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The Situated Materiality of Scientific Practices: Postconstructivism – a New Theoretical Perspective in Science Studies?[1]

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Abstract

For about 20 years, a rather wide range of conceptual approaches to the social study of science and technology have emerged which have occasionally been labelled "postconstructivist". Although these conceptions differ in various respects, they have in common a twofold opposition: against traditional representationalist realism as well as "classical" social constructivism established by the "sociology of scientific knowledge" (SSK). In order to escape the pitfalls of both these views (and to overcome the familiar, yet unfruitful opposition between them), postconstructivist perspectives understand and study the sciences primarily in term of their situated material and discursive *practices*. The present article starts with a brief retrospect on why and how since the mid-1980s postconstructivist trends have questioned not only rationalist and realist accounts but also the conceptual foundations and background assumptions of SSK's claim to explain sociologically the content of science. Subsequently, the central features of a postconstructivist perspective in science studies are outlined, referring to the key concepts of "knowledge", "practice", and "performativity". The fruitfulness of a theoretical approach focusing on scientific practices is illustrated using the example of the increasingly important issue of scientific non-knowledge: In the same way that knowledge is not to be comprehended as simply the mental "possession" of a knower, non-knowledge is not merely the lack thereof but an (unrecognised) implication of materially and socially situated research practices. Finally, it is emphasised that postconstructivist science studies should not be misunderstood as claiming (as do realism and constructivism) to provide a meta-theoretical explanation or legitimation of science. Instead, postconstructivism should be conceived as a situated critical effort to challenge one-sided accounts of scientific knowledge and foster more self-reflective research practices.

[1] I would like to thank two anonymous reviewers as well as the editors of the Special Issue for their helpful comments on an earlier draft of this article.

1 Introduction: Why Postconstructivism?

Introducing another "post"-term into social science debates (after postmodernity, poststructuralism and so forth) will doubtlessly raise a lot of well-founded objections. Nevertheless, in the present article I hope to successfully establish my thesis that the hitherto only sparsely used concept of "postconstructivism" is appropriate and important, if not indispensable, in order to denote a new and distinct research perspective that has emerged in science and technology studies over the last two decades. Although, in some cases, the "boundaries" might not appear to be clear-cut, this perspective on a conceptual level differs significantly from the "classical" social constructivist sociology of scientific knowledge, but no less, for instance, from the so-called "operative constructivism" of Niklas Luhmann's theory of social systems. In addition, postconstructivism might turn out to be a more promising approach to the empirical study of important issues in the area of science studies than social constructivism.

In order to substantiate these claims, I would first like to briefly review how and why it is that postconstructivist views have been emerging within science and technology studies for around 20 years or so. Second, I shall explain in greater detail the general outlines and central features of a postconstructivist perspective in science studies, focusing primarily on the work of the feminist theorist and physicist Karen Barad, the sociologist of science Andrew Pickering and the philosopher Joseph Rouse, all of them as yet not very broadly perceived within the German-speaking debate.[2] Thirdly, using the example of scientific ignorance or non-knowledge with special attention to what I have elsewhere termed "unrecognised non-knowledge" (Wehling 2004), I would like to illustrate that a postconstructivist perspective is able to provide new and fruitful approaches to both scientifically and politically relevant issues. Finally, I would like to explain briefly how the postconstructivist claim to move beyond the well-established opposition of realism and constructivism should be interpreted and justified. As a result, it might become clear that the somewhat artificial and perhaps only provisional term "postconstructivism" demarcates important differences to both (social) constructivism and (representative) realism and contributes to clarifying and developing the conceptual foundations of science studies.

2 The Emergence of Postconstructivist Perspectives in Science Studies

Why did postconstructivist interpretations emerge in the mid-1980s in implicit or explicit opposition to the constructivist "sociology of scientific knowledge" (SSK) that itself had become established as a new and quick-to-dominate paradigm in science studies only ten years before? In his book *Scientific practice and ordinary action*, published in 1993, Michael Lynch, from his ethnomethodological point of view, stated a crisis of the constructivist and relativist sociologies of science and assumed that one could

[2] For pragmatic reasons, I concentrate in this article primarily on science studies. Nina Degele has recently reclaimed the importance of a postconstructivist view for technology studies as well, in order to bring the "materiality of things" (back) to the fore (Degele 2002: 127). Yet, by speaking of "realist postconstructivism" with regard to Bruno Latour's and Michel Callon's *actor-network theory* (ANT) (ibid: 126) she invites two possible misunderstandings: *either* it is suggested that postconstructivism ultimately is a renewed and extended form of realism *or* that there might also be a "constructivist postconstructivism" as opposed to a realist variant. By contrast, what I shall attempt to show is that postconstructivism aims to question and transgress the entrenched dichotomy of realism and constructivism (cf. Barad 1996; Rouse 2002b; Asdal 2005).

observe, as a consequence, the emergence of "postconstructivist trends" (Lynch 1993: 107-113).[3] These various trends amounted to questioning the key terms of "the strong program's agenda to give sociological explanations of the content of science" (ibid.: 112): how can, for instance, "social" factors be discriminated from "cognitive" or "natural" ones, and what should be considered the "content of science"? According to Lynch, "the most radical and interesting of the postconstructivist sociologies of scientific knowledge" (ibid.: 111) at that time appeared to be the *actor-network-theory* (ANT) which, some years before, based on the work of Bruno Latour, Michel Callon and others, had developed into a novel and independent approach in the area of science and technology studies. With regard to the conceptual foundations of science studies, Latour (1992) had argued for "one more turn after the social turn" and criticised the "complete asymmetry" in David Bloor's famous symmetry principle: "Society was supposed to explain nature." (Latour 1992: 278) In their exchange with Callon and Latour, Collins and Yearley quite straightforwardly expressed this explanatory strategy by giving the following methodological advice to their adversaries: "We provide a prescription: stand on social things – be social realists – in order to explain natural things." (Collins/Yearley 1992b: 382). As Rouse convincingly argues, this "prescription" results in both "the reification of natural scientific knowledge as a determinate explanandum and the reification of some aspect of the social world as a potential explanans" (Rouse 2002a: 136). It was the implicit asymmetry in SSK as well as this twofold reification that ANT sought to overcome by its "extended symmetry principle" which ascribed the same explanatory power to non-human "actants" as to humans and refused to make any a priori distinction between them.

