

# Is 'Representation' a Folk Term? Some Thoughts on a Theme in Science Studies

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## Abstract

An influential strand within Science and Technology Studies (STS) rejects the idea that science produces representations referring to objects or processes that exist independently of it. This radical 'turn' has been framed as 'constructionist', 'nominalist', and more recently as 'ontological'. Its central argument is that science *constructs* or *enacts* rather than *represents*. Since most practitioners of science believe that it involves representation, an implication of the radical turn must be that 'representation' is a folk concept; perhaps even a myth or an ideology. This paper explores this anti-representationalism and its implications for the relationship between STS and mainstream social science, in part through drawing parallels with ethnomethodology.<sup>1</sup>

## Keywords

science and technology studies, representation, ontological turn, constructionism, ethnomethodology

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Science and Technology Studies (STS), and its predecessor the Sociology of Scientific Knowledge (SSK), have often been presented as rejecting ‘traditional’ philosophy of science, epistemology, and sociology of knowledge. Initially, this was on the grounds that these wrongly assumed that only false beliefs can be caused, or that only irrational or non-rational forms of thought are open to social science explanation. Subsequently, the radical character of STS has frequently come to be portrayed as principled denial of the idea that natural science produces representations of objects or processes that exist independently of it. Instead, it has been argued that science must be treated as *constructing* or *enacting* these objects or processes.<sup>2</sup>

This anti-representationalism has also been applied to *social science*; and, as part of this, some advocates of STS have directly challenged the methodological ideas that lie behind conventional social research, along with the traditional philosophy of science on which these are held to rely (Law 2003, 2004). And, certainly, anti-representationalism is at odds with the assumptions built into the *practice* of most social scientists, including those who express this doctrine themselves: while many are coy about using the word ‘truth’, they nevertheless present accounts that purport to represent accurately the characteristics of phenomena that exist independently of their research.

In this paper, I want to examine the implications of anti-representationalism for the relationship between STS and social science. In doing this, I will draw some parallels with ethnomethodology, which has been one of the main influences upon it.

## Anti-Representationalism

A convenient summary of anti-representationalism in STS is provided by Coopmans et al. (2014:2) in introducing a new edition of an influential collection of chapters on ‘representation in scientific practice’. They write that

Contributors to the original [...] volume made use of historical, sociological, ethnographic, literary, ethnomethodological, and conversation-analytic investigations, and sought to respecify ‘representation’ as practical action in social and material contexts. They stressed the roles of instruments and textual

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<sup>2</sup>This anti-representationalism dates back at least to Latour and Woolgar 1979. See the summary of its development in Fujimura 1991. Latour and Woolgar’s argument is that the phenomena scientists investigate do not exist prior to their investigations, but are brought into existence through a process of collective ratification, ultimately leading to their being black-boxed, in other words taken-for-granted and relied on in further investigations. Once this has happened there is resistance to dismantling black boxes because of the costs of this in effort and resources. However, should this happen, the previously existing phenomena would disappear.

formats, and the interactional and interpretive work surrounding them. Their emphasis on such public, practical, communicative, and textual work was set off against an established philosophical picture of representation as mental, verbal, or pictorial reference to features of an independent world.

While there is some equivocality in this passage (and I will go on to suggest that this is no accident), its radical message is carried by use of the term ‘respecify’, which is characteristic of ethnomethodologists’ discussions of the relationship between their work and that of conventional social scientists (Button 1991), by the apparent rejection of any referential relation between scientific accounts and reality, and above all by the implication that the features of the world studied by science do not exist independently of it.

