FQS FORUM: QUALITATIVE SOCIAL RESEARCH SOZIALFORSCHUNG

Volume 3, No. 3, Art. 15
September 2002

Signing for Reflexivity: Constructionist Rhetorics and Its Reflexive Critique in Science and Technology Studies

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Key words: reflexivity, Science and Technology Studies, representation, constructionism, relativism

Abstract: I argue that reflexivity should not be seen as being primarily about the relationship of scientific writing to the realities studied—as it is often understood. In trying to establish this point I examine the discussion about reflexivity in science and technology studies (STS). The STS reflexivists claimed that the relativist and constructionist STS undermined reflexively themselves by arguing that *all* knowledge is situated and socially constructed. In the face of this reflexive problematics they suggested that "new literary forms", which manifest the constructed nature of scientific text, should be adopted. It seems to me that this program of *inscribing* reflexivity was semiotically misguided, which contributed to its demise. On the other hand, I argue that the basic reflexive point about the paradoxicality of making general claims about the local and contingent "nature" of knowledge is sound and that it should have deserved more attention in the constructionist rhetoric.

The second part of my paper draws some more general methodological points from the STS case presented. I am especially interested in the performative aspects of signing for methodological novelties and ask whether it is sensible to talk about "reflexive methodology".

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> "They said that they were weavers, claiming that they knew how to make the finest cloth imaginable. Not only were the colours and the patterns extraordinarily beautiful, but in addition, this material had the amazing property that it was to be invisible to anyone who was incompetent or stupid."
>
> H.C. ANDERSEN: The Emperor's New Clothes

1. Introduction

There is both nothing new and yet something seemingly urgent about reflexivity. In philosophy it is a well-known phenomenon manifesting itself in logical and linguistic paradoxes but also used by the philosophical method itself. In philosophical argumentation it is common either to refer to what one is *in fact* committed to in making a given assertion, or to what one *must* be committed to if an assertion *in principle* is to be *meaningful*. The first one of these strategies makes use of the pragmatic, or performative, self-reference, whereas the second one has a transcendental orientation (BARTLETT 1987, pp.10-11, see also GASCHE 1986). [1]

The philosophical tradition notwithstanding, the reflexive problematic dubbed as "reflexivity" is a relatively recent topic that has been discussed from the 1970s onwards in various fields of empirical study, such as anthropology, sociology and science and technology studies. As the discussion of reflexivity has flourished, the term reflexivity has taken multiple meanings. The same term has been used when talking about modern societies or "modernity", "agents" or subjects, the "participant's" methods of accounting for their reality and finally about epistemological and methodological issues more generally (e.g. BOURDIEU & WACQUANT). This proliferation of reflexivities poses the question of why reflexivity seems to be a distinctively "postmodern predicament"? (LAWSON 1985) [2]

The first thing to note is that the issue of reflexivity has arisen typically in the discussions about representation and objectivity. Once it had become accepted that knowledge is socially and culturally produced and historically contingent, the stage was then set for questioning the objectivity of our scientific representations. This then lead to the reflexive problematics concerning the terms on which we are pursuing our research. Yet, the requirement of self-reflection has been for a long while a well-established part of what might be called a *good scientific practice*. What distinguishes, then, the reflexive worries from the more familiar urges on criticism and introspection is their nearly exclusive focus on the epistemological problems concerning representation. [3]

Indeed, if there is one common thread running through the diverse post-spirited discussions, a good selection for that is the so-called crisis of representation.

Reflexivity is an outgrowth of this concern. For instance ALVESSON and SKÖLDBERG define in their recent *Reflexive Methodologies* the question of reflexivity as "above all a question of recognising fully the notoriously ambivalent relation of a researcher's text to the realities studied" (2000, p.vii). They also emphasise the importance of ontology and epistemology of social scientific research instead of concentrating on the qualitative methods themselves—which, as they note, has created a burgeoning branch of literature recently. In their view, fixing the attention on procedures and techniques tends to draw "attention away from the fundamental problems associated with such things as the role of language, interpretation and selectivity in research work, thus underrating the need for reflection" (p.2). [4]

If we do not, thus, expect scientific method to be an automaton producing reliable representations of reality, we have to inquire about the role of subjectivity and discourse in scientific representation. A straightforward conclusion of this—it has seemed to many—is that we should somehow *inscribe* subjectivity and reflexivity in our texts. A new kind of scientific writing should emerge. In the field of anthropology, for example, this has meant attempts to implant different subjectivities (or "voices") into the texts, whereas in science and technology studies an even more ambitious project was undertaken—that of criticising the scientific writing in the form of scientific writing. [5]

Now the question is, granted that reflexivity makes us reconsider the ways we represent, whether it is a problem that needs to be solved, or endorsed, by writing otherwise. It appears to me that this is not the case. In the following I shall argue for this in two ways. Firstly, I shall give reasons for why the attempt of making more reflexive—or subjective—texts is bound to lead to just a new rhetoric. Secondly, I ask whether reflexivity should be regarded as a problem in the first place. It seems that the wholesale criticism of representation regenerates the epistemological "picture" it seeks to avoid and that this can happen easily to any other sort of radical criticism as well. Consequently, I argue that reflexivity and other like methodological "novelties" should be rather seen as manoeuvres to upset the established hierarchies and distinctions rather than any attempts to go beyond them—as some of the protagonists seem to imagine. [6]

