

**UNANALYSED RESIDUES:
REPRESENTATIONS AND BEHAVIOURS
A COMMENT ON W. WAGNER**

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Piaget famously remarked that logic is the morality of thought, and it is in this sense that Wolfgang Wagner's (1994a) recent paper on *The Fallacy of Misplaced Intentionality in Social Representation Research* presents us with a kind of morality tale whose object is to teach us some of the things we should avoid. For it is through the medium of the logic of scientific research that he tries to persuade us to see the importance of how we frame research questions in the terms of a theory of social representations, and of avoiding the pursuit of issues which lead away from the radical significance of social representations as a perspective in social psychology.

The center of Wagner's argument is that the use of verbally assessed beliefs as an independent measure of a representation which can be used to predict some subsequent behaviour reflects the importation of a misplaced intentionality into social representations research. This intentionality, with its assumption of a causal relation flowing from representation to behaviour, is misplaced because it misconstrues the relations between representation and behaviour. As I said earlier, Wagner establishes this argument through a consideration of the logic of scientific research, not least because this is the traditional idiom for discussing questions of causality in psychological research. But this argument can also be expressed in a less technical way. If, as Moscovici asserts, a social representation is a "system of values, ideas and practices" (1973, p. xiii), then a focus on the systematic quality of representations also indicates that it would be a fallacy to separate the values and ideas as antecedents to the consequent practices. On the contrary, from this perspective values, ideas and practices can all be taken as elements signifying the same representation, or, if one prefers, they can each be construed as expressions of the same representation. In these circumstances there seems to be no cogent reason for suggesting that it is possible to extract one or other element as logically prior to the others in such a way that it could be considered as a cause of the other elements. Each of these elements becomes significant because they are structured as a system, as a social representation.

Wagner's argument is that we cannot *prove* that actions are consequences of beliefs, since both actions and beliefs are expressions of underlying representations, which we infer through an interpretive procedure which can be applied equally to either actions or beliefs. I would also add that we cannot prove that representations exist, we can only demonstrate the power of this concept through the interpretations we can offer of social phenomena. And in this sense psychology finds itself in the same position as every other human science. A part of our problem is that psychology continues to function in an epistemological world that is distorted by positivism, which we ourselves find difficult to shake off. The productive aspect

of Wagner's paper is the way he makes us see the residues of this positivism in the continuing construction of research problems in terms of causal relations.

Wagner's paper can also be seen in a more ironic way. In articulating his argument he discusses three recent research studies, and he notes that the authors of these papers rarely use the language of causality explicitly, though it is implicit in their methodology. His analysis of these three papers is in fact a demonstration of an inconsistency between representation and behaviour in the conduct of research. And, as he makes clear, his argument should not be taken as a condemnation of these particular studies, but rather of a continuing problem in social representations research more generally. It is a sin which many of us commit, and I can certainly recognise it in some of my own work (when, for example, I've tried to use assessments of social gender identities to predict performance on interview tasks, cf Lloyd and Duveen, 1992). To appropriate a Freudian expression, we can observe "unanalysed residues" in our assimilation of the perspective of social representations, and the merit of Wagner's paper is that it brings these residues to consciousness. And like all good analytic interpretations, his discussion is as provocative as it is productive.

It might seem strange that after 30 years of research in social representations it is still possible for Wagner to write this paper precisely because the issues it takes up seem so crucial to the perspective of social representations. If one returns, as Wagner does, to some of the classic theoretical texts of this tradition it is immediately clear that there can be no question of separating representations and behaviour so that the former can be construed as the cause of the latter. To make such a separation would be to limit the conception of representation by excluding behaviour from its purview, and, from the other side, of seeing behaviour only as a consequence of this limited notion of representation. Yet, as Wagner also notes, while this point may be accepted theoretically, it is regularly suppressed in empirical research. At a theoretical level, social representations presented a critique of approaches which rested on a separation between cognitive and behavioural elements. What he points out is that the empirical habits we have acquired through our training in social psychology have not been subject to the same kind of critique. When we use research designs and analytical models adapted to the analysis of causal relations we cannot simply disavow this causality through an arbitrary act of conceptual redefinition. There is a dissonance here, which like all dissonances it is more comfortable to avoid. In this sense, the provocative purpose of Wagner's paper is to make it unavoidable.

The strangeness I spoke of can then be seen as a question: How is it possible for this dissonance to have remained dormant for so long? This is not a question which Wagner asks in his paper, which considers only the logical structure of the arguments. It is though an interesting question to consider, precisely because in doing so one is led to consider some of the wider issues raised by Wagner's paper. The reflections which follow have been stimulated by reading this paper, even if they do not provide a clear answer to the question as to why a sense of strangeness persists.

