

HOLOPHOBIA

Elisabeth Pacherie

Institut Jean Nicod
CNRS-ENS-EHESS, Paris
pacherie@ehess.fr

A few years ago, Jerry Fodor (1988) complained about the epiphobia which seemed to be spreading among philosophers of mind. My aim in this paper is not to comment on the relevance of this diagnosis but to investigate another phobia, holophobia, the most illustrious victim of which appears to be Fodor himself. Holophobia can, be defined as the "neurotic" fear that semantic holism, if not instantly extirpated by the most radical means, could well be a deadly threat to intentional realism. Fodor's fear has deep roots but the first explicit symptoms of that neurosis appear in his book, *Psychosemantics* (1987), where meaning holism is pilloried, and functional role theories of narrow content, considered irremediably contaminated, are abandoned in favor of an indexical or "no content" account of narrow content. The same worries are at the heart of several of the papers collected in Fodor's *A theory of Content* (1990), and the symptoms culminate in Fodor & Lepore's *Holism A Shopper's Guide* (1992) where a death struggle is engaged with holism.

My opinion is that Fodor overestimates the threat that meaning holism poses to intentional realism and to a viable account of narrow content. More precisely, I think that he overestimates the difficulties of confining meaning holism within reasonable bounds and that his exaggeration of the difficulties of such a task goes together with an overestimation of the relevance for intentional psychology of Quine's demonstration that a substantial analytic/synthetic distinction is out of reach.

Taking a course opposite to Fodor's, I shall argue that all that is needed to defeat the more radical varieties of meaning holism is a weaker distinction than the full-blown and allegedly inexistent analytic/synthetic distinction. What we need is a distinction between what we could call, if the terms were not misleading, the psychoanalytic and the psychosynthetic, and what I shall call, less whimsically, the quasi-analytic and the quasi-synthetic, - namely, a distinction between what is treated *as if* it were analytic by a cognitive system and what is treated *as if* it were synthetic.

I shall further argue that making an appropriate use of this distinction leaves us with a moderate and tractable brand of meaning holism which can be considered fairly innocuous from the point of view of intentional realism. Last but not least, we are not forced anymore to the desperate moves Fodor was advocating: functional role theories don't have to be sacrificed on prophylactic grounds and we can preserve a substantial notion of narrow content defined in terms of tamed functional roles.

According to Fodor, arguments in favor of semantic holism are usually nothing but elaborations of an "Ur-argument" the general form of which is the following:

Step 1: Argue that *at least some* of the epistemic liaisons of a belief determine its intentional content.

Step 2 Run a 'slippery slope' argument to show that there is no principled way of deciding *which* of the epistemic liaisons of a belief determine its intentional content. So either none does or they all do.

Step 3. Conclude that they all do (1, 2: modus tollens) (Fodor, 1987, p. 60)

Notice that the conclusion, if firmly established, does indeed pose a threat to a substantive intentional realism. The threat is not that mental states would be denied content; it is rather that a holistic account of the individuation of intentional states sets no limit to the diversity of contents. Fodor's main worry can be summarized as follows: if we are to individuate people's intentional states by the totality of their epistemic liaisons and, if, as is empirically quite probable, no two people are exactly alike in terms of the epistemic liaisons of their propositional attitudes, it will turn out that no two people will ever be in the same intentional state. Thus, there will be no point in trying to contrive intentional generalizations, since these generalizations will not in fact

find any application. This would be the end of a substantive intentional psychology the aim of which was precisely to enlist intentional generalizations in order to explain people's behavior.

Now, let us grant that the Ur-argument seems *prima facie* formally sound. So, if the intentional realist wants to avoid the distressing conclusion, he is faced with two options. Either he can reject the first premise and deny that any of the epistemic liaisons of an attitude are relevant to the determination of its intentional content, or he can dispute the second premise: here, the best way to proceed would be to produce some criterion allowing us to decide which epistemic liaisons of an attitude might be semantically relevant.

In *Psychosemantics* (1987), Fodor gives only a short try at the second option and, considering it unsuccessful, rapidly settles for a purely denotational semantics. Thus, he relegates epistemic liaisons to the limbo of semantically irrelevant facts.

The advisability of such a rash move appears questionable. As several commentators have pointed out, and as Fodor himself in his more lucid moments concedes, there is some explanatory work that epistemic liaisons, or more classically functional roles, were purported to do and that a denotational semantics alone definitely can't achieve. The Oedipus' case is exemplary and is well-known enough to need no further rehearsal. In his paper "Substitution Arguments and The Individuation of Beliefs" (1989), Fodor has tackled the problem and has tried patching the leaking story by giving an account of propositional attitudes as four-place relations. Whatever the intrinsic merits of the attempt, one cannot help feeling puzzled at the thought that such an account is provided by the hitherto most unrelenting advocate of strong intentional realism. Whereas Oedipus' deportment seems to be the perfect case of a behavior that is calling for an explanation adverting to intentional generalizations, we are told that the explanation should be phrased in terms of differences of vehicles and that, in the matter at hand, contents are causally inert!