In the following, I shall use the reflexive controversy in the field of science and technology studies (STS) as a practical example in developing my arguments. This debate is, to my mind, especially interesting, as, despite of its having been both radical and epistemologically subtle, it did not have much effect on STS scholars' ways of writing. This makes one wonder whether there was anything substantial about the debate, and if there was, then why did it peter out without apparently any lasting effect? I suggest that the fate of the reflexivity in STS tells us something about the functioning of our intellectual markets where anything fashionable is often accepted quite uncritically only to be discarded as easily on the arrival of the even more "new". [7]

This presentation proceeds as follows: After introducing the case of reflexivity in STS, I shall examine the main arguments and proposals of STS reflexivists from

both semiotical and philosophical point of view. Drawing on this discussion I shall, then, reflect the specific methodological lessons that can be learned from this case. [8]

2. Turn, Turn, Turn—Reflexivity in STS

> "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."
>
> COLLINS and YEARLEY 1992, p.304

For a scientific study of scientific study the reflexivity of that endeavour should appear obvious. However, the realisation of reflexivity was a matter of more than nanoseconds in STS and yet reflexivity was on the agenda more or less in 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 "sociology of scientific knowledge" (SSK) 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. [9]

These kinds of explanations came to be 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. [10]

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 claimed that "interest-work" was constitutive of

scientific practice, and, thus, interests should also be investigated instead of being used as unexplicated resources for explanation. WOOLGAR admitted that no study is able simultaneously to study all the features of "a scientist's world" but suggested that this can become problematic especially if we remain committed to the causal-type of explanation. [11]

Indeed, it is questionable to simultaneously try to approach the practices of any scientific group as historically contingent and culturally specific, and seek for generalisable causal explanations. Because the interest explanations are typically particular and descriptive, they do not provide much insight into why the factors they present should be preferred to those of the alternative explanations (see ROTH 1998). However, WOOLGAR was not just contesting causal explanation, his reflexive program was much more utopian and ambitious in character. WOOLGAR was targeting the scientific explanation per se. This becomes clear from his subsequent dialogue with Barry BARNES, who replied him in defence of interest explanations (BARNES 1981).[1] [12]

BARNES identified, in response to WOOLGAR'S critique, the ethnomethodological preoccupation in explicating how the concepts are used, but he wondered whether any concepts can ever be "explicated" given that it is an endless task. And if so, he argued, the conventional scientist does not use concepts in any different sense than an ethnomethodologist does. BARNES admitted readily that his conceptions of interests "have been constructed by the analyst so as perform his explanatory work" (cf. WOOLGAR 1981a, pp.372-373). What else should they be constructed for, BARNES asked, and added that nothing at all prevents their further study and criticism (1981, p.493). [13]

WOOLGAR'S answer to BARNES is revealing. He announces that any attempt to "methodically" arrive at more accurate descriptions of reality is misguided, since descriptions themselves—or "accounts" (for an ethnomethodologist)—are constitutive of reality and thus no "definite descriptions" of anything can ever be produced:

> "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 not be set aside in favour of further attempts at explanation." (WOOLGAR 1981b, pp.510-511) [14]

In retrospect, it seems surprising how openly WOOLGAR *founded* his reflexive argument on the "essentially flawed nature of explanation". But let us take WOOLGAR'S suggestion at its face value: Is the project of explicating explanation feasible? Can a continued ethnographic research itself really succeed in eschewing the conventional, explanatory schemes and avoid suspicions about

1 Donald MacKENZIE (1981), whose work was, too, criticised in WOOLGAR'S text, also took part in the discussion.

the constructed nature of its own accounts? Soon, WOOLGAR came to doubt that, too (WOOLGAR 1982, 1983, 1988). [15]

2.1 Science "as it actually happens"?

If production of knowledge is social activity, then *this* knowledge is presumably socially produced too. Can there, in these circumstances, be any privileged method or approach with the help of which we could somehow find out "what actually goes on in science"? One way of interpreting the laboratory studies that were cumulating at the turn of the eighties is that they tried to realise this possibility, inspired by ethnomethodology and micro-sociology.[2] Their 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). [16]

WOOLGAR criticised this urge of laboratory studies to describe science as it happens of an instrumental conception of ethnography, which 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, p.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, that it is as social, contingent, local, situated, and so forth, as any other activity. [17]

In and of itself the finding of the social character of science is not very stunning news, as WOOLGAR remarked. Consequently, an alternative had to be put up, with which the "new" conception of science could be contrasted. This strategy provided, for quite a long period, a popular way to open up a STS-article. The alternative, old-fashioned and even damaging view, was conveniently provided by the "philosophical version" of science as rational activity oriented at finding the truth, or by any other "traditional" or "common" view of science. Typically, not much was usually said about this view—it was merely alluded to, or presented very briefly, in an uncontextual and general manner.[3] 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 views professed.[4] [18]

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2 Three well-known laboratory studies are LATOUR and WOOLGAR (1979), KNORR-CETINA (1981a) and LYNCH (1985). For micro-sociology, see KNORR-CETINA (1981b).

