Human Affairs, Special Issue 'Action and Practice Theory' edited by Theodore R. Schatzki, vol. 17, no2, Dec.2007, pp. 138-153.

Participation of the Public in Science: Towards a New Kind of Scientific Practice

Isabelle Peschard [i.peschard@utwente.nl]

University of Twente (NL)
San Francisco State University

Abstract:

Participation of the public in science has been the object of an increasing number of social and political philosophical studies, but there is still hardly any epistemological study of the topic. While it has been objected that involvement of the public is a threat to the integrity of science, the apparent indifference of philosophers of science seems to testify to its lack of relevance to conceptions of scientific activity. I argue both that it is not a threat to science and that it is relevant to philosophy of science by showing that it constitutes a new kind of epistemic practice. Two main objections to the idea that the involvement of non-scientists, with their situated perspective and contextual values, can form an epistemic practice will be addressed: the first bears on the epistemic potentialities of the cooperation between scientist and non-scientists; the second on the possibility that this cooperation takes the form of a practice.

The great challenge that now faces philosophy of science [is] to develop methodologies, not for life in the laboratories where conditions can be set as one likes, but methodologies for life in the messy world that we inevitably inhabit.

(Cartwright, 1999, 18)

I. Introduction

Participation of the public in science has been, over the past 20 years, the object of an increasing number of social and political philosophical studies. These studies provide a thorough analysis of the sociopolitical aspects of this participation, reflecting on the conditions and legitimacy of involving the public in science policy and decision-making and the normative requirements regarding the conditions of this participation. There is still, however, hardly any reflection on the epistemic aspect of this participation, that is, any reflection on the significance of this participation and its normative requirements from the standpoint of philosophy of science. While it has been objected that participation is a threat to the integrity of science (Ezrahi, 1990), the lack of interest on the part of philosophers of science seems to testify to the lack of relevance of participation to philosophical conceptions of scientific activity. This paper aims to show, first, that it is not a threat to science and, second, that it is relevant to philosophy of science by showing that it constitutes a new kind of scientific practice. Two crucial objections to the idea that the participation of non-scientists, with their particular concerns, their situated perspective, their local knowledge, and contextual values can form an epistemic practice will be addressed: the first bears on the *epistemic* potentialities of the participation; the second on the possibility that the cooperation between scientist and non-scientists takes the form of a *practice*.

It is first important to be clear on what I call ‘participation’. As Row *et al.* (2004) notice, participative mechanisms vary not only in form but in intent, and the different forms of deliberative procedures that the authors gather under the label of participative mechanisms are not participation in the same ‘thing’. I want to focus on participation *in science*, as participation in the production of scientific knowledge. In most cases of deliberative procedures, speaking of participation in science would be like saying that one participates in making a dinner when one is merely asked to write down one's culinary preferences or make some comments on the menu. Even the case of consensus conferences, often taken as paradigm of participation in science (Joss and Durant, 1995), is not one of participation in the production of knowledge (Douglas, 2005). The conferences make possible a new form of communication from the experts to the citizens, and a new form of reflection on the part of the citizens. But no scientific knowledge is produced. The experts are there only to provide knowledge, and the consensus that is reached only concerns the citizens. This kind of procedure is still much too unidirectional to enable an effective participation of the public in science. It seems instead to exemplify the idea that "current democracy lacks the institutions to facilitate participation as knowledge production rather than to express one's concerns, interests or values." (Hisschemöller, 2005, 199)

By contrast, the kind of participation that I have in mind as an immediate challenge to philosophy of science is such that participation is not limited to the policy-making process … but "actually start[s] at the research level, where the knowledge-, information-, and/or data-basis for decision-making is created." (Jürgens, 2004, 87) That means that non-scientists interact directly with scientists and take part in the scientific research and resolution of particular problems. The extent to which that is possible is not always the same and the modes of participation are diverse (Leach and Scoones, 2007). But there are an increasing number of examples where, through a collaborative approach ('community-based participatory research' (Flicker *et al.,* 2007), 'collaborative analysis' (Douglas, 2005), 'Joint Fact Finding' in its collaborative research form (Campbell, 2006)), the problematic situation addressed by the scientists is resolved in a way it couldn't have been without this participation and the situated knowledge or considerations that non-scientists bring to the research.

The objection discussed in the first part of the paper is that participation, because it expresses particular concerns or interests, would be shaped by values that are relevant to the life of the participants, but not to knowledge. Accordingly, they constitute a threat to the production of *epistemic* judgments empirically well grounded. The core of my counter-objection will be that the contrast between, on the one hand, the necessary situatedness of participation, with its particular orientation and 'non-cognitive' values, and on the other, the conditions of formation of empirically well grounded epistemic claims, rests on an idealized conception of scientific activity, oblivious to the realization conditions of scientific activity as a practice, which is, as such, always already situated and oriented.

This answer, however, immediately raises another objection, drawing on the very idea that scientific activity is a practice. Scientific activity, as a practice, is seen as characterized by homogeneity in practical performances, in shared beliefs or in normative commitments of the practitioners. So, if a cooperation between non-scientists and scientists aims to bring together different ways of relating to a certain problematic situation, it can only be at the expense of the homogeneity seen as necessary to the practice through which scientific knowledge is produced. How could the involvement of the public be a participation in science? It can only appear as a disturbance of scientific procedures, or even an opposition to science. It seems that the best one can do to ‘involve’ the public in science while preserving the integrity of scientific practice is to develop a better communication of scientific results and to allow some recommendations that will not bear directly on scientific activity. This is not a participation in science, for the public does not interact with the scientists in a way that can affect the production of scientific knowledge. And such a participation is simply inconceivable if this interaction necessarily prevents the realization of the conditions of a scientific practice, as can only be the case if these conditions involve homogeneity in practical performances, in shared beliefs or in normative commitments of the practitioners.

