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RESPONSES AND REPLIES

Strong Constructivism — from a Sociologist's Point of View: A Personal Addendum to Sismondo's Paper

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A Reconciliation for Constructivism?

As a labelled constructivist, I turned with pleasure to Sismondo's paper,[1] expecting it to instruct me in what I was labelled for, and hoping to learn something about constructivism's variants and wrinkles. I was not disappointed. The field badly needed a first review and Sismondo's is a good one; it is informative about various brands of constructivism, and expert in pointing its fingers at sore spots. Sismondo is not passing by distinctions which have become important in the field and which are usually conflated, like that between constructivism (which one could call constructionism) and social constructivism. He realizes that constructivism has been reinvented in science studies rather than taken over from sociological precursors like Berger and Luckmann, and in many ways constitutes a different doctrine altogether. And he seems sympathetic at least to some of the challenges constructivism has posed, to the point of wishing to reconcile constructivism with empiricism and realism.

However, Sismondo's paper also puts into focus disagreements and differences in understanding between those who have been associated with constructivism and those who have not. To be sure, some of these differences are of the kind that one might be able to work out with a critic like Sismondo over dinner, but they are irritating nevertheless. For example, the desire which Sismondo manifests to reconcile constructivist science studies with more received views of science is one that puzzles me, not only in this author but also in others. Is what lies behind it the biblical mood of bringing home the sheep that has gone astray? What is constructivism supposed to be brought home to

- conventional realism and its problems? Or conventional positivist social science? Who profits from the return - constructivism or the homemaking party? If constructivism has been fecund, as Sismondo suggests (and as I too think it still is), should it not be left on the loose to roam the fields ploughed by traditional approaches in ways which have made it 'fecund'? What appears as a benevolent attempt at making constructivism more 'acceptable' by bringing it together with more 'plausible' views, may do a disservice to a programme that still spawns new versions and has not tested them out. More important, it may also do a disservice to the domains which cherish the plausible views.

Consider the challenges constructivism, and the sociology of scientific knowledge in general, have posed for traditional philosophical beliefs. These new approaches replace the view that observation and experiment play the dominant role in the specification of scientific facts by the view that these processes involve collective negotiations, interests and the infusion of experimental outcomes with contingent features of situations. What constructivism puts its finger on in these claims is that the social is part of the production of innovation: constructivism brings into view social processes, as opposed to the methodological and individual processes with which received views of science were concerned. Now, whether one agrees or not with some of the specific claims made in this respect is beside the point;[2] what seems not beside the point is the need for philosophy of science systematically to think through and incorporate the role of the social into normative pictures of scientific activity. In fact, it is hardly conceivable that a phenomenon like modern science, which is so intrinsically linked to modern society as an institutional and collective arrangement, should not itself display social features *which philosophy must come to grips with* if it is ever going to be *au courant* with the world in which it lives.[3] Equally, constructivism in particular has not stressed interests so much as features which one could roughly associate with a notion of social *practice* - features that are inherently linked to social situations (contingency, indexicality, opportunism, emergent outcomes),[4] to the pliability of rules and standardized criteria, to the situational role of power and the like. Besides putting its finger on the potential relevance of the social for understanding science, constructivism has raised the question of what role one should accord, in a theory of knowledge, to the *reversals* practice brings about - the reversal of universal standards through local conventions and opportunities, the

reversal of rules through power, and the replacement of social and other characteristics of persons through situational features. With respect to this role, philosophy is called upon to think beyond Wittgenstein and Heidegger and to notice, besides the work of great scientists and the work of methodological commandments, the work of situations.[5] Finally, there is another issue raised by constructivism which invites philosophical thinking rather than philosophical denial of existence. This is the hoary old possibility that science, too, may be based upon circular reasoning, with conclusions shaping experimental action just as the outcome of this action shapes conclusions.[6] Closed circuitries in science come in many shapes and variants.[7] If we must have foundations for scientific knowledge, is it not conceivable that we might work out circular foundations? What worries me about attempts at reconciling constructivism with established doctrines is that they foreclose such possibilities. In bringing us home to the received pillars of disciplinary belief, they prefer, within the perspectives from which they criticize constructivism, disciplinary regress to disciplinary progress.

Do Representations Create Their Objects?