3 Consider the following, not atypical, statement: "Orthodox historians, philosophers, psychologists, and sociologists of science tend to want science to stand still and remain in good health while they study it. But while they are studying science—usually after idealizing it, in part by purifying it of its social trappings—the science is changing. They marvel at its 'success' without considering the contribution of pragmatic criteria, 'tinkering', and trial-and error technology to the 'success' of 'pure' science. Finally they act as apologists and ideologues of science" (CHUBIN & RESTIVO 1983, p.69).

4 Apart from this customary contrast to "traditional" philosophy/sociology/history of science, many other contrasts have been flourishing in STS writing (much in the same fashion as in other constructionist genres, too). Thus there have been attempts to contrast what scientists say with

The *irony* about the situation is that the constructionist[5] is not herself practising what she is preaching. According to WOOLGAR, the preferred constructionist 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, p.245). This conviction is, then, seasoned with differing amounts (depending on the constructionist in question) of constitutive intuitions that we construct realities by way of accounting them. Yet, when it comes to contrasting the descriptions of the constructionist ethnographer and those of the scientists she studies (or philosophers and other traditionalists), there seems to be no doubt about whose story is supposed to *fit* the reality best. [19]

Thus it seems that the constructionist is not really playing a fair game or is at least playing two different games at once. When it comes to others' scientific work, to their representations, they are "shown" to be constructions—or "contingent local accomplishments". The implication is that no scientific representation should be regarded as a truthful portrayal of real objects or processes, that there is no definite correspondence between our scientific representations and the reality they aim to explain or describe. In arguing for this view, the constructionist has created one more representation, but now, *it* should be taken as a correct depiction of its object, the science (which is, simultaneously, claimed to be in no way distinct from any other "ordinary activity"). The question is: Is it possible to be consistently constructionist? What could that mean? How to meet the reflexive challenge? [20]

2.2 The possibility of a *writerly* mode of STS writing

> "Pourquoi le *scriptible* est-il notre valeur? Parce que l'enjeu de travail littéraire (de la littérature comme travail), c'est de faire du lecteur, non plus un consommateur, mais un producteur du texte."
>
> BARTHES 1970, p.10

Side by side with before mentioned epistemological reflexivity there has lived another concept of reflexivity in STS, that of ethnomethodological reflexivity.[6] It draws our attention to how any description is not just about something, but that it is also doing something in the same time. In consequence, descriptions "are not merely *representing* some facet of the world, they are also *involved* in that world in some practical way" (POTTER 1996, p.47). Thus many ethnomethodologically

what they "actually" do—or with what "in fact" happened—and that what they write with what scientific work "really" consists of, and so on.

5 I am using the word "constructionism" instead of "constructivism", which is more commonly used by STS writers. In this I am following SISMONDO (1996) and HACKING (1999) who want to distinguish constructionism from the other forms of philosophical constructivism.

6 As already noted, WOOLGAR'S reflexivism has its roots in this conception of reflexivity, but he has developed it into an epistemological direction.

oriented researchers, notably the discourse analysts, have thought they practice reflexive research by studying the interactional means through which the social order and its products are achieved.[7] But from the point of view of epistemological reflexivity this is self-deceptive, since one cannot evade the dilemmas of scientific representation simply by replacing the *why*-and *what*-questions by *how*-question. A description of *how* an account (or representation) is achieved is still a representation itself, too. [21]

What to do, then, about these paradoxes of representation? Since there seemed to be no adequate methodical solution to the reflexivity problem, the reflexivists suggested that instead of trying to solve the reflexivity 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, p.489). Such were, for instance, the "second voice device", a funny and witty example of the use of which is provided by WOOLGAR and ASHMORE (1988), and other kinds of (constructed) dialogues trying to display their reflexivity. The aim of this reflexive "wrighting" (ASHMORE 1989) was to shatter the reader's "naïve 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. [22]

In trying to evaluate the proposal of adopting "new literary devices", one could first ask what else could we gain by such a transformation of our styles of writing than the questioning of literary conventions and our alleged epistemological habits (i.e. naïve realism). Not much more, the protagonists admitted readily. 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). Thus the reflexivist proposal to turn literary has the odd consequence of transforming all texts into "epistemological" exercises. What a loss of content! [23]

If reflexivism was not even promising to offer much—at least to scientific audience-then what about its success in its outspoken mission? The literary strategy of "new literary devices" was motivated by the insight that the description produced by a scientist is but a text and as such it has to display its "referentiality" or "realism" by textual means. A narrative appears as "realistic" if it follows certain conventions. But this applies to reflexivity, too. Also the "reflexivity" of a text is an effect of using certain literary conventions. Consequently, no more than a text can be inherently realistic, can it be inherently reflexive, either. Instead of more realistic or reflexive texts we end up producing texts full of signs of "realisticness" and "reflexivity". [24]