Presumably, ANT is – particularly within the German-speaking scholarly discussion – still the best-known and most prominent conceptual effort to get beyond certain shortcomings of SSK's social constructivism (cf. Krohn 2000; Degele 2002). It nevertheless remains highly contested, not least with respect to the proclaimed "symmetry" between human and non-human actants.[4] However, within the field that might be characterised as "postconstructivist", there is, besides ANT, a wider range of different, yet no less important and perhaps even more sophisticated, approaches to the social study of science that have emerged during the last 15 years.[5] Some of those, as for instance Pickering's "pragmatic realism" or Karen Barad's "agential realism", label themselves "realist" in order to make still more explicit the conceptual difference from (social) constructivism. Against this background, the philosopher of science

[3] A few years later, in his outline of "a constructivist genealogy of social constructivism", Lynch (1998: 18) referred to those conceptual developments in terms of a "post history" of social constructivism in the course of which "hybrid constructivisms" proliferated and a loose consensus emerged that practice "is the heart" of the social study of science. Remarkably, Lynch (1993: 91) did not integrate the studies of laboratory research inspired by ethnomethodology (e.g. Lynch 1985) into the "constructivist line". In fact, those studies show at least as many conceptual intersections with "postconstructivist" accounts of science and technology as with classical social constructivism..

[4] For rather different objections see, for instance, Collins/Yearley 1992 a, b; Pickering 1995: 13-15; Weingart 2003: 76-77.

[5] The same applies for technology studies: apart from ANT, one could mention here, for instance, the so-called "workplace studies" which have emerged in recent years; these draw on ethnography, ethnomethodology, and conversation analysis and show a lot of overlaps with postconstructivist accounts of scientific practices. In workplace studies, technical work is conceived as both socially and materially "situated practice in which the context is part of the activity" (Orr 1996: 10). A survey of this field has been given by Knoblauch and Heath (1999).

Joseph Rouse in a recent review essay has re-adopted the term "postconstructivism" (without making any recognisable reference to Lynch) and stated that particularly "the work of cultural historians, anthropologists, and feminist theories of science has taken postconstructivist science studies in important new directions" (Rouse 2002b: 62). In his essay, nicely titled "Vampires: Social Constructivism, Realism, and Other Philosophical Undead", Rouse mentions as proponents and promoters of postconstructivism, among other scholars, Donna Haraway, Evelyn Fox Keller, Peter Galison and Hans-Jörg Rheinberger and considers the book *Science as Practice and Culture*, edited by Pickering (1992a), a "benchmark for sociologists' shift away from social constructivism and its underlying humanism" (ibid).

What precisely is at issue in the postconstructivist criticism and shift away from SSK's social constructivism? Despite the well-known multiplicity and heterogeneity of constructivist approaches within science studies, answering this question requires at least a preliminary, minimal definition of social constructivism to be offered. I borrow such a definition from Rouse's book *Engaging Science,* in which he characterises social constructivist science studies by the following two features: "First, all scientific beliefs must be accounted for by *social* factors, whatever that analytical category turns out to include; second, any adequate interpretation of scientific knowledge claims must be neutral with respect to their epistemic or political legitimacy and hence to that extent is committed to some form of epistemic relativism." (Rouse 1996a: 9 – original emphasis) Given this background, the objections raised by Latour, Pickering and others were directed against a tendency towards a sociological reductionism in science studies, accompanied by what Collins and Yearley had termed "social realism" which inclines to reify certain aspects of social life (interests, power relations, cultural identities and so forth) into a stable, self-evident and uncontestable reality. It was argued by SSK's critics that the exclusive focus on (supposedly) "social factors" tends to marginalise or even (almost) completely negate the importance for the establishment of scientific knowledge of non-social, material factors and objects. A striking example of this tendency can be seen in Collins' programmatic statement "that the natural world must be treated as though it did not affect our perception of it" (Collins 1983: 88). Consequently, Collins pleads (1981: 3) for an "explicit relativism in which the natural world has a small or non-existent role in the construction of scientific knowledge".[6]

While the "postconstructivist trends" were highly critical of such claims, they nevertheless refused to return to any form of "traditional" representative realism. Instead, the objective was to articulate a more adequate alternative to representative realism "while avoiding antirealism", as Pickering (1989: 279) has put it. The various postconstructivist approaches thus started to tentatively develop theoretical conceptions which, explicitly or implicitly, aimed to overcome the realism-constructivism-divide. I would like to illustrate this move referring to Pickering's aforementioned paper "Living in the material world", published in 1989, in which he describes his own view, maybe for the first time ever, as "pragmatic realism" in order to demarcate it from both social constructivism and representative realism. According to Pickering, on the one hand, "it is clear that material practice – interaction with the material world – can play a constitutive role in knowledge production" (ibid.: 280). Yet, on the other

[6] It is no coincidence that Niklas Luhmann (1990: 37) affirmatively quotes this statement. In spite of all other differences, the denial of a significant role of the "natural world" indicates and constitutes a remarkable affinity between Luhmann's "operative constructivism" and Collins' "Empirical Programme of Relativism".

hand, the resulting connection "between knowledge and the material world has (…) to be understood not in terms of fixed correspondence but rather in terms of local, potentially unstable coherences achieved between material procedures and conceptual models" (ibid.: 281). What should therefore move to the fore of science studies is "the *making* of coherence" (ibid.: 279 – emphasis added) in an open and contingent process of mutual "interactive stabilization" of cognitive expectations and the effects of experimental practices. In this manner, Pickering (and others) argued, the objectivity of scientific knowledge and its constitutive relation to material phenomena could be re-integrated into the social study of science without falling back into representational realism according to which nature has always existed "out there" exactly like it is depicted by the sciences. Thus, with regard to realism, the decisive shift leads from "representation" or "correspondence" as an abstract philosophical idea (in the sense of an adequation between things and concepts, between reality and theory) to the sociological study of various representational *practices* in science (Lynch/Woolgar 1990), or from "representation of" to "representation as" (Rheinberger 1997: 103).

