be a part of mathematics rather than of psychology. This statement was meant to be expository rather than provocative. In particular, I didn't mean to say that the two conceptions of semantics were irreconcilable. Rather, it seemed to me that those who thought of what they were doing as mathemtics and those who thought of it as psychology were using quite different methodologies, the former tending to be—well—mathematical, and thinking about truth, and the latter trying to be constrained, and thinking about cognitive representation. Also, there are differences in the troubles to which the two approaches seemed to be liable. The logical approach (in its best known versions) falls prey to difficulties with proposiitonal attitudes like 'believe', while the psychological approach seems to have trouble getting an explicit and reasonably comprehensive theory off the ground. Given these differences, I thought it would be helpful to bring them out in open, so that linguists could better understand what Montague was trying to do.

All this, however, is no reason why the two approaches should be incompatible. Indeed it has seemed to some people—philosophers like Max Cresswell and Asa Kasher, for example, and linguists like Barbara Partee[2] (at one stage anyway—I'm not sure whether she still feels this way)—that they *must* be reconcilable. After all, we understand natural languages, and these languages have logical properties that should be characterizable in model theoretic terms. And the obvious way to go about mediating the two would be to show how the apparatus of Montague's intensional logic (or enough of it to interpret a language) could be represented by an idealized finite mind.

The view is nicely stated in Partee.

> ...there may be no problem in principle in regarding a theory like Montague's as a

[2]Cresswell [2]; Kasher [7]; Partee [15] [?]

> kind of competence model. It could be telling correctly how we can determine the meaning of a sentence from the meaning of its parts, with "performance" factors like finite brains explaining why we don't have complete meanings of lexical items and why we sometimes make logical mistakes along the way.
>
> But such a story remains plausible only so long as we ignore the propositional attitudes. [16]

I believe that this is the wrong way around. On the one hand, there is no great difficulty in dealing with propositional attitudes within the context of a theory like Montague's: all that is needed is a more adequate reification of propositions than that afforded by possible worlds.[3] (This, however, is not the topic of the present paper.) On the other hand, serious foundational problems are bound up with the project of psychologically oriented semantics. As it is presently conceived, I believe the project fares about as well as Hilbert's attempt to provide a constructive foundation for mathematics—and for similar reasons.[4]

I hope it is clear now what the task of this paper is. It is to replace Frege's arguments against performance psychologism in logic and mathematics with arguments against competence psychologism in semantics.

These arguments are (I hope) rigorous and convincing. Gut I do not mean to suggest that they precipitate a stable and comfortable intellectual situation. If you take cognitive psychology seriously, it is hard to avoid accepting the competence/performance distinction, and modeling performance in terms of effective operations, and thinking of beliefs as structures that the ideal language user manipulates. And yet these assumptions appear to clash with the arguments that I will present.

The upshot is discomforting. But, as the Liar paradox shows, people can learn to live with this sort of discomfort, and even to make a certain amount of progress here and there. One way of looking at it is this: Benacerraf's problem in the philosophy of mathematics extends to semantics as well. This is the problem of reconciling ontology with epistemology in mathematics. Given the objects that mathematics seems to be about, how do we give an account of how these objects can be known?[5]

Semantics too has its ontological dimension: there is the task of defining the space of semantic values for the expression of a language. But, having defined this space, the epistemological problem arises: what is it to know a language, and how do speakers grasp the meaning of expressions?

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[3]See Thomason [22]

[4]But please don't associate the project of this paper with Lucas' attempt (see [12]) to argue that the incompleteness results somehow show that the abilities of human beings transcend those of an ideal computer. I feel that I know something about the latter; but I know very little about what human beings are like or what they can do. I am not concerned to put any constraints on how psychologists should study these things. All that I am saying is that there will be a gulf between theories of how human beings know language—even as competence theories—and semantic theories of the logical variety.