As reader-oriented literature studies have pointed out, any text becomes what it is, first in the process of its reading. Texts are elliptical and need a reader to fill in the missing information. For instance Umberto ECO (1979) describes this by calling texts "lazy machines" that need reader's collaboration. In consequence a

7 MANNING (1998) calls this variant of constructionism for "procedural social constructionism" as distinguished from "reflexive social constructionism".

text and a reader make an inseparable pair. It is difficult to draw a line between what is in the reader's experience and what is in the text (CULLER 1982, p.82). Thus what the text seems to be like is dependent on the background knowledge and practices that make up the context of the text's reading. When the context (and a reader) changes, so does the text. Consider, for instance, how our knowledge of the writer's advocation of new literary methods affects our reading of an article that is written in a customary idiom. (Aren't we inclined to suspect that there is something tricky hidden in the apparently straightforward style?). [25]

In fact, WOOLGAR'S reflexivist critiques were written in a form of a conventional critique. First after the point was successfully brought home, WOOLGAR and the other reflexivists started with (occasional) experimental writing. That the "new literary forms" should perform the epistemological task given to them depended thus critically on their being explained first in a straightforward manner. Hence, the conventions of discourse are not just constraints but something that enables communication and critique—also of themselves. The more experimental a text becomes, the more difficult to interpret it becomes. This is in line of the paradoxical characteristics of the "open" texts noted by ECO (1979). Even though the modernist texts give the impression of being open, they actually make heavy demands on their readers' competence and the way they should be interpreted to make any sense at all. This applies certainly to the reflexivist writings in STS, too, which explains partly the irritation reflexivity has awaken in those who have not dedicated themselves to its complexities. [26]

What is more, "the new literary forms" or other representative innovations, however critical and fresh in the beginning, tend to become in continued usage just another conventions; turning easily into superfluous epistemological hair-splitting or pretentious self-positioning. Thus it seems that one can fix no one way in how to write a reflexive text. There are no genuine "reflexive devices" nor any format for how reflexive texts should look like. And this is admitted by WOOLGAR and ASHMORE:

> "The interesting problem is to distinguish contributions which are patently not reflexive from those which are reflexive, but which deliberately conceal any ostensive signs of reflexivity. Apparently unreflexive texts may in fact be reflexive and vice versa ..." (WOOLGAR & ASHMORE 1988, p.6). [27]

This being so, is there any point in distinguishing "reflexive" texts? In retrospect, it seems fair to say that radical reflexivity of STS stumbled against its own impossibility. It's strategy, as argued above, was semiotically misguided in at least in two ways. On one hand, the reflexivists did not pay enough attention to the reception of the texts. On the other, they did not reflect that much on the possibility that any device taken into use becomes a convention itself, thus creating order, against which it was originally taken into use. Occasional celebrations aside, this failure is reflected in that the reflexivist critique did not have much impact on the way STS scholars write. Moreover, the active debate of the issue dried up already in the beginning of the 1990s, with the term "reflexive" remaining though, an established part of STS vocabulary used more or less

synonymously with the word "critical" (see, however, the Section 3.4 of this paper).[8] In my opinion, the reflexivists nonetheless made a genuine point about the epistemological dilemmas of whole-sale constructionism, which, as the prevailing rhetoric in STS shows, were passed by too easily. In the philosophical excursion of the next chapter I examine this insight. [28]

2.3 Reflexivity re-examined

The charge of unreflexivity levelled at the adherents of strong programme and constructionists was that their accounts of scientific study reflexively destroy the possibility of those accounts themselves.[9] This criticism makes use of the arguments from self-refutation commonly used in philosophy. As TOLLEFSEN (1987) notes, they have seemed attractive to philosophers because they do not appeal to any other claims than those made from the position thus attacked. Consequently, if successful they provide "conclusive stopping points in the dialectic of philosophical controversy. They establish limits beyond which that dialectic cannot go, beyond which significant philosophic controversy is impossible" (1987, p.209). [29]

There seems to be two principal ways of dealing with the reflexivity problem, either it is tried to be eluded or then it is accepted, even endorsed. To avoid the paradox induced by self-reference one can attempt to exclude from the reference of a claim the claim itself. This was RUSSELL'S well-known solution to his set-theoretical paradox. He argued that the problem was caused by illegitimate totalities, which involve themselves as a part of the collection they cover. A class, maintained RUSSELL, must belong to a higher logical level than the elements that belong to that class. Consequently RUSSELL'S move bans all-embracing claims such as "There is no truth". One can legitimately claim that there is not, for instance, any religious or aesthetic truths, but then at least some arena to the truth is still preserved (e.g. LAWSON 1985, pp.18-20). This strategy is implied by the research methods which assume that the social scientist operates on a different level than those studied. The difference between the levels is established by distinguishing between the "observer" and the "observed". The assumed "exoticism" of the object of the study makes it easier to maintain the difference (WOOLGAR 1988). When this device is borrowed from the study of "exotic" cultures to the study of our (sub-)culture(s) it gets some ironic overtones. WOOLGAR himself used this device in his laboratory study together with

