Reflexivity, representation, and the possibility of constructive realism

Tarja Knuuttila
University of Helsinki

Realism and constructivism are often considered to be incompatible with one another. This paper considers this topic from the perspective of scientific representation. It discusses how the most extreme constructivist claims turned out to be self-defeating in terms of the problem of reflexivity. It is argued that a more modest constructivist programme can fruitfully be combined with pragmatic accounts of scientific representation.

1. Introduction

If there is anything that unites realists and constructivists, those supposed adversaries, it is the supposition that realism and constructivism are incompatible with one another. Or at least this was the case still a bit more than a decade ago, in the aftermath of the infamous "science wars". One frontline was between realistically inclined philosophers of science and constructivist sociologists of science. Since then, however, the things have moved forward both in the field of science and technology studies and philosophy of science, and now it appears that they might have more in common than previously believed. On the one hand, as philosophers of science have become increasingly interested in scientific practice, they have become more receptive to some constructivist ideas. On the other hand, the most extreme constructivist tenets stumbled at their own absurdity, which is what the constructivists soon realized themselves in the discussion of the problem of reflexivity.

In what follows I will delineate some points made in this discussion and show how they are linked to the question of representation. What I aim to argue for is that the constructivist critique of the realist philosophy of science shared with (some versions of) it the same idea of representation, that of accurate representation, which they perceived as the root of “epistemological horrors” (an expression by Steve Woolgar). In a sense, in its wholesale criticism of “realistic” representation constructivism regenerated the epistemological outlook it sought to avoid. However, if one does not subscribe to the idea of accurate representation much of the constructivist critique of the philosophy of science becomes ineffective. Or, to put it in a more positive vein, it is possible to outline how the work done on the practices of scientific representation in the field science and technology studies could contribute to the pragmatic approach to scientific representation. I begin by discussing the problem of reflexivity, after which I move on to the recent philosophical discussion of scientific representation, concluding with a chapter juxtaposing the different versions of constructivism with the different philosophical accounts of scientific representation.

2. The problem of reflexivity in Science and Technology Studies

> “Within the first few nanoseconds of the relativist big bang, nearly everyone realized that the negative levers were equally applicable to the work of the sociologists and historians themselves.“
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> (Collins & Yearley 1992, p. 304)

In the 1970s and the beginning of 1980s the newly founded field of science and technology studies engaged in a head-on critique of the “traditional” view of science, typically exemplified by the philosophy of science or functionalist sociology represented by Robert Merton. The attack proceeded in two main fronts. The adherents of the sociology of scientific knowledge aimed to show that scientific knowledge was shaped by various social factors (e.g. Barnes & Dolby 1970, Mulkay 1976, Bloor 1976). On the other hand, the constructivists went into scientific laboratories to study how the objects of science were constructed (Latour & Woolgar 1979/1986, Knorr-Cetina 1983, Lynch 1985). However, they soon encountered the problem of how to justify their own accounts of science given that they aimed to show the interested and constructed nature of the claims of the researchers they studied. This problem became to be called the problem of reflexivity.

2.1. Reflexivity and the self-defeating character of STS constructivism

For a scientific study of scientific study the reflexivity of that endeavor should appear obvious. However, the realization of reflexivity was a matter of more than nanoseconds in STS and yet reflexivity was on the agenda more or less from the beginning of that movement. In David Bloor's influential "The Strong Programme in the Sociology of Knowledge" (Bloor 1991/1976) *reflexivity* was listed among the four tenets that the emerging "social studies of knowledge" should adhere to. The other tenets were *causality*, *impartiality* and *symmetry*. According to Bloor, the sociology of scientific knowledge has to look for the same kind of general causal explanations as other scientific disciplines. Specifically, it should be "concerned with the conditions which bring about belief or states of knowledge" (p. 7). For Bloor, Barnes and other researchers affiliated to the so-called Edinburgh school this meant finding explanatory factors such as cultural resources, social milieu as well as concerns and interests of different groups. Importantly, the explanations should be impartial with respect to purported truth or falsity of the investigated claims and the same types of cause should be used to explain, symmetrically, both true and false beliefs. Scientific knowledge did not deserve any special treatment and was not to be left to philosophers as putative experts on the rational method.