Yet, to consistently sketch out a postconstructivist perspective requires not only conceptual transformations in the traditional realist philosophy of science but major revisions of the constructivist sociology of science as well. There can be little doubt that these revisions will have to go even beyond the two above-mentioned objections to sociological reductionism and epistemological relativism. For, if one widens the critical stance on social constructivism, one will become aware that the sociology of scientific knowledge – despite all its criticisms of rationalistic philosophies of science – inadvertently shared the premises and hidden assumptions of the latter (in particular a cognitivist focus and fixation on scientific *theories*) to a much greater extent than is usually acknowledged. Before I refer to this point in greater detail in the next section, I would like to briefly draw two rather preliminary conclusions from this brief historical retrospect.

First, it has become clear that postconstructivism should not be understood in a merely temporal sense, as something which simply comes "after" constructivism. Instead, it primarily includes a conceptual dimension which *presupposes* and *builds upon* constructivist science studies and their objections to ahistorical realist and rationalist explanations of scientific knowledge. Accordingly, postconstructivism means and implies a self-reflection of (social) constructivism, not a return to any kind of "pre-constructivist" realism and objectivism. At most, one could speak of a "re-entry" of realism into constructivism, as does Wolfgang Krohn (2000), using the vocabulary of Luhmann's systems theory, with regard to Latour's work. One should, however, not fail to see that such a reentry does not leave unchanged the two seemingly contradictory and incompatible views. For postconstructivism ultimately aims to overcome the rigid and highly polarised opposition of the "philosophical undead" (Rouse) realism and constructivism by questioning the supposedly self-evident premises and hidden assumptions on which this opposition is founded.

Secondly, within the area of science studies there is no *single* established and consistent postconstructivist theory or approach but a rather wide range of theoretical perspectives and research programmes which might be labelled "postconstructivist", though these themselves use quite different terms for their self-description. Besides Latour's and Callon's ANT, these perspectives include, for instance, Pickering's "pragmatic realism" (Pickering 1995), Rheinberger's "epistemology of experimentation" (Rheinberger 1997), Rouse's philosophical "naturalism" (Rouse 2002a), or

Barad's "agential realism" (Barad 1996) and other feminist accounts of science. Nevertheless, in spite of all the differences between theoretical backgrounds, disciplinary contexts, and so on, these perspectives have one crucial and fundamental feature in common which, in a more general sense, allows postconstructivism to be spoken of in terms of an emerging new perspective in science studies: science is conceived and analysed primarily in terms of *practices*, especially of material and performative practices. An important impulse for this shift "from science as knowledge to science as practice" (Pickering 1992b) has doubtless been given by the re-discovery and revalorisation of the experiment and its significant role in the production of scientific knowledge during the 1980s in philosophy, history and sociology of science (see for instance Hacking 1983; Gooding et al. 1989).

In the following section I would like to illustrate some of the theoretical implications and consequences of those developments in greater detail by sketching out the central features of postconstructivism (or, rather, the various postconstructivist perspectives mentioned above). As I indicated at the outset, I shall refer mainly to the work of Barad, Pickering and Rouse. Rouse's sometimes rather pointed reflections and statements are particularly well-suited to making explicit the characteristics of postconstructivism and its theoretical differences from social constructivist sociologies of scientific knowledge.

3 Outlines of a Postconstructivist Perspective in Science Studies

It has become clear that – beyond "bringing back in" material factors and their importance for the production of scientific knowledge – postconstructivism implies a critical, self-reflective evaluation and revision of the premises and background assumptions on which SSK, more implicitly than explicitly, is based. I would like to illustrate the characteristics and the reach of such a self-reflective turn with regard to the following three key concepts and issues: *knowledge*, *practice*, and *performativity*.

3.1 A "Deflationary" and "Non-Reifying" Conception of Knowledge

What "is" knowledge, and in particular scientific knowledge, and how can it be conceived of in a theoretically appropriate and productive manner? Usually, it is understood as something that is "possessed" and "applied" by a knower and transmitted by communicative interaction (cf. Rouse 1996b: 406). Contrary to these common-sense notions Rouse has developed a "dynamic" and "deflationary" account of knowledge, drawing on deflationary conceptions of truth: "In both cases, truth and knowledge, the deflationary move is a shift from thinking about a putative object that a concept could describe to thinking about the practices in which the concept is used." (Rouse 1996a: 199) According to this "practice turn" (Schatzki et al. 2001), knowledge is not to be understood (and "reified") as an independent and coherent entity or object which is discovered by science and thus explains and justifies scientific practices. As Rouse has put it: "There are many appropriate ascriptions of ‚knowing' within the multifarious practices of assessing, attributing, relying upon, or contesting understanding and justification, but there is no *nature* of knowledge underlying these ascriptions." (Rouse 2002a: 179 – original emphasis).[7] Instead, (scientific) knowledge "consists" of nothing but those prac-

[7] In deflationary theories, the same applies for truth: in this case "the truth predicate and the capacity to use it are recognized as indispensable to linguistic and epistemic practices, even though no underlying nature of truth unifies or reifies the instances of its appropriate application" (Rouse 2002a: 179).

tices of generating, attributing, and justifying knowledge, and "the historically situated and contested development of the practices themselves suffices us to understand them" (Rouse 1996a: 200).

From this non-essentialist account of scientific knowledge three far-reaching conclusions can be drawn:

1) First, knowledge is an effect and an implication of situated practices and can only partly and "artificially" be separated and isolated from them: "Knowledge is embedded in our research practices rather than being fully abstractable in representational theories." (Rouse 1987: 24). This insight not only implies that scientific knowledge is far more intransparent, ambiguous and "intrinsically open to multiple interpretations" than rationalistic philosophies of science usually admit (Rouse 1996a: 25-26). In addition, scientific knowledge frequently or even regularly encompasses what Collins (2001: 72), in an illuminating attempt to discriminate different forms of tacit knowledge, has termed "unrecognized knowledge" and "uncognized/uncognizable knowledge". With these categories, he refers to cases when scientists are able to successfully conduct an experiment without being fully and explicitly aware of why and how it works. One should therefore always take into account the possibility of unexpected, unrecognised or only partly recognised effects and implications of research or the technological implementation of its results. I shall refer to this point more broadly below.