[5]See Benacerraf [1]. I believe, by the way, that the sort of argument I will give would also go through in an intutionistic setting. If this is correct, then intutionistic logic will not provide a solution to Benacerraf's problem, though it may seem in many ways to do better in this respect than classical logic.

2 Informal presentation of the argument

The psychological approach to semantics begins with the idea that people know the language they speak, and that part of every language is its semantic component. An idealized person can be viewed as a finite symbol-manipulating mechanism. So semantic theories ought to address themselves to systems of symbolic structures that can be manipulated by such mechanisms. Or at least, those theories that claim to be psychologically real should do so.

Many authors have aimed at theories conforming to this idea: the program J. Katz calls "semantic theory,"[6] the recent ideas of both interpretive and generative semanticists,[7] the idea (going back to Carnap) that meanings are equivalence classes of phrase structure trees modulo intensional isomorphism, and any theory that takes semantics to consist in translation into any of the standard systems of logic. Probably the various semantic theories beginning to emerge from artificial intelligence should be included as well, but I am not well enough acquainted with them to judge this myself.

The model theoretic approach does not conform to this conception; this has been clear ever since the beginning. The fundamental result of Tarski [20] is that the relation between a sentence of arithmetic and its truth value is not arithmetical. (I.e., the relation cannot be expressed in arithmetical terms.) As a corollary, the relation is not recursive; therefore it cannot be modeled on an ideal computer.

I want to show that this is not a chance pecularity of just one of the important notions of the model theoretic approach. Rather, it is something that will hold in general of the relation established by a semantic theory between a sentence of a language and its semantic representation. If this relation is recursive, paradoxical consequences will ensue.

The method I apply is a diagonal argument of the kind that has been current in logic and recursion theory for about forty years. The version that I use is adapted from Montague. [14] If the reasoning is correct, then the project of constructing a comprehensive cognitive semantics is comparable to that of squaring the circle with compass and straightedge.

Summary of the argument: if a theory conforming to the psychogogical ideal were correct, and intelligible, an ideal speaker could formulate the theory, including its expression relation—the relation between a sentence and its semantical representation. Suppose that the ideal speaker could formulate the hypothesis that truth is a property of the semantical representations of sentences. We could then construct by standard procedures a sentence of the ideal speaker's language saying that its semantical representation is not true. Thus the hypothesis leads to inconsistency, and so it would be problematic to regard at least one propositional attitude—truth - as a property of the propositions of a psychological theory. And, if certain assumptions about knowledge and belief are granted which are plausible in the context of cognitive psychology, the result concerning truth extends to these propositional attitudes as well.

Thus, if certain natural assumptions about truth, knowledge, or belief are added to such a semantical theory, and the theory is then taught to the ideal speaker, the ideal speaker will be inconsistent. The difficulty is that, on the psychological ideal, the notions of semantics will

[6]See Katz [8] and [9].

[7]For interpretive semantics, see Jackendoff [6] and the references given there. For generative semantics, see lakoff [11] and the references given there.

be recursive. Since such notions can be captured in simple formal languages, and certainly in human languages, there is nothing preventing the theory from being taught to the ideal speaker.

3 The argument formally presented

We assume a semantic theory for an ideal speaker—say, Karl—whose language is disambiguated, and capable of saying some arithmetical things: In Karl's language there are standard names $\overline{0}$, $\overline{1}$, $\overline{2}$, ... of the nonnegative integers, and there are linguistic constructions corresponging to disjunction, negation, and variable binding by the quantifiers of first order logic. We assume that Karl has a theory expressed in his language, and that it contains a minimum of arithmetic (this can be made precise by saying that it contains Robinson's arithmetic, **Q**). Such a language has the power to express any recursive function on the natural numbers: i.e., if F is such a function then there is a two-lace predicate F (perhaps complex) of Karl's language such that for all n, if $f(n) = m$ then $\vdash (x)[F(\overline{n}, x) \leftrightarrow x = \overline{m}]$. (Here, '$\vdash$' means 'is provable in Karl's theory.)

