



# **RADICAL REFLEXIVITY**

A comment on the use of recursivity in Sociology

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## INTRODUCTION<sup>1</sup>

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Towards the end of the 1980s, Steve Woolgar and Malcolm Ashmore created quite a bit of stir in the Sociology of Scientific Knowledge community<sup>2</sup> by insisting that the constructivist methods which STS had deployed upon various bodies of science could — indeed should — be deployed on STS itself. Such a deployment would be ‘radically reflexive’ because, as an STS account, it would bring out the socially constructed character of STS’s own accounts. For a short while, this proposal generated a heated debate in which the sides neither reached a rapprochement nor gained or gave ground. Eventually, as usually happens in these things, the participants seemed to tire of it all and moved on to other topics and interests.<sup>3</sup>

Being preoccupied by other matters, we took no part in the dispute over radical reflexivity in STS, remaining distant and somewhat bemused observers. We would not be picking over the bones now were it not that interpretations of the positions pressed by Woolgar and Ashmore are now being taken as inspiration for some of the dottiest ideas to have emerged in sociology for a long time. These ideas form the core of a proposed research method entitled “autoethnography”, a flavour of which can be gained from the following manifesto statement by some of its UK leaders.

*Autoethnography.....often explicitly challenges the exclusivity of supposedly value-neutral, rationally-based categorical thinking and abstracted theory in explicitly celebrating emotionality, political standpoint position and social activism. Many autoethnographers, explicitly and implicitly, do this in pursuit of a social justice agenda, aiming for the reduction of the oppression of individuals and groups within broader socio-political structures .....The aim of this pursuit is to positively impact on and change the world, in line with an aspirational utopian ideal (SHORT, TURNER & GRANT, 2013, P.5)*

What autoethnography thinks it has found in radical reflexivity is license both to challenge conventional forms of sociological reportage, especially the forms used in ‘Qualitative Sociology’, and to promote the recounting of the sociologist’s personal experience, not as a resource for sociological

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<sup>1</sup> One way of thinking about this piece is as a warm up exercise for the much more extended treatment of sociology as a mode of reasoning about modes of reasoning that, having finished it, we now see is needed.

<sup>2</sup> At the time, the community called itself the Sociology of Scientific Knowledge (SSK). This was revised to Science and Technology Studies (STS). For consistency, we have re-written history and referred to STS throughout.

<sup>3</sup> The key references for this are [WOOLGAR \(1988, 1992\)](#), [COLLINS AND YEARLEY \(1992\)](#) and [ASHMORE \(1989\)](#).

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reflection, but as sociological reflection itself. Such personal experience is expressed as autobiography, fiction, poetry, transcribed debates, stream of consciousness and other literary genres.<sup>4</sup>

As we say, we think this is all quite dotty. However, unlike [SARA DELAMONT \(2007\)](#) and [PAUL ATKINSON \(2006\)](#), we don't believe it is merely idleness or naiveté. Neither do we think, like [LEON ANDERSON \(2006\)](#), that the dottiness can be alleviated with a bit of analytic leavening. For us, autoethnography displays symptoms of a deep and prevalent tendency present throughout sociology, one which is certainly on view in the row over radical reflexivity in STS itself. This is a willingness to give credence and hence argumentational space to what are taken to be absolutely prior philosophical or quasi-philosophical questions. Once allowed to take grip, because there seems to be no logical basis for stopping them, such questions run away with the sociological arguments. In the end, the sociologist ends up feeling the need to promote views of the kinds autoethnography sets out or engage in the kinds of exercises Woolgar and Ashmore did. We think none of this is necessary. It is simply a consequence of muddle and misunderstanding about which methodological premises we have to accept and which we don't, and when. Once we are clear what can be forced on us when we do sociology and what can't, what we have to accept before we begin sociology and what we don't, we will no longer feel impelled to indulge in this kind of runaway reasoning. Our aim here is to offer some suggestions for how we might gain that clarity. We will begin by briefly summarising what radical reflexivity was supposed to be and how it has been translated into autoethnography. We will then lay out why we find both to be less than compelling. We will finish with some initial general reflections on the nature of sociological reasoning

## RADICAL REFLEXIVITY AND AUTOETHNOGRAPHY

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### STS after Quine and Kuhn

In his classic paper *Two Dogmas of Empiricism*, [W. V. O. QUINE \(1953\)](#) knocked away two of the bulwarks of positivism, the distinction between analytic and synthetic truths and the verificationist theory of meaning. Having considered a number of ways in which the concept of analyticity might be explicated, Quine concludes no clear definition can be arrived at. It is worth quoting his words at length.