2) Given this background, it would be severely misleading to conceive of knowledge primarily or even exclusively as a "possession" or "property" of individuals or social groups such as certain scientific communities. Instead, the attribution of knowledge is "more like a characterization of the situation knowers find themselves within rather than a description of something they acquire, possess, perform, or exchange" (Rouse 1996a: 133). As Rouse emphasises, this does not mean simply rejecting our ordinary ways of speaking and thinking about knowing. "It can be perfectly appropriate to ascribe knowledge to a knower, so long as we understand that correct ascription of knowledge depends on how the knower is situated within ongoing practices rather than simply on whether the knower 'possesses' the right beliefs or skills (...)." (Ibid.) For better understanding, one should be aware that Rouse's concept of practices, following Donald Davidson, includes linguistic or discursive practices as well (cf. Rouse 1996a: 205-236; see also Section 2.2). "Knowing" certain theories or "understanding" certain scientific concepts thus implies and means competently participating in discursive practices of connecting those theories and concepts to other theoretical models and/or phenomena and situations in the world. To illustrate this point, Rouse uses the following example: "Biologists (...) employ a rich terminology to articulate structural features of living cells: nuclei, ribosomes, mitochondria, membranes, Golgi bodies, and so forth. The application of these terms was regularised and is now learned through the use of multiple ‚models'. Schematic diagrams depict these components in structural relationships. These diagrams are connected to cells through various laboratory manipulations (...). With these models available, it is perfectly straightforward to learn to understand (and to utter understandably) sentences employing terms like ‚mitochondria', whose truth conditions encompass events taking place in unexamined cells outside the laboratory setting." (Rouse 1996a: 229).

3) The deflationary, non-reifying account of knowledge results in a significantly modified and extended conception of what should be comprehended as the "content" of science. This point is crucial in order to fully realise the "postconstructivist" objections to social constructivism. As is well-known, the

so-called strong programme outlined by David Bloor in 1976 aimed to explain sociologically "the very content and nature of scientific knowledge" (Bloor 1976: 3). In this context, however, knowledge, or the content of scientific knowledge, were conceived of primarily in terms of cognitive beliefs; this becomes clear when, for instance, Bloor's famous "symmetry principle" claims to explain "true and false beliefs" by the same type of causes (ibid.: 7). The same applies for systems theory which de facto reduces knowledge to communication and the employment of distinctions of "true vs. untrue" (which obviously are qualifications of beliefs or propositions) in the medium of meaning (Sinn), while (material) research practices do not matter in Luhmann's sociology of science. In this manner, constructivist sociologies of science implicitly and inadvertently share the same cognitivist reduction of scientific knowledge to theories, systems of belief and true-or-false-distinctions as did their "counterpart", representative realism (cf. Rouse 2002a: 136). The main reasons for this specific framing of SSK's object of study are to be found in the history of the field, especially in the strong programme's explicit opposition to Mannheim's and Merton's approaches. As is well-known, both of them had ultimately exempted the content of scientific knowledge from sociological study, thus leaving its explanation to traditional rationalist accounts. To sociologically challenge this exemption would seem to be facilitated, as Rouse argues, "if the contested turf were described commensurably" (ibid.: 143).[8]

In contrast, a postconstructivist perspective gives rise to a completely different account of the "content" of science: "Is the content of a science its verbal representation of the world, or the reconfiguration of the world itself through practical engagement with things, people, and prior patterns of talk? The more radical postconstructivist claim is not that the content of a science can be explained by social rather than material or rational 'factors', but that the only coherent notions of content or meaning incorporate the social, material and discursive setting of a science." (Rouse 2002b: 73).

Such a reflective, postconstructivist interpretation of the content of scientific knowledge not only demarcates a crucial difference to social constructivism in terms of theory but also has significant consequences for the analysis of empirically relevant issues, as I will demonstrate later, referring to the example of scientific non-knowledge. Apart from this, there is another important implication of Rouse's claims that I can mention only briefly here. If the "content" of a science not only consists of beliefs and theories but of the entire (i.e. also institutional) setting of scientific practices, then the opposition and presumed incompatibility of an "institutionalist" and a "sociology of knowledge paradigm" that has emerged within German-speaking science studies in the mid-1990s (cf. Schimank 1995a, 1995b; Amann 1995) turns out to be based on questionable premises and appears to be misleading (cf. Böschen/Wehling 2004: 22-25).

[8] It is in this context where Rouse locates the most important and fruitful contributions by scholars of feminist science studies (as for instance Donna Haraway, Evelyn Fox Keller or Karen Barad) to overcoming the tacit continuities of constructivist sociologies with traditional philosophical accounts of science. In particular, "feminist science studies shift their primary object of study from the semantic content of knowledge or belief to a concern with relationships (...) between knowers and known" (Rouse 2002a: 146-147). Their interest in the materiality (and accountability) of these relationships led feminist scholars to oppose also the forms of relativism and "detachment" which seem to be constitutive for SSK's explanatory programme (cf. ibid.: 151-159).

3.2 The Situated Materiality of Scientific Practices

Over the last years, "practice" has become one of the most important but also most strongly contested concepts in contemporary social theory and sociological research (cf. Schatzki et al. 2001; Reckwitz 2003). Given the multiplicity of perspectives from which practices are studied and theoretically understood, "it is not surprising that there is no unified practice approach" (Schatzki 2001: 2). How, then, is the key concept of (scientific) practice or practices to be comprehended within a postconstructivist framework? First, it is important not to misunderstand scientific practice (for instance in narrow terms of experimentation) as opposed to and strictly distinct from theory. Leaving out of account the widely acknowledged "theory-ladenness" of observation and experimentation, one should better conceive of scientific theories "in terms of *theoretical practices* of modelling particular situations or domains; articulating, extending, and reconciling those models and their constituent concepts and techniques; and connecting theoretical models to experimental systems, rather than in the classical sense of *theoria* or through more recent analyses of theories as axiomatic or model-theoretic systems" (Rouse 2002a: 163 – first emphasis added). Against this backdrop, it becomes clear that scientific practices may not be reduced and narrowed to *material* practices but necessarily encompass *discursive* dimensions as well. This understanding of scientific practices as inherently discursive is opposed to both a representionalist account, according to which language simply expresses the given "objective" meanings of things, and a presumedly "materialist" underestimation of the significance of scientific language, reducing it to "mere" rhetorics or literary technologies. As Rouse (1996a: 153) rightly remarks, "(s)ignification in scientific practice (including metaphors and models as well as supposedly 'literal' discourse) is too rich, inventive, and important to be adequately understood in these terms". The eminent role of discursive practices in the sciences as well as their mutual interactions with (if not inseparability from) material, experimental practices are highlighted in Lily Kay's illuminating account of the history of the genetic code. "Encompassing activities such as naming, describing, interpreting, analogising, and signifying discursive practices have formed the conceptual framework guiding molecular biologists in their theorising, experimental design and interpretations (...)." (Kay 1999: 15). Discourses are therefore "a way of thinking and doing" (ibid.: 16).