A semantic theory of Karl and his language conforming to the psychological ideal will provide semantic representations for the formulas of Karl's language—the *propositions* of the theory.[8] These propositions will be finite structures, and the set of them will be a recursive set. (This is so because the theory conforms to the psychological ideal; that this set is recursive will be part of the explanation of how Karl can store his semantic knowledge of his language.) These propositions will presumably contain meaningful parts: for instance, if $\overline{2} + \overline{6} > \overline{5}$ is a sentence of Karl's language, then the corresponding proposition will contain a part corresponding to the numeral $\overline{2}$.

Since Karl's language is disambiguated,[9] each of its sentences will express exactly one proposition.[10] *this relation of expression will be recursive: expressing will be a recursive function.*

[8]If we wanted to make distinctions that are natural, but not needed for our argument we could call the semantic representations of open formulas *propositional forms,* and those of closed formulas it propositions. I will use 'proposition' to refer to propositional forms, as well as to propositions proper.

[9]This is a simplifying assumption. If you like, you can think of Karl's disambiguated language as an artifact resulting from the analysis of Karl's used language.

[10]Another simplifying assumption. If we give up the assumption that each sentence of karl's language expresses a proposition, arguments similar to those I give will still go through, with consequences that are almost as damaging. For instance, we will be able to formulate a formula ϕ of Karl's theory that is provable, but such that according to the theory it is nonsensical. (This will be a sentence saying of itself, in the case of truth, that if it expresses any proposition then this proposition is not true.)

Also, it is generally thought to be part of the repertoire of the ideal speaker to determine whether or not an expression has a semantic value. If so, then using partial functions will not recover what is required by the psychological orientation. It looks as if under reasonable constraints on the size of the set of sentences that should receive a semantic value, the domain of the function relating a sentence to the proposition that it expresses will have to be very undecidable. In this connection the following result (communicated to me from John Burgess, via Anil Gupta) is suggestive. Let E be a partial assignment of truth values to the sentences of a first-order language containing Robinson's **Q** and suppose that E interprets a syntactic predicate E of the language. Then if E is consistent and its domain is closed under three-valued logical consequence and contains every Kripke-grounded sentence (see Kripke [10]) of the language, this domain is at least hyperarithmetical.

I put this last bit in italics because I believe this is the critical assumption. Among the hypotheses we have made, it is the one that I would consider to be false. The following two remarks may help to clarify the role of this assumption. First, it is a consequence of the psychological ideal; it is part of the explanation of how Karl's semantic knowledge is stored. The assumption is not true of model theoretic sematnic theories. For extensional theories, where propositions would simply be truth values, expressing will relate a sentence to its truth value. For arithmetical languages, this relation is not even arithmetical, much less recursive. In nonextensional theories, the assumption would continue to fail in cases where the theory of propositions has any plausibility. It fails, for instance, in the case of the possible worlds theory.[11] Take a version of such a logic in which you can also talk about sets of such worlds (not necessarily all such sets, but a family of them) and membership in these sets. Assume that our language is also adequate for arithmetic. If the relation of expressing were expressible in this language by a predicate E, then $\lambda x[F(x) \wedge (y)[W(y) \rightarrow y \in x]]$ will satisfy conditions (10) to (13) below, where F expresses the property of being a formula, W the property of being a world, $\in$ the relation of set memberspip, and '$\vdash$' is constued as truth. It follows that there can be no such predicate E. So in this case, expressing is not even arithmetical.

Now all this can be reflected in Karl's theory. We can Gödel number linguistic expressions and propositions so that syntactic predicates will appear as predicates of Gödel numbers of formulas, and predicates pertaining to semantic structure as predicates of Gödel numbers of propositions. Expressing will then show up in Karl's language as a two place arithmetical predicate, E. Since this was a recursive function, Karl can prove certain things about it. In particular, the following holds.

(1) For each i, if i is the Gödel number of a formula of Karl's language and j the Gödel number of the proposition that i expresses, then $(x)[E\overline{i}, x) \leftrightarrow x = \overline{j}]$.