Given the problematic outcome of Fodor's chosen move, one wonders whether it would not be worth one's while to try harder to force one's way through the other

open option. Was not Fodor showing a premature and unusual defeatism when granting the second premise of the Ur-argument after scarcely a skirmish or two? Did he have compelling reasons for so quickly throwing the towel in? Well, let us simply say he thought he had.

According to Fodor (1987), although confirmation holism does not by itself imply holism about meaning, confirmation holism, taken together with the further premise that at least some of the epistemic liaisons of an attitude are determinants of its intentional content, inescapably leads to the dreadful conclusion. As Fodor construes it, Quine's argument in favor of confirmation holism is based on the rejection of semantic localism. Semantic localism presupposes that strictly semantic implications can be distinguished from merely empirical ones. But Quine has shown the impossibility of the analytic/synthetic distinction needed by the semantic localist. Now, the rejection of semantic localism together with the acceptance of confirmation holism does not imply semantic holism. At least two other options are open: meaning nihilism and purely denotational theories of meaning. If, however, you cling to the idea that epistemic liaisons must somehow be semantically relevant, meaning holism is your fate.

Briefly put, if you consider that at least some epistemic liaisons are determinants of content, it is useless to try to find a way of deciding which epistemic liaisons are semantically relevant because what you would need to effect such a sorting out is precisely the analytic/synthetic distinction which Quine has shown was not available.

But, to my mind, such an answer won't do. For even if one agrees that Quine has convincingly shown the impossibility of a substantial analytic/synthetic distinction, it would seem that what we need here is not a full-blown analytic/synthetic distinction, but a distinction between what is treated as if it were analytic by cognitive systems and what is treated as if it were synthetic. In other words, insofar as the notion of "epistemic liaison" we are interested in is really, as Fodor acknowledges, "a *psychological* notion, not an *epistemological* one" (1987, p. 56), it seems that what we need in order to discriminate among epistemic liaisons also has to be a psychological distinction, and

not an epistemological one. Thus, it seems that, for lack of a distinction between the analytic and the synthetic *per se*, a distinction between the quasi-analytic (in the sense of treated as analytic by a cognitive system) and the quasi-synthetic would meet our needs.

So here comes a new question: what is preventing Fodor from considering this alternative distinction? That the confirmation of scientific theories be inherently holistic is one thing, but that the same be true of other types of knowledge or theories is quite another matter. It is not obvious, *pace* Quine (1953, p. 79), that "science is a continuation of common sense" and that by way of consequence, scientific confirmation inherits its holistic characteristics from common sense. This is however an idea to which Fodor readily subscribes, telling us that "if holism seems a plausible account of how the scientific community achieves the semantic evaluation of its theories, it also looks not bad as a psychology of how individuals achieve the fixation of their beliefs" (1987, p. 63). In other words, according to Fodor, belief fixation shares its main characteristics with scientific confirmation. Quineanism and isotropy are the hallmarks of both enterprises and the attempt to draw a distinction between the quasi-analytic and the quasi-synthetic is doomed to failure for the same reasons that the attempts made in favor of the analytic/synthetic distinction were destined to fail.

Fodor's dismissal of the possibility of the psychological distinction that I am advocating thus depends on the analogy he draws between scientific confirmation and belief fixation. But is this analogy a sound one? It seems to me largely misguided. Indeed, it presupposes that the rationality involved in scientific dealings is of the same type as the rationality involved in the individual's ordinary psychology and behavior. Quine's picture of science is that of a project guided by an ideal of absolute rationality. The scientific enterprise is a long-term collective enterprise which seeks to free itself from the limitations of individual rationality. Whatever the accuracy of such a picture, it is not one that easily lends itself to an analogy with belief fixation. The rationality manifested in our everyday conduct and actions is in essence a limited rationality. As Proust (1991) has pointed out, Fodor misses completely the pragmatic dimension of

rationality. One of the main characteristics of the rational behavior of a finite being is to develop a strategy of belief-fixation and decision-making which takes into account consideration of available cognitive resources, constraints on the task (including time constraints) as well as hierarchies of goals.

Thus it seems that Fodor largely overestimated the relevance of the parallel between scientific confirmation and belief fixation. One of the most puzzling consequences of embracing this analogy is the claim made by Fodor that the frame-problem is insolvable. The short way to undermine the analogy is simply to point out that, contrary to computers, humans usually do not have the frame-problem. Consideration of this fact and of what it might involve concerning human rationality makes it all the more surprising that Fodor seems not even to have looked at possible relevant data in clinical neuropsychology, in experimental psychology, and in anthropology.

Current psychological research, and especially work on domain-specificity, strongly suggests that domain-specificity is not a property true only of peripheral systems. This characteristic is shared also by more central cognitive systems. There is now growing evidence in favor of the existence of innate constraints on categorization and inductive processes. Moreover, the available evidence suggests that to different semantic domains correspond different sets of constraints and that those sets determine the conceptual organization of the domains to which they apply. I wish to explore the idea that the role played by an epistemic liaison in the determination of the intentional content of an attitude is a function of the relation this liaison sustains with innate constraints on categorization. The closer the association of an epistemic liaison with those constraints, the stronger its semantic significance.