This anti-representational stance is, of course, analogous to some more general versions of what has been referred to as social constructionism or social constructivism, for instance those which emphasise the constitutive role of discourse. Whether counted as constructionist or not (see Button and Sharrock 1993), some forms of ethnomethodology also display this anti-representationalism. The argument here is that, rather than language being viewed as a means of representing phenomena, it must be regarded as ‘performative’ in character – it is used to *do* things. More than this, its use creates the world we experience, rather than ‘mirroring’ an independent reality (Mehan and Wood 1975; Pollner 1978, 1987). An implication sometimes derived from this is that ‘representation’ is a folk concept; that it is ideological or mythological (Woolgar 1988:14,36 and *passim*), in the sense that a deeper understanding reveals that what is involved is actually ‘construction’, ‘constitution’, ‘performance’, or ‘enactment’.<sup>3</sup>

More recently, a further twist has been added via the notion of an ‘ontological turn’. Woolgar and Lezaun (2013:322) outline this as follows

If, as Viveiros de Castro (2004: 483) noted, modern philosophy is characterized by ‘the massive conversion of ontological into epistemological questions’, then the turn to ontology operates as a reversal of this trajectory: it short-circuits the tendency to rephrase questions about the reality of multiple worlds as questions about the multiple ways in which a singular world is represented, and in so doing stimulates an alertness towards forms of difference that cannot be reduced to a disparity of ‘worldviews’.

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<sup>3</sup>An influential source for the idea of enactment is Mol 2002. One effect of this anti-representationalist point of view is, effectively, to erase any distinction between science and technology, which perhaps reflects the development of what Ziman (2000) calls ‘real science’.

The ontological turn, in various forms, is also to be found elsewhere, notably in anthropology – not least in the work of Viveiros de Castro himself (1).<sup>4</sup> Jensen (2017) has outlined some key differences among versions of the ontological turn, and I cannot discuss them all here. I will focus entirely on STS, and primarily on discussions by Law (2003, 2004) and Woolgar and Lezaun (2013). However, the discussion has wider relevance.

## **Equivocation**

My argument will be that there is an unavoidable equivocation in anti-representationalism, and that this renders it untenable. A useful starting point is with what seems to have been one of the main influences on anti-representationalism in STS: ethnomethodology.<sup>5</sup>

### *The Case of Ethnomethodology*

There has long been equivocation among some ethnomethodologists about the relationship of their approach to conventional social science. On the one hand, from early on, several influential advocates have criticised conventional sociology as misconceived (Zimmerman and Pollner 1970; see Hammersley 2018, 2019a and b; Lynch 2019). The key argument here has been that, contrary to its claim to scientific status, sociological research trades on common sense assumptions, rather than treating these as part of the focus of inquiry. And ethnomethodology sometimes appears to be offered as a replacement science, designed to avoid this problem. On the other hand, ethnomethodologists have also frequently suggested that conventional social science is to be regarded simply as one set of members' practices amongst others. In these terms, Garfinkel (2002:ch2) has argued that ethnomethodology is 'incommensurable with, and asymmetrically alternate to' conventional social science. An implication of this is that social science practices are open to ethnomethodological investigation, this focussing on how they are implicated in the constitution of the reality they document. For example, in

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<sup>4</sup>Of course, long before this, a 'crisis of representation' had been announced in that discipline and in others (Clifford and Marcus 1986; Denzin and Lincoln 1994), and (at least rhetorical) moves against representationalism have been common in several parts of social science. Also, Heidegger's work has been presented as an ontological turn that reacted against neo-Kantian epistemology (Bambach 1995). While there are some parallels with this earlier 'turn', there are also significant differences.