It is in (1) that the psychological ideal shows up, in that here we have used the recursiveness of expressing in Karl's language: (1) is going to get us into trouble.

Now, besides being objects of propositional attitudes, propositions are traditionally taken to be bearers of truth. Thus, if Karl's language has a truth predicate like that of 'It is true that snow is white' (an indirect discourse predicate, not a predicate of sentences), it is natural to suppose that this sense of 'true' functions in Karl's language as a property of propositions. Without losing any generality, we can substitute the corresponding property of Gödel numbers. Let T express this property in karl's language. We then will have the following in Karl's theory, assuming that Karl is able to prove the minimum you would expect about indirect discourse truth.

(2) For all i, if i is the Gödel number of ϕ then, for all j, $\vdash E(\overline{i}, \overline{j}) \rightarrow [T(\overline{j}) \rightarrow \phi]$.

The next bit is a digression, designed for readers who look at (2) and say "Convention **T**—of course there will be a paradox!"

[11]There are versions of modal logic in which quantifiers over possible worlds are available; see, for instance, Hazen [4].

It shouldn't be thought that the paradox of the Liar has been smuggled aboard (2). Truth as a property of propositions is not paradoxical. Frank Ramsey's propositional theory of truth in [17], for instance, has been criticized in various ways—but not as being inconsistent. Maybe it will help to clarify things if we let T_ϕ represent the formula in the language ascribing truth to the proposition expressed by ϕ. This notation says nothing about logical form; it assumes only that ϕ appears as a syntactic part of T_ϕ. Now, (3) is trivial.

(3) $\vdash T_\phi \leftrightarrow \phi$

On Ramsey's theory, for instance, this would amount to no more than $\phi \leftrightarrow \phi$. The hypothesis that truth is a property of propositions is less trivial; it comes to (4) in the present context, where ϕ expresses the proposition with Gödel number $\overline{m}$.

(4) $\vdash T_\phi \leftrightarrow T(\overline{m})$

Still, it leads to no inconsistency; both (3) and (4) can be satisfied in models of first order arithmetic, with propositions construed simply as truth values.

Our task now is to derive a contradiction from (1) and (2). First, we note that there is a three place predicate D in Karl's language for which the following holds.[12]

(5) For all i, j, and k, if i is the Gödel number of a formula and k is the Gödel number of the result of replacing all occurrences of the variable x in this formula by $\overline{j}$, then $\vdash (x)[D(\overline{i}, \overline{j}, x) \leftrightarrow x = \overline{k}]$.

Now consider the following formula—which, say, has Gödel number n.

(6) $(\exists y)[D(x, x, y) \wedge (\exists z)[E(y, z) \wedge \neg T(z)]]$

Let ψ be the result of substituting n for x in (6); this formula, say has Gödel number m. Using (5), we see that $\vdash (x)[D(\overline{n}, \overline{n}, x) \leftrightarrow x = \overline{m}]$. This and first order logic yield

(7) $\vdash \psi \leftrightarrow (\exists z)[E(\overline{m}, z) \wedge \neg T(z)]$

Now, let ψ express the proposition p. where p has Gödel number h, we then have $(x)\ [E(\overline{m}, x) \leftrightarrow x = \overline{h}]$; so from (7) we have

(8) $\vdash \psi \leftrightarrow \neg T(\overline{h})$.

But since $E(\overline{m}, \overline{h})$, in view of (2) it follows that $\vdash T(\overline{h} \leftrightarrow \psi$. This and (8) lead to a contradiction in Karl's theory:

(9) $\vdash T(\overline{h}) \leftrightarrow vnotT(\overline{h}).$

[12]There will be such a predicate, since what is represented is clearly a recursive function.

So commitment ot a recursive semantic theory, through the resulting collapse of the semantic metalanguage into the object language, makes it impossible for us to think of its propositions as truth bearers. If such a theory relates sentences to thoughts, the thoughts cannot be true and false.