A second key element of the postconstructivist account of scientific practices is even more crucial, and presumably more unfamiliar and contested within social theory: from the reflections on knowledge and the content of science portrayed in the previous section it follows that practices in this context may by no means be reduced to the doings of social actors (e.g. scientific researchers) "as distinct from the material setting of what they do" (Rouse 2002a: 163). Instead, an adequate conception of (scientific) practices has to encompass the material "configuration of the world" (Rouse 1996a: 133) which makes the activities of individual or collective agents become significant, coherent and intelligible. In explicit contrast to widespread notions of practices as rules and regularities of social actors' doings, Rouse stresses that "practices are not just patterns of action, but the meaningful configurations of the world within which actions can take place intelligibly, and thus practices incorporate the objects that they are enacted with and on and the settings in which they are enacted" (ibid.: 135).[9] This claim is not to be interpreted in terms

[9] In the wider context of the abovementioned *workplace studies*, a similar conception of situated practice has been outlined by Suchman (1987).

of an extended and radicalised "symmetry principle", as suggested by Latour, but more in the sense of a "priority of the situation", whereby "situation" is understood as "the relational complex of embodied agents in meaningfully configured settings for possible action" (ibid.: 150). By contrast to Latour, Rouse is less concerned with ascribing symmetrical explanatory power to human and non-human "actants" than with explaining what renders the performances of human actors meaningful and intelligible. As he argues, "one cannot engage in skillful activity without the right sort of equipment in the right surroundings" (ibid.), whereby skills are not fixed once and forever but develop and change in interaction with the material setting.

One a more general level, this emphasis on the situated materiality of practices has far-reaching implications for epistemology as well as social theory which are diametrically opposed not only to realist and representationalist assumptions of independently given "natural" objects of cognition but also to Luhmann's "autopoietic" model of operationally closed observing (social) systems.[10] The profound differences between these conceptual approaches and postconstructivism are highlighted by the following statement: "If the post-constructivist tradition denies that there is any role for 'unreconstructed nature' in our understanding of science, it is not because we are unable to get 'outside' of a relatively self-enclosed social world, but because we have never been 'inside' one in the first place. The question is not how we ever get from our social world to a transcendent nature, but how meaningful language and other practices are sustained as part of the ongoing reconfiguration of a reliable and meaningful environment." (Rouse 2002b: 69)[11] The basic and fruitful idea behind this seemingly extravagant claim is not some kind of metaphysical monism but the rejection of understanding scientific practices, both material and discursive, in terms of representation or mediation. Practices (or representations as their stabilised results) are themselves configurations *in* and *of* the world; they neither represent a given "natural world" supposed to exist "behind" those configurations nor mediate it with a distinct "social world" (cf. Rouse 1996a: 150-151, 2002a: 173). Scientific understanding, according to Rouse (2002b: 69), is "not 'inside' minds or cultures, but embodied in worldly phenomena, skills, equipment, institutions, and situated discursive exchanges that cut across the traditional bounds of natural objects and social or cultural meanings". This reflection leads to a third important feature of postconstructivism: an account of scientific practices in terms of their temporality and performativity.

3.3 The Performativity of Scientific Practices

Perhaps even to a greater extent than practice, "performativity" has developed over the last years into a very prominent and widely used but equally ambiguous and contested concept, particularly in philosophy and cultural or

[10] Something that systems theory and postconstructivism doubtlessly have in common is a shift away from representationalism. As defined by Barad (2003: 804), representationalism is "the belief in the ontological distinction between representations and that which they purport to represent; in particular, that which is represented is held to be independent of all practices of representing". However, Luhmann, somewhat paradoxically, seeks to escape the pitfalls of that belief by entirely cutting off any epistemologically significant relationship between an operationally closed observing system and its environment (cf. Luhmann 1990, 1995). In a way, he thus even radicalises the representationalist background assumption of a clear-cut distinction between the "knower" and the "world" (for critical discussion see Christis 2001; Wehling 2002).

[11] In such reflections one will find the reasons why postconstructivism is considered to be a fruitful and promising conceptual approach in areas such as environmental history (see for instance Asdal 2003).

gender studies (cf. for an introduction Wirth 2002). If one tries to pick out one feature that (almost) all of the different references to performativity have in common, then the best candidate might be its non- or anti-essentialist impetus: performativity is not concerned with substantial things but rather with the (temporal) effects of "doings" and "performances", of repeated actions of some sort. It is this basic idea of performativity that has been attractive for that branch of science studies which seeks to move beyond representionalism (Barad 2003: 805):[12] the objects studied by science are not independently given, stable "things" awaiting discovery but instead *temporally emergent phenomena* that are produced (or co-produced) in their specific forms by and within the scientific practices themselves. According to Pickering, a performative account of science is one "in which the performances - the doings - of human and material agency come to the fore. Scientists are human agents in a field of material agency which they struggle to capture in machines. Further, human and material agency are reciprocally intertwined in this struggle. Their contours emerge in the temporality of practice and are definitional of and sustain one another." (Pickering 1995: 21)