For a long time the most public face of social representations was that which it presented as a critique of social psychological theories of attitudes. Indeed, I suspect that it was exposure to this debate which first drew many researchers into the orbit of social representations. But this debate has left its shadow in the way that questions for research are posed. Within attitude theories the relations between attitudes and behaviour have been a continual source of disquiet and discomfort, since the assumption that attitudes should somehow predict behaviour proved so difficult to sustain. Fishbein and Ajzen's theory of rational action is intended to soothe this discomfort by providing the balm of behavioural intentions as the central mediator between attitude and behaviour. I don't want to comment

on this theory in any detail, except to point out that it accepts as given a separation between the cognitive elements of attitudes and the external actions of behaviour. Indeed, we could say that the problem of attitude-behaviour relations stems from a conceptualisation that effects an initial separation between these elements while asserting at the same time the necessity of a causal relation between them. The radical imperative of social representations theory was its refusal to accept such a separation, insisting rather that attitudinal and behavioural elements had to be seen as both contained within the same symbolic structure. From this perspective the attitude-behaviour problem is a pseudo-problem, an issue which will always disappear through a deeper analysis of the symbolic aspects of the attitudes and behaviours expressed. An yet reading Wagner's analysis of the three research studies he considers it is difficult to avoid the impression that the design of these studies reflects a continuing concern with analysing attitude-behaviour relations. It is here that the shadow of the debate between social representations and theories of attitudes is most apparent, where the research questions have been structured by empirical habits adapted to a causal view of the relations between attitudes and behaviours. Wagner's elegant logic illustrates the extent to which researchers are still trying to use the theory of social representations to solve the problems of other forms of social psychology, rather than establishing problems in terms of the theory of social representations itself.

Wagner suggests that these old empirical habits survive in contemporary research when social representations are considered as a distributed property of individuals because the researchers themselves are too close to a folk-psychology of the individual which emphasises a causal intentionality. No doubt he is right about this, and Farr's discussions of the ideology of individualism in social psychology serve to underline his argument (eg Farr, 1991). But there is, I think, also another reason which he touches on only partially. This is the undervaluation of practice in empirical studies of social representations, an undervaluation which is so prevalent that I am tempted to describe it as systematic.

The neglect of practice in empirical research in social representations is an extraordinary phenomenon. While there are one or two notable exceptions (eg Jodelet, 1991; Lloyd and Duveen, 1992) for the most part studies of social representations have limited themselves to the investigation of verbal material. An even where research has drawn on non-verbal evidence (eg De Rosa, 1987) this has not generally been linked to an investigation of practical activities. In this sense the three studies examined by Wagner are themselves exceptional in trying to link verbal expressions of representations to some form of practice. What makes the neglect of practice so extraordinary is that theoretically it has always been assumed that social representations inform practices just as much as they structure values and ideas. And yet we pursue research as if a focus on values and ideas were sufficient, so that as long we take care to examine these, we can simply assume that the practices will follow naturally. Except, of course, that we know perfectly well that practices do not just follow in this way, that there can be all manner of disjunctions and inconsistencies between verbal expressions of values and ideas and practices undertaken in relation to the same objects. To Wagner's fallacy of misplaced intentionality, we could add the illusion of consistency in social representations research. Why should this illusion persist? Again, without claiming to provide a comprehensive answer, I should like to indicate a number of points.

At one level the issue here is the bias which exists towards assessing representations through verbal expressions, and in his discussion of this issue Wagner emphasises the sense in which verbal expressions are always themselves the focus of an interpretive effort on the part of the researcher before an underlying representation can be inferred. And in this sense verbal assessments stand in the same relation to an inferred representation as any other form

of activity, so that it is only the imposition of a folk-psychology which leads to the identification of the verbal data as antecedent to other forms of data. Privileging verbal data in this way also reflects a view that such data are somehow closer to the cognitive aspects of representations. In part, of course, this view draws sustenance from the fact that as researchers we have no alternative but to express the representations we investigate in language if we wish to communicate them. But the role of language in the communication of research ought not to be taken to mean that language has a decisive role in the shaping of representations. Here again we can see how our habits of thought remain subject to influences from more traditional forms of psychology, particularly contemporary cognitive psychology. Indeed, we could say that as well as a folk-psychology of individualism influencing social representations researchers, there is also a folk-psychology of cognitive psychology which exerts an influence through its privileging of the cognitive over the behavioural and the verbal over the non-verbal.¹