If this difficulty could be confined to truth, perhaps we could live with this conclusion and banish truth from psychological semantics. But generalizations of Tarski's theorem on truth erode this alternative. The results of Montague [14] show that the above difficulty with truth also infects the capacity of such semantic theories to deal with certainty,[13] triviality, probability, and idealized knowledge. Where $Gn(\phi)$ is the Gödel number of ϕ, Montague showed that there can be no consistent language containing an adequate numberical theory, and meeting also the following conditions for all formulas ϕ and psi, where L is any predicate.

(10) $\vdash L(\overline{Gn(\phi)})$, if ϕ is an axiom of first order logic

(11) If $\vdash L(\overline{Gn(\phi\psi)})$ and $\vdash L(\overline{Gn(\phi)})$, then $\vdash L(\overline{Gn(\psi)})$

(12) $\vdash L(\overline{Gn(\phi)}) \to \phi$

(13) $\vdash L(\overline{Gn(L(\overline{Gn(\phi)}) \to \phi)})$

Since we are dealing with a theory that is motivated by psychological considerations, let's illustrate the point with constructions like 'Karl knows that snow is white'. On the assumption that these involve a relation between a person and a proposition, and suppressing the first of these relata, we have a predicate N which holds of i in case i is the Gö number of a proposition that Karl knows. Using the two place predicate E, we can convert N into a predicate N^* that applies to Gödel numbers of sentences. The following condition amounts simply to the definition in Karl's theory of N^*

(14) $\vdash (x)[N*(x) \leftrightarrow (\exists y[(x,y) \wedge N(y)]]$

Now, if Karl is logically omniscient, N^* will meet conditions (10) to (13)/ Condition (10), for instance, merely requires that Karl know any proposition that is expressible by means of an axiom of ifrst order logic (an axiom with respect to your favorite axiomatization). It follows from Montague's result that N can't, on pain of inconsistency, be treated as a predicate of propositions.

This cuts closer to the bone of the psychological orientation. But perhaps a psychologically motivated theory could stick to belief, making no attempt to account for truth or knowledge. Even this line of retreat, however, is cut off ty Tarski-style arguments, as is shown by the following variation on Montague's theorem.

Consider a theory that has a version **Q** of Robinson's arithmetic as a subtheory, and that meets, as well as (10), (11), and (13), the following condition, for all formulas ϕ.

(15) $\vdash N(\overline{Gn(N(\overline{Gn(\phi)}))}) \to N(\overline{Gn(\phi)})$

[13]As in 'It is certain that pigs can fly', not as in 'I am certain that pigs can fly'.

Let α be a single axiom for **Q**. Then for all formulas ϕ, we have $\vdash N(\overline{Gn(\alpha)}) \rightarrow N(\overline{Gn(\phi)})$.

This result extends the argument to idealize belief, showing that any speaker who develops a theory of the kind we have been considering will be able to show that if he believes a fairly minimal amount of mathematics he then believes a contradiction.

4 The force of the argument

The impact of this argument may seem diminished by the fact that real people are not logically omniscient. But remember, we are arguing against modern theories of competence. Of course real people aren't logically omniscient. But the ideal speakers of these theories are.

Notice that it is natural to think of logical omniscience as an ideal case that is conceptually simpler than the real case. Early work in epistemic logic, like Hintikka [5], adopted logical omniscience as a simplifying assumption. Although we may well expect such ideal cases to be fictional in some respects, we at least don't expect them to tell a story that is internally incoherent. In fact, what makes them *idealizations* is that the story they tell us is more palpable intelligible than one that tries to be realistic. And the above argument does not merely end with the conclusion that a logically omniscient being doesn't exist *as a matter of fact*; the end result is that the nonexistance of such a being would be provable in semantic theory.