*The statement 'Brutus killed Caesar' would be false if the world had been different in certain ways, but it would also be false if the word 'killed' happened rather to have the sense of 'begat'. Thus one is tempted to suppose in general that the truth of a statement is somehow analysable into*

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<sup>4</sup> See [ELLIS & BOCHNER \(1996\)](#) [CHANG \(2008\)](#) and [REED-DANNAHAY \(1997\)](#) for introductions. As has now become commonplace, neighbouring disciplines such as teacher education and HCI have started to import the approach. See [GRANGER \(2011\)](#) and [O'KANE ET AL \(2014\)](#)

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*a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements. But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all, is an unempirical dogma of empiricists, a metaphysical article of faith.*  
*(QUINE, 1953 PP 36–37)*

Reductionism is the complement to verificationism in the positivist theory of meaning. Analytic statements are confirmed no matter what, while synthetic statements might fail the tests we put to them. Notoriously, for Logical Positivism if a statement cannot be verified, it has no meaning. Reductionism proposes that, in principle, confirmatory statements can all be reduced to statements about our experience of what were and are called ‘sense data’. The implication of this is that we can, again in principle, separate some unique set of sensory events whose occurrence increases the likelihood of a proposition being true from some other possible set of sensory events whose occurrence makes that less likely. For Quine, any attempt to discriminate discrete sets of experiences will fail because, as he puts it, “our statements about the external world face the tribunal of experience not individually but as a corporate body” (p. 41). This is his famous doctrine of *holism*. With the rejection of reductionism and verificationism, the last support for the axiomatic status of the analytic/synthetic distinction goes. Analyticity cannot be defined as what is maximally verifiable. In determining the meaning for our propositions, we simply cannot separate out the factual elements which contribute to their truth from the logical ones.

On Quine’s view, our beliefs (about what is true or is the case) form a web with, as he says, only the outer fringes connecting to experience. Revisions at the edges lead to revisions elsewhere in the web, but what will be revised is under-determined by the logical relations between the beliefs. Most importantly, any statement can be held to be true providing drastic enough changes are made throughout the web of other beliefs. There can be no point by point, step by step calibration of beliefs against the world. In the end, the distinction between scientific concepts we take to be logically certain and those which because of the possibility of gradual verification, we pragmatically accept as ‘true for now’ falls away. All beliefs are pragmatic. They are given certainty by their so-far unrevised place in the totality of the web.

With holism, decisions over the truth and falsity of individual propositions are made against the total body of our beliefs. One popular way of talking about this was to say that truth (or falsity) was relative to some conceptual scheme. The categories we use to organise the content of our experience (and hence which tell us what we are experiencing) are structured within a common scheme — that is, one we share with others and one that we consistently use. Change in conceptual schemes is evolutionary rather than catastrophic. The network of connections between concepts gradually adjusts to changes at various points. So although we can say that *as a whole* the conceptual schemes of pre-

Copernican society and post-Copernican cosmology are ‘incommensurable’, we also allow for transition and hence translation as we gradually move from the one to the other. The truth of propositions about the orbits of the planets, and hence the meaning of the concept ‘planet’, is relative to the scheme in place.

Holism, together with a particular interpretation of Wittgenstein’s later philosophy, was translated into sociology as the vaguely specified idea that conceptual frameworks were self-sealing and incommensurable.<sup>5</sup> Despite objections from many philosophically inclined sociologists, this quickly became the received view, in part because it resonated well the linguistic and cultural relativism associated with the Sapir-Whorf hypothesis.<sup>6</sup> On that hypothesis, the ‘world views’ expressed in the languages of many non-modern societies are not translatable into Western European languages and their shared world view. With the widespread take up of [KUHN’S \(1962\)](#) concept of ‘paradigm’ and his use of it to describe the punctuated evolution of science, this vague holism became a central explanatory thread in STS. The ‘strong programme’ in STS proposed that, at any point, the meaning of the terms in the conceptual scheme underpinning science was determined by social context and not, as the standard epistemology of science proposed, by the structures of reality. The categories and content of scientific knowledge and beliefs were constructed within the boundaries of the conceptual scheme and, as such, the acceptance of ‘findings’ and ‘discoveries’ served to reinforce that scheme. What counts as a ‘discovery’ or a ‘finding’ is given within the dominant conceptual scheme (paradigm) and the complex of norms, values, practices, types of accepted equipment and instruments and forms of reporting legitimated by it. Science, that is the corpus of scientific knowledge, theories, explanations, and so on, is to be seen as a social construction shaped by and fitting within the wider social structure. STS’s task was to trace the determining (or causal) connections from the latter to the former and the feedback relationships between them as changes in scientific understanding permeated social institutions.

### The Reflexive Shuffle

In *Science: The Very Idea*, [STEVE WOOLGAR \(1988A\)](#) turned this position back on itself. If, according to STS, the meaning of the propositions given in the accounts science provides are constructions, why aren’t the accounts STS gives of science equally constructed? Leaning heavily on a particular reading of some work in ethnomethodology, Woolgar insists there are no in principle grounds for halting the reflexivity of meaning at what might be thought of as second order accounts (accounts of the accounts).<sup>7</sup> Instead of focussing on theories and explanations, Woolgar looks at texts. The scientific text as an account or representation of the world is much a constructed social object as the theories and formal bodies of knowledge it sets out. He points to the ways the conventional formats of scientific texts construct the

<sup>5</sup> It is no part of our task here to assess the degree of convergence between Quine and Wittgenstein, save to say this has been hotly disputed (see the contributions to [ARRINGTON & GLOCK 1996](#)). Nor are we concerned with the consistency between sociology’s received view and the views of either Quine or Wittgenstein.

<sup>6</sup> See [WILSON \(1970\)](#) for an array of the various positions.

