

IN DEFENSE OF THE SOCIAL PSYCHOLOGY OF SCIENCE (A REJOINDER TO MARKOVA AND ELEJABARRIETA)*

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What is the relationship between science and philosophy? Do they compete with one another or speak of different worlds? Neither position is acceptable. (Roy Bhaskar, 1979, p. 5)

In a recent issue of this journal, I argued that the theory of social representations, when applied to issues relevant for the psychology of science, leads to the prediction that amateurs will be more likely than scientific experts to believe in the ontological realism of scientific entities and theories (Jost, 1992). This hypothesis was derived from the writings of Moscovici on the diffusion of scientific concepts into common parlance, especially the process of 'objectification' whereby abstract theoretical entities are endowed with real, concrete properties by the mass public (e.g., Moscovici, 1981, 1984). Moscovici & Hewstone (1983) applied the analysis of objectification and social representation specifically to the domain of scientific beliefs, drawing comparisons between the beliefs of amateur and professional scientists. For instance, they observe that:

Certain physicists go so far as to hesitate in their belief in the reality of material phenomena such as 'wavelengths', 'particles', 'fields' and 'black holes'. Representation has a propensity to make qualities and forces correspond to ideas or words -- stated baldly, to give ontological life to something that is no more than a logical, even verbal, 'being' (p. 112).

It seemed to me that there was a parallel here between the belief that would result from this process of objectification and the philosophical position known as ontological realism. More specifically, I argued that a consequence of Moscovici & Hewstone's view is that scientific experts may be expected to believe *less* in the reality of their concepts, which may serve largely heuristic or instrumental purposes for them, than scientific amateurs, who tend to 'objectify' and 'reify' the theoretical concepts of the scientist. In arguing for this connection, however, I fear that my motives may have been misunderstood.

It is a rare intellectual pleasure to have international experts in one's field comment in a serious, challenging, and timely way to one's own work. I am indeed grateful for the thought-provoking critiques of Marková (1992) and Elejabarrieta (1992). In responding to their points, I shall try to sort out misunderstandings of my position (which I think comprise the majority of the issues) from substantive disagreements between us. Both Marková and Elejabarrieta suggest that my proposals either violate logical principles or are otherwise

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incompatible with the aims of the theory of social representations; this is because they think I have committed the 'naturalistic fallacy', which I hope to show is not the case. If disagreements between us persist after this objection is eliminated, they concern the potential role of a field of study to be known as the 'social psychology of science' (cf. Fiske & Shweder, 1986; Fuller, De Mey, Shinn, & Woolgar, 1989; Gholson, Shadish, Niemeier, & Houts, 1989; Kruglanski, 1989). I think that both of my critics are too pessimistic about the opportunities for psychology in general and the theory of social representations in particular regarding fields traditionally reserved for philosophers.

I. DID I COMMIT THE NATURALISTIC FALLACY?

In commenting on my article, both Marková and Elejabarrieta accuse me of committing the "naturalistic fallacy" of conflating philosophical (normative) questions about what 'ought' to be and psychological (descriptive) questions about what 'is'. For example, Marková suggests that I was trying to "reduce ontological issues into epistemological ones" (p. 125), and Elejabarrieta thinks that I was arguing that "if we accept [the theory of social representations] then -- scientifically -- we can not adopt a realist position" (p. 134). They go on to criticize me for overstepping my psychological bounds and drawing ontological conclusions which are either unwarranted or incompatible with the aims of the theory of social representations. I hope now to clarify my original objective, which was not to prescribe philosophical positions but to generate some empirical consequences of the theory of social representations¹. Following Moscovici & Hewstone (1983), I offered some hypotheses about what attitudes and beliefs scientists and laypersons might actually hold, not what they *ought* to hold. My attempt was to formalize and expand on the interesting claims of Moscovici & Hewstone and to tighten the links between the theory of social representations and the social psychology of science. At times Marková and Elejabarrieta simply appear to misunderstand my purpose as philosophical rather than psychological, but at other times they seem to rule out the possibility that the theory of social representations could be used to study the (ontological) beliefs of scientists and amateurs. *A priori* resistance to endeavors of the latter type strikes me as counter-productive and bad for the theory.