There are other differences I have with Sismondo which are more substantial. One concerns his view of constructivist conceptions of the relation between the world and its representations. Sismondo alleges that (some) constructivists conflate ontology with epistemology, or the existence of the world with what we know about it, by claiming that material objects are constructed out of world views. Though I am not specifically accused by Sismondo of this 'epistemic fallacy', I have been accused of it in the past.[8] Also, I find a version of the thesis Sismondo objects to quite plausible, and want to make clear why. My version of the thesis has been that science secretes an unending stream of entities and relations that make up 'the world'.[9] Now whether this thesis is rejected (as Sismondo would have it) seems to me to hinge not on whether one grants the pre-existence of an (unknown) material world, which to my knowledge every constructivist has granted, but on whether one assumes the pre-existence of specific objects before they have been delimited by science in precisely the way they are delimited by science. It is the latter view which some constructivists find implausible, and replace by a less transcendental view of ontology.

Consider some problems. Science changes its views about the character of natural objects, so the precise sense in which these objects are supposed to have pre-existed as scientifically delimited objects *independent* from us is not very clear. The existence of a scientifically delimited world is a variable over time, not a presuppositional constant that fulfils the requirements of a humanly independent ontology. One could hold that TRF always existed in the way described by Schally and Guillemin only if one believed that every Tact' turned out by science is an eternal truth, a belief contradicted by history and by common sense. What pre-exists before scientifically delimited objects are culturally delimited objects, those humans pick out and encounter and deal with in everyday life. This sort of existence is (or used to be) relatively independent of science, but it is not independent of space and time. In other words, existence is again a variable, one that flows from, and is captured by, cultural designation. Is there another kind of existence? What about non-designated, not distinguished material forces which make their presence felt by unaccountably affecting us one way or another? They too can be granted, and perhaps science, and other cultural universes, can make inroads upon them. But it is clear that only after they are somehow designated or distinguished, built into our accounts and represented, recurringly encountered or recurringly avoided, can they be said to 'exist' in any specifiable, concrete sense of the word.

Existence may not require 'knowledge', but it does require the making of distinctions, recurrent forms of interaction or reference, and the like. As a constructivist, one subscribes to an ontology that anchors existence in the world, not outside it. While the existence of the world as a material, physical entity independent of us may be granted on principle, the existence of specific objects identified in terms of their character cannot so be granted. Specific ontologies flow from cultural practices and hence must be seen as secondary, not primary. Pre-existence itself is a historically variable phenomenon; what objects are thought to have pre-existed changes with these cultural practices and with scientific belief. Thus specific scientific entities like subatomic particles begin to 'pre-exist' precisely when science has made up its mind about them and succeeds in bringing them forth in the laboratory. In all this, there is no conflation of epistemology and ontology because existence can be established on grounds other than representations and 'knowledge'. On the other hand, within science, existence does seem to flow from beliefs which involve what we customarily call 'knowledge'.

There is another sense in which Sismondo, in my opinion, misconstrues the thesis that the world is a consequence, rather than merely a precondition, of scientific accounts. This is the sense in which he makes it appear as if the material world were an automatic consequence of scientific knowledge, as if 'giving rise to an object' were the act of the snap of a finger rather than of long processes of accomplishment, struggle and failure. In other words, Sismondo's formulation ignores *the work* of translating scientific accounts into practice, of solidifying objects which exist only on a laboratory scale or only as representations, of making these objects recur outside the laboratory, of construing contexts in which occurrence and encounterability are possible, of construing a new world together with new scientific entities. While this process may be triggered by scientific representations once they exist, it involves, like the accomplishment of these representations, material labour and cultural (and social, political, economic and the like) intervention. The constructivist literature documents at least part of this labour. It also documents failures to bring certain objects and effects into existence, in the laboratory and in the wider context.

Strong Constructivism Is Constructivism On Its Own Terms

In defining ontologies as part of historical experience, constructivism makes a characteristic shift: it takes a philosophical question or concept and reconstructs it within the domain of empirical analysis and theory. Constructivism owes many questions to philosophy, a fact picked up very well in Sismondo's paper. What is often less well understood is that, for constructivist studies, what is important is not just the question, it is the shift. In other words, these studies display a 'take the question and run' mentality toward philosophical concepts, and it is where they run to and how they reformulate the issue that accounts for their interest. However, their ambivalent relationship with philosophy also gets these studies into trouble - the trouble consisting in the belief that constructivism must be understood, or must prove itself worthy, in relation to philosophical doctrines. Sismondo, for example, calls a strong constructivist thesis one that is least plausible given philosophers' conceptions of ontology. From a sociological perspective, however, it is precisely the reformulation of ontologies as a consequence of science that brings into focus the instrumental, symbolic and political work required to refurnish the