These kinds of explanations were called "interest-explanations" since especially the interests of different groups played a central role in the empirical studies of the Edinburgh group. However, even if reflexivity was on Bloor's list, it did not really have any prominent place in the emerging sociology of scientific knowledge. It was rather acknowledged as a consequence of the need to seek for general explanations, since in that case the patterns of explanation would have to be applicable, "in principle", also to sociology itself.

For the adherents of the "strong programme", the attempt to specify the interests giving rise to scientists' actions meant revealing the social character of scientific knowledge. But if scientific knowledge was regarded as a social product, then what exempted interests from the same kind of scrutiny? This was asked by Steve Woolgar—who was to become the leading figure of the reflexivists in STS—in his seminal "Interests and Explanation in the Social Study of Science" (Woolgar 1981a). Woolgar urged that the interests should also be investigated instead of being used as unexplicated resources for explanation. In an answer to Woolgar, Barnes readily admitted that his conceptions of interests had been "constructed by the analyst so as perform his explanatory work". What else should they be constructed for, Barnes asked, and added that nothing at all prevents their further study and criticism (1981, 493).

Woolgar's answer to Barnes is revealing as in it the full-blown constructivist agenda starts to take shape. He announces that any attempts to "methodically" arrive at more accurate descriptions of reality are misguided, since descriptions themselves are *constitutive of reality*:

> I'm not saying, then, that the work of MacKenzie and the interest theorists is any more wrong than other attempts at explanation: The artful concealment to which I refer is to be understood as symptomatic of *all* explanatory practice, not as reflection of the motives of particular individuals. So I make no apology for pointing out the significant sense in which *all* such work is *essentially flawed*. The essentially flawed nature of explanation demands our analytic attention and this task should no be set aside in favour of further attempts at explanation. (Woolgar 1981b, 511).

What Woolgar was in effect suggesting was that instead of explaining what takes place in science, sociologists of science should engage in explicating the various uses of concepts, such as explanation, that they as well other scientists were using. This project was ethnomethodological by nature: The idea was to study the interactional means through which the social order and its products are achieved—or constructed. A good example of this kind of work had already been provided by Michael Mulkay (1976/1991) who had subjected the Mertonian norms to "discourse analytic" scrutiny. From Mulkay's perspective Mertonian norms should not be understood as ideal standards that regulate scientific activity. Instead they are discursive resources that scientists *mobilize* *strategically* to describe and judge their own and their colleagues' professional behaviour. For Mulkay as well as for many other STS scholars Mertonian norms presented an overly idealized image of science that was being used by scientists to justify the special status of science as an activity not to be interfered with from the outside.

While the emphasis was first on the discursive practices, the constructivist programme was soon extended to cover also other aspects of doing science. The turn of 1980s marks the publication of several important laboratory studies whose method was, to cite one of the protagonists, "direct observation of the *actual site of scientific work* (frequently the scientific laboratory) in order to examine how objects of knowledge are constituted in science" (Knorr-Cetina 1983, p. 117, italics are those of the original; see also Latour & Woolgar 1979/1986, Lynch 1985). The new focus was on how discursive construction was woven together with the experimental, material construction taking place in scientific laboratories. This was especially highlighted by Latour and Woolgar who in their book *Laboratory life* (1979/1986) asked how the material process taking place in the laboratory was turned into pictures and graphs published in scientific articles. During his two-year ethnographic study at the Salk institute, Latour observed the transformation processes taking place there: Animals were killed, various materials used, the resulting extracts were put into an apparatus, and a sheet of figures was obtained. Now the focus shifted from tubes to figures that were used as input into a computer, which, in its turn, printed a data sheet. The data sheet was worked on into a curve that was then further processed, redrawn and discussed in the research group, finally ending up as a diagram in a published article. Latour and Woolgar wrote:

> Once the data sheet has been taken to the office for discussion, one can forget the several weeks of work by technicians and the hundreds of dollars, which have gone into its production. After the paper, which incorporates these figures has been written […] it is easy to forget that the construction of the paper depended on material factors […]. Instead, "ideas", "theories", and "reasons" will take their place. (ibid., 69).