<sup>7</sup> Quite the best catalogue of the various ways that the concept of reflexivity has been tortured by the social sciences remains [LYNCH \(2000\)](#)

'facticity' of the claims and propositions made. *Mutatis mutandis*, this applies to STS as well. The ethnographies of scientific practice STS provides are themselves social constructions and the accounts of science they give contain equally constructed 'facticities'. In science and in STS, it is the production of the text along with other institutionalised practices which *constitute* the facts. What he calls "radical reflexivity" embraces the recursivity of construction by attending to the methods of its own production, becoming, in intent and in effect, an ethnographic representation of ethnographic representation.<sup>8</sup> In that sense, radical reflexivity adopts what Woolgar terms an 'ironic' posture towards its own third order reflexive accounting of meaning. The "instrumental reflexivity" of conventional STS is content to offer second order reflective commentary and analysis. However, for Woolgar, being trapped in representation is not a problem but a challenge.

*Without wishing to try and escape representation, it is nonetheless worthwhile to pursue the possibility of developing alternative forms of literary expression. The idea is that this approach might modify existing conventions and thereby provide new ways of **interrogating** representations.*  
([WOOLGAR 1988A, P 94](#). *Emphasis in original*)

The Introduction to *Knowledge and Reflexivity* ([WOOLGAR 1998B](#)) is a demonstration of the pickle Woolgar has got himself into. Written with his colleague Malcolm Ashmore, the piece is very aware that without explicitly demonstrating the practice of radical reflexivity, a call for radical reflexivity leaves itself open the charge of failing its own tests. As a consequence, Woolgar and Ashmore produce what they call "a conventional empiricist monologue occasionally enlivened with dialogical discussion — a form which the authors emphatically repudiate" (p 10). Any attempt to say anything definitive has to be undercut by an "ironicising" commentary (including the above comment) that "deconstructs" its definitiveness. In the end, the whole thing descends into a kind of Morecambe and Wise routine (but without the jokes) or a scene from *Waiting for Godot*. Comment is piled upon comment seemingly unendingly until, at last, Woolgar intervenes to exercise senior author's prerogatives and terminate the discussion.

*Malcolm. This is what we have so far. I'm afraid things are still very obscure. And I am not at all sure about this ending. Do we need to start again?*

*Steve. This is what we have so far. I'm afraid things are just as obscure as they were, though I have tried to improve the ending. I have done this, as you will realize, by inverting your final question. Rather that re-start, I have re-ended. Do we need to end again?* ([WOOLGAR 1988B P 11](#))

The most extended case of this all-consuming circularity is [ASHMORE'S \(1989\)](#) *The Reflexive Thesis* which, as Woolgar underlines in his Foreword, is not actually a book written for publication (one genre)

<sup>8</sup> In his extended discussion of ethnography, [WOOLGAR \(1988B\)](#) aligns this approach to the 'critical ethnography' of Clifford and Marcus (1986).

but a thesis written for submission as a PhD (another genre). Ashmore has two academic objectives (we will leave out the career objectives, though presumably these are — or should be — susceptible to Ashmore’s ironicising): to apply the case study method used by STS on itself and to “interrogate” the thesis format as the means by which findings, conclusions, representations, renditions (or whatever) are to be communicated. He does this by couching his presentation in a number of different formats within which the usual tasks of setting the background, identifying the relevant concepts and authorities, evaluating contesting positions and summarising results and conclusions are carried out. We have a public lecture and discussion, lists of encyclopaedia entries, a standard academic discussion, an annotated transcript of a fictional interview and, of course, a fictionalised oral examination. Just in case we might be in danger of missing it (given the number of times Ashmore tells us this is what he is doing, it is hard to imagine how), Woolgar spells the point out in his Foreword.

*.....the form of Ashmore’s argument is absolutely crucial to its effect.....the play between thesis-as-argument and thesis-as-occasioned-product.*  
(ASHMORE 1989 P XIX)

This “play” is designed to enable us “to grasp the enormity of the consequences of scepticism relentlessly and authentically pursued” (p xviii). According to Woolgar,<sup>9</sup> in so doing Ashmore ‘problematizes’ the problem of reflexivity and sustains and explores “the paradoxes which arise when we attempt to escape the inescapable, not to attempt their resolution” (p xix). Notice the terms being used here. For Woolgar, what Ashmore is demonstrating is what a thoroughgoing scepticism with regard to ‘thesis-writing-method’ would look like. His ‘non-argument’ revels in the paradoxes, antinomies and absurdities which such a line would force on us. His purpose is not a positive one, for the sceptic has no positive arguments. It is entirely destructive. The irony of the permanent possibility of radical reflexivity (and *that* is the premise Ashmore does insist on) is that it leaves us adrift in an ocean of representations with no way other than personal preference or institutionalised authority to differentiate among them; that is, a resort to subjectivity on the one hand or normativity on the other. Both are grounded in values. Scepticism over method halts at the value laden nature of representation. Choices over representation are choices over values. And of course, in the modern world, the relativity of values is axiomatic.

