

# Cognitive Science and the Analytic/Synthetic Distinction: Comments on Horwich<sup>1</sup>

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1. Quine is usually read as arguing either for a *non-factualism* about analyticity:

- (1) The property of being true by virtue of meaning alone does not make coherent sense.

Or, at the very least, for an *error* thesis about it:

- (2) Although the idea of an analytic sentence does make some sort of sense, there aren't and couldn't be, any instances of it.

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<sup>1</sup>The present paper is a comment on Paul Horwich's "Chomsky and Quine on the Analytic-Synthetic Distinction", *Proceedings of the Aristotelian Society*, 1991. It is that paper that Horwich read at the meeting in Salamanca and on which I commented. Unfortunately, it proved impossible to publish that paper in these proceedings; what appears instead is a closely related companion piece. My comments remain largely unchanged.

These attributions—including the stronger non-factualist thesis— seem licensed by many passages, including the famous one which concludes Quine's discussion in "Two Dogmas":

...for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith. (p. 37)

2. Nevertheless, Paul Horwich does not wish to read Quine as endorsing either (1) or (2). He certainly does not wish to attribute (1) to him. And he wishes to attribute only a *restricted* version of (2). What this restriction is and why Paul wants to make it are matters I'll come to in just a moment.

3. Before going on, however, I wish to point out that it strikes me as a *very good thing* for a philosopher to distance himself from claims (1) and (2), especially if he is not simultaneously prepared to subscribe to *meaning-indeterminacy*—i.e. to the view that there are no determinate facts about what linguistic expressions mean—and to its natural accompaniment, *meaning-eliminativism*.

Quine, famously, traveled the road from a denial of the analytic/synthetic distinction (in "Two Dogmas") to belief in meaning-indeterminacy and ultimately meaning-eliminativism (in *Word and Object*).

One of the most puzzling aspects of the contemporary reception of Quine, however, has been that although philosophers have gone along overwhelmingly with the denial of the analytic/synthetic distinction, they have not necessarily been willing to follow Quine to meaning indeterminacy and meaning-eliminativism. That is, they have thought of the denial of a/s as an *insight* into determinate meaning facts, rather than a rejection of them.

However, this strikes me as an impossible combination of views. Let me briefly indicate why.<sup>2</sup>

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<sup>2</sup>The argument here is spelled out in detail in my "Analyticity", forthcoming in Hale and Wright (eds.): *Blackwell's Companion to the Philosophy of Language*.

Suppose meaning facts are determinate, so that there is an objective, determinate fact about what expressions mean. To make things vivid, imagine that the set of all possible concepts is arrayed neatly in Plato's heaven:  $C_1, C_2, C_3, \dots$  etc. . . . And that for any expression, 'A', 'B' etc. . . ., there is a fact of the matter about which of these various concepts it expresses. (This picture does not, of course, rule out *vague* concepts.)

Then, how could there fail to be a fact of the matter about whether any two expressions do or do not mean the same? By hypothesis, there is a fact about what concept 'A' picks out and what concept 'B' picks out.

Now, if they do pick out the same concept, then (assuming 'A' and 'B' are of the appropriate grammatical category)  $A = B$  will express an analytic sentence.

Or, if you think for some or other reason that we can't use the identity sign as we actually have it for these purposes, we can imagine that there might have been a connective % whose meaning was precisely as follows: Sentences of the form  $A \% B$  are true if and only if 'A' and 'B' have the same meaning, false otherwise. Given that we are operating on the assumption of determinate meaning facts, we are entitled to assume that there might have been such a connective. Similarly, we are entitled to assume that there might have been synonyms. The upshot is that, if meaning realism is true, I don't see how either of Quine's skeptical theses about analyticity could turn out to be true.

4. So, my problem with Quine's thesis (2) —I shall henceforth concentrate only on this weaker and hence more plausible thesis— is that it leads to the very radical doctrine of *global indeterminacy*. And I don't think either that global indeterminacy is true or that Quine's arguments for it are very compelling.