Two observations concerning these laboratory studies seem especially important. First, the label of social constructivism, not to mention any extreme social constructivism (see Searle 2009) does not apply to them. Construction is a material-cum-social process. In fact, the materiality of this process is even underlined, as the critique is directed against those philosophical and other accounts that tend to forget the material basis of scientific representations. The second observation that is especially important as regards the question of reflexivity concerns that method of these laboratory studies. Namely, how should one understand the articles written about them qua representations of how science *actually* works. Was not there something deceptive about this aim given that what these studies set out to argue was precisely the constructed nature of any scientific accounts. Could such ethnographic studies, or texts reporting them, really succeed in avoiding the conventional explanatory schemes themselves and circumvent suspicions about their own constructed nature?

Indeed, the STS reflexivists, especially Steve Woolgar and Malcolm Ashmore, soon launched a forceful critique of the ethnomethodological pretension of being in the possession of a privileged method with which one could somehow find out “what actually goes on in science“? It is telling that Latour and Woolgar added to the second 1986 edition of their *Laboratory* life a postscript that discussed the ethnographic method and reflexivity. Also the word “social” was dropped from the subtitle of the book that now became: “The construction of scientific facts”. This is no surprise as Woolgar had already by then engaged in the critique of also laboratory studies attacking them of an instrumental conception of ethnography. Such conception applies relativist epistemology only *selectively*—to other scientists’ accounts—whereas one’s own accounts are presented realistically. An instrumental ethnographer, according to Woolgar, tends to be after news, of “finding things to be other that you supposed they were“ (1982, 485). In this case, the news was, more often than not, that scientific facts are constructed and that science does not differ from non-science—it is as “social”, “contingent”, “local”, “situated”, and so forth, as any other social activity.

In and of itself the newly found social character of science was not very stunning news. To render it such, an alternative "traditional" conception had to be put up, with which the novel constructivist conception of science could be contrasted. This strategy provided, for quite a long period, a popular way to open up a STS-article (which is still in use, actually). The alternative, old-fashioned and even damaging view, was provided, more often than not, by the "philosophical version" of science as rational activity oriented at finding the truth. Typically, not much was said about this view—it was merely alluded to, or presented briefly, in an uncontextual and general manner. The impression a reader easily gets from this is that the "traditional view" invoked is a rhetorical construct, the rationale of which is to underline the novelty and epoch-making character of the (constructivist) views professed.

The irony about this situation is that the constructivist is not herself practising what she is preaching. According to Woolgar, the preferred constructivist position on reality and its representation is the mediative one according to which "there is nothing inherent in the character of real world objects which uniquely determines the accounts of those objects" (Woolgar 1983, 245). This conviction is, then, seasoned with differing amounts (depending on the constructivist in question) of constitutive intuitions that we construct realities by way of accounting them. Yet, when it comes to contrasting the descriptions of the constructivist ethnographer and those of the scientists she studies (or philosophers' and other traditionalists' accounts), there seems to be no doubt about *whose* story is supposed to *fit* the reality best. In other words, according to constructivists, scientific representations should not be regarded as truthful representations of real objects or processes: there is no correspondence between our scientific representations and the reality they aim to explain or describe. Yet in arguing for this view, the constructivist has created one more representation, but now this representation should be taken as a truthful depiction of its object, that is science, as it actually happens. The question, then, is how to meet the reflexive challenge?