But is it true that a practice is best characterized in terms of homogeneity? As we shall see, this conception of practice cannot even account for crucial features of what is already recognized as a practice, linguistic or scientific. An alternative is to understand scientific practice dialogically (Risjord, forthcoming). According to such a conception of practice (Rouse, 2002), what holds together the practitioners, and more generally the constituents of the practice, is their interaction with each other as submitted to a mutual accountability with respect to certain norms, themselves dynamically constituted in the practice and the interaction. According to this conception of practice, there are no conditions of homogeneity, and therefore no *a priori* objections to a positive, productive interaction between non-scientists and scientists. And nothing more, so to speak, is required than such an interaction and a mutual accountability to regard the non-scientists as taking part in scientific practice, that is, as practitioners. Participation is then nothing else than the development of a new kind of scientific practice, which is of a new kind only in the sense that the space of the responsive and responsible interaction that constitutes the practice has been opened to non-scientists.

II. Is Cooperation a Threat to Epistemic Content?

1. Conflict of Values?

The first objection, which aims to ‘save’ the epistemic content of scientific claims, rests on two inter-related presuppositions: one is that values can be categorized *a priori* as of different kinds such as cognitive, epistemic, social, ethical. The other is that science can be divided into separate moments (Lacey, 1999): choice of problem, of kind of phenomena to be investigated; problem-solving and more specifically theory choice; application of scientific knowledge. That values play an important role in science is not controversial but it is regarded as essential to scientific knowledge that only the first and third moments be affected by non-cognitive or non-epistemic values. I am not interested in the debate between realists and anti-realists as to whether cognitive values are epistemic or not, in the restricted sense supposed by such a dispute. What is at issue in what follows is the idea of *an priori* distinction between the values that play a role in the formation of knowledge claims and those that do not, between epistemic values, broadly conceived as bearing on qualities of theories, such as simplicity, scope, explanatory power, and values that are 'instead' embedded in human life as always situated and oriented towards particular concerns. And the response to this will depend on whether the process of production of scientific knowledge allows for considering 'problem choice' and 'solution choice' as moments that are distinct in such a way that they would involve values distinguishable in terms of non-epistemic vs. epistemic.

1.1. From abstract theory-choice to situated modeling activity

The threat that participation seems to represent for the epistemic content of science is that value judgment would replace the factual judgments that ground scientific argumentation. Admittedly, value judgments play a role in science: for instance, outcomes of experiments count as the content of factual judgments only under certain experimental conditions, those recognized as *good* ones or the result of the analysis of data count as a data-model because, among other things, it results from a *good* method of analysis. But these are deemed reducible, at least in principle, to factual judgments: what it is for experimental conditions or methods of analysis to be good can be expressed in terms of factual evaluation of their conformity to certain standards. And when one asks about why *those* standards, the answer is that they are those that are the best suited for the aims or values of scientific activity (Laudan, 1984). Whatever these values are, reliability of the procedures, simplicity or explanatory power of the theory, they are meant to be assessable independently of the particular social, ethical or otherwise situated commitments of the scientists. And it is true that, when scientific activity is viewed as mere evaluation of theories, with the aims and the norms adequate to these aims taken for granted, it is difficult to imagine that there could be room for values related to particular situations, orientations, interests or concerns (Potter, 2006, 165). Theories are already there, regardless of the particular conditions in which they were obtained, as if waiting to be chosen, purporting to represent the same thing, which thing is also already there, as if waiting to be represented, abstracted from the conditions in which it came to be understood as what is to be represented. If it is from this retrospective standpoint that we look at scientific activity, in a view 'from above' as van Fraassen calls it (forthcoming), then the values or norms that it involves can only be seen as independent of anything related to particular situations or concerns, as instead responsive to natural or rational necessities. And participation can only appear as a threat to this responsiveness.

This conception of scientific knowledge is unable, however, to make sense of most scientific practices, of how scientific knowledge is obtained and how it 'works', of what scientists are doing when they produce scientific understanding of a particular phenomenon. For scientists are not working with general theories but with particular models (Hughes, 1996; Morgan and Morrison, 1999; Bailer-Jones, 2003). The point is not merely to substitute assessment and choice of model for assessment and choice of theory. It is to be more realistic about scientific activity by attending the way in which models function and the conditions under which they are *obtained.*

1.2. The role of situated values in the production of models

Modeling takes place in the context of a *situated* activity, a practice. Modeling activity in natural science, is situated somewhere and at a certain moment, and whether it is a 'model of' or a 'model for', it is oriented and produced as a response to a problematic situation in the world (by contrast to a mathematical problematic situation). Modeling takes place in the world and is directed at the world, in the context of a certain domain of action, intervention in the world, transformation of the world (Rouse, 2002, 177). The intended domain of intervention conditions what kind of model will count as relevant depending on what matters, what is important in that context, what has to be accounted for and what has to be taken into account: "It is through the model users that a model can be intended to have a certain function and that it can be intended to be about certain aspects of a phenomenon." (Bailor-Jones, 2003). Models are tested against factual statements, sometimes even against each other and arguments for choice may then well appeal to values that are traditionally identified as 'cognitive' such as simplicity, computational cost, empirical precision, scope etc. Commitments to such values may contribute to which methodological procedure is preferable, and what counts as factual judgment.