<sup>5</sup>It is perhaps necessary to say that, while there is ethnomethodological work within the field of STS, the main authors whose work I discuss in this paper are not, and do not (as far as I know) claim to be, in that tradition. But they do draw on some of the arguments of ethnomethodologists, and for this reason these can be used as a useful comparison.

this spirit, explicitly on analogy with much STS investigation of the work of natural scientists, Greiffenhagen et al. (2011, 2015; Mair et al. 2016) examined the work of both qualitative sociologists and statisticians, documenting how they dealt with the routine troubles that arose in the course of their analytic work, with a view to challenging the idea that this is simply a matter of their following methodological rules, or that the differences in the forms of reasoning they employed simply reflected the qualitative/quantitative divide. However, these authors specifically deny that their account implies any criticism of the social scientists they studied.<sup>6</sup>

One way of trying to account for this equivocality would be to say that what is wrong with conventional social science, from an ethnomethodological point of view, is only that it claims scientific status; as a lay practice it is as legitimate as any other. But this seems to treat ethnomethodology as playing an overseer role – as able to determine the true status of other practices.<sup>7</sup> In effect, this involves claiming scientific status (of a rather old-fashioned kind) for it; and there have been influential ethnomethodologists, notably Lynch (1993, 2016), who have rejected this claim as incompatible with ethnomethodology's radical character, and in doing so have ruled out any methodological distinction between scientific and folk practices. The implication seems to be that ethnomethodology is simply one amongst a range of incommensurable practices, presumably with conventional social science also included in these. While ethnomethodology may be capable of documenting these practices, it would also presumably follow that conventional sociology can provide its own account of ethnomethodology; and that each must be treated as legitimate in its own terms (see Hammersley 2019c).<sup>8</sup>

It may be instructive at this point to note Pollner's (1978, 1987) very different way of dealing with this issue, since it bears on the appeal to reflexivity that sometimes accompanies anti-representationalism in STS (Latour and Woolgar 1979; Woolgar 1988). He argued for the superiority of ethnomethodology on the grounds that it reflexively *recognises* that the picture it produces – of reality as ongoingly constructed in and through processes of

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<sup>6</sup>For a more detailed discussion of this body of work, see Hammersley 2020.

<sup>7</sup>While it can be argued that ethnomethodologists have assessed conventional sociology on its own terms, in that much 1950s sociology in the US *did* claim to replace common sense assumptions with scientific ones, this has been less obviously true of much work in the discipline from the 1960s onwards, especially that employing qualitative methods. Furthermore, criticism of conventional sociology was the other side of ethnomethodology's own claim to analytic legitimacy, this often drawing on a parallel with the rigour of phenomenology (see Hammersley 2018:ch3).

<sup>8</sup>There is the further problem that if conventional sociology is to be accepted as, like other lay practices, legitimate in its own self-constituted terms, it is hard to see how its claim to scientific status can be extracted from that self-constitution.

social interaction – is simply one construction amongst others; whereas conventional social science, and what Husserl called the ‘natural attitude’ on which it trades, assume that the picture *they* project corresponds to a reality independent of them. Here, Pollner makes a virtue of a particular form of reflexivity, using a sense of that term which is very different from the one used by other ethnomethodologists.<sup>9</sup>

### *The Case of Anti-Representationalism in Science and Technology Studies*

Applying STS anti-representationalism to social science practice results in similar equivocality, I suggest. On the one hand, there has been investigation of ‘the social life of methods’ much of which has been presented as simply descriptive. On the other hand, the claims of conventional approaches to social science have also been challenged, on the grounds that these assume that the phenomena they refer to exist independently of them (Law et al. 2011). The blurb to Law’s book *After Method* declares that it is about ‘what went wrong with “research methods”’. And it continues

John Law argues that methods don’t just describe social realities but also help to create them. The implications of this argument are highly significant. If this is the case, methods are always political, and this raises the question of what kinds of social realities we want to create.

Law argues that: ‘[...] the problem is not so much lack of variety in the practice of method, as the hegemonic and dominatory pretensions of certain versions or *accounts* of method’ (p4), and subsequently he adds that his ‘aim is [...] to broaden method, to subvert it, but also to remake it’ (p9). Law’s main argument is that reality does not have the character that conventional social science assumes, since much of it is ‘messy’ (in the sense of inchoate and heterogeneous, but also ‘ephemeral’ and ‘fluid’), and that attempts to treat it as if it could be reduced to clean, fixed lines distort it. Instead, what are required are ‘ways of living with and *knowing* confusion’ (Law 2003:4).<sup>10</sup> Law presents this as a rejection of ‘common-sense realism’. This phrase is revealing because it indicates that, as with Pollner, it is not just the representations produced by social science whose status is being ‘re-specified’ but

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<sup>9</sup>For a dissenting view on this concept from within ethnomethodology, see Lynch 2000. On the notion of the natural attitude, see <https://www.oxfordreference.com/view/10.1093/oi/authority.20110803100225205>.