Although the reflexivists' critique of the paradoxes of wholesale constructivism was perceptive, their proposed solution to the problem was less so. Since it seemed to them that there was no adequate solution to the problem of reflexivity, they suggested that instead of trying to solve the problem it should rather be “celebrated“. It was proposed that with different textual methods the “monster“ of reflexivity could be “simultaneously kept at bay and allowed a position at the heart of our enterprise” (Woolgar 1982, 489). Such methods were, for instance, the “second voice device“ (Woolgar and Ashmore 1988), and other kinds of (constructed) dialogues trying to display their reflexivity. Their aim was to shatter the reader’s supposed “naive belief“ in the text and make her aware of the text’s artificial nature by constructing it so that it more or less deconstructs itself. As a result the text becomes more of an epistemological project directed to question our alleged epistemological habits (i.e. naïve realism) than any scientific representation of the empirical subjects studied. According to Woolgar “reflexive ethnography need not entirely exclude the production of news about laboratories; this becomes an incidental product of research, rather than its main objective“ (1982, p. 492).[1]

No wonder, then, that the reflexivist programme never took off in science and technology studies: readers were still more interested in the news about laboratories. Indeed, one big problem of the STS reflexivism was its disregard of the reception, in other words the readers, of texts. Especially when it comes to articles published in scientific journals, the scientists reading them are certainly not naïve realists, unaware of the laborious experimental and theoretical processes behind the published results. It is as if the reflexivists fell prey to their own trap in fixing their gaze on the finished product, the text, instead of its production processes. Interestingly, this was precisely the direction to which they turned after a less than a decade of reflexive “wrighting“ (Ashmore 1989).[2]

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[1] Of course this proposal was not restricted to science and technology studies only, but was instead a part of a larger current sweeping over humanities and social sciences in the1970s and early 1980s. In the field of historiography, Hayden White (e.g. 1973) urged historians to pay attention to the historical narratives themselves, to their fictive and artificial nature. Clifford Geerz (e.g.1973), in turn propagated for the same kind of programme in anthropology. Spencer notes how as a result of Geerz’s emphasis on writing, his hermeneutic approach “tries above all to close the hermeneutic cycle by limiting his readers' access to that which he wants to interpret himself" (1989, p.149).

[2] To be sure, the reflexivist agenda did not disappear from the STS, or sociology in general, but it got new forms. Consider for instance how Woodhouse et al. advocate activism: "[I]n as much as there always are more research questions than time to study them, it seems hard to miss the possibility of extending the individual-level reflexivity of the 1980s to the field more generally:

2.2. The practices of scientific representation

In retrospect the collection *Representation in Scientific Practice* edited by Lynch and Woolgar (1990) paved the way for how the constructivist STS has ever since approached scientific representation.[3] Several contributions of the book meticulously follow the “assembly line”, the processes of constructing scientific representations. From this point of view scientific representation appears as a subtle “dialectic of gain and loss“ (Latour 1995). It is not just a question of reduction or simplifying. Some methods of representation further fragment, upgrade or define the specimen in order to reveal its details, whereas others add visual features for the purposes of clarifying, extending, identifying, etc. Often the aim of scientific representation is to mould the scientific object so that it can assume a mathematically analysable form or to be more easily described and displayed by using different textual devices (see also Latour 1990, Lynch 1985 and 1990). Scientific representation widens in these studies into an expanded process of circulating and arranging diverse pictures, extracts, “tissue cultures”, photographic traces, diagrams, chart recordings and verbal accounts. Representations become things that are worked upon, being ultimately “rich depositories of ‘social’ actions“ as Lynch and Woolgar sum up the approach in their introduction to the volume (p. 5).

Any ambitious epistemological programs are largely left behind in the book, although for some authors of the book still aim to “explode“ the supposed homogeneous conception of representation in order to make room for the “deeds performed, when those [representational] items are embedded in action“ (Lynch 1994, 146). However, it seems that this constructivist agenda is rather traditional when it comes to its approach on scientific representation. Firstly, in an effort to deconstruct the notion of representation

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what social processes are setting our collective agendas; is the agenda-setting process a laudable one […] (2002, 307).

[3] The book *New Representation in Scientific Practice* (Lynch et al, in press) takes its inspiration from its predecessor, taking into account novel forms of image production, especially such as digital image processing and new kinds of tactile and haptic representations.

the protagonists rely on what is commonly taken as representation already in their choice of case studies. Thus rather than exploding the notion of representation these cases reveal what a complicated phenomenon scientific representation is. Secondly, what these studies seem to be doing is to show us what *really* goes on in scientific representation. That is, the reflexivist worries are clearly ignored. Thirdly, although these studies focus on scientific representations and not on the phenomena they are supposed to be representing, they give us clues as to how through the laborious art of representing scientists are seeking and gaining new knowledge.