Directedness and situatedness do not allow, however, 'epistemic' to be understood as necessarily independent of social or ethical considerations, nor, more generally, of values as belonging to a certain category, in the abstract, independently of the situation in which they come to play a role. Consider the scientific prediction that radioactive contamination in Cumbria's hills, after the Chernobyl nuclear accident, would soon decline – justifying the policy of restricting sales and movement of sheep in the area. That prediction was based on a model of cesium distribution for clay mineral soil, explaining how new grass growths are uncontaminated thanks to radiocesium being immobilized in the soil. It was certainly a good model, for *that.* Unfortunately it was not a good model for Cumbria' geological and vegetal environment, where acid peaty soils predominate. (Wynn, 1989; 1996, 64-66) The model for *that* situation could have been improved by taking into account the local knowledge of farmers, as was the scientific understanding of flooding in the Kanyapella Basin when the local knowledge about past flooding, ecology and management of the basin was taken into account (Roberston and McGee, 2003). What the example of the prediction of the evolution of radiocesium contamination shows clearly is that whether a model is a good model, whether it provides a good account of a phenomenon, and what sort of consideration are relevant for its construction depend on the purposes for which one seeks an understanding of the phenomenon (Cartwright, 1999). It depends on what is considered, in Rouse's terms, as being at issue and at stake: whether for instance it is to find a model of a laboratory phenomenon, for the sake of producing a general explanation or a model of a real world phenomenon, which matters to the conditions of life of a group of people. Considerations related to what a model should be a model of or a model for, and to what kind of model is then to be sought, obviously, have a social or even ethical dimension. However, if by contributing to the formulation of the problem, of what we want a model of or for, these considerations and the values that motivate them contribute also to the definition of what can count as a solution, do they not partake in the production of knowledge and therefore pertain to the epistemic realm?

2. Participation in What? Problem-choice *vs.* Problem-solving.

It is here that the distinction, in the development of scientific knowledge, of three separate moments comes into play. The argument runs as follows. It is one thing to say that social or ethical values contribute to the formulation or the choice of a problem. it is a very different thing to claim that they have a cognitive/epistemic function. Social, ethical and economical values can play a role in the choice of problem, but not in solving the problem, and must not, for the sake of science impartiality (Lacey, 1999). Accordingly, they are not cognitive/epistemic.

2.1. Participation must be limited!

From this perspective participation is a problem for epistemology, for it generally aims at much more than the choice of problem. Consider the mobilization against vaccination in the UK (Leach, 2005; Leach and Scoones, 2007). Those who are reacting are not simply pointing to a problem, the consequences of the vaccination, they are contesting a certain formulation of the problem, and the way the situation is addressed, for example, what counts as evidence or as a good reason. In fact, they are discussing the very terms of scientific debate (Epstein, 1996, 12-13 in Leach, 2005, 3). Participation often aims to take part not merely in the choice but also in the formulation of the problem, that is, the determination of what has to be accounted for and what has to be taken into account, what is at issue and what is at stake. What was at issue for the Cumbria farmers was the remaining contamination of their soil, the adequacy of experts' models, what had to be taken into account (specific features of their environment, like the soil or the proximity to the nuclear center Sellafield). As with Cartwright's wish that the life of women dying of cancer, rather than explanatory or unificatory power, become what is at stake in scientific research (Cartwright, 1999, 18), participation often aims to change the stakes of research. The formulation of the problem, in terms of the formulation of what has to be accounted for, what has to be taken account, what are the stakes, goes well beyond pointing to a certain problem. It puts a constraint on the way in which a model should represent or address a phenomenon, a constraint on what can count as a data-model of this phenomenon and on the norms of evaluation of the correctness of the theoretical model.

The idea that participation beyond the mere choice of problem can be an epistemic threat rests on the same presumption as does the categorization of values into epistemic and non-epistemic kinds. According to that presumption, choosing a problem is merely pointing to something to be modeled or to be measured construed like an external constraint on what a good model, a good experimental procedure, or a good measuring device, are. This choice may well involve non-epistemic values. Solving the problem, on the other hand, is a matter of finding the adequate measuring device or the adequate model, and the judgment of adequacy, if it is to respect this external constraint, can only allow values strictly related to the evaluation of the experimental procedure or the model. Knowing is here somewhat like the children's game where one has to find, among different pieces with different shapes, the one that fits the shape cut on top of the box. Objects measured and measuring device, structure modeled and modeling structure, are logically independent from one another, but related to one another, thanks to scientific skills, by an external, contingent relation. What is to be measured acts as a selector of the measuring device that will be able to measure it, just as what is to be modeled does with respect to the right model. If the norms governing the production of scientific knowledge were indeed responsive to a constraint prior to and defined independently of scientific activity, transcendent to the practice itself (Rouse, 2002, 74) then, yes, participation should be strictly contained. For otherwise it would make the norms that govern inquiry sensitive to particular, contextual considerations embedded in the situation from which non-scientists perceive the problem, thereby rendering the inquiry non scientific.