In Woolgar’s hands, radical reflexivity has a purely sociological point. The aim was to open up sociological texts (STS ethnographies) to sociological (STS-type) analysis. For Ashmore, that is not sufficient. The purpose of radical reflexivity is to “debunk” the claims that the accounts (either those of science or of STS) make. Since science plays such a powerful role as an institution in our society in determining our structure of knowledge (the web of beliefs, to re-use Quine’s phrase), it is democratically important to question, appraise and reveal the basis on which these knowledge claims

<sup>9</sup> We are not going to make the mistake of pointing out that although Woolgar and Ashmore embrace scepticism over method, this does not prevent them from affirming premises, forming arguments and pressing conclusions. All that would provoke would be the (ironic) retort that consistency is the hobgoblin of rationalist minds.

are being made. Description becomes interrogation but of a distinctively different kind to that suggested by Woolgar. What was purely sociological has now become politicised. As a consequence, “epistemological radicalism” is transmuted into “politico-moral radicalism”. The point of the latter is not to provide an even handed (“symmetric”) reflexive de-construction of the debates over science, technology and innovation, but to choose sides, or rather to choose to be on the side of the “underdog” (usually identified as the poor, the marginal, the least powerful). Indeed, the claim is that in not choosing sides, the “symmetric” approach actually reinforced the power of the powerful. Symmetric epistemological reflexivity is inherently conservative. Faced with this recursive undercutting of his own position, Ashmore entered a plea for tolerance.

*I want to propose a different description of the two radicalisms and their relation. Politico-moral radicalism should be conceived, not as partisan, not as on a side, not as endorsement, but as a critique of dominance, a nay-saying refusal to accept the ruling relations. Intellectual (epistemological) radicalism, as the effort to extend scepticism as far as possible (and further), to doubt the indubitable, to unsettle the certainties of science, common-sense and self-evidence - and those of certainty-unsettling practice (through reflexivity) - is the model. For a radical practice, whether its focus is 'power' or 'knowledge' (or both; with nods to Foucault and Latour....) is not centred nor comfortable nor secure nor expert nor respected nor honoured. It is, however, tolerated in one place - the liberal University. This is its place on the margin. Let us tolerate this toleration and carry on with our work ([ASHMORE 1996, P.316](#)).*

Moral-political radicalism is one of the wellsprings of autoethnography.

### Autoethnography

The path from radical reflexivity to autoethnography should now be pretty clear. Once the traditional ethnographic form is defined as ‘objectivist’ and ‘objectifying’ and deploying an ‘abstracted’ voice, all of which are expressive of the power of the ‘scientistic’ mode of thought, it becomes obvious that new literary genres such as those articulating subjectivity and personal experience, must be used to challenge the scientism of institutional authority and its value structures. This is the starting point for autoethnography.

Another premise is to be found in the questioning of the ethnographic stance as an investigative mode in anthropology. Following the early work of [CLIFFORD AND MARCUS \(1986\)](#), anthropology had become very self-conscious of its position. When ‘they’ are the same as ‘us’, what becomes of a discipline premised in explicating cultural difference? These reflections took an institutional and a personal form. The institutional form drew on [DAVID HAYANO’S \(1979\)](#) original call for ‘autoethnography’ in anthropology as a response to the fact that the communities being studied share the same ‘modernist’ culture and aspirations, and hence are as ‘westernised’, as the anthropologist undertaking the study. The personal



form was a reaction to the bewilderment those making their way in the discipline felt on discovering that their subjects already have and use sociological accounts of their lives to guide their actions (sometimes even the same sociological account as that proffered by the investigator). The collapsing of the sociological premise of 'them' and 'us' seems to render the whole endeavour incoherent. According to [HELENE RATNER \(2012\)](#) this is just what seems to have happened to Annalise Riles when she undertook a study of NGOs in Fiji.

*In her study of a network of NGO's in Fiji, lobbying for women's rights at United Nations meetings, Riles find that her "informants" are already understanding and producing written artifacts representing their organization based on the sociological concept of the "network." As Riles had imagined that this exact concept would work as one of her primary analytic tools, the effect is analytic bewilderment. This form of "data," according to Riles, reduces "anthropological analysis...to restatement [and] to repetition" (Riles 2000:5). Or it affects an analytical collapse between theory and data: "everything that can be explained has already been explained indigenously; there seems to be no way to analyze the phenomenon beyond the explicit language it offers for itself" (RILES 2000:26). When the "network" simultaneously serves as an explanatory analytical device and as an empirical object, the classical purpose and auto-relevance of explanatory analysis is up for grabs. (RATNER, 2012 P. 73)*

Somehow or other, a new voice, a new frame of reference has to be found to overcome the "levelling of the conceptual and the empirical" (p 75).

Within sociology, radical reflexivity was taken to imply that since meaning is constructed in social context, there are no 'in principle' and 'external' grounds on which to prefer one account of social life over another. The production of the plausibility structure of an account of some phenomenon is an internally secured 'performance'. This rejection of modernist epistemology (usually given the shorthand designation of 'positivism') entailed the collapse of traditional sociological forms of representation, authority and power, rooted as they were in the mirage of a value-free social science. In their place must be put radical self-expression which challenges all forms of oppression. Since the form of oppression endemic in sociology is enshrined in the concepts of standardised methodology, structured investigation and the objective research report, these too must be cast aside and replaced by genres drawn from literature, the arts and beyond, fashioned to confront sociology and its association with the value system of the wider society. This is how Short and his colleagues put things.