What is essential for this constructivist perspective is the focus on what one does with scientific representations and how various representational media are utilized in the process of scientific representation. The artificial features of scientific representations do not render them into contingent social constructions but rather result from well-motivated epistemic strategies that in fact enable scientists to know more about their objects. Thus our possibility to represent the external world and to gain knowledge from it through representation, is not threatened, just the idea that scientific representations would be some kind of transparent imprints of reality with a determinable relationship to their targets. Now the question is, what could possibly be so threatening about this view on scientific representation? Does it make constructivism and scientific realism incompatible with each other? The answer, I suggest, depends on how we conceive of scientific representation. With this in mind let me consider next the recent philosophical discussion on scientific representation.

3. From semantic to pragmatic accounts of representation

The question of representation arose in the philosophy of science only relatively recently although the idea of representing the world accurately has been central to our common conception of science and to the philosophical discussion of realism (see e.g. Godfrey-Smith 2003, 176-177). Yet it was not until the beginning of the 2000s that representation as a specific topic began to interest philosophers of science more generally. Once started, the philosophical discussion focused almost exclusively on scientific representation in the context of modelling. According to the received wisdom, models give us knowledge because they represent their supposed real-world target systems more or less accurately, in relevant respects and to a sufficient degree. This kind of formulation already suggests that there is a special sort of relationship between a model and its target, and the question became how to analyse such relationship. Could representation be analysed in such terms as isomorphism or similarity, or is something else needed to establish the representational relationship? To this question philosophers of science have given various answers, which have far-reaching implications for how the epistemic value of models is to be understood.

The conviction that representation can be accounted for by reverting solely to the properties of the model and its target system is part and parcel of the semantic/ structuralist approach to scientific modelling. According to the semantic conception, models specify structures that are posited as possible representations of either the observable phenomena or, even more ambitiously, the underlying structures of real-world phenomena. The semantic/ structuralist conception of scientific representation was originally cast in terms of isomorphism: a given structure represents its (real-world) target system if they are structurally isomorphic to each other (e.g. van Fraassen 1980, Suppe 1974, 1989). Isomorphism refers to a kind of mapping that can be established between two structures and preserves the relations among the elements. Giere (1988) in turn suggested similarity as the basis of the representational relationship in his reformulation of the semantic approach, but he has later come to think that similarity fits better the pragmatic approach to scientific representation (see below).

The recent philosophical discussion has found the analysis of representation in terms of isomorphism lacking in many respects. Firstly, isomorphism does not have the right formal properties to capture the nature of the representational relationship: it is a symmetric, transitive and reflexive relationship whereas representation is not.[4] Secondly,

[4] These points derive from Nelson Goodman's famous critique of similarity (Goodman 1968). For reasons of space I cannot deal with them in detail, and readers are referred to Suárez (2003) and Frigg (2006). Suárez has also directed this line of critique towards the similarity account, but it seems to me that philosophers of science currently favoring a looser (i.e. not mathematical) notion of similarity all take into account users and use (e.g., Giere 2004, 2010, see above).

it does not leave room for misrepresentation. The idea that representation is either an accurate depiction of its object or not a representation at all does not fit actual representational practices. Thirdly, structure sharing is not necessary for representation. Scientific practice is full of examples of inaccurate models, which are difficult to render as isomorphic with their targets. Fourthly and perhaps most importantly, isomorphism does not capture the directionality of representation. We usually want the model to represent its target but not vice versa.

Structuralists have tried to counter these criticisms in two ways, either amending the structural account in adding to it directionality (e.g., Bartels 2006), or trying to weaken the conditions that isomorphism imposes on representation by suggesting different morphisms such as homomorphism (Lloyd 1988, Bartels 2006, Ambrosio 2007) or partial isomorphism (Bueno 1997, French & Ladyman 1999, da Costa and French 2003). Both of these notions attempt to do away with the problems of misrepresentation and non-necessity. It is worthy of note that in defending homomorphism as an alternative to isomorphism Bartels (2006) suggests that it has to be complemented with a representational mechanism connecting the representational vehicle to its target. Thus, whereas Bartels makes an effort to give a fully-fledged analysis of representation, it is indeed questionable whether other structuralists have attempted to present any necessary and sufficient conditions of scientific representation. Yet it seems that in their conviction that "it involves isomorphism" (French 2003) they have usually left the rest unexamined. Structural relations provide the *privileged foundation* on which our knowledge rests.