2.2. Measuring, Modeling: Interaction vs. Intra-action

The image of an external and contingent relation between what is modeled and what is modeling, what is measured and what is measuring, which support the idea that science can be divided in two separate moments, first, choosing the problem, then solving it, rests on a retrospective and static view of scientific knowledge. It is oblivious to the conditions under which scientific knowledge is produced in real time, in practice, oblivious to the conditions under which something can be recognized as what is measured or what is modeled. It is precisely to counteract this conception of measurement that Karen Barad (2007) develops the conception of *intra-action*, which Rouse extends to the other relational performances that constitute the practice. I see the possibility for a similar development, regarding the practice of modeling, in Mauricio Suárez' view (Suárez 1999) of the target of the model as 'built-in' the model. The relation between the model and what it represents would then be understood as an intrinsic relation, emerging through the practice of modeling. What is the problem that is addressed by a certain type of scientific investigation can be seen as the starting point only retrospectively and by taking a snapshot of the open-ended process of investigation; in the real time of scientific activity, the problem, what is at issue, is specified in the course of the investigation. The formulation of the problem and the form of the investigation are dynamically and intrinsically related to one another, in the same way as what is measured and what is measuring, or what is modeled and what is modeling, are.

Instead of being logically independent and contingently related, through interaction or similarity, what is measured and what is measuring, what is modeled and what is modeling, have to be understood as co-emerging, co-constituted in the practice of measuring and modeling. Object measured and measuring device are not defined independently of one another and of the particular context of an experimental arrangement: "[C]oncepts are defined by the circumstances required for their measurement" (Barad, 2007, 109). Karen Barad (2007,

114) gives the example of two mutually exclusive experimental set-ups using the light scattered by a particle. When the light is directed towards a fixed photographic plate, the light is a part of the measuring device recording the position of the particle; when the light scattered encounters a plate, which instead of being fixed is movable, it is the light's momemtum that is measured, and the light is part of the object measured. Rouse (2002, 276) proposes another example: in one case, a collection of plants are grown under similar environmental conditions, in another, clones of the same organism are grown under different environmental conditions. In the first case, what is measured is the differing adaptive response of organisms to a specific selective environment, whereas in the other it is the relevance of these environmental differences for the phenotypic expression of the organism. What is measured and what is measuring are not determined independently of a particular experimental arrangement, as if the only function of the experimental procedure was to put them in relation with one another and to enable the former to have an effect on the latter. The general lesson to draw is that we can only speak of what is measured by specifying a procedure of measurement. It is not the trivial claim that different procedures are required to ascribe values to different concepts; it is that the procedure is constitutive of the concept. As says Wittgenstein, "what 'determining the length' means is not learned by learning what *length* and *determining* are; the meaning of the word 'length' is learnt by learning, among other things, what it is to determine length." (Wittgenstein, II, xi,) And scientific *investigation* is generally not just a matter of applying procedures of measurement already established, but of specifying, in new circumstances, what counts as procedure of measurement, and thereby specifying what the concepts referring to the quantities that are measured are and what sort of objects the objects that are described with these concepts are. What will be, at some point, and maybe temporarily, understood as what is measured depends crucially on the experimental arrangement and different arrangements manifest different orientations as to what is important, what is interesting, what has to be taken into account. Which experimental arrangement is appropriate is certainly a normative question in the sense that not anything can count as a good experimental arrangement. But the norms cannot be seen as bearing on scientific practice as from outside. They depend on what is viewed, at some point, as what is at issue and what is at stake in a particular type of investigation. If what is at issue is to understand what is happening in the messy world, as in Cumbria in 1987, laboratory arrangements oblivious to the particularities of this situation will not be appropriate.

The distinction between theory and experiment as two distinct moments of scientific activity certainly makes the situatedness of scientific activity difficult to accept. Theories are seen as formalizable structures, independent of any particular situation. Even when theories are conceived in terms of models, but abstracted from the practice of modeling, what is modeled can only be seen as contingently related to the model, as the object measured is contingently measured by a certain apparatus. Considered in the context of their construction, a model aims to fulfill a certain function, and the choice of function depends on which knowledge we want, what we want our model to account for and to take into account. A problem is not chosen; it is formulated. The formulation of the problem depends, contextually, on what knowledge we already have, what we are interested in, what are the priorities and goals of the research. And it conditions what may count as an empirical ground for the assessment of the model, what is relevant and how it is relevant. Johnson (2002) presents a striking example. He considers three competing models of attention in cognitive psychology and argues that each of them, "involves distinct ontologies of entities and processes and entails a specific set of values that guide research on attention". They are not three models of the same thing; it is rather that they start from different conceptions of what attention is, of what has to be accounted for, and of what will or what can count as relevant evidence. The same applies to models of cognition: the models developed in the context of computationalist and enactive perspectives are not of the 'same thing'. The former seek to account for computational abilities to solve well-defined problems, independent of the cognitive system, whereas the latter sees cognition as creative and situated, and knowledge as depending "on being in a world that is inseparable from our bodies, our language, and our social history- in short, from our embodiment." (Varela, 1991, 149) What is considered as what has to be accounted for and what has to be taken into account is different for these two approaches and that conditions what kind of model is relevant.

When the model is considered retrospectively, in isolation from the context of its production, the representational power of the model, that it is a good representation of a certain phenomenon, seems to be as an additional feature, bestowed on the model by an external relation of, say, (partial) isomorphism or similarity that it entertains with the data-model of the phenomenon. When one, by contrast, considers a model as emerging from a process of construction, its representational content, what and how it represents, appears as 'built in' the model through the practice of modeling; it is inseparable from the process of construction. It is that and how it can be used that makes the model a representation. The intended use of the model is not something external to the model and defined independently of the process of modeling, it is “an essential part of the model itself” (Suárez, 1999). If what is modeled is constituted through the practice of modeling, the formulation of the problem, of what we want a model for or a model of, cannot be conceived, in general, as a moment separate from, prior to the production of the model -- it is an intrinsic part of the practice of modeling.

