SPEAKING IS ACTING IS REPRESENTATION
COMMENTS ON THE REPLY BY A. ECHEBARRÍA

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In his reply, Agustín Echebarría Echabe (1994, this issue) discusses and criticizes my paper "The Fallacy of Misplaced Intentionality in Social Representation Research" (Wagner, 1994a). Echebarría's reply as well as my comment may be difficult to appreciate without knowing the original paper we are referring to, since it was not published in the present journal *Papers on Social Representations*. This is the reason why I repeat some of my arguments here.

Echebarría comments on several levels: *(a) a specific defense of his and his collaborators' work*, which supposedly was attacked in my paper, *(b) a critical appraisal of my linguistic style and argument* and *(c) a general criticism of my article's message.* I will primarily comment on the third point but I also wish to comment briefly on the first and second. Let me just add that I enjoyed reading Echebarría's comments although I disagree with many of his annotations.

I think communication is a difficult enterprise. Seminars on group dynamics and communication constantly remind us of the dangers inherent in not having, or having only half understood a communicator's message. I do not consider myself immune to these problems and it may well be the case that I have seriously misunderstood Echebarría's paper and perhaps also the other two papers I am quoting in my theoretical exposition. However, Echebarría also seems to have misunderstood the central theses of my paper. If he means that "we do not feel identified with the portrait which Wagner has drawn of us", I also feel that he has not understood my point either; but this probably is the fate of much scientific discourse. This leads me directly to my first point:

(a) It was not at all my intention to criticize a specific research or work in my paper, neither Echebarría and Gonzalez's (1993) nor Thommen et al.'s (1988) nor Kempton's (1987). As stated in my paper, these three research reports were used simply as illustrations or examples of a *style of empirical research* in the domain of social representations which inherently follows the logic of using representations (social or cultural) as an independent, and subsequent behaviour as a dependent variable, irrespective of whether the authors *verbally refer to the terms "independent" and "dependent variable" in their text or not*. Hence, it was not the *quality* of these works – of course they represent high quality work – which motivated me to use them as examples, but their *inherent logical structure.* I consider the structure of their empirical design as being very similar to the classical *"beliefs/attitudes cause behaviour"-designs* we know so well from social cognition research. I do not refer to their verbal super-structure. Consequently I can hardly be accused of criticizing Echebarría's and the other authors' messages, neither can I be obliged to consider all the papers a respective author ever has produced. Let me just mention that I know Echebarría's work on social representations and I regard it highly.

(b) Echebarría accuses me of using what he calls a "rhetorical strategy", i.e. logic, specifically formal logic, to prove the superiority of my position over the others' positions. Let me be brief on this: rhetoric always is a necessary form of any verbal statement, be it written or spoken. If I use formal logic to make a certain point more precise, I do this simply because definitions – in this case it is the definition of what I consider a causal closure – in my experience can be made much more precise by formal notation than by informal words. Otherwise we would not need mathematics, nor programming, which are languages constructed precisely to allow unequivocal statements. I would add that Echebarría also uses rhetoric in his reply by referring to, e.g., Wittgenstein, Foucault and Billig, or by using a figure to make his point; yes, even the use of a name or a figure is rhetoric!

My "logical rhetoric" seems, however, to have lead Echebarría astray when he comments that one can only critizise mainstream social cognition, if one abstains from using formal logic in a paper. It is hard for me to follow this argument, because I see no necessary connection whatsoever between the form of a logical statement and its theoretical content and context. In fact, from my paper it would be rather hard to conclude that I subscribe to the "amateur" or "naïve scientist" approach of social cognition and its view of biases and shortcomings in everyday cognition.

(c) Let me finally come to the points which refer to the theoretical arguments put forward in my paper. I think that one important contribution of the social representations paradigm has been to draw our attention towards the importance of *what people think* as a determinant of *how people think*, and to the related fact that all of peoples' discourse is socially situated. If we investigate *what people think* it is the contents of this thinking which is the object of research interest. The research *object* is the specific *content*. We speak and write about this content from a scientific point of view which simply means that we describe it in theoretical terms within its social frame of reference. In our description we may use the same or a similar language and concepts, as used in everyday language. However, we must, and usually will not confuse the ideas *in the object* with the ideas and concepts used *in our theory* which permits us to describe the object.

Smedslund, in his series of papers and books on Psycho-Logic, researchers working on indigenous psychologies, as well as philosophers in cognitive science have similarly drawn our attention towards the fact that much of cognitive psychology's conceptual vocabulary stems from our *Western indigenous folk-psychology*. It is easy to see that it poses a specifically crucial problem for an *international scientific psychology* (as well as pedagogic) if its central concepts are culturally biased. (This, however, is not the present topic.)