My aim in the original article was *not* to answer philosophical questions about which epistemology (realism or anti-realism) is best, most useful, or true. Rather, I intended to stick to the empirical issues raised by Moscovici & Hewstone (1983), namely the ways in which experts and laypersons might be dissimilar in their attitudes and beliefs with respect to science. The task I set for myself, therefore, falls squarely within the boundaries Marková delineates for the theory of social representations, including:

the psychological and sociological processes that facilitate and interfere with the formation and maintenance of people's images, concepts and beliefs; with the processes by which people construct natural and social phenomena including scientific entities; with the question of how social representations give rise to new representations; and so on (p. 126).

My predictions were offered about what scientists and laypersons *actually* think (the psychological question), without any presumption that the results would tell us which epistemology we *ought* to believe (the philosophical question). So, when Marková writes that the theory of social representations "is not concerned with the philosophical question as to whether the real objects that are antecedents of social representations, *really* exist" (p.

¹ Following Elejabarrieta's advice, I shall refrain from excessive personification of the theory of social representations and avoid referring to it as "Moscovici's theory".

126), my response is simple: neither was my paper. My points were not ontological but psychological. Similarly, when Elejabarrieta suggests that "by comparing the social object with the scientific object, and not vice-versa, we introduce criteria of value and truth that destroys a typical focus of the [theory of social representations]" (p. 132), I must point out that the only comparison I suggested was between social objects themselves: the thoughts and beliefs of professional scientists as compared with the thoughts and beliefs of laypersons.

I hope it is now clear that I never attempted to "reduce ontological issues into epistemological ones" (p. 125), as Marková suggests. My aim was to contribute some new hypotheses (based on the theory of social representations) to the social psychological study of science. These predictions concerned the actual beliefs of scientists and amateurs; the type of beliefs to be studied happened to be *beliefs about* ontological reality. By studying these groups' ontological beliefs, however, I did not presume to be studying the nature of ontological reality itself. For example, I did not claim that because scientific experts may be more likely to *believe* in an instrumentalist or anti-realist position than laypersons, this position is *correct*.

Some of the misunderstandings of my position may be due to an implicit privileging of scientists' views relative to amateurs'. This introduces value judgments which were not in my paper but, to be fair to my critics, may be present in the writings of Moscovici. Marková, for example, equates anti-realism with scientific sensitivity, writing that "beliefs in the ontological reality of phenomena are associated with lack of awareness rather than with great awareness" (p. 128). She likens the scientist to an expert driver who is attuned to the feel of the car and the road, while the amateur "only knows superficially" about such things². One way in which I avoided committing the naturalistic fallacy is by *not* claiming that the scientists' ontological beliefs are better or more sensitive to the truth than those of the amateur.

Elejabarrieta also seems to think that I took the professional scientist's belief as an "objective reference" point by which to compare the amateur's "distorted reproduction" (p. 132). This was certainly not my intention. My hypotheses do not depend upon any objective reference of this sort, only that it is possible to show attitudinal differences between two groups of people, scientists and laypersons. When Elejabarrieta asks "How then will we know what we are comparing?" (p. 133), my answer is that the situation is no different here than with any between-group comparison of attitudes or beliefs, with the limitation that the subjects (amateur and professional scientists) are not randomly assigned to groups. It may be true that when we compare the beliefs of scientists and laypersons we are comparing different *processes* of belief construction as well as different belief products, but this difference in process (and its consequences) may be the very phenomenon we wish to study. Groups assigned to different experimental conditions may also go through different 'processes', and this itself may be part of the hypothesis being tested. Strangely, however, Elejabarrieta claims that "the comparison between systems of thought... falls beyond the