III. Can Cooperation Form a Practice?

The above response to the alleged epistemic threat of participation makes visible some characteristics of the production of scientific knowledge, the situatedness and directedness of scientific activity as a practice. Is this enough, however, to make it conceivable that the involvement of the public in scientific activity can take the form of constructive interaction, cooperation between non-scientists and scientists constitutive of an epistemic practice? Is it enough to make it conceivable that the involvement of the public in science can take the form of a participation in science, that is, of a new kind of scientific practice?

1. What Binds the Practitioners Together?

1.1. <u>The Externalist View</u>

The involvement of the public in science seems to contradict a pervasive way of conceiving of what makes for the unity of a practice and of what binds its practitioners together: regularities in performances grounded in something practitioners tacitly share. The problem, on this conception, is not that non-scientists’ judgments are situated and oriented in opposition to scientific judgments. The problem is rather that they are not situated in the same way. Think of farmers and experts regarding radioactive contamination, or of parents and of scientists regarding vaccination. Their dealings with and understandings of the world are undoubtedly associated with different performances, expressing different skills, presuppositions and beliefs. How farmers and epidemiologists relate practically and linguistically to a sheep is so different that one may say that the sheep has, in fact, several identities belonging to different practices, different ways of locating ‘sheep’ in distinct networks of rational and practical performances (Law and Mol, forthcoming).

Because of this, it seems as difficult to understand how farmers could take part in a scientific practice as it is to imagine, to take another but telling example, how phenomenologists could cooperate with neuroscientists in a scientific study of experience. It seems that what ‘experience’ means and what experience is, for phenomenologists and neuroscientists, is simply not the same thing. For the unity of a practice to be ensured non-scientists would have to assimilate scientists' ways of acting and thinking; this would make it pointless to speak of the participation of non-specialist. In fact, however, phenomenologists do take part in neuroscientific studies of consciousness, through what is known as 'neuro-phenomenological practice' (Varela, 1997; Gallagher and Varela, 2003). The phenomenologists that are involved see this cooperation as a source of enrichment for the phenomenology of experience, whereas the neuroscientists see it as the only way to carry out a scientific study of experience. The possibility of neuro-phenomenological *practice* is, however, difficult to reconcile with a conception of practice as grounded in a principle of homogeneity.

As Turner (1994) argued at length, how such a principle could explain regularities in practitioners' performances is in fact difficult to understand. What kind of thing could it be that would be non-public, since it is tacit, and common since it is shared? What kind of transmission among people would allow for a transmission with no alteration, and in what sense of explanation, by of what kind of process, would the regularities be explained? Turner proposes that regularities are emergent patterns as in connectionist networks (Turner, 2001). The trouble with that analogy, however, is that either the connectionist system is simply seen as an example of auto-organized system, and then the analogy is oblivious to the normativity that characterizes practice by opposition to habits (Barnes, 2001, 26). Or, it is seen as a cognitive model, and then it supposes an external norm built-in the system by the modeler. There certainly is a tendency, as Risjord (2007) argues, to conceive of the norms of evaluation of individual performances as external to the practice itself and according to which the limits of the community of practitioners could be drawn. That may well reveal the importance norms play in practices but it is in full contradiction with certain crucial features of the practice of science. In that view, practitioners are seen, to use Barnes' terms, as a "unitary collective entity" of individuals oriented by, "moved by a single object or essence" (p.24), identified to 'the practice'. Their performances are submitted to and evaluated according to norms that are constitutive of 'the practice' and these norms are external in that they are not affected by what the practitioners are doing. But the way in which Galileo was measuring the temperature, with an instrument also sensitive to air pressure, or in which he ascribed 'same velocity' to falling objects, are wrong. The way in which Faraday proposed to conceive of and experiment on magnets was completely different from the way it was done before. Scientists have their papers reviewed and their arguments and methodologies criticized. Is their status as practitioners called into question? Doesn't that show, on the contrary, that they are recognized as practitioners?

In his alternative to both individualistic and collectivist approaches, Barnes emphasizes the interaction between practitioners and their mutual susceptibility. Practices are collective accomplishments of individual "concerned all the time to retain coordination and alignment with each other *to bring them about*". (p.24, it. added) The individuals are responsive to one another as they interact "*in order to* sustain a shared practice." (p.25, it. added) Even though Barnes insists that the practitioners "are oriented towards each other" rather than towards "a single object or essence", the interaction is still a means for a collective object. There is something that is collectively accomplished and whether an individual qualifies as a practitioner depends on whether s/he does things in such a way that s/he can contribute to the enactment of this collective object. The mutual susceptibility of the practitioners is a means for alignment, uniformity. If performances are accomplished differently, it is only "slightly differently" and only "in varying conditions and circumstances." (p.25) This view may be adequate for the practice of riding in formation, that Barnes takes as an example of practice, but it falls short of doing justice to the heterogeneity and dynamics of scientific, and more generally, linguistic practice. In these cases, doing things wrong or in a new way does not amount to not being a practitioner, not even to being a bad practitioner. But doing just the same as the others might well.