8 One of the latest disputes on reflexivity took place in *Science, Technology, & Human Values* where WOOLGAR (1991) attacked the emerging "social study of technology" (SST) for uncritically applying the insights of sociology of scientific knowledge (SSK) to technology. In consequence, according to WOOLGAR, some of the "epistemic significance" of SSK gets lost in this constructionist move from SSK to SST. For an interesting exchange around this topic, see PINCH (1993) and WOOLGAR (1993). See also GIERE (1999) for a positive assessment of this "turn" to technology. For GIERE "a concern with technology counts against extreme views and thus for an enlightened post-modern synthesis" (1999, p.57). The connection between the postulated entities and the experimental reality can be tenuous in natural sciences, but this is not true of typical technological innovations (ibid., p.60).

9 I distinguish between the strong programme and constructionism, since not all constructionists think that it involves epistemological relativism (e.g. KNORR-CETINA 1982, p.321). The problem of relativism can be treated, in this case, as one variant of the problem of reflexivity, so that both the constructionism and relativism can be discussed in terms of reflexivity.

LATOUR (LATOUR & WOOLGAR 1986), but the irony behind the expressions such as "tribes of scientists" went largely unnoticed (WOOLGAR 1982) and some analysts continued, in a rather complacent style, to regard themselves as "ethnographers of science". [30]

What LATOUR and WOOLGAR'S *Laboratory Life* (1986) exemplify is that cultures and practices familiar to us can be made appear "strange" by using methods of detachment as well as one can strive to make one's ethnographic methods less privileged by reducing the distance between the "observer" and the "observed" by different research and representative methods. But this is bound to lead the analyst to reflect one's own practices in relation to those studied. In the case of science studies especially, it seems that the familiarity of the activities of objects of study cannot be played down easily so as to achieve an objective position that does not turn back against itself.[10] [31]

How can the relativist or constructionist theses of STS then be defended without giving them a privileged position? One way is provided by the interpretative flexibility of the component parts of an argument. Mary HESSE (1980) has used this strategy in her "equivocation defence" of the relativist claims of the strong programme. She claims that a relativist means different things than an objectivist by such expressions as "truth", "knowledge" and "grounds". A "truth" for a relativist, for example, means that what meets the criteria of truth in local culture. Therefore the non-relativist attempt to show that the relativists' claims about the relativity of the "knowledge" and "truth" to a local culture are self-defeating, since it makes use of the senses of "true" and "knowledge" excluded by the relativists' claims. But now a new problem appears: it is not clear that relativist and her critic are engaged anymore in a philosophic controversy (see TOLLEFSEN 1987, p.211). In anticipating this criticism, HESSE suggests that the relativist thesis should not be construed as a conclusion inferred from established premises but rather as a hypothesis about which we could then "consider whether its consequences are consistent with the rest of what we wish to affirm about knowledge" (1980, p.42). [32]

A non-relativist might counter this proposal by claiming that it works as long as the relativist's redefinition of the terms is ignored, and that any attempt of the relativist to offer evidence to her own position fails, because if it is to have any force, it has to happen in objectivist terms. Thus the equivocation defence removes relativist's position as such from the dialectic of philosophical discussion (TOLLEFSEN 1987, p.215). This is a kind of transcendental argument commonly used by various non-relativists (rationalists, realists etc.), according to which, if

10 This has not been accepted by all the participants in STS. Consider Harry COLLINS'S claims on replication that have been studied by ASHMORE (1988; 1989, Ch. 4). On COLLINS'S view scientific "replication" is a problematic and complex process, which involves social negotiation over the perceived similarities and differences between the events (e.g. COLLINS 1985). However, on the basis of the studies on replication he concludes that "one of the most well *replicated* outcomes of social studies of knowledge concerns the social negotiation of reproducibility" (COLLINS 1982, p.304). Collins explains away the apparent paradox by claiming the difference between natural and social world. Since social world can be treated by us as real, but natural world cannot, the social scientists can happily replicate their findings about the indeterminacy of replication in natural sciences (e.g. COLLINS 1981).

we are to have any philosophical discussion at all, we have to accept certain arguments as compelling and consequently as objective (e.g. HOLLIS & LUKES 1982). But if the burden of proof is placed on the objectivist's shoulders, the outcome is not that favourable to her anymore as she is ultimately unable to ground her position uncontextually. In the face of this deadlock of linear argumentation, dialectical thinking, which tries to overcome or mitigate the seemingly necessary opposites, suggests itself. Another way of looking at the problem is to admit that neither objectivism nor relativism can defend itself without appealing to the other. This calls for seeing the strife between objectivism and relativism as a continual "tensional transformation" in which challenge and co-operation cannot be told apart (see BERGGREN 1984). *Ergo*, perhaps the contending parties should pay attention to this situation of mutual dependence. [33]