A second issue which needs to be addressed is the lack of clarity about what we mean by the term *practice*. Is it simply a synonym for other terms such as *behaviour* or *overt action*, or does the concept of practice signify something beyond such discrete notions? On this point Wagner's paper is curiously mute, shifting from the use of *behaviour* to speaking about *practice* without articulating any distinction between them. But some distinction is necessary. In Jodelet's (1991) study of social representations of madness, for example, it is not the particular action of some village host in separating the clothes or utensils of a lodger before washing them which enables her to identify a particular practice. Rather, it is the sense that such examples form part of a systematic pattern of activity extending across a range of different actions which is important here. The notion of practice, however, implies something more than just the identification of a systematic pattern of activity. The sense of practice emerges when these activities can be interpreted as meaningful actions. In Jodelet's example (as Wagner notes) it is the beliefs about contagion in the social representations of madness which renders these actions meaningful. Here, though, the word *meaningful* is potentially ambiguous, since it can conflate the perspectives of the actors and the researchers. In fact it would be better to say that from the researcher's perspective, the interpretation which they offer renders activities and behaviours *intelligible* in terms of a particular social representation, and that a part of this interpretation is also the assumption that such activities and behaviours are *meaningful* for the actors involved since they are participants in the symbolic universe of this representation.

In the context of this discussion of Wagner's paper I should add to say that representations render patterns of activity intelligible is not to claim that the representations can be considered as a cause of the activity. Rather, it is to claim that actions become practices when they can be interpreted within the structure of a representation. Representations, as Moscovici emphasises, are always the representations of someone or some group, and the practices which form part of such representations are also always the meaningful actions of someone or some group. Jodelet's claim, then, is twofold: first, that these actions are meaningful for these actors because they express a particular representation of madness; and secondly, that her interpretation renders these activities intelligible. Here the

¹ There is another dimension to this question which lies in the practical exigencies of undertaking research in the contemporary academic world. To put it bluntly: an investigation of practices (for which the most appropriate method remains ethnography, cf Duveen and Lloyd, 1993) takes more time than administering and analysing questionnaires or even interviews. This may be a less theoretical reason for the neglect of practice, but it is no less powerful for being merely contingent. Time has become an ever more valuable commodity in the political economy of the modern academy and each of us has become more or less of a miser in the expenditure of this scarce resource.

appropriate category to describe the link between representation and practice is not an *explanation* but *intelligibility*. In making an interpretation, the researcher is of course claiming that there is an intentional causality on the part of the actor (i.e. they undertake these actions for specific representational purposes), but the researcher's analysis is not constructed in terms of what Wagner calls a "misplaced intentionality".

As these remarks indicate there is a clear need to distinguish between the perspectives of the researchers and the actors who are the focus of their analytic efforts. Indeed, the need for this distinction is one of the central nodes in Wagner's argument, which he encapsulates in his blunt warning not to "confuse contents of data with theoretical categories". In another recent paper he states this idea more explicitly when he writes that "research explaining behaviour by representations misplaces a belief on the part of subjects at the theoretical level. Such research takes what *is* a mental content of the subjects and introduces it into the theory *about* the mental contents of the subjects. This is not legitimate in the same sense as it would be illegitimate to take the subjects' beliefs *about* madness and introduce them into a scientific theory *of* madness" (Wagner, 1994b, p. 178).

While I think Wagner is correct to insist on the necessity of making this distinction, I think that sustaining it is more problematic than his texts admit. In his paper Wagner counterposes the perspective of the researcher which is located in that conceptual space we call science, to that of the actors engaged in the social phenomena under study. A simple counterposition of perspectives leaves the impression that the researcher occupies a position from which social phenomena can be viewed objectively. But as researchers we do not operate from some objective position outside the cultures we inhabit, we are deeply implicated in them and influenced by them. How else should we construe the continuing presence of these residues of positivism within social representations research which Wagner himself so clearly brings to our attention? An interpretive stance cannot be limited to the researcher's view of the phenomena under study, it must necessarily become reflexive, since the researcher too is an actor engaged in the production of social phenomena. In short, I want to argue that the interpretive approach which is characteristic of social representations research also needs to be adopted in discussing the activities of social representations researchers themselves. Like any other scientist, we are engaged in a practice characterised by a dialectic of representations and influence (cf Moscovici, 1993).

Distinguishing between the perspectives of researchers and actors is, of course, at the center of the weaknesses which Wagner identifies in the research studies he discusses. As a remedy for the continuing influence of a folk-psychology in scientific research, he suggests focussing more particularly on the collective aspects of representations. I am not, though, convinced that this is in itself a sufficient remedy. If this folk-psychology continues to influence our work it is because it expresses a representation through which we are linked to the culture we inhabit, and it is this which makes it difficult simply to leave folk-psychology behind, or, as they say in German, to throw it on the ashes. We may be scientists, but this does not make us immune from the symbolic entanglements of the representations of our culture. Untangling the kind of confusions which Wagner identifies ought to be one of the functions of communication and discussion amongst ourselves, and it is in this kind of debate that I would see a stronger medicine. The best remedy, if I can appropriate Freud once again, would be a "talking cure". And in the way it stimulates reflections on a wide range of substantive theoretical concerns, not only about the characteristics of the theory of social representations but also about the characteristics of social psychology as a scientific practice, Wagner's paper provides a notable intervention.

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