*.....researchers within the burgeoning autoethnographic movement have eschewed the assumptions and practices of traditional qualitative approaches, including the privileging of researcher over the subject, an over-concern with method at the expense of story, and pre-occupations with outmoded conceptions of validity, truth, and generalizability..... Thus, grand*

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*theorising, the façade of objectivity, the decontextualising of research participant and the search for single truths are all rejected. This allows for the emergence of new forms of subjectivist writing, which focus on the local and the particular, and which synthesise autobiography with cultural critique, utilising creative written and analytical practices, including literary tropes. (SHORT, TURNER & GRANT, 2013, P.3)*

Pushed to its extreme, this line of thinking leads to the wish to divest sociology of forms of reporting which express.....

*..... an ideology of hegemonic cultural practice, the smooth operation and management of social, political and organisational structures is thus left minimally challenged or disturbed. This is played out in the politics of autoethnographic representational practices, with normative cultural assumptions arguably frequently forming the framing backdrop in autoethnographies which tend towards the tradition of conventional qualitative inquiry and voice..... In contrast, those in the autoethnographic communities who embrace more of a critical and poststructural edge to their work might regard such representational practices as anathema to trenchant and reflexive cultural interrogation. Textual practices which expose oppressive, deadening and creativity-stifling societal practices and experiences are key in challenging cultural hegemony. (IBID PP 5-6)*

On this view, reflexivity is the stance one takes towards one's own experience but also to the way one represents that experience. If the standardly available modalities of such representation are “oppressive, deadening and creativity-stifling” — as they surely must be — then other forms, other genres must be adopted. Ethnography as poetry, ethnography as satire, ethnography as stream of consciousness become not just acceptable but *de rigueur* as sociological forms of social criticism.

Autoethnography, then, shares three central assumptions with the radical reflexivity of Woolgar and Ashmore: (a) the assumption that reflexivity refers to an attitude or state of mind, by means of which one strives to reveal and scrutinize the basis on which the terms of one's description and analysis are fixed whilst in the midst of giving that description and analysis; (b) the assumption that the recursive logic of the radical reflexivity of meaning is irresistible; (c) the assumption that since claims to knowledge are constructs grounded in social given values, all knowledge — scientific, sociological, whatever — is, at root, value-laden. In our view, all three of these assumptions are misguided. We will now show why.

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## UNRAVELLING REFLEXIVITY

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### Reflexivity is not a state of mind

Woolgar introduces reflexivity as one of the “methodological horrors” of science. Since his formulation is key to the misunderstandings we have identified, we will quote it at length.

*The relationship between representation and represented object is reflexive in the particular sense is due to Garfinkel. That is, the intimate interdependence between representation and represented object is such that the sense of the former is elaborated by drawing on 'knowledge of' the latter, and knowledge of the latter is elaborated by what is known about the former. The character of the representation, as perceived by the actor, changes to accommodate the perceived nature of the underlying reality and the latter simultaneously changes to accommodate the former. The establishment of a connection between representation and object is thus a back-and-forth process. In Garfinkel's terminology, 'members' accounts are consistent features of the settings they make observable'. This means' in particular, that it is not possible to conceive of the component parts of any representation-object couple as straightforwardly independent. The consequences for certain forms of interpretive practice are profound. For example, in models of causal explanation, the horror of reflexivity suggests we recognize explanans and explanandum as intimately and inextricably intertwined. Indexicality, the documentary method of interpretation and reflexivity as a constitutive of social action. (Woolgar 1988a, p 33 references omitted).*

It is the last section of the quotation to which we draw particular attention; the claim that the implications of Garfinkel's observations are profound for practices such as causal explanation in science. This is a complete misunderstanding of Garfinkel's work and its import. The *sociological* character of Garfinkel's investigations is neither designed for nor has metaphysical or epistemological consequences. To think that it does is to completely misunderstand Garfinkel's intent and programme. For reasons he sets out at length (see [GARFINKEL 1967](#)), Garfinkel's interests are methodological not philosophical. His concern is with what it would take to implement sociology rigorously according to its own precepts and what consequences any choices made about the framing of that implementation would have for the study of social action. He is not concerned with developing or defending propositions about how the world, social life or anything else 'really is'.