<sup>10</sup>In line with his commitment against ‘clarity’ and ‘definiteness’, Law’s position does not seem to be entirely consistent, since in other places he suggests that STS defines reality in and through its operations.

those of lay people as well. In effect, what is being denied is a key element of ‘the natural attitude’: the taken-for-granted assumptions we all routinely make about the world in the ordinary course of living, notably that most of the objects we come into contact with exist, and have particular characteristics, independently of our awareness of them. To reject this clearly *is* a radical stance, of an ontological kind.

Other advocates of anti-representationalism in STS have adopted a slightly different approach. For example, Woolgar and Lezaun (2013:322) write that

[...] the purpose of [STS] investigations was not to provide more satisfactory answers to old epistemological questions, but rather to displace the framework that accorded them their central, obtrusive quality (Lynch, 1993). Having developed its characteristic analytical sensibilities in a series of moves of deflation and deflection, it would be odd if STS were now to embark on a project to champion one or another version of ontology.

Woolgar and Lezaun are surely right that there is a problem here: Law’s argument raises the question of how he knows what reality is *really* like (messy rather than fixed and clean-cut); after all, this cannot be by relying on common sense experience (he ascribes it to ‘intuition’: Law 2003:3). Equally important, he is making an ontological and therefore a representational claim: there is an assumed correspondence between his statement that reality is ‘messy’ and an independently existing messiness.<sup>11</sup> He apparently believes that social scientific analysis must reproduce the mess if it is to be genuine. Furthermore, as with some kinds of ethnomethodology, his position appears to involve assigning superior epistemic status to his version of STS compared to conventional social science. Yet the implication of the ontological turn is surely that these constitute incommensurable realities. From this point of view, when STS or ethnomethodology studies the work of conventional social scientists, the attitude should be to document it as producing a distinct reality rather than to challenge this reality on the basis of an alternative ontology. At most, any criticism must be restricted, as in the case of Pollner, to its claim to document the one and only reality.

However, Woolgar and Lezaun themselves do not escape this sort of difficulty, as to whether they are challenging conventional social science or simply treating it as another form of practice that is open to investigation. In places they make similar claims about the nature of reality to Law: at one point

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<sup>11</sup>It is possible that he assumes that the true nature of reality is immediately accessible, but this is implausible – in fact, he seems to be assuming that only STS provides true access to reality. Nor is his argument entirely new: it is very similar to a common, and far from unproblematic, theme in qualitative research about the complexity of reality (see Hammersley 2008:ch2).

they refer to ‘the inconstancy and fluidity of different varieties of “being”’ (p325). But, even putting this on one side, there are, in any case, problems with the notion of incommensurable realities: what these authors refer to as ‘forms of difference that cannot be reduced to a disparity of “worldviews”’ (p322). They write that

Exploring how objects are ‘enacted in practices’ implies, first, a refusal to draw on ‘context’ as an explanatory or descriptive tool. Objects do not acquire a particular meaning in, or because of, a *given* context; they cannot be accounted for by reference to the external circumstances of their existence. Rather, objects are brought into being, they are *realized* in the course of a certain practical activity, and when that happens, they crystallize, provisionally, a particular reality, they invoke the temporary action of a set of circumstances. (pp323-4)<sup>12</sup>