3. Methodological Discussion

> "Everything changes and everything stays the same."
>
> BARNES 1981, p.489

Writing to a journal dedicated to qualitative methods I'm expected to provide some practical guidelines for doing qualitative research. Yet the main thrust of my article has been to show that ambitious methodological programs turn easily against themselves when complied too dogmatically. Instead of telling what should be done and how one should write I conclude by drawing together some lessons and implications of the STS case. In doing so I hope to provide some arguments through which one can reflect and evaluate the diverse methodological "standpoints" that have inspired much qualitative research lately. [34]

3.1 Global localism?

The same kinds of doctrines circulate in the post-modern fields. It has been popular for quite a while already to find out, or simply presume, that the activity one is studying is social, situated, locally accomplished, contingent, and so forth. This has been accompanied by certain methodological prescriptions. It has been supposed that if any phenomenon *is* locally accomplished, then it has to be studied as confined to a certain local situation and by using ethnographic participant-observational methods. Still, we might do well to reflect a bit the claims on which we thus found our methodological preferences. Specifically, the assertion that *all* activities and knowledge are local, situated and socially accomplished contingent achievements seems to be either trivially true or leads to reflexive paradoxes. In arguing for the "inherent" locality and contextuality of any activity or knowledge one is making general (and thus global) claims. Accordingly not even a devoted constructionist can avoid making general and essentialist claims. [35]

What this amounts to is that one should be more modest in one's claims—and less strict about one's method—and concede that one's adversaries usually have

a point, too. This is one way to interpret what ALVESSON and SKÖLDBERG (2000) are after when they propose that reflexive methodology consists of "various mixes of empirical work, meaningful interpretations, critical reflection and linguistic-textual self-reflection" drawing insights from as different traditions as grounded theory, hermeneutics, critical theory and postmodernism. Reflexivity, for ALVESSON and SKÖLDBERG, arises when these different elements or levels of interpretation "are played off against each other" (p.249). Be that as it may, I suggest that for this kind of approach to lead to results instead of ending up in confusion, the old-style reflection focusing "upon a specific method or level of interpretation" (p.2) is still needed. Actually, it seems that many empirical studies have already been drawing from different kinds of sources and moving in various interpretative levels. Hence, what ALVESSON'S and SKÖLDBERG'S "reflexive methodology" does is to legitimise the existing practice. [36]

Last but not least, if locality and situatedness are taken too seriously, as a methodological prescription to do only in-depth, on-site and small-scale ethnographic research, one is left without means of studying many phenomena including the question of how and why the ideas motivating this kind of research are disseminated globally. [37]

3.2 The "impossibility of objectivity" and the novelty of the new

In addition to the "inherent" locality and situatedness of any knowledge, the "impossibility of objectivity" is repeatedly affirmed in recent methodological discussions. What is more, this is often taken to imply that some "new" kind of methodology, which does not hang on the illusionary promise of objectivity, is needed. Thus for instance it is argued that if an enquiry cannot but be partisan, it should be openly and purposefully so (see HAMMERSLEY 2000 for a critical evaluation of these claims). Now according to the problematics of reflexivity treated above, any attempt to claim objective status for the assertion about the impossibility of objectivity is contradictory. This does not mean that partisanship methodologies per se should be rejected. I am simply claiming that the decision of whether or not to adopt a partisanship strategy in one's research should be done on more specific reasons than on claiming that objectivity—or neutrality—is unattainable anyway. Besides, as the claims of the reflexivists and advocates of partisanship show, it is extremely difficult, if not impossible, not to be committed to objectivity and continue doing science. As we have seen, most STS constructionists claimed, at least implicitly, objective status to their own findings. reflexivists, for their part, wanted to contract out of the objectivity altogether with the proposal of "new literary methods"—that for obvious reasons didn't attract those still interested in scientific research. In the case of the partisanship these problems are encountered in a more practical level. If a researcher qua researcher is practising partisanship, then is she not still trading on the assumed objectivity—or validity and representativeness—of scientific research? [38]

More often than not the "new" is presented and justified as a critique of the "old". Because criticism is easily parasitic on the view it criticises, the "new" approach then tends to reproduce part of the problem it tries to avoid. This is exemplified by

the reflexivist claim that the relativists and constructionists have remained captive of the realist view of science they tried to contest. But then, the reflexivists did not fare much better, either. For them the realisation that representations are constructions, which do not correspond to the reality in any definite way, awakened "epistemological horrors". Why? The key to this question is, in my view, that reflexivists were still inclined to assess representation according to its correspondence to the world. Though this time negatively. If representation does not correspond to reality as it should, our obligation as responsible researchers is to make this "fact" manifest in our texts. [39]

The situation changes if we allow that to give us knowledge about the world a representation needs not be a faithful reflection of it. If we treat representations as more or less descriptively accurate inference devices instead of treating them as something that stands for something else (the reality!), we can understand how and why representations are useful even though they are artificial and conventional constructions. Interestingly, in STS there has been some insightful studies done on scientific representation showing that the purposeful constructed-ness of representations and the artificial features added to them enhances rather than diminishes their epistemic value. Furthermore, scientific representations are often manipulated precisely in an effort to make them more true to the "nature"! (e.g. the essays in WOOLGAR & LYNCH 1992 and GALISON 1998) [40]