world with new, scientifically derived objects, work that may involve displacing older centres of authority like the church, that involves rewriting textbooks and history, and so on. A strong constructivist thesis, with respect to ontology, is one that shows how the world is slowly moulded into shape in ever new ways through successive generations of (scientific) practice. On a general level, it is one that most felicitously performs the reformulation of recurrent questions, the felicity condition being that reconstruction leads to new enquiries and fresh food for thought. This, in any case, was the strength of the constructivist thesis in the original laboratory studies: they shifted the question from the consideration of the relationship between the world and scientific representation which had been addressed by realist, scepticist, instrumentalist and such-like doctrines, to an enquiry into the constructive process of world making.

Other shifts of a similar kind have been fruitful in the past, or would appear likely to be so in the future. One can move from working out philosophical epistemologies to trying one's hand at identifying the concrete relationships between the knower and the known that different sciences implement in what they do. Or one can move from seeing the laboratory as a non-essential environment for the performance of experiments to seeing it as an agent that brings epistemic revenue to experimental science. For example, if the laboratory is made up of scientific reconfigurations of the natural in relation to the social order from which epistemic profit can be reaped, its artificiality becomes a condition for the success of science. With respect to this success, other problem shifts are thinkable. Sismondo acknowledges, but does not seem to believe, the lesson one can learn from the example I once cited of the mouse that successfully runs from the cat: the lesson is that we need not assume that the mouse carries a correct representation of the cat's enmity in its head. Sismondo, however, appears to continue to think that correct representations are necessary to explain the success of science. But what if we merely assume that a science, in picturing the world from within the closed circuitry of its own reconstructions, simply reacts to failures to make things work by changing its procedures until they work? Success in this case is explained by the use of strategies that accommodate the obdurateness of the worked-on world, not by correct representation of the world. Scientific enquiry can, while producing fictional representations of the world, still be empirical in the sense of such reactive adaptations.[10]

The philosophical beliefs in terms of which constructivism in science studies is usually evaluated and critiqued have another disadvantage: they prevent us from criticizing constructivism on its own terms, perhaps as an empirical theory of knowledge. Consider what I think is one of the major flaws of constructivist thinking in science studies: its conception of construction in terms of the moves and activities and negotiations of individuals. Callon and Latour have opened the gates to other entities such as non-human beings, but in conceiving of these as actors (or actants), they close them again on a world locked into strategic action. However, construction today is also, and perhaps increasingly, the work of machineries of construction. Machineries involve the orchestrated work of layers of entities superimposed upon each other in more than one dimension, and fuelled by more than one source of energy. They display a certain viscosity which we need to penetrate if we want to shed light on the conjoint functioning of their elements. The trouble with an often historically oriented study of science is that it leads us back to an individualist conception of society, either because it works on a period in history when individuals mattered in a sense different from today, or because we lack the records of the larger arrangements that existed. What sociology could infuse into contemporary pictures of science is a notion of these more entrenched arrangements. Of course, some scientific fields may still run on the kind of interactional patterns which the negotiation metaphor and its correlates suggest, but not all do. Even if they do, it may be wise to consider the individuals that make up the interaction as themselves the product of expedients and mechanisms which enhance agency, and which need to be studied.

There are other questions constructivism raises - for example, whether it might not itself be a variable characteristic of certain historical developments. If one looks at fields like Artificial Intelligence, Economics, Experimental High-Energy Physics and Genetic Engineering in Molecular Biology, it could be argued that science itself has become more constructive in the past decades - that it is turning away from a kind of empiricism that contrasts with constructivism. This, of course, introduces another wrinkle of the constructivist argument, another addendum to the Sismondo paper. What I want to conclude with, rather, is a brief methodological note. Constructivism has proven to be a useful tool in supplementing philosophically intuited accounts of science by analyses of scientific practice - that is, by turning the methods of science upon itself. In my

opinion, it is not, however, a world view, or a life-time occupation, or an optics that is equally fruitful regardless of the domain. Conceivably, there will be a time when it is more useful to take the questions and run from constructivism. If constructivism gets in a bind with realism and, on the sociological side, gets stuck with working out negotiations, this time will come soon.