More precisely, it seems that by exploiting the available data on domain-specificity, we should be able to distinguish among three types of epistemic liaisons: first, the quasi-analytic liaisons which are semantically relevant; second, those quasi-synthetic that also are semantically relevant; and third, the quasi-synthetic liaisons that are merely encyclopaedic and have no direct semantic relevance. To take an example,

one might have a certain set of beliefs about tigers, for instance, that tigers are animals, that they are striped, and that the tiger in the London zoo suffers from rheumatism. Intuitively, we would want the first of these beliefs to be classified as quasi-analytic, the second as quasi-synthetic but semantically relevant, and the third as simply encyclopaedic.

The distinction between the quasi-analytic and the quasi-synthetic was in a way adumbrated by Putnam in his famous paper, "The Meaning of Meaning" (1975). In this paper, he acknowledges the special status of certain semantic features and introduces a qualitative distinction between what he calls "semantic markers", on the one hand, and features which are simply typical and belong to the stereotype, on the other hand. Thus, in the case of "tiger", a feature like "animal" is considered a semantic marker, whereas a feature like "striped" is simply typical. It happens that the features Putnam classifies as semantic markers correspond to the ontological categories towards which we have, according to psychologists Frank Keil (1979, 1983, 1986) and Rochel Gelman (1990) an innate cognitive bias.

Faithful to Quine however, Putnam contends that this should not be taken to mean that the statement "all tigers are animals" is a logical truth but simply that this statement is more central, and thus harder to revise than the statement "all tigers are striped". However, Putnam's analysis seems to me to be insufficient. What distinguishes the two statements is not simply that one is harder to revise than the other, it is also that they are not liable to the same type of revision. One way to revise the statement "all tigers are striped" is to replace it by "most tigers are striped", but this will not do for "all tigers are animal". If we were to discover that some (and only some) of the beings we had hitherto called tigers were in fact robots, we would not say that our original statement must be revised and replaced by "most tigers are animals". Rather, we would say that some of the beings we took for tigers were in fact not tigers. It is only if we were to discover that all tigers were robots that we would revise our statement. The way to revise it would then be to say that "no tiger is an animal". Thus, we have a criterion for quasi-analyticity: a statement of the form "all As are Bs" is

quasi-analytic if and only if the only way to revise it is to replace it by "no A is a B". We can see that this criterion is psychological and not epistemological since quasi-analyticity is strongly tied to our innate cognitive biases

Given that we have a criterion for quasi-analyticity, our last problem is to distinguish among the remaining epistemic liaisons (the quasi-synthetic ones) those that are semantically relevant and those that are merely encyclopaedic and of no direct semantic relevance. Once again, we should lend an ear to the suggestions of psychologists. Of special interest for the present problem is the proposal put forth by Gelman:

I propose that implicit domain-specific principles specify the core of many of the concepts and categories with which young children learn to sort the world. Domain-specific principles function to direct attention to the objects, events, or attributes that are relevant exemplars. The exemplars in turn feed general processing abilities, including the ability to extract from stored information the predictive validity of the characteristics of the items assimilated to a domain" (Gelman, 1990, p. 90)

The feature of this proposal most interesting to us is that the task devoted to general processing mechanisms is not to extract the defining characteristics of items belonging to a category but simply to extract characteristics having a high cue-validity. In Gelman's model, a clear difference is made between what defines a category - its conceptual core - and what commonly determines which objects are assigned to which categories. Thus, this model acknowledges the distinction between those determinants of content that are quasi-analytic and those that are not. Also, the model accounts for the difference between the epistemic liaisons which are determinants of content and those which are not. Thus, the belief that tigers are striped is semantically relevant because even if being striped is not a necessary condition for belonging to the category of tigers, it is, however, a high-validity cue. On the contrary, my belief that the tiger of the London zoo is rheumatic does not have to be treated as a determinant of content because neither being rheumatic nor being a resident of the London zoo are reliable indicators of tigerness.

As a final point, it should be noted that what we have here is not a sharp distinction between semantically relevant and semantically irrelevant epistemic liaisons, but a graded notion of semantic relevance. Is this a weakness of the position I have been proposing? I think not. It would be a weakness, if we didn't have a solid grasp on the conceptual core of categories, but the criterion of quasi-analiticity gives us this grasp. It seems, on the contrary, that this criterion of quasi-analyticity, together with a graded notion of semantic relevance for non-quasi-analytic epistemic liaisons, leads to a view of intentional content which makes it possible to conjoin stability and flexibility of content. If the crucial question for functional role semantics is the question of what counts as identity and difference of functional role, and if, as Ned Block (1986) is contending, we have, in order to answer this question, "to move to a scientific conception of meaning that does away with the crude dichotomy of same/different meaning in favor of a multidimensional gradient of similarity of meaning" (Block, 1986, p. 629), then the proposal put forth in this paper should be considered as an attempt to move in this direction.

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