I would to like illustrate the fundamental differences between a "traditional", representionalist approach to science (the basic assumptions of which are at least partly shared by social constructivism) on the one hand, and a performative (and "postconstructivist") account on the other, by referring to Pickering's critical discussion of the concept of "constraints". Usually, constraints are conceived as some kind of external (social, institutional, technical, natural, etc.) condition that objectively limits scientific activities as well as pushing them in certain directions.[13] Pickering criticises this widespread notion of constraint for drawing too static a picture of the relationships between scientific practices and their objects, and also their cultural and institutional contexts and surroundings. Constraints, as he argues, are traditionally understood as "temporally nonemergent", thus "preexisting practice and enduring through it": they "are always there" (ibid.: 65-66). In contrast, Pickering proposes introducing the concept of "resistances" in order to adequately take into account the temporality of the relationships between science and its various contexts. Contrary to constraints, resistances are "genuinely emergent in time, as a block arising in practice to this or that passage of goal-oriented practice" (ibid. 1995: 66). Against this background, scientific practice consists of a performative intertwining of emergent resistances on the one hand and repeated efforts to overcome them on the other. Pickering speaks of "a dialectic of resistance and accommodation, where resistance denotes the failure to achieve an intended capture of agency in practice, and accommodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it" (ibid.: 22).[14] Under happy circumstances, this dialectic may result

[12] While the concept of performativity as yet has only sparsely been used in an explicit manner in science studies (cf. Pickering 1995; Barad 2003; Kroß 2003), there is nevertheless a wider range of scholars propounding performative understandings of science without making express reference to the concept. Barad (2003: 807) names among the latter Haraway, Latour and Rouse.

[13] However, I do not agree with Pickering's (1995: 65) statement that constraints are usually restricted to the distinctively human realm. Frequently, also technical or natural conditions of scientific activities are conceived as objectively given "constraints".

[14] One should note that Pickering here does not employ any idea of a "symmetry" between human and non-human actants, or between human and material agency.

in "interactive stabilisations of machinic performances and conceptual strata" (ibid.: 182), whereby the latter include an "interpretive account" of how the involved apparatuses of observation and measurement work, as well as a "phenomenal account" of the aspects of the material world under consideration (cf. ibid.: 68-96). In cases of successful stabilisation and alignment of these three elements, one could speak of the generation of new, objective knowledge, with the passage trough the "mangle of practice" (Pickering) defining "a rather severe criterion of objectivity" (ibid.: 195). In order to avoid realist or representationalist misinterpretations: this knowledge does by no means "reveal" what and how an independently given object "really is" but is a local and temporal achievement due to the "constitutive intertwining (...) between material and human agency" (ibid.: 15). The stability and validity of this knowledge therefore depends on the maintenance and repeatability of those practices (in Rouse's broad sense) that had both coproduced the respective "machinic performances" and allowed them to be connected to cognitive expectations and theoretical reflections.

It is important not to misunderstand this performative account of scientific knowledge and practice as if phenomena or resistances which are temporally emergent from and within practices were less "real" or "material" than stable things that are "always there" (see for instance van den Belt 2003: 209). Instead, the concept of performativity results in a different understanding of what Barad (2003: 815) has termed the "primary epistemological unit". This "unit" is no longer to be found in independent objects with supposedly inherent properties but rather in *phenomena* which indicate, as Barad argues following the physicist Niels Bohr, the "inseparability of 'observed object' and 'agencies of observation'" (ibid.: 814). Thus, while doubtless being a relational term, "phenomenon" signifies "relations without preexisting relata" (ibid.: 815). The "observed objects" neither are accessible "outside" of their constitutive relations to agencies of observation, nor do they exist "behind" or "beyond" these relations. This by no means denies the materiality or reality of the phenomena, insofar as, according to Bohr, phenomena necessarily involve "things" which ultimately admit of the observation but may not be reified as existing independently of the (material) practices of observation (cf. Barad 1996: 176). What is observed is, in other words, "not a property of the object in isolation but of the phenomenon as a whole" (Rouse 2004: 148). Or, as Barad has put it: "Reality is not composed of things-in-themselves or things-behind-phenomena, but 'things'-in-phenomena." (Barad 2003: 817) One should add "that material resistances are only manifest relative to prior expectations; they have no existence in the absence of such expectations" (Pickering 1989: 281).[15] Temporally emergent resistances or phenomena are recognisable only when they can be captured and connected, within material-discursive practices, to the cognitive expectations of an individual scientist or a scientific community. This points to the question of how the issue of scientific non-knowledge might be comprehended within a postconstructivist framework. As I shall argue in the next section, postconstructivism offers new ways of adequately understanding this important and contested issue, in particular its most difficult aspect: unknown or unrecognised non-knowledge.

[15] This claim might appear to be misleading; yet, again, it does not deny the materiality of the setting from which resistances might emerge. But in the absence of cognitive expectations, resistances do not become manifest, they have no manifest existence.

4 The Embeddedness of Scientific Non-Knowledge – A Postconstructivist Account

For about 15 or 20 years, the novel and unfamiliar issue of scientific ignorance or non-knowledge has increasingly gained attention, both in the (social) sciences and in the general public (cf. for instance Wynne 1992; Luhmann 1992; Wehling 2001). Moreover, the focus has shifted to what Jerry Ravetz (1990) has termed "science-based ignorance", that is non-knowledge *generated* by science itself. The media researcher Holly Stocking had therefore suggested a few years ago the project of a "sociology of scientific ignorance (SSI) to complement and expand the existing sociology of scientific knowledge (SSK)" (Stocking 1998: 173; cf. also Wehling 2004). In this section, I would like to substantiate my thesis that a postconstructivist conceptual approach is most appropriate to grasp the full range of the processes of generating scientific non-knowledge and especially to adequately understand the key phenomenon of "unrecognised non-knowledge" or, as it usually is termed by British and American scholars, of "unknown unknowns" (cf. Kerwin 1993; Grove-White 2001; Wynne 2002). By this notion, situations are characterised in which the sciences don't even know what they don't know (cf. Wehling 2004: 71-72). The almost "classic" example of this state of complete unawareness is the depletion of the ozone layer by CFCs which, even more than 40 years after mass production of those substances was started around 1930, remained entirely beyond the scope of scientific expectations and cognitions (cf. Böschen 2000: 41-104). In recent social conflicts over new technologies, for instance over genetically modified organisms (GMOs), questions of the possibility, probability, or even unavoidability of unknown unknowns are highly contested and increasingly coming to the fore (cf. Grove-White 2001; Wynne 2002).[16]