The point of my paper is that even such plausible ideas as (1)*"Thinking (wanting) X makes (causes) me (to) do Y"* are part and parcel of folk-(psychological)-beliefs and hence part of the object (the contents of everyday thinking), which social representation research sets out to describe. Describing such a belief as part of *psychological folk-science* is one thing. Integrating it into the language of theory used for description, as plausible as it may be, is quite a different thing. But it is exactly this confusion, the *fallacy of misplacing folk-beliefs in theory*, which I am criticizing.

Many, though not all, of the investigations conducted in social representation research at least implicitly subscribe to this *intentional causality* by designing their research plans according to the folk-belief of intentional causality. Usually the authors first apply interviews and/or questionnaires, and secondly they observe the subjects' behaviour (or even only ask the subjects themselves for their behaviour – as in the case of Echebarría's study). The rationale is that the interviews and questionnaires "assess" the beliefs or the representation towards a specific object and that the subsequent behaviour is a consequence of the assessed

beliefs and representations. Hence, the underlying assumption is that (2) *"Thinking X makes (causes) the subjects do Y"*. The ideas (1) and (2) are the same in content but situated at different levels, i.e. (1) is at the everyday psychological level and (2) is at the theoretical level of the investigator's research designs. I discuss this issue in two respects: (aa) with respect to the functions of intentional causality in everyday social life, and (bb) with respect to the assumed validity of verbal and overt behavioural measures. Therefrom I conclude that (cc) representation and behaviour cannot, or should not be conceptually separated.

(aa) Mistaking a content of folk-beliefs as a notion on the theoretical level blinds us to seeing the folk-political and socially situated meaning of an everyday statement like (1). Imagine asking your friend why he or she did Y, e.g. buying a sweater. If he or she answered *"I have no idea why I did Y"* or *"Well, it just happened that I did Y, it doesn't mean anything"*, you probably doubt the mental state of your friend. Stating that he or she did Y because of *thinking something* or *wanting it*, is quite a different thing. It is an everyday-political statement (re-)affirming that your friend is a sensible and proper person, or, in theoretical terms, a rational person. One needs to give good reasons when answering such a question, reasons which, from a different angle, appear as causes of the subjectively explained behaviour. Being sensible is probably a value in all, but certainly in our Western societies and cultures. It goes without saying that the "games" of taking or rejecting responsibilities in our society, i.e. stating *"I did Y because I thought or intended X"* etc., have been objectified in the institutions and ritual procedures of politics, justice, and everyday life. Hence, even such a simple statement serves its socially situated function and needs to be described and investigated as such.

I think this short presentation of my main points makes clear that the type of rationality I am talking about is the subjective (or social, if you want) rationality, or *sensibleness*, of everyday people. It is part of their discourse and part of social representations. It is only in a second step that we could ask on the theoretical level, whether belief X "really causes" behaviour Y, i.e. the *theoretical rationality model*, or whether belief X is a secondary rationalization of behaviour Y, i.e. the *theoretical rationalization model.* Neither of the two is favored in my paper, nor would it play an important role. Echebarría is mistaken if he sees my paper to be placed in the rationalization "box".

(bb) The arguments in the foregoing paragraphs also give an answer to the question some people, not primarily Echebarría, have asked: isn't it the case that often people *do not act* in accord with what *they tell us that they believe*. (Mind, I am saying "what they tell us that they believe" and not "what they believe".) This is foremost a methodological and not a theoretical question. My answer is: "how do we as researchers know what the people believe?". If *we think we know* what people believe by asking them, i.e. using interviews or questionnaires, and then observe their subsequent behaviour, we certainly often observe discrepancies. However, why should we consider the people's answers to an interviewer's questions or to questionnaires as a more valid information source for what people think than their own behaviour? Look at it the other way around: why don't we try to infer from their behaviours what people think and only secondarily wonder why their verbal answers to interviews and questionnaires are discrepant? It is obviously a methodological decision which method, verbal responses to questions, or observing behaviours, we consider as being a more valid indicator of what people believe.

(cc) One may doubt whether this problem can be resolved empirically or whether it must be solved on a theoretical level. I think it is the latter because it boils down to the theoretical suggestion I made several times with regard to social representations and behaviour. Why do we separate the two? Why don't we take seriously what Moscovici wrote, "qu'il n'y a pas de

coupure entre l'univers extérieur et l'univers intérieur de l'individu (ou du groupe)" (1969, p.9; see also Abric, 1994) which comes to saying that representations comprise the stimulus as well as the response (1984, p.60f; cf. Wagner, 1993). I do not know exactly which rationale Moscovici had in mind to justify his statements, but my analysis – which probably is different from his rationale – points to the very same theoretical necessity of considering representations, i.e. cognitive, affective, iconic and symbolic content, as comprising also behaviour as an *integral part of the representation* and not as *being linked to* or *determined by a representation.* This constitutes a crucial difference not only for theory, but also for methodology and research designs.