3.3 Field-methods vs. discourse

The reflexive obsession with the possibility or, rather, impossibility of representation as "the mirror of nature" (RORTY 1980) shifts focus from the problems of scientific method to those of scientific representation. This contrasts with empirical scientists' predisposition to justify what they have written in "field-method terms". It is thought that the research process itself bears on the acceptability of the claims of a scientific paper. According to, for instance, Clifford GEERTZ anthropologists have tended to trace their difficulties in constructing an ethnographic description to the problematics of field work rather than to those of discourse. Thus they have assumed that if the relation between the observer and observed can be managed, the relation between author and text will follow by itself (1988, p.10). It is precisely the certainty of this link that reflexivity interrogates. [41]

Even though reflexivism in STS did not succeed to free itself from the epistemological problems it tried to avoid, in concentrating to representation it nevertheless drew attention to the point that scientists' principal scientific products are usually texts. Therefore, the question of how to write is not a minor problem if only because what the scientists want to communicate to their community has to be mediated by their writings somehow. For example, the STS cases make often painful reading with their detailed accounts of specialised sciences. For anybody who is not a specialist in those fields these descriptions do not largely make sense and if, for some reason, a specialist would read these STS narratives, she would be seeking for other kind of information. Then what are these details for? Roland BARTHES (1982) has argued that the function of

the seemingly insignificant details that can be found from all Western stories is to create "the effect of the real". Seen from this point of view, the detailed accounts aim to mediate us the presence of the scientific work, but actually they just provide us signs of the text being grounded in "actual practices". In consequence the reader usually just skims them through, and rightly so. Certainly, the task facing those writing about scientific research to non-specialists is anything but simple. [42]

The metaphysical views or research methods subscribed to do not necessitate any certain style of writing. The link between a method and a certain style of writing is a product of a convention creating consequent expectations. In my view, many styles of writing will do as long as the reader is provided the information she might need in interpreting and assessing the work she is reading. That the conventional style of writing is usually chosen—even though many scientists admit that they would prefer to read more entertaining texts—results from its not making so heavy demands on the writer and the reader. Accordingly, we can approach rhetoric as not predominantly manipulation and one-sided persuasion but more positively as reaching out to our readers. Seeking to give the reader a good sense of the material and presumptions on which the conclusions of the study are based, we give her also a possibility to make her own connections and disagree with us.[11] [43]

Important as the interest in scientific writing has been, reflexivity should not be restricted to the questions of discourse only. If it does so, it ends up being an inverted image of the field-method approach—or "recipe-book-research"—which envisages research process as that of collecting, processing and analysing data. In either case the supposed research process itself is distinguished from its presentation in writing, as if one first did the study and then wrote down the "results". [44]

Reflexivity expands into dialogical dimension once attention is paid to the readers and their expectations. Another form of dialogicality is that between the researcher and the subjects studied. Once again, I do not think that there are any ready answers to the question of how this dialogue should be played out. I have myself been studying language-technological research and usually my informants, most of them familiar with humanities, were very eager to know about the aims of my study and my methods. As I tried to explicate these things to them, many interviews I (we?) did turned into interesting conversations about science, too. I also got useful tips from the subjects of my study and even started a common writing project with one of them (KNUUTTILA & VOUTILAINEN 2002). But of course, all this is possible because of the relative proximity between the competence and activities of myself and my informants. In the research group I am working in, we have tried to think what a dialogical approach to studying science and technology could be (MIETTINEN & HASU 1999). Minimum is that

11 Writing about "anthropology as writing" SPENCER notes how ironic it is that GEERTZ, who is the most hermeneutical of anthropologists, "adopts a literary practice which tries above all to close the hermeneutic cycle by limiting his readers' access to that which he wants to interpret himself" (1989, p.149).

we are committed to reporting to the people—whose work we have been studying—our preliminary results from the research process and to discuss our interpretations with them. This has proven also to be a valuable way of getting new information. Naturally, this dialogical process should be somehow made detectable and discussed about in the articles written. [45]

3.4 From the ethical and performative point of view

As reflexivity should not be too obsessed with a single researcher's text, it is bound to lead us to consider the larger contexts of research-process, readership—and scientific activity in general. Here, the reflexive, ethical and performative aspects of doing research mingle. For instance, we can reflect on ambitious (and often purist) methodological declarations from the performative point of view. What is it that is being attained: New strictures? Rhetorics against "tradition" or others in an attempt to win academic prestige? Rhetorics at the expense of fairness? [46]

Indeed, applying relativism, constructionism or any other form of criticism to other's research (or other activities and claims) should lead us to examine critically our own research. It should induce us to think through how we construct our subject-objects or opponents and how we present the views of our predecessors or other disciplines. This is especially important since at every moment there is a new generation of scientists growing, who form their opinion of history (or philosophy or sociology etc.) predominantly by reading recently produced texts in their own fields. The (modernist) imperative of science to find continuously something new is aided by the ignorance of the past. Should we enhance this state of things on purpose? [47]