• NOTES

1. Sergio Sismondo, 'Some Social Constructions', *Social Studies of Science*, Vol. 23, No. 3 (August 1993), 515-53.

2. The claim that has been most criticized with respect to the role of social factors is the notion that social interests affect the making of knowledge. For a recent criticism, see T. Kuhn, 'The Trouble with the Historical Philosophy of Science', Robert and Maurine Rothschild Distinguished Lecture (Cambridge, MA: Harvard University, Department of the History of Science, 1992).

3. To be sure, some philosophers have already made a start: see H. Putnam, 'The Meaning of Meaning', in Putnam, *Mind, Language and Reality: Philosophical Papers, Volume 2* (Cambridge: Cambridge University Press, 1975), 215—71. See also Joe Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca, NY: Cornell University Press, 1987).

4. These features are all addressed in the first laboratory studies cited by Sismondo (summarized in Chapter 2 of my 1981 book). See also K. Knorr-Cetina, The Micro-social Order: Towards a Reconception', in N.J. Fielding (ed.), *Actions and Structures* (London: Sage, 1988), 21-53, esp. 27-32.

5. For a philosopher who has noticed the work of situations but still reverts to explaining science in terms of individual mental events and their development, see Ron Giere, *Explaining Science: A Cognitive Approach* (Chicago, IL: The University of Chicago Press, 1988).

6. This point has been argued theoretically most forcefully by Woolgar with regard to the new sociology of science's neglect of its own circularities: see, for example, S. Woolgar and M. Ashmore, 'The Next Step: An Introduction to the Reflexive Project', in Woolgar (ed.), *Knowledge and Reflexivity* (London: Sage, 1988), 1–11, esp. 7-9. For an example from the natural sciences, see K. Knorr-Cetina, 'Epistemic Cultures: Forms of Reason in Science', *History of Political Economy*, Vol. 23, No. 1 (1991), 105-22.

7. For a science that turns closed circuitries into a principle of knowing see experimental high-energy physics (K. Knorr-Cetina, *Epistemic Cultures*, Chapter 3, forthcoming 1994).

8. The offending assertions are summarized, for example, in my chapter Towards a Constructivist Interpretation of Science', in K. Knorr-Cetina and M. Mulkay (eds), *Science Observed: Perspectives on the Social Study of Science* (London: Sage, 1982), 115–40, esp. 123-35.

9. Ibid., 135.

10. The lesson to be learned form the mouse is reinforced today by similar conceptions which come out of the biology of cognition. For a sociological reformulation of concepts derived from it, see N. Luhmann, *Die Wissenschaft der Gesellschaft* (Frankfurt: Suhrkamp, 1990).

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Responses & Replies (continued)

Response to Knorr Cetina

Sergio Sismondo

Ambivalence to Philosophy

Karin Knorr Cetina quite rightly points out that constructivist studies of science have an ambivalent relation to philosophy of science: on the one hand they 'take the questions and run', leaving philosophical methods and presuppositions behind; on the other hand constructivist authors attempt to compare their work to, and justify it with respect to, those same methods and presuppositions. Knorr Cetina's 'Addendum' reflects this ambivalence:[1] she claims that her 'strong constructivism' (roughly my 'neo-Kantian constructivism')

is a fruitful methodological precept and presumably doesn't need justification in any but pragmatic terms, but she also wants to claim that it (or at least her own nominalist version) is *philosophically* justified.

Perhaps it is difficult to take the questions and run because constructivism's proponents tend to see constructivism as fundamentally a metaphysical position, a school unified by a core belief: neo-Kantian constructivism, the thesis that representations construct their objects. That is certainly the impression that I get from Knorr Cetina's response; although she agrees that there are different versions of the neo-Kantian thesis, she also implies that constructivism is committed to one or another version.[2] In contrast, I find it fruitful to think about constructivist science studies as unified (to the extent that it is) by a collection of tools and a presupposition that science is to its core a social enterprise. Leaving behind neo-Kantian constructivism in favour of other uses of the constructivist metaphor moves us away from the ambivalence that Knorr Cetina notes: by not seeing constructivism in science studies as a metaphysical thesis about the non-independence of causal structures, science studies can lay aside that argument with philosophy of science.

I am not convinced that neo-Kantian constructivism is in fact a fruitful methodological precept, at least not in the way that Harry Collins's 'relativism' is fruitful. Constructivism in social studies of science is many things - different types of entities can be socially constructed, in a number of different ways - and the neo-Kantian thesis is not a part of most of constructivist studies, except as an occasional flourish. In addition, by making ontology necessarily culture-dependent, it obscures the mechanisms by which scientists and others sometimes materially construct the world; in these cases it points us in the wrong direction. Thus it is difficult to see how neo-Kantian constructivism can be methodologically advantageous.