Can this strange, double negative notion of unknown non-knowledge or unknown unknowns be sociologically (or philosophically) understood in a meaningful and consistent way at all? For, contrary to what Robert Merton (1987) has coined "specified ignorance", unrecognised non-knowledge is by definition *not* present and observable in the form of a certain individual's or group's explicit recognition of what they don't know. But how to explore what is completely absent or, at least, appears to be completely absent (cf. Weinstein/Weinstein 1978)? At this point, the postconstructivist, non-reifying and non-representationalist account of knowledge outlined above proves to be fruitful with regard to the following three closely related aspects: First, if Rouse's claim is right that knowledge is "embedded" in situated research practices and not "fully abstractable in representational theories" (Rouse 1987: 24), then the same applies for non-knowledge. If, secondly, it holds true that knowledge is only poorly understood as the "possession" of certain knowers, for instance a group of scientists, then non-knowledge may not simply be reduced to the mere "absence" or "lack", individual or collective, of such a possession. Thirdly, identifying the "content" of scientific knowledge not simply with its verbal (or mathematical) representation of the world but instead with the "reconfiguration of the world itself through practical engagement with things, people, and prior patterns of talk" (Rouse 2002b: 73) gives rise to a

[16] In order to avoid misunderstandings, one should emphasise that talking and debating about the possibility of unknown unknowns does not necessarily mean that one becomes aware of what is not known (or of what eventually might happen when GMOs are released to the environment). The crucial point in social conflicts is that, by definition, the occurrence of unknown unknowns can neither be proved nor refuted in advance.

more comprehensive understanding of scientific non-knowledge: no less than knowledge, non-knowledge is embedded and inscribed in practices conceived as material reconfigurations of the world. Thus, the (more or less) explicit recognition and scientific "specification" of what is not known in terms of theories or hypotheses is only *one* dimension of the problem which certainly is important but at the same time extremely dependent on highly contingent and precarious preconditions. To put it differently: unknown unknowns, or unrecognised non-knowledge, are inherent in the situated materiality of scientific practices; they are elements and (possible) effects of the material settings which nevertheless are not manifest (or do not exist, as Pickering has argued) in the form of temporally emergent "resistances" or interactively stabilised "phenomena".[17] For unknown unknowns to become manifest, above all appropriate material and discursive practices are required, including the formation of adequate cognitive expectations as well as technical equipment of observation and measurement. There is, however, no guarantee of the successful alignment and interactive stabilisation of "on the one side, captures and framings of material agency, and, on the other, regularized, routinized, standardized, disciplined human practices" (Pickering 1995: 102). On the contrary, one can by no means rely on the assumption that the various elements of the configurations in which scientific or technological practices are enacted will, sooner or later, "manifest themselves" due to their "sheer" materiality and therefore be fully transparent and controllable.

The importance of such a non-reifying, postconstructivist account of non-knowledge which is not centred on knowing (or not knowing) minds and subjects immediately comes to the fore if one understands the technical implementation of scientific knowledge primarily in terms of an "extension of scientific practices beyond the research setting" (Rouse 1996a: 131). What is crucial in this regard is "the reconstruction of the surrounding world to resemble the laboratory in important respects. Objects and substances created in and for the laboratory are introduced into other settings. Partitions and enclosures are built to prevent unwanted or unaccountable mixtures. Actions and events are more carefully sequenced and timed. Instruments to register and interpret the signs first elicited from objects in laboratories become standard equipment elsewhere." (Ibid.). The issue of non-knowledge, in particular of unknown unknowns, becomes relevant here in two respects: first, the strategies of partitions and enclosures to prevent "unaccountable mixtures" will always tend to be limited and incomplete; the complex social or natural world can not really be made into the controllable "micro-world" of the laboratory. The metaphor of "society as a laboratory" (Krohn/Weyer 1989) therefore remains a metaphor, if, of course, an illuminating one; unforeseen and/or unrecognised effects can certainly not be entirely prevented. Second, if one takes into account that scientific practices even *within* the laboratory are not always fully transparent and recognisable (cf. Collins 2001), then unknown

[17] Alexander Bogner's criticism misses this point by confusing the postconstructivist emphasis on the embeddedness of (non-) knowledge in material configurations with a realist and representationalist notion of reference. In a postconstructivist view, (unrecognised) non-knowledge does not *refer* to "more or less objectively knowable phenomena", as Bogner (2005: 23) suggests, but *is embedded in* and *incorporates* a setting of material entities, agencies of observation, established spatial or temporal "horizons" of attention, and so on. Within this setting it is of course a crucial question whether (and when) at least some of its elements can be "captured" and connected to cognitive expectations by situated practices, both experimental and discursive. But this is by no means a retreat to representational realism. By contrast, Bogner again traps the sociological analysis of non-knowledge in the ritualised dichotomy of realism and constructivism.

non-knowledge embedded in the research setting will inadvertently be "exported" into the surrounding natural and/or social worlds with possibly unforeseeable consequences. Given this background, it comes as no surprise that the hitherto uncontested authority of science over the definition of ignorance and non-knowledge is increasingly challenged by social actors, resulting in a remarkable and far-reaching "politicization of ignorance" (Stocking/Holstein 1993) which includes above all the questioning of the dominant framings of scientific non-knowledge (see for instance Grove-White 2001). An attempt, as made for instance by van den Daele (1996), to restrict the "relevant non-knowledge" to the *known* unknowns, that means to the "specified ignorance" of the experts in the respective fields, is therefore not only dubious in terms of risk regulation and public policy (cf. Wehling 2003: 129-131). In addition, it sticks to exactly that narrow, representationalist conception of knowledge (and non-knowledge) in terms of a "possession" of a certain scientific community that postconstructivism seeks to overcome. What follows from a postconstructivist account of scientific (non-)knowledge, is, in contrast, the demand to extend the accountability of the sciences *beyond* what is explicitly known or not known, thus encompassing the material configurations in which scientific practices are enacted.