At the same time, these authors distinguish their position from that of Actor Network Theory and ‘new materialisms’. They write that there is

an interesting tension between the general applicability of the notion of ontological enactment and the tendency to assign agential priority or causal primacy to the ‘materiality’ of objects [...]. Despite many claims linking an interest in ontological matters with the emergence of ‘new materialisms’ [...], there is no obvious reason why the ‘ontic turn’ should imply or be coupled with a presumption of the agential power of matter. Quite the opposite, the physical identity, durability, obduracy and recalcitrance of material objects – in short, all the traits that would qualify a certain entity as ‘material’ – should in principle be treated as practical achievements, as qualities that are also ‘enacted in practices’ [...]. In other words, ‘materiality’, just as ‘context’ and its cognate terms, needs to be understood as the contingent upshot of practices, rather than a bedrock reality to be illuminated by an ontological investigation. (p326)

This argument involves a direct parallel with ethnomethodology. Nevertheless, there is considerable opaqueness about the nature of ‘practices’, and about their location (for instance, are they within the realities they generate or outside of them?). And it appears to be assumed that these practices have a different ontological status from what they generate (more well-defined and stable, less inconstant and fluid, therefore more easily specifiable?).

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<sup>12</sup>They comment that ‘This position echoes the distinction made by early ethnomethodology between “context of action” and “context in action” – that is, between context as an explanatory resource available exclusively to the analyst, and context as an emergent property of interaction available to its participants [...]’ (p324). In rejecting the former in favour of the latter, their argument here seems very close to that of Zimmerman and Pollner 1970.

Furthermore, in declaring that there are multiple realities these authors are still projecting an ontology, one that is incompatible with the assumptions underpinning conventional social science. There is also the problem that their argument implies the existence of some container (Reality with a capital 'R'?) within which multiple realities or ontologies exist.<sup>13</sup> Yet, from another point of view these multiple realities presumably exist only within a reality that Woolgar and Lezaun themselves project or co-construct through *their* practices, not independently of it. In which case, they leave intact, and beyond legitimate criticism, conventional social science's assumption of a single reality.<sup>14</sup> There is a labyrinth of paradoxes here arising from applying anti-representationalism to itself. Yet we must do this, unless we are to ditch a commitment to consistency as well, which would make any sort of scientific discourse pointless.

### *No Escape From Realism or Normativity*

My line of criticism is not new, of course – it is a version of one that has long been directed against relativism, to the effect that it involves a performative contradiction.<sup>15</sup> But, apparently, deployment of this criticism continues to be necessary to underline the fact that there is no escape from realist commitments. Woolgar had it exactly right when he referred to 'ontological gerrymandering' (Woolgar and Pawluch 1985): this is precisely what he and Lezaun are engaging in (see Pinch 1993). He seems to regard this as unavoidable, with the 'solution' being (more or less in line with Pollner) to produce accounts that are self-subverting (see, for an illuminating example, Ashmore 1989). But, in effect, this amounts only to what Goffman (1961) referred to as role distancing: claiming one's independence from a role *while still playing it*. It is a long way from genuine subversion, it is a form of make believe.<sup>16</sup> Furthermore, ontological gerrymandering is actually only an affliction of radical constructionism. And the remedy, I will suggest, is de-radicalisation.

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<sup>13</sup>On the container metaphor, see Lakoff and Johnson 2003.

<sup>14</sup>As a result, the Pollner escape strategy, the claim to superiority on the basis of reflexive recognition (in this case of the 'fact' that there are multiple realities), does not work, because recognition of multiple realities is only a virtue from the point of view of this form of STS (or from within its reality, if you prefer), not from the point of view of conventional social science, for which (generally speaking) there is only one reality.

<sup>15</sup>Collins and Yearley (1992) famously described it as a game of epistemological chicken. A performative contradiction is a statement whose assertion contradicts its content.

<sup>16</sup>For discussion of Woolgar's reflexivity argument from a somewhat different angle, and reaching different a conclusion, see Hadden 1992.