To see this, we have to go back to what stimulated Garfinkel's studies in the first place and the distinctive form that they took. As has been widely documented, the origin of Garfinkel's work lies in an encounter with Parsons' theory of social action.<sup>10</sup> Like all such theories, Parsons' theory is the embodiment of a set of choices (Garfinkel calls them "pre-theoretical elections") which are implemented through the theory. Parsons adopted a version of Kantian analytic realism. Since all sociology begins with pre-theoretical choices, Garfinkel suggests there is no way for us to choose amongst them on the basis of the sociologies they provide. The sociologies are instantiations of the elections not tests of them. As a consequence, he sets them aside as topics for investigation and simply asks, given a particular set of choices, how could they be implemented? There is a constraint on this implementation though. Since the theories aim to describe social life, rigour requires the studies licensed by any implementation preserve the concepts and relations set out in the theory as observable phenomena in the flow of ordinary action within social life. The way to see if this constraint is met is to treat theories *as if* they embodied sets of instructions for the production of social life. This is the infamous "praxeological rule". In Garfinkel's early studies, this is precisely what he tried to do utilising Parsons' theory. The theory of social action hinges on two assumptions: social action is rational action — that is the result of shaping means to ends; and social action is facilitated by motivated compliance on the part of social actors in the sharing of a common culture (sets of understandings, beliefs, values and so on). Garfinkel asks: how could we make these two visible and observable instead of simply assuming them? It is here that he makes his first distinctive move. He suggests we could do so by dropping the assumptions from the theory. That is, for the purposes of making studies, we could drop the presumption that activities obviously and unproblematically do make rational sense to the actors concerned. Notice, and this is the most important thing, this is a methodological move, the exercise of a theorist's right to vary assumptions in a theory, not a metaphysical or an epistemological one. The much discussed 'breaching experiments' were no more than a demonstration of what kinds of studies might be undertaken if one dropped, first, the assumption of shared meaning on the part of the actors, and second the assumption that they shared a common form of rationality.

The result of Garfinkel's attempts to proceduralise Parsons' theory according to the praxeological rule did not lead to the expected outcomes. Despite framing the experimental conditions so that norms and expectations were violated, social action did not become impossible. For Garfinkel, this implied that, under the praxeological rule, we should treat the rationality, the sense, the understandability (that is, what he calls the "accountability") of actions not as a premise but as an outcome of those actions. In other words, the accountability of action is to be treated as *reflexive* on the actions themselves. Reflexivity thus becomes a feature of all social actions in all social settings, *not* an attitude or state of mind which social actors display on the occasions when they reflect on their own actions. In order to undertake studies of such reflexivity in social life, Garfinkel draws on the phenomenological sociology of Alfred Schutz. Once again, the status of the pre-theoretical elections which Schutz makes, is set

<sup>10</sup> See [HERITAGE \(1984\)](#) and [SHARROCK AND ANDERSON \(1986\)](#)

aside. Instead Garfinkel asks how the central concepts of intersubjectivity, finite provinces of meaning, and the primordality of the natural attitude can be used to make the production of social order visible and hence available for investigation. Instead of Parsons' motivated compliance, what has to be investigated are the ways by which the recognisability of a finite province of meaning is displayed and shared within the flow of common experience and shared knowledge without actors knowing in advance just what knowledge is to be relevant or how that experience will turn out. The ways social actors make finite provinces of meaning observable, recognisable and so shareable is what reproduces social life as the ordinary unproblematic affair it routinely is. Under this use of Schutz's rubric to construct sociological studies, what any finite province of meaning might be, what any action is, what it means, is taken to be something which social actors themselves investigate and display. To produce and reproduce social order, the accountability of action, its place in the structure of action, must be made visible.

It is this line of methodological reasoning which leads to the maxims which underpin all ethnomethodological studies:

1. Treat all activities as reflexively accountable;
2. Treat settings as self-organising and the common sense knowledge shared in that setting as an occasioned corpus of knowledge;
3. Treat social actors as inquirers into those settings and accounts.

Let us underline the key point once more. Garfinkel is saying *if* you want to use Schutz's conceptual apparatus to mount studies of social action, these are the maxims he recommends you adopt. He is making no claims about the essential, real, proper, inherent character of social actors and social life. That is, he is making no claims about how these things are independent of the ways we choose to study them. To use a term which has been much misunderstood, he is "indifferent" to these matters. He is indifferent simply because, as we said earlier, he sees no way to settle them outside the sociologies we deploy; that is, outside the sociologies which instantiate them.

With these considerations in mind, let us now turn back to the claim that reflexivity has profound implications for things like causal explanations in science. For Garfinkel, the steps taken to render the central set of postulates of a sociological theory visible and hence available for investigation within a particular social context such as the practice of science, cannot have any bearing on the epistemological basis of science's explanations of its phenomena. Garfinkel's concerns are methodological not conceptual. Woolgar can, of course, use the concept of reflexivity in pretty much any way he likes. What he cannot do is use it to ascribe an attitude or state of mind to a set of social actors (*viz.* scientists or sociologists) who are engaged in the performance of activity (*viz.* science or sociology) and claim legitimacy for that attribution from Garfinkel's investigations. Under the maxims set out above, indexicality, the documentary method of interpretation and reflexivity are constitutive characteristics of social action according to an investigative protocol. Garfinkel's studies provide no

grounds for Woolgar's claims concerning their epistemological import. Indeed, Garfinkel's own work on science makes this abundantly clear. To repeat our own words, what Garfinkel's studies of science show is that.....

*.....(s)cientific work, as an integral and central part of its nature, is a self-reporting activity.....Rather than set out to examine scientific work and see if reports on it are true or proposing that since science is self-reporting we can rely on the reports it produces and need not examine ur activities for ourselves, (we can) examine those activities and reports....as providing the organisation-and-product of a self-reporting system which leads us to look in a fresh way at how activities (such as those making up the daily life of science) hang together. (SHARROCK & ANDERSON, 1986, P77)*

The point is not to provoke trouble for scientists (after all they have troubles enough of their own) through the studies being carried out, nor to attribute attitudes, assumptions, theories and positions to them on the basis of what they say or do. Rather, it is to provide a way to frame and undertake sociological investigations of science or any other domain of social life as practical activity, studies which it is hoped, will be distinctive from those which have been and are dominant in the discipline. The findings of these studies bear on the sociology of science not on its philosophy. This is a distinction to which we return in our concluding section.