What I have said so far refers primarily to the individual level of assessment whereas social representations are equally situated at the collective or social level. This is due to the fact that the vast majority of empirical research, including the three investigations referred to in my paper, is situated on the individual and not on the collective level of *assessment* (for the term level of assessment and its consequences for social representation research, see Wagner, 1994b). Also Echebarría's *empirical* part is at the individual level of assessment, which should not be confused with Doise's (1986) level of analysis. It simply means that his methods, questionnaires and interviews, were applied to identifiable individuals and not to collective entities like, for example, institutions per se. Even if Echebarría stresses that his analysis refers to *Social Practices*, writ large, i.e. to the social *institution* of voting, this is true only for his level of *theoretical* reasoning and not at all for his level of *measurement* in the empirical report. In this context I accept his reproach that I omitted giving an explicit definition of what I consider to be a social practice in contrast to behaviour and action. It is true that his theoretical rationale is explicitly situated at the level of social practice as a collective institution, but his data and statistical results are *not.* because he asked *individuals* and *not an institution* after the elections, whether they had voted or not (Echebarría & Gonzales, 1993, p. 33). This is basically the same thing as asking, whether they have "put a ballot paper in the ballot box". (Such discrepancy between levels of assessment and levels of theoretical analysis is not a problem per se and common practice in research.) – But because I refer to the logical structure of his *empirical* investigation he is wrong in accusing me of having confused the individual act (putting a ballot paper) with the social practice of voting.

In my paper I put the rhetorical question whether there is *anything* which social representations can explain, if behaviour must be considered part of the representation and not be explained by it. My conclusion is that it is the social facts, the action outcomes, which result from a group of people enacting concertedly and collectively a specific representation, which constitutes a good candidate for such an *"explanandum"*. Action outcomes are different from the action itself, and they are *contingently* and not *logically* connected to representations-including-behaviour as, for example, Elster in his trilogy on rationality and irrationality (1983, 1984, 1989) convincingly points out.

Action *outcomes*, however, are not the same as the *functions* social beliefs fulfill in social groups. Echebarría misses the subtle difference in the epistemological status of functions and causal consequences in the last paragraph of his reply. Unfortunately this is not the place to elaborate on what functions are in contrast to consequences. Suffice it to say that functions or *"functional explanations"* are (pseudo-) explanations giving merely *epistemic reasons* for a fact. Giving epistemic reasons means to explain the presence of a "feature in terms of its beneficial effects on the system" (Little, 1991, p. 91f). They are not explanations in a strict sense and they derive historically from teleological world views. A causal explanation, even in social science, is an answer to the question "How does X come about?" by giving one or

more causes for X to come about (for the explanation issue cf. Stegmüller, 1974). Therefore my suggestion cannot be equaled to Durkheim's or Halbwachs's functionalist views.

Let me conclude by asking *"What's in a theoretical exposition?"*. A theoretical exposition certainly is a different matter than an empirical report. An empirical report gives empirical evidence in favor of or against a position, a theoretical paper gives theoretical analysis and seeks to provide logical evidence in favor of or against a position. Giving logical evidence means to look for contradictions between statements of a theory. Such contradictions may not only exist between theoretical statements per se, but also between theory and its implicit and explicit operationalization in empirical work. Logical or theoretical evidence can be provided by constructing counter-examples, by pointing out logical incoherence and by extending theoretical statements to the limits of their validity. Such evidence can be presented to the reader in its pure theoretical form or in the form of fictive or factual *illustrations*. Illustrations often take the form of rhetorical figures and appeals to plausibility.

It seems that Echebarría mistook some of the *illustrations* of theoretical arguments I use in my paper as being the theoretical essence itself. One gets this impression when he quotes some of my statements doubting the *factual* validity of my *illustrative* statements. But such *illustrative* statements gain their validity only within the *theoretical* context of the respective paragraphs and chapters and obviously not when they are taken as isolated sentences as is the case in Echebarría's penultimate paragraphs.

In his reference to Wittgenstein Echebarría correctly points out that disagreement between meta-theories cannot be resolved. However, neither is the discrepancy between a rational and a rationalizing theoretical approach a *meta*-theoretical discrepancy, nor is my paper based on a (meta-)theory different from social representation theory. The former could be settled by showing empirically the limits of validity of either one, since both may be correct in different contexts; the latter is no discrepancy at all. My paper is just an analysis of a specific problem in social representation theory, giving a modest suggestion of how one might understand the theoretical relationship between representation, behaviour and action outcomes in the light of the theory itself.

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