Before going on to discuss this, however, there is one further aspect of the argument I must mention. As with Law, what Woolgar and Lezaun are rejecting is not just the ‘mono-realism’ of ‘traditional’ philosophy and methodology but also their normativity. In parallel with some ethnomethodology, these authors appear to be denying that there are better and worse ways of doing science that can be formulated independently of how it is actually done; and, especially, that such formulations could provide guidance for its future practice. So, ruled out here is the practical usefulness of ‘rational reconstructions’ (Reichenbach 1938) or ‘reconstructed logics’ (Kaplan 1964), along with the very idea that philosophy of science or methodology can evaluate and correct actual scientific practice, or even formulate a general account of the nature of scientific methods. There is no scope for methodological recipes or rules, nor presumably even for statements about the advantages and disadvantages of particular methods of data production or analysis; at least ones that are not situated accounts produced by scientists on particular occasions.<sup>17</sup> And the value even of these would not be seen as lying in their representational capabilities but rather in their functionality. In Woolgar’s (1988:17) terms, they are simply ‘*post hoc* rationalisation’.

It seems to follow that the character of any normative statements about method for these authors is the reverse of how philosophy and methodology treat them: such statements are either external fabrications with no intrinsic relationship to scientific practices or they are situated glosses. In neither case do they amount to guidance that scientists could *use*. So, scientists are portrayed as simply doing what they do pragmatically, according to the situation. What scientists say about how they do this is, at best, simply a *post hoc* account that is reflexively tied to particular moments of practical activity. It follows that neither philosophy of science nor methodology is viable as a separate discipline; or, at least, neither actually does, nor can do, what it claims to do. More generally, this is a rejection of the claims that have frequently been made for the role that ‘Reason’ can play in relation to practice. Indeed, it is the sharpest of challenges since it questions the character of science and philosophy, which are often regarded as the very exemplifications of Reason; placing them alongside other forms of practice, denying their claims not just to superiority but even to fundamental distinctiveness.<sup>18</sup>

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<sup>17</sup>While Law does recommend new methods, this is largely in the name of diversity and novelty. Of course, where he insists on criticising conventional research methodology as at odds with the nature of reality he is necessarily adopting a normative stance, wittingly or not.

<sup>18</sup>That there are problems with the concept of Reason, and with use of the term ‘rationality’ more widely, is undoubtedly true. The most fundamental of these, in my view, is a failure to recognise that ‘rational’ is an evaluative term, that the evaluative criteria that it invokes can vary, and that it is essential to spell these out on any occasion of use.

This account of science must apply to social scientists' practices as well: they, too, can only do what they do; there is no guiding function for methodology. This appears to rule out both the formulation of principles as a means of learning from one situation about others, and any consistency of action (even in relation to ethical considerations). Practices are simply a matter of adhocery or expediency. However, it is worth remembering that this is only true within the reality constituted by anti-representationalist practices. So, as with Wittgenstein's philosophy, this approach actually leaves everything as it is as far as conventional social science is concerned, despite Woolgar's (1988) claim to have engaged in radical critique.

Again, there is a parallel with ethnomethodological criticism of conventional sociology (see Hammersley 2019a). What is operating here, in part, is a sharp contrast between, on the one hand, a conception of following a rule which treats it as algorithmic in nature, this being taken to be an essential component of conventional social science, and, on the other, the treatment of judgement as *entirely* determining what any rule means in a particular situation, the position apparently adopted by much ethnomethodology. However, this dichotomy is not warranted: in most rule-following the rules constrain behaviour, even if there are some cases where they have to be 'bent' or even suspended. Equally, judgements about what to do in any situation necessarily rely on the resources available to make them.<sup>19</sup> As ethnomethodologists originally emphasised, using rules always depends upon background knowledge, and we can add that the amount and nature of this knowledge partly determines whether the rules can be followed relatively straightforwardly or whether following them is seriously problematic (Hammersley 2018:100-1). Moreover, rules and principles have an important function in summarising what has been learned in the past, even though they do not carry entirely determinate implications for what should be done in the future. There is no good reason to reject the idea of algorithmic rules in favour of adhocery, as if there were no middle ground. In effect, the critics are here taking over a positivist conception of the requirements of conventional social science as an evaluative standard, one that they know is unachievable. This is another aspect of a false radicalism.