1.2. The View from Within: Practice as Normative Accountability

Rouse endorses Turner's critiques of the concept of practice, but far from following him in rejecting the concept, he reads the argument as revealing the importance of conceiving practice normatively rather than as regularities (Rouse, 2002, 168). According to Rouse, "not all practitioners perform the same actions or have the same presuppositions, but practitioners and other constituents of a practice are accountable for performances or presuppositions that are inappropriate or otherwise incorrect." (p.169) What binds the practitioners together is not something they mysteriously have in common, or something they all contribute to realize, but instead that they interact and are accountable to each other and to certain norms for what they do and what they say.

This shift is obviously crucial for the possibility of regarding collaborative interactions between non-scientists and scientists as cases of scientific *practice*. What is important to qualifying as practitioners in the same practice is not sharing certain ways of doing, beliefs or presuppositions, it is being accountable to certain norms, it is one's performances and utterances being subject to questions, to demands of justification, to criticism, to constructive elaboration, being something that matters to the other, something that can make a difference to their own performances and utterances. One of the main complains expressed by the Cumbria's farmers, indeed, was that they were ignored, that what they were saying when confronted with discrepancies between the prediction of the experts and what they experienced was not heard, not even criticized or corrected; it simply made no difference to the way the situation was handled. By contrast, practitioners' performances respond to one another, whether critically or constructively.

How could there be heterogeneity in performances and accountability to the same norms? One may say 'scientists have certain norms that structure their activity, but they are obviously different from those farmers are accountable to'. How could they participate in the same practice? It would be a mistake to regard norms as fulfilling the function supposedly played by something that would be external to the practice in the sense of being independent of the particular conditions in which the practice is instantiated, and structuring and delimiting, as from outside, the interactive space of the practice. Scientific practice is constitutively heterogeneous. Take an experimenter trying to model the behavior of the flow behind a cylinder. He is interacting with engineers for the construction of the experimental system, with theorists and mathematicians for the construction of the mathematical model, with computer scientists for the simulation of the models, maybe with civil engineers for the safety of the products he has to add to the flow to enable visualization, and he is interacting with the material set-ups that realize the experimental system and the instruments that enable measurements. They are all constituents of the practice of modeling the flow and they are all, things and practitioners, submitted to normative assessment. The norms that are binding upon the constituents of the practice are not homogeneous; they depend on what is assessed, by whom, at what moment. Just as for Pickering (1995) the practice of modeling is an open-ended process with historically situated moments of stabilization but no external constraints, the normativity of practice according to Rouse comes "from being in the open-ended contingencies of a historical-material situation rather than in a relation to something 'outside' or 'beyond' it." (Rouse, 2002, 76). The practitioners participate in the same practice in that what they do, what they say, is accountable to certain norms constituted from within, in the dynamics of the practice, in response to what they recognize as what is at issue and at stake in the practice.

What is at issue and at stake may not be, and is generally not, completely defined; it is what motivates the practice but it is also what the practice aims to clarify. It is because they are responsive to what is at issue and at stake in agents' performances that the norms, to which these performances are accountable, are neither fixed nor definable from outside, independently of the situatedness and directedness of these performances. Take the convergence of certain computational and connectionist cognitive research programs, previously competitors, and the drift of two conceptions of cognition towards a hybrid one in terms of connectionist-symbolic systems (Cooper and Franck, 1993; Cummins and Schwartz, 1991). With this convergence, different sets of norms distinctively relevant to different conceptions of cognition evolve into a new set relevant to a new conception of what is at issue. Or consider the contrast between representationalist and non-representationalist neuroscientific research programs. What is at issue, recognized as what has to be understood, modeled, or explained in a representationalist approach to cognition is, say, the general identification of neural patterns which count as representations of items in the world. This identification will appeal to the establishment of correlations between certain patterns of neural activity and some features of the world observed by the scientist. For a critical view on the meaning of such correlations see Noë and Thompson (2004, 10-13). In a non-representationalist, enactive approach, what is issue is the emergence of a world, as content of experience, through the sensori-motor interaction with a surrounding; what this content is to be is not specified by the scientist. The criteria for identifying a neural pattern as cognitive will have to make room for the first-person report of the experience. What is at stake here is not simply making sense, theoretically and practically, of what happens to the cognitive system; it is reconciling the scientific study of our cognitive relation with the world and the experience we have of our being in the world, reconciling science and the actuality of human experience: "to deny the truth of our own experience in the scientific study of ourselves is not only unsatisfactory; it is to render the scientific study of ourselves without a subject matter". (Varela *et al.*, 1991, 13)

It is the goal of such a reconciliation that led some neuroscientists to develop neurophenomenological practice. I will finish this paper with a closer look at this practice, for it is deeply instructive regarding the general conditions of realization, in *concreto*, of a practice of participation of non-scientists in science. First, it looks like a most 'unfavorable' case regarding the possibility to achieve a constructive, epistemic cooperation. But more seriously, it strikingly illustrates both the primacy of mutual accountability, rather homogeneity, in constituting a practice, and the internal dynamics of the production of normativity.

2. Mutual Constraints as Condition for Normative Accountability: Insight from Neurophenomenological Practice

The image of a gap between experience and neural description speaks against the possibility of a constructive interaction between phenomenology and neuroscience, just as objections appealing to differences in shared values, performances, or presuppositions speak against the possibility of epistemic participatory practice embracing non-scientists and scientists. Similarly, the project of reducing first-person discourse to neural description resembles the idea that participation would require non-scientists to acquire the ways of doing and thinking of scientists. In both cases, there is no cooperation because only one type of discourse remains. The image of a gap, where experience is supernatural, and the idea of reduction, where the experiential dimension of cognition is irrelevant, are both oblivious to the embodiment of experience or cognition (Depraz, 2002, 86). It is the acknowledgment of this embodiment that leads the participants in neurophenomenology (1) to see cognition as an experience and experience as natural and (2) to reject both the image of a gap and the idea of reduction for a program of coordination of first-person discourse and neural description.