I also don't find the arguments for neo-Kantian constructivism *per se* very convincing, though the issue is more complicated; I only made a gesture toward arguing that case in my paper,[3] and I can do little more here.

Cat and Mouse Games

Mice run from cats, at least sometimes. To explain this we need not assume that mice correctly represent the danger they are in when

faced with a cat, only that they respond in the right way. However, I am unconvinced by the analogy between mice and scientists: some of scientists' successes in manipulating, explaining and predicting features of the world need to be explained partly in terms of representations. Knorr Cetina asks: 'what if we merely assume that a science . .. simply reacts to failures to make things work by changing its procedures until they work?'[4] Such an evolutionary picture of science requires accurate representation no more than does an evolutionary picture of mice. The evolutionary picture is attractive, but it still leaves us with the task of explaining how science manages to react as well as it does, given what we should know about the tasks of prediction and control.

I take it as a given from Knorr Cetina's discussion that she agrees that scientific knowledge is sometimes pragmatically successful - that it seems to account for what we see, at least in the laboratory. I also take it as a given that our knowledge is extremely underdetermined by the evidence; this is one of the premises of much work in science studies.[5] Then it seems that the most we can say about our best scientific knowledge is that it is successful. But that successful knowledge is built on the backs of other successful knowledges, in a number of senses. For example, any particular piece of knowledge is tested in only a few, select arenas, against a small number of alternatives; the selection of these alternatives is made on the basis of prior assumptions or other knowledge - in Knorr Cetina's terms, it is 'decision-impregnated'. Thus even the judgement that something has been tested typically rests on a body of assumptions - assumptions which, among other things, indicate the plausible alternatives. If these assumptions were not sometimes approximately true then it would be extremely difficult to understand how scientists achieve the pragmatic successes they do. The evolutionary process would not get off the ground, for any tests would be close to meaningless. In traditional philosophical terms, the theory-dependence of successful methods should lead us to expect the approximate truth of the theories on which they depend.[6]

Although this argument is aimed at Knorr Cetina's empiricism, it also suggests that her nominalism can't give us the whole story. She says that 'while the existence of the world as a material, physical entity independent of us may be granted on principle, the existence of specific objects identified in terms of their character cannot so be granted. Specific ontologies flow from cultural practices and hence must be seen as secondary, not primary'.[7] Of course, some portions

of this nominalism are right: there are plenty of alternative ideas of ontology, and how we carve the world up is culture- and interest-dependent. But the above argument claims that we need some (perhaps small) measure of approximate truth about the properties of objects in order to start getting pragmatically successful knowledges. And I don't know how to think about approximate truth about properties in conjunction with a wholly nominalist view of ontology: *some* conceptions of ontology must map the kinds of things there are in the world better than others.

'Science changes its views about the character of natural objects',[8] but that doesn't get us very far in the direction of nominalism; all it tells us is that there have been different attempts at representation. We usually understand this when we are talking about more mundane objects, and there's no obvious reason why this should be any different for TRF.[9] And, contrary to Knorr Cetina, such a view doesn't require one to believe that every scientific 'fact' is an eternal truth: it requires exactly the opposite, that some representations are better than others. Were every 'fact' an eternal truth, we would have to accept the materiality of contradictions galore.

Motivations

Knorr Cetina asks about my motivations for engaging in this type of philosophical discussion of constructivism, for apparently trying to reconcile social studies of science and philosophy. One of the reasons is simply a perverse interest in metaphysics on my part; Knorr Cetina points out that I am not alone among constructivists in this. I am also not alone among constructivists in having difficulties with the arguments for neo-Kantianism; Bruno Latour, for example, somewhat disingenuously says that 'it is absolutely impossible to be convinced by a constructivist argument for more than three minutes. Well an hour, to be fair'.[10] In the case of my review, the meaning of a 'reconciliation', such as it is, between constructivist science studies and more received views is simply helping constructivists to avoid a less than completely plausible position. I do this in part by promoting some other types of social construction, in part by showing the inadequacy of some arguments for neo-Kantianism.