## A Remedy: De-Radicalisation

Anti-representationalism operates as a radical epistemological/ontological argument; and this is true even when it seeks to deny that it is

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<sup>19</sup>That the non-algorithmic character of social rules, whether geared to norms or interests, causes problems for sociological explanation is true enough. But there is no ethnomethodological escape from this problem if empirical findings are to be produced. Even if the task is restricted to description, this still requires putting actions and other phenomena into categories, however fuzzily defined.

philosophical in character. What I want to suggest is that it would be wiser for STS to avoid the philosophical battlefield and simply to suspend, for methodological purposes, the issue of whether scientific accounts refer to, or represent, phenomena that exist independently of them. Then the focus could be solely on documenting and explaining scientific practices and their consequences. The original principle of symmetry (Bloor 1976) tells us that no evaluation of the truth of scientific accounts, or of the rationality of scientific practices, is required in order to carry out this task. In these terms, STS can be treated as a separate discipline from philosophy, social research methodology, and other kinds of social science – one that focuses on the character and role of scientific practices. This avoids the philosophical conundrums I have outlined. Also, potentially at least, it offers the prospect of interdisciplinary peace. STS would not be automatically at odds with these other disciplines, it would no longer challenge them; though, of course, it will still be in competition for resources. Some researchers under the STS banner already do this.

It is perhaps necessary to point out that this de-radicalised version of science studies is by no means a new idea, Reichenbach (1938:1) proposed something similar a long time ago

Every theory of knowledge must start from knowledge as a given sociological fact. The system of knowledge as it has been built up by generations of thinkers, the methods of acquiring knowledge used in former times or used in our day, the aims of knowledge as they are expressed by the procedure of scientific inquiry, the language in which knowledge is expressed – all are given to us in the same way as any other sociological fact, such as social customs or religious habits or political institutions [...]. Knowledge, therefore, is a very concrete thing; and the examination into its properties means studying the features of a sociological phenomenon [...]. It follows, then, that epistemology in this respect forms a part of sociology. (quoted in Richardson 2000:S154)

I recognise that, for some, Reichenbach's advocacy of this strategy will be no recommendation. Even aside from this, there are those in STS (and, for that matter, in ethnomethodology) who will not find de-radicalisation an appealing prospect. The word 'radical' has acquired a routinely positive connotation in social science and the humanities in recent times. But there is little justification for this. As an illustration of the point, take the extreme example of Hans Freyer, a German sociologist who supported the Nazis (Muller 1987). His views were undoubtedly radical, not least in senses of that term which prevail today: he was anti-positivist and anti-capitalist, for example. However, I doubt that many sociologists today would view his radicalism positively. What this indicates is that the value of any particular kind of radicalism has to be justified. And, for the reasons I have outlined, there is no coherent justification

for the sort of epistemological and ontological radicalism discussed in this paper.