The conditions for such coordination are not simply given. It requires a conception of cognition "as belonging to embodied, situated agents – agents who are in-the-world", and a conception of phenomenology as subject to naturalization. (Gallagher and Varela, 2003, 93) This conception of cognition is in fact an analog of that proposed by Rouse concerning scientific activity. In both cases, it amounts to thinking of performances of cognitive agents as situated and oriented, dynamically intra-acting with the world (Varela *et al.*, 1991, 173-174; Rouse, 2002, 173, 253) In both cases what disappears is the image of an external, transcendent constraint on the norms that guide agents' performances in favor of an image of dynamic, co-constitutive exploration. The re-conception of phenomenology also echoes what was said about participation. Just as the situatedness of value judgment was not peculiar to the non-scientists, subjectivity is not peculiar to phenomenological discourse and phenomenological discourse can and must be inter-subjective, submitted to common norms (Depraz *et al.*, 2003).

In the neurophenomenological study conducted by Lutz et al., several subjects perform identical cognitive tasks. Before, during, and after their performance, while their neural activity is recorded, they describe their phenomenological experience in terms of degree of attention, expectation, surprise. With the disappearance of the idea of an external normative constraint on neural activity the scientist loses the epistemological 'right' to identify, on the sole basis of correlations between cognitive activity and features of the world that he specifies, what counts as neural patterns of cognitive activity; he becomes accountable to the phenomenological discourse concomitant to neural descriptions. As the same time, phenomenological analysis of experience becomes accountable to intersubjective norms of categorization relevant to neuroscientific study.

Speaking of mutual constraints or mutual accountability in terms of coordination could misleadingly suggest that it is a matter of establishing relations between two languages that are independent of one another, of creating external relations. The same misunderstanding threatens the conception of participation in science. In both cases this misinterpretation would depict the dynamics of constructive cooperation as simply a confrontation between different systems of norms where conflicts in assessment are settled authoritatively. In fact, the mutual constraints are generative, they create 'generative passages' (Varela, 1997, 372) where descriptions of neural activity and phenomenological accounts not only constrain but enrich one another (Depraz, 2002, 91). The normativity of this cooperative practice is itself embedded in the dynamics of this co-constitution. I will conclude with a programmatic example. Among the questions that phenomenology has investigated, like how we come to perceive certain things, what part of the body we are aware of, there is what the experience of temporality is. Temporality is also central to neuroscience in relation with the dynamical structure of neural activity. Neurophenomenological investigation provides a framework to articulate these two domains of description and interpretation (Gallagher and Varela, 2003): "phenomenology of time-consciousness can resolve certain problems found in static cognitive accounts of experience" and "the study of cognitive dynamics can contribute to a better understanding of time-consciousness". For instance, Husserlian analysis of time-consciousness in terms of impression, retention and protention can serve as a constraint and resource for the study and interpretation of the neural activity in terms of auto-organization and on-going, historical and contextual processes of emergence of instable neural patterns of synchronizations (pp.122-124). In return, the dynamical description of cognitive activity offers a conceptual resource to reconceive certain phenomenological aspects of time experience, for instance the disruption of protention, and a guide to explore or refine others, like the inter-relation between different components of phenomenological experience or the temporal structure of consciousness. It is expected that, in return, this exploration or refinement will provide new phenomenological constraints on the description of the neural dynamics. As for any scientific practice, the normativity of a cooperative practice can only be a dynamic normativity, generated from within, in response to the elucidation and reformulation of what is at issue.

REFERENCES

Bailor-Jones, D. When Scientific Models Represent. *International Studies in the Philosophy of Science*, 17, 59-74, 2003.

Barad, K. *Meeting the Universe Halfway*. Durham&London: Duke University Press, 2007.

Barnes, B. Practice as collective action. In T. Schatzki, K. K. Cetina and E. von Savigny (eds), *The Practice Turn in Contemporary Theory*. New York: Routledge, 2001.

Campbell, L. Science Impact Collaborative Decision Analysis and Joint Fact Finding, LMIT-USGS, 2006.

Cartwright, N. *The Dappled World: A Study of the Boundaries of Science.* Cambridge: Cambridge University Press, 1999.

Cooper, R. P. and Franks, B. How Hybrid should a Hybrid Model Be? In *Proceedings of the Workshop on Combining Symbolic and Connectionist Processing.* 11th European Conference on Artificial Intelligence. Amsterdam, August, 59-67, 1994.

Cummins, R. and Schwarz, G. Connectionism, Computation and Cognition. In T. Horgan & J. Tienson (eds.), *Connectionism and the Philosophy of Min*d. Dordrecht: Kluwer, 1991.

Depraz, N. Francisco Varela's Neurophenomenology of Radical Embodiment. *Phenomenology and the Cognitive Sciences*, 1, 83-95, 2002.

Depraz, N., Varela, F.J and Vermersch, P. *On Becoming Aware*. Amsterdam: John Benjamins, 2003.

Douglas, H. Inserting the Public into Science. In S. Maassen and P. Weingart (eds), *Democratization of Expertise ? Exploring Novel Forms of Scientific Advice in Political Decision-Making. Sociology of Sciences*, 24, 153-169, 2004.