A further reason for looking critically at these different constructions is an interest in maintaining constructivism's critical potential. For the political critic of science (for example, the feminist critic), there is a problem with neo-Kantian constructivism much more obvious than any of the ones that I give. One of the things against which the feminist wants to argue is specific scientific claims about women: in short, the 'feminist empiricist' project is not something that any feminist doing science studies would want to abandon lightly. When nineteenth-century doctors advised pubescent girls to stay indoors, get lots of rest and do light housework, they were participating in the social construction of middle-class housewives. This form of social constructivism few feminists would want to deny. But along with the prescription came some reasons. These doctors claimed that girls' ovaries would not form properly if they received too much exercise, and that sterility would result. The strict neo-Kantian constructivist would have to say that the doctors were right, that their consensus created the truth.[11] The feminist empiricist, and for that matter almost any feminist, wants to argue that these doctors were simply *wrong* in their consensus, and that women's bodies are not malleable enough to be constructed so easily by scientific representations of them.

To me the rift between these two positions looks too great to be healed. Neo-Kantian constructivism gives the political critic some tools with which to work: a debunking attitude toward science, and some interpretative skills for recognizing interestedness. But it withholds a key tool: the concept of *mis*representation. This constructivism does not allow for misrepresentation in science, because representations create their objects. For the critic who is particularly concerned about science's attempted representations of herself and her society, the concept of misrepresentation is the crucial tool.

The problem goes slightly further than misrepresentation, for the political critic certainly does not want to accept that she is constructed (in this sense) to meet science's images. And since she knows that she has not been so constructed, she knows that this form of constructivism does a poor job of diagnosing power relations. The bodies, brains, intellects and societies that science sometimes misrepresents are not as powerless in the face of description as neo-Kantian constructivism would have it. They do not simply change with the changing approaches of the biologists, psychologists and anthropologists. So in this fairly trivial way,

neo-Kantian constructivism makes the asymmetries of power much larger than they actually are.

When trying to understand what is right about neo-Kantian constructivism, I prefer pluralism to nominalism or idealism. Ontology is interest-dependent because where we look has effects on what we find. What pieces of equïpment scientists can put in their laboratories, what other resources they have access to, and how they are trained help to determine what they can study. Pre-SSK historians and philosophers of science taught us that observation is theory-laden; SSK and social studies of science more generally teach us that knowledge is interest-laden, culture-laden, practice-laden and context-laden. There are many potential sustainable scientific practices that we can construct, and these hook into the world in different ways. This is to recognize the 'construction' metaphor as a metaphor, but to take it seriously as such.

NOTES

1. Karin Knorr Cetina, 'Strong Constructivism - from a Sociologist's Point of View: A Personal Addendum to Sismondo's Paper', *Social Studies of Science*, Vol. 23, No. 3 (August 1993), 553-63.

2. Thus the focus of Knorr Cetina's 'Addendum', and consequently my response to it, is neo-Kantian constructivism. Whether this use of the 'construction' metaphor is or is not central to constructivist science studies, the issue of the status of neo-Kantianism seems central.

3. Sergio Sismondo, 'Some Social Constructions', *Social Studies of Science*, Vol. 23, No. 3 (August 1993), 515–53.

4. Knorr Cetina, op. cit. note 1, 560.

5. See, for example, H.M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Chicago, IL: The University of Chicago Press, 2nd edn, 1992).

6. This argument is a very pared-down version of one given by Richard Boyd, for example, in his The Current Status of Scientific Realism', in Jarrett Leplin (ed.), *Scientific Realism* (Berkeley, CA: University of California Press, 1984), 41-82.

7. Knorr Cetina, op. cit. note 1, 558.

8. Ibid.

9. There *is* a sense in which TRF, as described by Schally and Guillemin, is not independent of humans, because before the mid-1960s TRF didn't exist isolated from bodies, whereas now that is a perfectly normal state. Endocnnological work ignores as much of that bodily context as possible.

10. Bruno Latour, 'The Force and the Reason of Experiment', in H.E. Le Grand (ed.), *Experimental Inquiries* (Dordrecht: Kluwer, 1990), 49-80, at 64.

11. Knorr Cetina (op. cit. note 1, 559) points out that I misrepresent neo-Kantian constructivism a little, by downplaying the *work* that scientists have to perform in order to construct the world. She is right, and I admit to bending the position for rhetorical value, but not much hinges on it. Neither the arguments that I sketch in my paper (op. cit. note 3), nor those here, change at all if we put in place a slower-acting constructivism.

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