### Growing out of reflexivity

Radical reflexivity insists that the meaning of any action is permanently open to question and challenge. Each time an account is offered of how meaning has been secured, the question can be re-applied. Radical reflexivity is the recursive questioning of meaning without a stopping rule. Scepticism has precisely the same form. It applies the question 'How can you be certain?' to every set of propositions we offer to ground our knowledge. Although no serious philosopher has actually claimed to be a sceptic, the fact that, should such a person appear, there is no straightforward or easy way to answer them on their own terms has exasperated generations of philosophers. Equally, the fact that their colleagues could not refute Woolgar and Ashmore or that Delamont and Atkinson cannot rebut autoethnography has, for them, been just as exasperating. This exasperation derives from a feeling that scepticism, radical reflexivity and autoethnography ought to be answerable and ought to be answered. If they aren't, some chink, some weakness, remains in the fabric of our reasoning. It was Wittgenstein's insight that it was not the sceptic (or, in our case, the radically reflexive ethnographer) who is the cause of our exasperation but our feeling that we have to respond to them: that they can and must be countered.<sup>11</sup> If we can gain the self-confidence, the sociological maturity to see not just that they cannot be so countered but that they do not need to be countered, then the exasperation will dissipate. We will feel no further need to take them seriously. [STANLEY CAVELL \(1979\)](#) once called the pursuit in philosophy of

<sup>11</sup> See [WITTGENSTEIN \(1969\)](#)

this kind of maturity “education for grown-ups”. One of the ways of becoming fully adult in sociology is by growing out of the temptation to struggle with the chimera of recursivity.

How might we work our way to this position? Citing some set of sociological facts or findings or pointing to the superiority of some sociological description or theory is patently not going to help. They are the very grist of the reflexive mill. We have to go further back, to the nature of the assumption itself. The reflexive question is always open, always on the table. Since any account can be subjected to the reflexive question at any point, then in principle all accounts can be so subjected at all points. But does that conclusion make sense?

Think about what we are being asked to do. Allow, for the moment, that we can talk of a socially organised frame of reference as the structure within which we construct a meaningful description of a set of activities, we are being asked both to hold the frame of reference in place and at the same time to subject it to investigation. But we cannot investigate the frame of reference without reframing it. Holding and not holding the frame of reference constant is the recursive conjuring trick. The point, of course, is that we cannot do it (which is why it is a trick). To hold some sets of concepts in place in order to question the basis of their meaning, other concepts have to be held constant in terms of which to review, assess, or analyse them. Paraphrasing Wittgenstein, we could call these latter, “hinge concepts”.<sup>12</sup> The analysis of the concepts we are examining turns on the concepts we are not examining. Of course, what counts as the relevant hinge concepts will vary from context to context across different enquiries. They are quintessentially “occasioned”. While we can imagine investigating any particular set of hinge concepts, what we cannot imagine is doing so without putting other hinge concepts in place in order to do so. The challenge set by the radical reflexivity of meaning turns out to be a sociological conceit. No-one has done it; no-one could do it. But that doesn’t stop us being tempted by the conceit and the need to defeat it. Unless, that is, we abjure the temptation altogether and thus release ourselves from its grip. The fact that autoethnography and its opponents are still gripped by the temptation of the recursive radical reflexivity of meaning, either in arguing for it or against it, is a sign of just how much growing up some parts of the discipline still have to do.

### Facts and Values

The adoption of vague holism in STS made it easy — not to say obligatory — to make a connection between statements of (scientific) fact and statements of value. Instead of pointing to a merely contingent historical co-relation between scientifically accepted facts about the nature of the material world and conventionally accepted values in the social world, STS sought to ‘demonstrate’ their linkage within a web of belief. The exact specification of this linkage varied across the STS community, but in every case the purpose was to ‘reveal’ the entanglement (to use a term of Hilary Putnam’s ([PUTNAM 2012](#))) of facts and values. Given radical reflexivity’s concern with the constructed nature of any and all scientific accounts, this revelation of entanglement took on a ‘debunking’ character. Claims of fact or

<sup>12</sup> See [WITTGENSTEIN \(1969\)](#) §341



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necessity set out in scientific accounts could be shown to be ‘constructions’ of values, and hence reduced to social and/or economic interests. Radical reflexivity’s erasure of the praxeological rule allowed epistemological and ontological claims to be substituted for methodological stipulations. Scientists, technical professionals, the leaders of science based companies, industries and interest groups propose the things they propose (new theories, new drugs programmes, and new technologies) because they serve personal or institutional interests.