Many of the aforementioned problems are directly related to the fact that scientific representation is a relation between a representational vehicle (e.g., a model) and a real target, and thus a mere mathematical relation between two structures fails to capture some of its inherent features – and makes too stringent demands on actual scientific representations. According to the pragmatists these problems will be cured if it is recognized that representation cannot be based only on the respective properties of the representational vehicle and its target system. For instance, users' intentions (or intended uses) both create the directionality needed to establish a representative relationship and introduce the necessary indeterminateness into the representational relationship (given that human beings as representers are fallible). However, this comes at a price. When representation is grounded primarily on the specific goals and representing activity of humans as opposed to the properties of the representative vehicle and the target object, it is deprived of much of its explanatory content: not much insight into the epistemic value of modelling is gained in claiming that models give us knowledge *because* they are *used* to represent their target objects.

One strategy to deal with this problem is to add to one's account of representation a further stipulation concerning its success. Rather unsurprisingly, then, what has earlier been presented as an analysis of the representational relationship, i.e., isomorphism (van Fraassen 2008) or similarity (Giere 2010), is now suggested as a success criterion. As for isomorphism, it poses too stringent a condition on the success of representation in the light of scientific practice. The case of similarity is trickier. On the one hand, it does not really supply any user-independent success criterion in that it is the users who identify the "relevant respects and sufficient degrees" of similarity. Giere (2010) admits this, arguing that an *agent-based* approach "legitimates using similarity as the basic relationship between models and the world".

Another possibility for a pragmatist is to go deflationary all the way, as Suárez (2004, 2010) has done, and resist saying anything substantive about the representational relationship or its success, in other words whether they rest on isomorphism, similarity or denotation, for instance. According to Suárez, substantive accounts of representation err in trying to "seek for some deeper constituent relation between the source and the target" which could then, as a by-product, explain why the source is capable of leading a competent user to consideration of a target, and why scientific representation is able to sustain "surrogate reasoning". Hence he explicitly denies any privileged relationship between a representational vehicle and its target. Instead, Suárez builds his analysis directly on the aforementioned by-products. His inferential account of scientific representation is two-sided consisting of *representational force* and the *inferential capacities* of the representational vehicle. Representational force results from the practice of using a particular representational vehicle as a representation, determining its intended target. In addition to that the vehicle must have *inferential capacities* that enable the informed and competent user to draw valid inferences regarding the target. The success of representation also implies that there are some norms of inference in place distinguishing correctly drawn inferences from those that are not (Suárez, 2010). All these features of Suárez's proposal in particular, and those of pragmatic accounts of representation in general, make them interesting from the perspective of the possible reconciliation of constructivism and realism.

4. The possibility of constructive realism

One strand of the constructivism versus realism debate has revolved around the question of representation. What constructivists in the field of science and technology studies have typically wanted to contest is the idea of science providing us accurate representations of the world. This critique, however, soon turned against the STS constructivists themselves in the form of the problem of reflexivity, as they nevertheless wanted show how scientific representations, facts and objects were constructed in *actual* scientific practice. Thus while the STS scholars aimed to expose others' scientific representations as interested and constructed, they simultaneously treated their own accounts as realistic depictions of science. This problem was soon noticed without any viable answer given to it, and eventually the STS constructivists turned into studying the practices of representation instead of worrying about the relationship of scientific representations to their supposed real-world targets. However, it seems fair to say that the underlying motivation of contesting the idea of scientific representations as accurate depictions of real-world phenomena stayed the same.