² Incidentally, I do not think that Marková's car analogy is an appropriate one for my hypothesis. The belief in ontological realism is not simply a matter of being aware of the differences between phenomena; it involves a belief about what exists and what does not. Thus, if expert drivers were found to be more likely to believe that "rack-and-pinion" steering is not a real thing, but a hypothetical entity or process, while amateurs tended to believe that it was an actual object or place on the car, this would be analogous to my hypothesis about scientific beliefs.

application of [the theory of social representations]" (p. 133). It seems to me that if the theory has *anything at all* to say about attitudes and beliefs, then it should be capable of producing empirical hypotheses concerning the differences between commonsense and expert conceptions of science. Indeed, this is the point of the paper by Moscovici & Hewstone (1983), and I see no reason to rule out the social psychological study of scientific beliefs or the theory of social representations' participation in such a study.

II. THE POSSIBILITY OF NATURALIZED EPISTEMOLOGY

While my objective in the original paper was not to argue for philosophical theses, I think that Marková is too dismissive of the possibility that social psychology has anything to contribute to the philosophy of science. For instance, when Marková asks whether a similarity exists between the process of objectifying scientific concepts and belief in ontological realism, she concludes that the theory of social representations and the concept of objectification have nothing to do with (philosophical questions about) ontological realism (pp. 125-6), and that the question is not answerable because objectification and ontological realism "belong to different categories" (p. 127). Marková argues, in effect, that there is philosophy and there is psychology, and never the twain shall meet.

I agree that the correctness of a philosophical position needs to be demonstrated on philosophical grounds -- it is in principle independent of any person's belief in it. At the same time, however, it is at least a plausible *philosophical* position that traditional questions about epistemology may be informed by psychological data. Thus, when Marková writes that the concepts of (psychological) objectification and (philosophical) realism cannot even be compared because they "belong to different categories" (p. 127), she has single-handedly dismissed what are arguably two of the fastest growing philosophical disciplines in Northern America and Western Europe, respectively: 'naturalized epistemology' and the 'sociology of science'. The contemporary movement to naturalize epistemology began with Quine's (1969) celebrated denial of the Kantian distinction between analytic and synthetic ways of knowing and the corresponding disciplinary separation between philosophical logic and empirical science. Quine's position has been further developed by Goldman, Campbell, Giere, Stroud, and many others who have sought to 'psychologize' questions that have traditionally belonged to philosophical epistemology (see Kornblith, 1985). The so-called 'strong programme' in the sociology of science (e.g., Barnes & Bloor, 1982; Bloor, 1976) and the related movement to observe laboratory life from a sociological perspective (e.g., Latour, 1987; Latour & Woolgar, 1979; Knorr-Cetina, 1981) have likewise inspired numerous attempts by Collins, Mulkay, Gilbert, Shapin, Pinch, Pickering, Fuller, and others to link empirical social science with issues typically handled by philosophers of science. Despite important differences between naturalized epistemology and the sociology of scientific knowledge, proponents of both positions are confident that social psychology will have plenty to say about the philosophical or ontological questions. While there are several reasons to be wary of the naturalist thesis, as is Marková, there are also many plausible arguments in favor of it (e.g., Bhaskar, 1979). It seems to me that it is too early to tell whether some form of socio-naturalistic epistemology is the best philosophical position, but it is certainly not as foolhardy as Marková would have us believe.

I do not claim, therefore, as do Marková and Elejabarrieta, that there is no conceivable connection between the philosophical and psychological questions, only that we are not presently warranted to assert such a connection. One day, for example, we may decide on

the basis of the division of intellectual labor (cf. Putnam, 1975) that the scientists' philosophy is preferred or, alternatively, that democratic concerns (cf. Feyerabend, 1978) favor the position of the more populous laypeople. If the merits of naturalized epistemology are judged to outweigh its demerits, social psychology *could* play a central role in addressing issues which have been assigned traditionally to philosophy. Thus, philosophers have already begun to draw on empirical work by Kahneman & Tversky and Nisbett & Ross in assessing epistemological and philosophy of science claims (e.g., Kornblith, 1985; Solomon, 1992).