Paul has a different, if related, worry about (2) —the view that there couldn't be sentences that are true or false purely by virtue of their meaning. He doesn't want to attribute an unrestricted acceptance of (2) to Quine because he believes that (2) is in fact denied by contemporary empirical theories of linguistics and psychology. So Paul sets himself the task of answering the question:

What thesis should we attribute to Quine which would both preserve the spirit of Quine's critique of a/s and which would not be falsified by cognitive science?

The answer he proposes is interesting and elegant. The main idea, as I understand it, is this: The sentences we assert have *two kinds of meaning*. On the one hand, they have the sort of meaning that we attribute to them ordinarily, in our normal folk-psychological idiom. According to Paul, this sort of meaning—he calls it *public language meaning*—is a *purely behavioral* property. Attributions of it presuppose nothing about the mind or brain which underlies linguistic activity. (p. 2)

On the other hand, they have the sort of meaning that is attributed to them in cognitive science—what Paul calls *I-language meaning*. And, according to Paul, this sort of meaning isn't a purely behavioral property: attributions of it may, typically will, take into account facts about the cognitive architecture and organization of the mind/brain which underlies linguistic activity.

With this distinction in place, Paul argues as follows:

- (3) There can be no analytic sentences defined over public language meanings.
- (4) There *can* be analytic sentences defined over I-language meanings.
- (5) Only the sort of analyticity defined over public-language meaning can be put to the sort of epistemological use that Quine found objectionable. Hence, the fact that (2) is not true *simpliciter* doesn't affect any real interest of Quine's.

5. There's a lot to discuss here. In this space, I will only have time to concentrate on the first and most important claim—that the ordinary folk-psychological notion of meaning won't support the existence of sentences that are true or false purely by virtue of their meanings. What is the argument for this claim?

The basic idea seems to be this: Public-language meanings are purely behaviorally constituted. But the totality of

the behavioral facts involving a person's use of a word don't settle what he means by that word, what rule of use is governing his use of the word. Hence, if that person's use of the word *changes* (think here of Paul's example involving the sentence 'Distinct straight lines don't intersect') then there won't necessarily be a fact of the matter as to whether this change in use reflects a change in the meaning of the word — a change in the rule governing its use— or whether it merely reflects a change in some *empirical belief*. Hence, there isn't a distinction between analytic and synthetic sentences.

In short: the argument is that behaviorally constituted meanings are *indeterminate*; hence, since public-language meanings are purely behaviorally constituted, they cannot support objective synonymies or analyticities (see p. 6).

Notice, first, that this reasoning doesn't flout the claim I was outlining above, that a denial of the a/s isn't consistent with meaning determinacy. On the contrary, the failure of an a/s distinction is traced precisely to an alleged failure of determinacy. However, Paul is not very expansive about the grounds for believing the major premise—that behaviorally constituted meanings are indeterminate. The argument here (pp. 5-6) goes by very quickly. Essentially two points are made.

The first is that since confirmation is holistic—since sentences don't have individual verification conditions in isolation from the overall theory in which they are embedded—a *verificationist* about meaning won't be able to characterize the notions of synonymy and analyticity.

One problem with this point is that even if it were true it would apply only to a verificationist theory of meaning, and that is not a particularly plausible view. The second problem is that it doesn't seem to me to be true in any case. A verificationist could say that *all* of the inferential liaisons postulated by a theory for a sentence are constitutive of its meaning, so that *any* change in the theory would lead to changes in the meanings of all the constituent expressions. This may not be a very plausible analytical-synthetic distinction, but plausibility wasn't the point at issue. If it were, we wouldn't be discussing verificationism in the first place.

Paul's second point is that Quine's web-of-belief model eschews semantical notions like analyticity, thus suggesting that they play no explanatory role in understanding the evolution of science.

I must say that I don't really see what's going on here. To the extent that the web-of-belief model is understood as already *incorporating* the idea that nothing is true by virtue of meaning alone, then it already *presupposes* the denial of the a/s distinction and so cannot be viewed as an argument for it.

So, I don't really see that we've been given a good argument for the claim that behaviorally constituted meanings are semantically indeterminate.

6. However, even granting this point, it seems to me that there is very little reason to believe the minor premise of Paul's argument —namely, that the meanings attributed in folk psychology are purely behaviorally constituted properties, properties whose attribution presupposes nothing about the character and nature of the mind/brain that underlies linguistic activity.