And this is just the sort of idealization to which the versions of the psychological approach we are considering are committed. The idea is that a viable level of psychological explanation will concern itself with ideal automata, free from mechanical error and built-in vagueness, and without a fixed upper bound on memory. It is at just such a level of "competence" that logical omniscience becomes possible. For this reason, it seems fair to conclude that the motivations underlying competence theories of language become at least partially incoherent when applied to semantics in a setting that assumes classical logic.[14]

Can someone who accepts the psychological ideal, and who also wants a semantic theory, afford to ignore these considerations? Are they entirely external to the development of such a theory?

Maybe partly external in the case of truth, since psychological considerations do not seem to force us to give truth a central role in semantics. Nevertheless, sentences like 'it is true that snow is white' (indirect discourse constructions, remember) do arise, and the problem of accounting for them will be internal to any semantic theory committed to studying languages in which they are found. Since these theories will assign a semantic representation to a sentence, which can be thought of as the meaning or the (idealized) thought associated with the sentence, it is at least an anomaly that the theory cannot say that these are true and false. And it makes the interpretation of sentences like 'It is true that snow is white' problematic. Perhaps such sentences could be paraphrased away, but (a) it is well known that this is no trivial task, in view of examples like 'What the policeman said is true' and (b) the resulting rift between 'that' complements of 'true' and those of verbs like 'seem' will

[14]It is not clear that the motivations can be maintained by manipulating the logic, though work such as [18] suggests that it might be made coherent in an intuitionist setting.

complicate the syntax, or the relationship between the syntax and its interpretation, in ways that would probably be very awkard.

The problems of knowledge and belief hit harder. since logically omniscient speakers like Karl fall within the scope of modern psychological theories of semantic competence, it is hard to see how such theories can be successful on their own terms. Their propositions can be objects of neither knowledge nor of belief. An something will have gone wrong with the rationale of semantic compositionality. This rationale (which is part of a story intrinsic to the psychological ideal, explaining our semantic competence) represents complex meanings as functions of simpler ones. We then use the theory, for example, to account for why Karl says ϕ in response to prompting by a 'yes-no' question: part of the explanation is that ϕ expresses a certain proposition. It is part of Karl's semantic repertoire to know this. And Karl believes this proposition. Such accounts are not available if the objects of belief differ from the semantic values of sentences. But unfortunately our argument shows that the semantic representation of 'Snow is white' cannot play this role in determining that of 'Karl knows that snow is white', or of 'karl believes that snow is white'.

The conclusion seems to be that if the psychological ideal has these sorts of limitations built into it, then the motivation that has been given for this ideal needs to be radically rethought. We need to have a better and more modest conception of what sorts of explanation a theory of this sort can be expected to provide.

Such a conception would invite—perhaps even require—another kind of semantic theory, that will do justice to our intuitions relating to truth, propositional attitudes, and compositionality. My own feeling is that this is the kind of theory on which logicians (for the most part) have been working all along.

The relation between these two semantic theories would, I'm afraid, be a philosophical problem. maybe if we could get clearer about what specific forms the theories should take in the case of natural languages we would be able to recast this problem in technical terms.

In a way, this may shake the foundations of cognitive psychology, but it would be wrong for cognitive psychologists to be disconcerted about this. Genuine sciences are self-supporting, and are not in any danger of crumbling if their foundations fail inspection by a term of philosophers. It almost seems as if the way for a science to gain respectability is for its foundations to seem problematic to philosophers. The arguments of Section 3 also have a technical dimension—they suggest, for instance, that a psychologically oriented semantics will have difficulties with quantifiers. But so little work has been done along these lines that hardly anything in the existing literature is actually affected.

5 Knowing a language

Perhaps some will notice that the conception of semantic representation I argue for here (and in general, the model theoretic approach to semantic representation) suggests that speakers of a language do not know their language.

I do feel that more needs to be said about what it is to know a language, and about the competence/performance distinction in cognitive psychology. One consequence of what I've said is that these matters need to be rethought, perhaps in radical ways. But I want to repeat, in conclusion, that I feel that these are philosophical issues, whose uncertainty does not threaten any of the substantive achievements of generative grammar or cognitive psychology.

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