At any rate, the social psychology of scientific belief is a legitimate field of study in its own right, whether or not its data will ever help to decide between philosophical or epistemological positions. Comparisons between lay and scientific conceptions have been a favorite topic of psychologists from Heider to Kruglanski, Ickes to Antaki, and Tomkins to Furnham. It is also a non-controversial assumption of post-Kuhnian philosophy that descriptive accounts of scientific activity are important and useful. I am inclined to share the optimism of Farr (1984, 1990), Ibañez (1992), and Potter & Billig (1992), all of whom recognize the applicability of the theory of social representations to issues raised by the philosophy and sociology of science.

III. MORE ON THE PROCESS OF OBJECTIFICATION AND THE BELIEF IN ONTOLOGICAL REALISM

After arguing that philosophical and psychological concepts cannot be compared in principle, Marková finally grants that there is a "superficial" similarity between objectification and belief in ontological realism (p. 127). While I cannot argue for the depth of my insight, I can certainly object to her characterization of the similarity in terms of the vague proposition "X has existence independently of human beings", where X refers to scientific entities according to ontological realism and X refers to social representations according to objectification. Thus, she thinks that the point of my argument was to show that (1) "According to the process of objectification, social representations have existence independently of human beings" and that this is similar to the proposition that (2) "According to the philosophy of ontological realism, scientific entities have existence independently of human beings". While (2) is true by definition, the meaning of (1) is far from clear. If objectification refers to a social psychological process, as I think Moscovici intended, rather than an ontological theory of reality, then by Marková's own claims it cannot lead to an ontological thesis such as "social representations have existence independently of human beings". What the process of objectification *can* lead to, and this is what I argued, is the *belief* that *scientific entities* (not just social representations, about which most objectifiers have no beliefs whatsoever³) "have existence independently of human beings". If I am right, then the connection between the process of objectification is much closer to the *belief* in ontological realism than Marková allows: objectification is hypothesized to lead to the expression of beliefs which are consonant with ontological realism.

³ If we wanted to study the impact of the theory of social representations on the mass public, then we *would* be interested in the beliefs the mass public has about the theory and its entities. Thus, we could ask laypersons and professional social psychologists: "Do social representations have existence independently of human beings?" The hypothesis I derive from Moscovici & Hewstone (1983) is that laypersons are more likely to respond "yes" to such a question than the scientists. In other words, one can be a realist or an anti-realist about the theory of social representations, and this may be predictable on the basis of social experience and group membership, among other things.

Elejabarrieta argues that my hypotheses might be true, but not necessarily because of what Moscovici calls 'objectification'. For example, Elejabarrieta claims that professional scientists are instrumentalists by trade because of the social pressures to publish in scientific journals and because the "hyperspecialization of science has led scientific work to become a task of fine detail in which 'philosophical' digressions have no place" (pp. 134-5). The problem is that Elejabarrieta does not say *why* a particular ontological attitude should follow from these circumstances. I can think of two reasons why the pressure to publish might result in instrumentalist beliefs: (a) one might be able to do scientific work faster or more efficiently by adopting an instrumentalist position, and (b) one might obtain more publications by conforming to the norms of the scientific community, and these norms might be largely instrumentalist. It seems to me that (a) is a far more controversial hypothesis than any of mine, but interesting (if unlikely) nonetheless, and (b) merely begs the question -- we would still need to explain why the social norms of the scientific community would favor instrumentalism. Furthermore, it is not obvious to me that the position of ontological anti-realism involves fewer "philosophical digressions" than the position of ontological realism.

Marková finds my hypotheses to be obviously true -- an occupational hazard, I suppose, of preaching to the converted. She assumes, for example, that instrumentalist beliefs should follow intuitively from practical or professional experience with "the problematic nature of the phenomenon" (p. 128); it is as self-evident as the prediction that expert drivers will be more sensitive than novices to differences between types of cars. I take some satisfaction in the fact that an expert such as Marková agrees that my hypotheses indeed follow from the theory of social representations and that I have not done serious disservice to it. Assuming, though, that most philosophers, psychologists, and other people do not (yet) subscribe to the theory of social representations, I think that they are in for a minor surprise if it indeed turns out that ordinary people believe more strongly in the ontological reality of the quark than do professional physicists. That the predictions of the theory are "counter-intuitive" was not a major point in my argument, only that they were not trivial or self-evidently true. Marková's lack of enthusiasm, if it is representative of other social representations researchers, does not in my opinion bode well for the constructive expansion of the theory into domains such as the philosophical and social studies of science. I am reminded of Potter & Billig's (1992) recent quip in this journal that "some of the most interesting analytic and theoretical work on science, particularly sociology of scientific knowledge... has tended to be ignored by SR workers" (p. 17).