At the same time, it is necessary to recognise a potential drawback to de-radicalisation as a solution to this problem. We must ask: how much of the news value possessed by the kind of STS I have been discussing arises from its anti-representational stance, and from the resulting challenge it poses to conventional philosophy of science, methodology, and social science? I suspect that the answer is ‘a great deal’. Radicalism in the academic world fulfils the same function as sensationalism does in the news media: it gains attention and notoriety. To illustrate this potential problem, we can ask: what is the news value provided by [Greiffenhagen et al.’s \(2011, 2015; Mair et al. 2016\)](#) research on social science practices, mentioned earlier? This is a tricky question to answer, and I can only sketch what is involved here (see [Hammersley 2020](#)). To put it in a nutshell, these authors largely (though not entirely) avoid radical claims, and (perhaps as a result) I suspect that most conventional social scientists would find relatively little of interest in their work. For example, while they point out that quantitative research is based on qualitative knowing, this is a point that was made many years ago by [Campbell \(1978\)](#). One reaction to this would be that such a response results from absence of the capacity to appreciate ethnomethodological work, a failure that in itself reflects what is wrong with mainstream social science. After the necessary scientific revolution (in something like a Kuhnian sense), it could be suggested, appreciation for this kind of work would become more widespread.<sup>20</sup>

While I take the point that assessments of any type of sociological work are dependent upon background assumptions, I do not find that answer entirely convincing. One reason for this is that the conventional news values of social science are those that arise from its social function – they are shared with lay audiences. And while more esoteric news values can be adopted, as they are by linguistics for example, these must ultimately have some link with lay interests. It is not clear (to me) that there is any such close connection in the case of [Greiffenhagen et al.’s](#) work. Indeed, its main messages seem to recapitulate existing ethnomethodological themes, rather than producing new knowledge.

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<sup>20</sup>Interestingly, [Anderson and Sharrock \(2018:ch11\)](#) appear to adopt a similar view to the de-radicalisation proposal I have made here. They argue that ethnomethodology should be regarded as just one sociology amongst others, each legitimate in its own terms – these effectively being seen as separate disciplines. However, the authors spoil this spirit of tolerance by then insisting that these various sociologies are not all on the same level, that ethnomethodology is a ‘first sociology’, on analogy with phenomenology’s claim to be a first philosophy.

However, I believe that these authors nevertheless indicate where the solution lies. Perhaps in order to maximise the chances of getting their articles published in social science journals, they sought to link their analyses to a methodological issue. Thus, they present their work as clarifying the question of how sharp a difference there is between the practices of qualitative and quantitative researchers. While I do not believe that they add much to what is already known about this topic (perhaps because theirs was a very small-scale project), such investigations of social science practice could be very instructive. But these *would* need to be pursued within a relevance framework concerned with methodological issues, and this rules out anti-representationalism and perhaps even the adoption of an ethnomethodological perspective more generally.<sup>21</sup>

## Conclusion

In this paper, I have argued that the attack on representation by influential figures within STS is a radical move that leads to paradox: it is not possible to escape from realist commitments. Furthermore, this move results in antagonistic relations with traditional philosophy of science, methodology, and social science that are unnecessary. What is particularly disturbing about anti-representationalist arguments is that their paradoxical character is precisely what some of their proponents find appealing. In effect what is involved here is a news-making strategy, parallel to the sensationalism of tabloid newspapers. Indeed, there are perhaps proponents who would not deny the parallel. It is interesting, for example, that [Woolgar and Lezaun \(2013\)](#) use a story from *The Sun* as a source of data in their article, and they offer an analysis that takes no account of the character and function of that ‘newspaper’, examining instead how the story purportedly fixes the character of an object – a ‘bin bag’.<sup>22</sup> Provocation is the central intent here, as elsewhere, and it points up the parallel I have been drawing.

Instead of anti-representationalist sensationalism, I have suggested de-radicalisation. What this amounts to is methodologically suspending the issue of representation solely for the purpose of studying and documenting scientific practices, *rather than denying that science can represent features of a single reality*. I recognise that this may seem a much less exciting prospect. However, there are more refined forms of excitement than ‘Épater la bourgeoisie’.

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<sup>21</sup>Note that I am not suggesting that such studies ought to be aimed at *resolving* methodological issues – as Reichenbach indicated, they ought to be concerned with documenting facts – my point is rather that they should seek to describe and explain social science practices in ways that are relevant to those issues.

<sup>22</sup>See also [Woolgar and Neyland 2013:ch2](#).

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