Epstein, S. *Impure Science: Aids, Activism and the Politics of Knowledge*. Berkeley: University of California Press, 1996.

Ezrahi, Y. *The Descent of Icarus. Science and the Transformation of Modern Democracy.* Cambridge, MA: Harvard University Press, 1990.

Flicker, S., Savan, B., Kolenda, B. and Mildenberger, M. A Snapshot of Community-Based Research in Canada: Who? What? Why? How? *Health Education Research*, 2007.

Gallagher, S. and Varela, F.J. Redrawing the Map and Resetting the Time. *Canadian Journal of Philosophy*, 29, 93-127, 2003.

Hisschemoller, M. Knowledge Production and the Limits of Democracy. In S. Maassen and P. Weingart (eds), *Democratization of Expertise ? Exploring Novel Forms of Scientific Advice in Political Decision-Making*. *Sociology of Sciences*, 24,189-207, 2004.

Hughes, R.I.G. Models and Representation. *Philosophy of Science*, 64, S325-S336, 1997.

Johnson, M. Metaphor-Based Values in Scientific Models. In L.Magnani and N.J. Nersessian (eds.) *Model-Based Reasoning: Science, Technology, Values.* Kluwer Academic Publishers: New York, 2-19, 2002.

Jurgens, I. Science-Stakeholder Dialogue and Climate Change. Towards a Participatory Notion of Communication. In F. Bierman, S. Campe and K. Jacob (eds) *Proceedings of the 2002 Berlin Conference on the Human Dimensions of Global Environmental Change*. Amsterdam, Berlin, Postdam and Oldenburg: Global Governance Project, 87-101, 2004.

Lacey, H. *Is Science Value Free? Values and Scientific Understanding*. London and New York: Routledge, 1999.

Laudan, L. *Science and Values*. Berkeley: University of California Press, 1984.

Law J. and Mol, A.M. The Actor-Enacted: Cumbrian sheep in 2001. In L. Malafouris and C. Knappett (eds.) *Material Agency: Towards A Non-Anthropocentric Approach.* Springer, forthcoming.

Leach, M. MMR Mobilisaton: Citizens and Sciences in a British Vaccine Controversy. Working Paper 247, Sussex: Institute of Development Studies, 2005.

Leach, M. and Scoones, I. Mobilising Citizens: Social Movements and the Politics of Knowledge. Working Paper 276, Sussex: Institute of Development Studies, 2007.

Morgan, M. and Morrisson, M. *Models as Mediators*. Cambridge: Cambridge University Press, 1999.

Noë, A. and Thompson, E. Are there Neural Correlates of Consciousness? *Journal of Consciousness Studies*,11, no1, 3-28, 2004.

Pickering, A. *The Mangle of Practice*: *Time, Agency, and Science.* Chicago: University of Chicago Press, 1995.

Potter, E. *Feminism and Philosophy of Science. An Introduction*. Routledge: New York, 2006.

Risjord, M. Who are ‘We’? Dissolving the Problem of Cultural Boundaries. *The Modern Schoolman*, forthcoming 2007.

Robertson, H. and McGee, T. Applying Local Knowledge: the Contribution of Oral History to Wetland Rehabilitation at Kanyapella Basin, Australia. *Journal of Environmental Management*, Vol. 69, 275-287, 2003.

Rouse, J. *How Scientific Practice Matter. Reclaiming Philosophical Naturalism*. Chicago: The University of Chicago Press, 2002.

Rouse, J. Social Practices and Normativity. *Philosophy of the Social Sciences*, 37, 46-56, 2007.

Rowe, G., Marsh, R. and Frewer, L.J. Evaluation of a Deliberative Conference. *Science, Technology and Human Values*, vol. 29, no1, 88-121, 2004.

Turner, S. Throwing out the Tacit Rule Book: Learning and Practices. In T.R. Schatzki, K. Knorr Cetina and E. von Savigny (eds.) *The Practice Turn in Contemporary Theory*. London: Routledge, 120-130, 2001.

Turner, S. The Social Theory of Practices: Tradition, Tacit Knowledge, and Presuppositions. Cambridge: Polity Press; Chicago: University of Chicago Press, 1994.

Suárez, M. Theories, Models and Representation. in L.Magnani and N.J. Nersessian (eds.) *Model-Based Reasoning in Scientific Discovery*. Kluwer Academic Publishers: New York, 75-83, 1999.

van Fraassen, B. C. *Scientific Representation: Paradoxes of Perspective*. Oxford University Press. forthcoming.

Varela, F.J., Thompson, E. and Rosch, E. *The Embodied Mind. Cognitive Science and Human Experience*. Cambridge, MA: MIT Press, 1991.

Varela, F.J. The Naturalization of Phenomenology as the Transcendence of Nature. *Alter*, 5, 1997.

Wittgenstein, L. *Philosophical Investigations*. Oxford: Blackwell Publishing, 3rd. ed, 2001.

Wynne, B. Sheep Farming after Chernobyl. A Case Study in Communicating Scientific Information. *Environment*, vol.31, no2, 11-39, 1989.

Wynne, B. May Sheep Safely Graze? A Reflexive View Of The Expert-Lay Knowledge Divide. In S. Kash, B. Szerszynski and B. Wynne (eds.) *Risk, Environment and Modernity: Towards a New Ecology*. Thousands Oaks, CA: Sage Publications, 44-83, 1996.