There are many things that could be said here, but let me confine myself to this. Given the singular lack of success of behaviorist accounts of our other folk-psychological concepts, it would be very surprising indeed if behaviorism were true about our folk-psychological concept of *meaning*. At any rate, the *argument* for the claim that it is such a notion would have to be very strong, if it is to overcome an understandable resistance.

Paul's argument for the claim that ordinary public language meaning is of a familiar type; it can be found in the writings of Dummett, and others in the Wittgenteinian tradition.

The idea is that meanings (and other folk psychological notions) must be notions of behavioral properties, else how are we to explain the fact that we so confidently infer what they are from the mere observation of a person's behavior.

The possession of these [meaning properties]... is confidently inferred from everyday behavior. We observe the manner in which Mary uses words and the manner in which she responds

to the use of words by others. On this basis, without any esoteric source of information about her mind or brain, we unhesitatingly conclude that she [speaks] English. (p. 2)

I have two difficulties with this sort of argument. In the first place, I think that it is possible to give a satisfactory account of the epistemology of other minds, one that would explain the confidence and ease with which we attribute folk-psychological properties, even if those properties are construed mentalistically.

Although this is obviously not the time to go into this in detail, and although it could benefit enormously from a more refined treatment than it has hitherto received in the literature, the rough idea is familiar: We justifiably infer to the properties of another person's mind from the observations of their behavior and by analogy from our own case.

In the second place, what arguments like Paul's tend to underestimate, it seems to me, is the extent to which behavioral properties are not *themselves* easily observable properties. In particular, those of my behavioral properties that are the only remotely plausible candidates for constituting what I mean are not easily observable.

The point is that there is no way to reconstruct what I now mean by an expression from *my actual behavior with it right now*. That behavior, as Kripke's famous discussion of the concept of addition vividly shows, is consistent with my following an infinite number of rules.<sup>3</sup>

Hence, at a minimum, the behavioral properties that are to constitute meanings, on a behavioral view, must be highly dispositional, counterfactual and idealized.

Now we may ask: How much do you really know by *mere observation*, about a person's behavioral dispositions, with respect to expressions?

The answer is very little. Think again here of Kripke's famous discussion of '+'. Any bit of my behavior that you may have seen conforms to a large, indeed infinite, number of

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<sup>3</sup>See Saul Kripke: *Wittgenstein on Rules and Private Language*, Harvard University Press, 1982.

dispositions. Which disposition I have is clearly not a merely observational matter.

And what this shows is that even a behaviorist, by his own lights, will have to use the sort of analogically mediated inference that I described the mentalist as exploiting, if he the behaviorist is to have an account of the epistemology of other minds.

But, then, the main motive behind being Behaviorist about public-language meaning disappears. There no longer remains any real reason for thinking that public-language meaning is behaviorist. And, hence, so far as any argument that either Quine or Paul has given us, there remains no reason to think that public-language meaning is indeterminate and, hence, no reason to think that it won't support objective synonymies or analyticities.

7. If what I've been saying is right, then the way is paved for a different and, I think, more satisfying picture of the relation between scientific investigation of meaning and the ordinary notion we attribute in common sense.

For Paul, it is crucial to claim that the notion of meaning studied in cognitive science is really a *different* notion from the one we ordinarily talk about, a change of subject matter. I think, on the contrary, however, that anyone not antecedently committed to a behaviorism about the ordinary notion would concede that it is heavily implicated in assumptions about the character and nature of the mental apparatus that underlies linguistic activity, assumptions that it is in part the proper business of cognitive science to investigate, refine and fill in.

In sum: I think Paul's attempt to reconcile Quine's rejection of a/s with the determinacy of meaning postulated by cognitive science fails. I think that Paul is right to think that Quine's rejection of a/s and his belief in indeterminacy rest largely on his behaviorism about the ordinary notion of meaning. But I think that behaviorism is wrong. I think the ordinary notion is mentalistic, determinate, supports an analytic/synthetic distinction and is much more smoothly related to the empirical study of meaning than Paul's picture allows.