Coming back to STS case, one would have expected the constructionist writers to be reflexively outspoken about what they were themselves *accomplishing* by their own rhetorics. Yet this has not been the case until recently.[12] The constructionist movement in STS was launched with a fierce polemic against the "traditional philosophy of science" and the "standard view of science". But to argue what kind of an activity science "really is" or is not, is to be engaged in philosophical discussion. As I have argued, to seek simultaneously to avoid the established epistemological problems and to contest them, to show them wrong, is to end up in being self-contradictory. Proceeding this way the constructionists have certainly not gone beyond any traditional distinctions they so eagerly challenged. [48]

In giving a charitable reading to constructionists, one can interpret them as practising deconstruction. The constructionist move can be understood as not

12 In a thoughtful article advocating activism—or "reconstructivist agenda"—WOODHOUSE, HESS, BREYMAN and MARTIN write: "[I]nasmuch 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: what social processes are setting our collective agendas; is the agenda-setting process a laudable one ... A criticism worth considering is the possibility that STS as a field of inquiry has tended to produce the hierarchies of the scientific research fields, which in turn reflect the funding priorities of a political economy of science ..." (2002, p.307).

getting rid of the distinctions handed down to us by tradition but as that of reversing them, of contesting the hierarchies invested in the distinctions such as global/local, theory/praxis, general/specific and so on. If such move is successful —and constructionism certainly has been that up to this moment—then, as the effects of it work themselves through academic and other communities, there is a change that a different kind of thinking emerges. [49]

The revolutionary and, yes, hostile STS rhetorics served its purpose for a while but now it is about to become counter-productive. It is rigidifying into new strictures, which can be seen in the way the texts produced display their methodological, political and theoretical correctness by using certain kind of language and words. They function as signals helping the members of the academia to identify their "own" group, despite the cherished "pluralism". The reflexive critique, in spite of its sensitivity to the paradoxes involved in making all-embracing claims, did not but reproduce the all too familiar pattern of sweeping critique and a new beginning by ending to propose "new" literary forms. [50]

Finally, the recent debate about activism or "politics of STS" has served to highlight the performative paradoxicality of STS constructionism.[13] The problem, quite simply, is the following: Given the debunking mode of much STS research, it is difficult to see how it could function as a basis for positive policy proposals. STS research has been most interested in deconstructing knowledge claims, yet in practice decisions have to be made and knowledge, however infallible, is needed for that. Researchers have taken two principal stands to this dilemma. On one hand there are those, who want to go "beyond epistemology" (JASANOFF 1996) and who seek for "extended reflexivity" that consists of "a more institutionally and politically located reflexivity" (WOODHOUSE, HESS, BREYMAN & MARTIN 2002). Typically these contributors think that constructionist STS *have* increased our knowledge about the nature and dynamics of science and technology notwithstanding their relativist thrust. On the other hand, many STS scholars have remained committed to the epistemological radicalism of STS. Thus for instance SINGLETON, after a careful empirical study on UK's cervical screening programme defends her denial to draw any normative conclusions from it on the grounds that "should discourses" be "oppressive and exclusionary" (1996, p.461). According to her, she has, in her study, suggested the "multiplicity and mutability" of her own identity and reflected her "contingent, ambivalent, multiple and mobile" stance (SINGLETON 1998, p.336). If there is, then, anything that brings together these rather different stands, it is the STS position, that according to JASANOFF "is always oppositional to other accounts that exist in parallel, often in widely accepted versions, in the academic literature or in life" (1996, p.412). As this is not perhaps the best way to enter into the political dialogue—or "negotiation"—with others, the constructionist rhetoric seems to turn against itself. [51]

13 See e.g. the Special Issue of Social Studies of Science, Vol. 26, no.2, May 1996, 219-468, edited by Malcolm ASHMORE and Evelleen RICHARDS, on "The Politics of SSK: Neutrality, Commitment and Beyond".

4. In Conclusion

I have argued that attempts to find a formula for writing reflexively (or subjectively) establish merely a new convention that functions as a sign of "reflexivity" (or "subjectivity") thereby naturalising and rigidifying the discourse once again—something that was precisely what one wanted to avoid. Consequently, instead of epistemological awareness one produces just another style of writing offered as a methodological recommendation. Is "reflexive methodology", then, a misnomer? Can there be any (one) methodology that is in and of itself reflexive—and consequently sticking to that methodology be a way to produce better research? At one point LATOUR writes that methodology should be replaced by style (1988, p.170). I think that the problem is exactly the contrary. Methodology too easily becomes just a style of doing research. A methodology thus transformed into a style is not reflexive, since no way of acting or representative convention per se is necessarily reflexive; to do so is giving up on reflection. [52]

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Citation

Knuuttila, Tarja (2002). Signing for Reflexivity: Constructionist Rhetorics and Its Reflexive Critique in Science and Technology Studies [52 paragraphs]. *Forum Qualitative Sozialforschung / Forum: Qualitative Social Research*, *3*(3), Art. 15, http://nbn-resolving.de/urn:nbn:de:0114-fqs0203152.

Revised 6/2008