IV. MODIFICATIONS AND ADDITIONS

I think that Elejabarrieta is right to criticize me for drawing a stronger association than necessary between the belief in physicalism (the thesis that theoretical entities have physical or material existences) and the belief in ontological realism (the thesis that theoretical entities have existences apart from the theorist's conception of them). In order to operationalize the attitude of ontological realism, I incorporated some aspects of physicalism as a (limited) way of tapping into the belief that something "really exists". For support, I quoted Greenwood's (1989) definition of ontological realism as the thesis that "scientific theories have real existence *in precisely the same sense as physical objects*" (p. 38, emphasis added). Belief in physicalism and belief in realism may not be entirely unrelated; it is hard to be a physicalist about most scientific entities and also be an anti-realist, though one could certainly be a realist and an anti-physicalist. However, I do not agree with Elejabarrieta that the process of

objectification results in the belief in physicalism but *not* the belief in ontological realism (p. 134). It follows from the theory of social representations that amateurs are more likely than scientists to believe, in Marková's terms, that "scientific entities have existence independently of human beings" and also that this existence is material or physical. I should have said that objectification results in the belief in physicalism *and* the belief in ontological realism. Interestingly, Moscovici (1981) includes components of both physicalism and realism when he describes objectification as the tendency to endow abstract theoretical concepts with a "nearly *physical, autonomous* existence" (p. 200, emphasis added).

Marková may be right that my use of Bhaskar's (1978) discussion of ontological realism falsely suggests that *he* was advancing an empirical hypothesis about the actual beliefs of scientists. My main purpose was to illustrate what one might believe if one believed in ontological realism. It is true that when Bhaskar writes that "the ontological independence of the event is a condition of the intelligibility of its description" (p. 190) he means to be making a philosophical point. Nevertheless, naturalistic epistemologists may well consider psychological data when evaluating claims about 'intelligibility', thereby opening the door again for the social psychology of science.

Elejabarrieta suggests that technological applications move more quickly into the realm of ordinary discourse than does scientific knowledge. This seems to be a criticism directed at Moscovici & Hewstone rather than at me, although I admit that I aimed to develop their arguments. I think that Elejabarrieta could be right that technological innovations are *more* influential in society than conceptual innovations, and yet we would still be interested in comparing the (scientific) beliefs of amateurs and experts, perhaps as a way of studying the impact of technologies on the public (cf. Kipnis, 1989). My project, therefore, would be compatible with the notion that beliefs are transmitted through practical experience with new technologies.

For the record, I did not claim that the theory of social representations is *correct*. I only attempted to formalize it and state hypotheses which were not self-evident and which had some bearing on the social psychology of science. This would appear to be worthwhile, since the theory has often been criticized for its lack of precision (e.g., Billig, 1988; Harré, 1984; Jahoda, 1988; Potter & Litton, 1985; Rätty & Snellman, 1992). However, my proposals encountered considerable resistance from the commentators, largely because I attempted to link the theory of social representations to philosophical beliefs about ontological reality. If this resistance was due to a misunderstanding of my purpose, namely that I had sought to reduce philosophical questions about what people *ought* to believe into psychological questions about what people *do* believe, then I hope to have removed some doubts. If, however, my critics still wish to claim that hypotheses about the scientific beliefs of amateurs and scientists are incompatible with the spirit of the theory of social representations (Elejabarrieta, p. 132; Marková, pp. 126-7), then I think they are far too shy about the social psychology of science.

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