As for the philosophical discussion of scientific representation, one central disagreement has precisely concerned the question of whether scientific representation should be understood in terms of accuracy or not. But what is meant by accuracy? Nothing even distantly like mirroring or copying seems to work when it comes to scientific representation. Accuracy in this context is captured by the idea of structure sharing:

"isomorphism provides us with a criterion for what counts as accurate" (Frigg 2006). Consequently, a scientific representation is accurate if it depicts a structure that is isomorphic with the underlying structure of its real-world target system. Clearly, also the other kinds of structuralist accounts based on various types of morphisms still attempt to latch onto this kind of accuracy.

From the perspective of the constructivist epistemological program it seems clear that the semantic/ structuralist account provides precisely the kind of philosophical view that it seeks to challenge. This becomes apparent once we consider some central features of the semantic/ structuralist view. The first thing to note about it is that it provides a *strong realist account* of representation that simultaneously gives an analysis of scientific representation and an objective criterion for its accuracy.[5] Secondly, it provides us a dyadic analysis of representation that (largely) reduces the relationship of representation to the properties of representational vehicles and their targets. Consequently there is neither room, nor function for users and the various intended uses of models. In other words, although no proponent of the semantic/ structuralist view would certainly deny that models are being used in actual scientific practice, these social aspects of representing are not deemed crucial when it comes to accounting for the epistemic value of models. Thirdly, the attempt to reduce the relationship of representation to isomorphism extracts from the actual scientific models a privileged layer, the structure, in virtue of which accurate representation is possible. What this amounts to is the claim that "the specific material of the models is irrelevant; rather it is the structural representation, in two or three dimensions, which is important" (French and Ladyman 1999, 109).

To sum up the argument so far: The structuralist / semantic account provides a strong realist account of representation that has no place for either the social aspects of representing, or the various characteristics of the actual representational media used—that is whether the model is expressed, for example, with symbolic, iconic, or

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[5] This needs not to be the case, however. One might also pursue an empiricist argument as van Fraassen (1980) does. For him the isomorphism at stake concerns the relationship between the model and the structures of (empirical) "appearances".

diagrammatic means, or as a 3D physical object. Clearly, this kind of conception of scientific representation is incompatible with constructivism. Yet, it provides just one philosophical approach to representation. Thus it seems that in their wholesale attack on representation the reflexivists (and some representation hostile neo-pragmatists alike) were themselves relying on this stringent notion of representation.

The pragmatic approaches to representation are, in contrast, inherently triadic meaning that the users and the purposes that the scientific representations are constructed for cannot be neglected in accounting for the representational relationship and its success. This means that the basic unit of analysis as regards scientific representation cannot be restricted to the model-target dyad (see e.g. Knuuttila 2009, 2011) that opens up space for considering the social aspects of representation and the process of constructing and using representations. To be sure, none of the pragmatic accounts of scientific representation has so far targeted the epistemic value of the *process of* constructing representations, but on the other hand several accounts of model construction have been presented recently (e.g. Peschard 2011, Knuuttila & Boon 2011). Another aspect of representation stressed by the constructivist studies is due to the way scientists utilize the specific characteristics of various representational media. This line of investigation has only just begun in the philosophy of science (e.g. Gelfert 2011, Knuuttila 2011, Vorms 2012, Chandrasekharan & Nersessian, forthcoming). It is however implied by the deflationary account of scientific representation by Suárez (2004, 2010) that stresses the importance of the inferences that scientific representations license. For these inferences the representational media used clearly matters that is already a well-researched fact in cognitive sciences (e.g. Zhang 1997, Hutchins 1995).

To conclude, there is a substantial overlap and hence a possibility for a fruitful dialogue between the pragmatic understanding of scientific representation and the constructivist studies of the representational practices in science. Both approaches are also compatible with moderate constructive realism (Giere 1988, 2006) that does not expect our representations to be accurate depictions of real-world phenomena, yet approaching them as important mediators of scientific knowledge. Last but not least, from the perspective of better understanding representation in scientific practice, the deflationary nature of the pragmatic account need not be regarded as its weakness. Quite the contrary, it might prompt us to provide more empirical flesh to it through an interdisciplinary study of the human use of signs and tools.

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