With respect to this demand, different scientific "cultures of non-knowledge", understood as practices of generating, recognising, defining and communicating non-knowledge, move to the fore.[18] As Karin Knorr-Cetina (1999) has shown convincingly in her study on the "epistemic cultures" of high-energy physics and molecular biology, the sciences differ widely in their ways of "making knowledge". Drawing on Knorr-Cetina's findings one can suppose that these epistemic cultures do not only encompass "cultures of knowledge" but also, at the same time, cultures of non-knowledge, i.e. specific practices and routines of dealing with what is not known. Whereas, according to Knorr-Cetina (ibid.), high-energy physics inclines to actively search for "liminal" or "negative" knowledge, that means knowledge of the limits of its knowledge, molecular biology employs an epistemic strategy of "half-blind variation": if experiments fail or show unexpected and unexplainable results, the scientists usually do not have much interest in carefully exploring the reasons why but vary some of the elements of the experimental setting until it works and delivers explainable and usable results. The study of such routines, mainly tacit, of dealing with (self-generated) non-knowledge might offer fruitful perspectives for initiating more self-reflective research practices, especially when such contrasting scientific cultures of non-knowledge are confronted with each other in public arenas, as in the controversy over GMOs.

5 Conclusion: Beyond Realism and Constructivism?

In his discussion of how to deal with material objects and experimental practices in science studies, Henk van den Belt maintains that postconstructivism as outlined by Rouse, in spite of its "deceptive label", is "really just another version of radical constructivism" (van den Belt 2003: 216), which, according to him, "makes the existence of an object depend on human knowledge" (ibid.: 209).[19] Other critics might

[18] The exploration of such "cultures of non-knowledge" (*Nichtwissenskulturen*) is the aim of a research project conducted at the Environmental Science Center of the University of Augsburg, using the examples of agrobiotechnology and mobile phone communication (cf. Böschen et al. 2005).

[19] Van den Belt aims to defend a "moderate constructivism", as advocated in particular by the "strong programme", against this "radical constructivism" to which he attributes, besides Rouse, the work of Ashmore,

consider postconstructivism as nothing but a retreat from "strict constructivism" (Bogner 2005) to an at best slightly more sophisticated version of traditional realism. Can the postconstructivist claim to move *beyond* the unfruitful dichotomy of the "undead" realism and constructivism nevertheless be substantiated and justified – or is there no escape from the pitiless rule of being *either* realist *or* constructivist (and from being misinterpreted from both sides)?

Usually it is taken for granted in the ongoing discussions on these issues that there are indeed fundamental differences between realism and constructivism that render the two opponents more or less incompatible. Without denying such differences, one should not fail to see that there are also, more often implicit than explicit, striking continuities and correspondences, beginning with a reifying notion of knowledge which is tied to the semantic and representational "content" of scientific knowledge in the form of theories, propositions, mathematical calculations, and so on (see above, Section 2.1.). It is at this point that postconstructivism intervenes: it does not seek to "overcome" the realism-constructivism divide by successively weakening and playing down the differences between them until they meet somewhere "in the middle" (in the shape of "moderate" versions). On the contrary, the critical strategy of postconstructivism aims at transforming (or at least irritating) the dichotomy itself by questioning the hidden background assumptions on which it is founded.

Representationalism is, according to Barad (2003: 812), "a prisoner of the problematic metaphysics it postulates". This metaphysics "separates the world into the ontologically disjoint domains of words and things, leaving itself with the dilemma of their linkage such that knowledge is possible" (ibid.: 811). From a non-representationalist perspective, however, one becomes aware that the question of whether scientific knowledge is to be explained by natural or cognitive rather than social factors (or vice versa), of whether it reveals the "objective truth" of independent things or is "nothing but" a more or less arbitrary social construction, only arises if we understand knowledge as a "coherent domain of determinable facts susceptible to and in need of explanation" (Rouse 2002a: 136). If, in contrast, scientific knowledge is conceived as embedded in research practices, in material configurations that cut across the boundaries between the supposedly distinct "natural" and "social" realms, it becomes meaningless to ask whether those practices *either* are determined by the reality of natural objects *or* constructed by social actors and influences. The three postconstructivist key concepts outlined above, namely the deflationary account of knowledge, the notion of the situated materiality and discursivity of scientific practices, and the concept of performativity, therefore challenge and transform the shared background assumptions of realism *and* constructivism - and thus elude and abrogate the dichotomy itself.[20] Moreover, compared to realism and social constructivism, these concepts are able to contribute to a more adequate and empirically rich image of the sciences and their achievements and successes as well as their risks and "blind spots" (cf. Section 3).

Callon/Latour, Knorr-Cetina, Pickering and Woolgar (van den Belt 2003: 203). Yet, as I have demonstrated in Section 2, the basic assumption of postconstructivism is almost directly opposed to van den Belt's assertion: according to postconstructivism, knowledge is embedded in research practices and "depends" therefore on material configurations of the world.

[20] Apparently, this does not prevent these concepts from being misinterpreted either as traditional realism or radicalised constructivism.

Yet one should not underestimate the persistent influence and attraction of realism and constructivism as supposedly coherent world views. It is in this sense that Rouse has ironically termed them "vampires" or "philosophical undead" which, in spite of all critical objections that have been raised, "still haunt our concepts and interpretations of nature, culture, and science" and "continue to function even when the explicit positions and arguments have become otiose" (Rouse 2002b: 63). Against this background, postconstructivism may not be misunderstood as itself being or claiming to be a coherent philosophical or even metaphysical account "above" or "outside" of the practices of generating, justifying or contesting knowledge. As I would like to suggest, it should instead be conceived as a self-reflective and critical discursive strategy that aims to continuously question and undermine reifying, one-sided interpretations of scientific knowledge (cf. Asdal 2005: 259). As a consequence, realism and social constructivism might lose their position of meta-theoretical certainties and guarantees: scientific knowledge can no longer be explained and legitimised (or de-legitimised) with reference to either "nature" or "society". What remains is "merely" the socially situated study of the scientific practices themselves and their reliability and accountability, for instance in terms of relationships between "knowers" and the "known", risks and benefits, or known and unknown unknowns.

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