Once again, having got a place to grip, the recursive argument runs away with itself. And once again it is because we have not had confidence in the maturity of our sociology. Go back to the original assertion. Facts are grounded in values. At a very important level, this is undoubtedly true. When, for example, scientists make judgements about the acceptability of one theory over another, they do so by expressing a preference or theories that are more powerful, simpler, more consistent and cohesive, have broader scope, are more elegant etc. etc. than others. These are value judgments. Simplicity, consistency, cohesion, scope, elegance are epistemic values. But they are not the *only* values. There are, of course, aesthetic, ethical, political and economic values and many more as well. The recursive argument gets its grip when we allow the reduction of epistemic values to ethical values and then to political and ultimately economic values (that is, self-interest). And, once we have allowed the recursive argument to start up, it is impossible, within its own terms, to prevent it being continuously applied.

The only way to stop the recursion is to prevent it from starting and to do that we have to deny it a place to grip. This means we have to respond not by pointing out that the recursive argument is based in moral-political values. That simply grants the recursive premise. Rather we have to ask how we could make judgments about preferences with regard to philosophical, sociological or scientific arguments without appealing to some values, unless it is being said there is something else, such as a set of facts which underpins a set of values, something which radical reflexivity would seem to deny. How are we to judge the recursive case and so be convinced by it, if we don’t appeal to epistemic values? It is precisely because of a judgment of its power, consistency, coherence, simplicity and elegance that its adherents press the recursive argument. To re-paraphrase Wittgenstein, these are hinge values for the assessment of arguments in whatever domain they are made. But, these judgments are made by looking to and relying upon the practices through which they are constructed. On the methodological premises which the radical reflexivists adopt, there is nothing outside the practices on which to rely. It is this which makes the judgments reflexive. Under the praxeological rule, saying there is only the practices to be relied upon is not camouflage for immanent values. It is a way of beginning sociology. Once we have confidence in this, the recursion falls away. We can look to debates in science, debates in the sociology of science, debates in the application of the sociology of science, debates in the economic, social, moral and political spheres and ask about the practices by which the epistemic values they are found to display are constituted without proposing that they must be reducible to some master set. We can accept with confidence the assertion that within our sociology, findings, generalisations and so on are



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grounded in value. For, given our sociology, how could they be otherwise? Confidence in the security of an appeal to epistemic values is, surely, a sign of a grown up sociology.

## CONCLUSION

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One of the things we have been trying to avoid here is getting sucked in to an all-encompassing “What’s wrong with sociology?” discussion. In part this is because we have already said the kinds of things we might want to say about reflexivity and autoethnography as species of sociology.<sup>13</sup> In part, too, it is because neither is substantial enough nor sociologically fertile enough to sustain such an extended treatment. However, we know we would have to look at both the treatment of science and sociology’s treatment of itself should we ever examine how sociology, as a mode of reasoning, approaches other modes of reasoning.

By way of conclusion to this particular discussion, we will use radical reflexivity to sketch one of the general features of sociology’s approach. The hope is that this will be provocative enough to stimulate others to reflect on these issues and a strong enough prod to ourselves to encourage us to complete a job that, in this discussion, we have only half started. The feature we have in mind is the notion of ‘problematization’ — a term we would much rather not have to use, but since it is widely deployed in contemporary discussions of sociology’s stance, we are resigned to doing so. Problematization involves applying a particular form of the method of doubt to the claims, justifications, rationales, explanations or accounts that members of some specific cultural group (scientific disciplines, technology innovators, other cultures, business managers, professional groups — whoever) offer for their actions. The account *they* give of their actions is subjected to the method of doubt within *our* (sociological) treatment. Such problematization cannot help but be framed as critique, a consequence which then throws up interesting ‘ethical’ challenges when the group so ‘problematized’ is said to be marginalised or oppressed and on whose side the sociologist obviously wants to be. Here is a classic example of how problematization works. It is from a discussion by John Law of scientific representations.

*There’s a lot of metaphysical — not to say social and material — work being done in the representations of technoscience, and it’s not all agreeable. The again, it’s also important to understand that no particular allegorical reading tells us the truth. Such readings are better understood as alternative practices for making literal. They have their confusions too, it could be no other way, but even so they interfere with technoscience’s own*

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<sup>13</sup> See Anderson and Sharrock (2012 & 2013)

*understanding of itself. In so doing they render what previously lay at the periphery of technoscientific vision, that which was indistinct or confused, explicit.*

*Perhaps, and this is the real hope, they also render patches of it sufficiently literal to make these discursively and politically contestable. (LAW, 2014, P 340)*

Since the “technoscience’s own understanding of itself” is its philosophy of (techno)science, then what is being made “discursively and politically contestable” is that philosophy. The *modus operandi* of this contesting turns out to be the substitution of sociology’s sociological account of technoscience for technoscience’s philosophical one. Sociology problematises the philosophy being articulated by means of the substitution. What is then offered is a sociological *rationalisation* of the rational structure technoscience’s metaphysics, epistemology, ontology and values.

Problematisation by substitution is the most obvious aspect of this feature of sociology’s reasoning. In effect, it is a method of deconstruction through premise denial, a term which comes from David Chalmers’ recent reflections on why there is so little progress in philosophy. Among the reasons that philosophical debates tend to be non-convergent, he suggests, is that they are often constructed around the device of premise deniability.

*When we address arguments against our views, we sometimes work backwards from our rejection of the conclusion to see which premises we have to deny, and we deny them. In the best cases, we learn something from this, and we take on commitments that we might have antecedently found surprising. But these commitments are rarely untenable to maintain. (CHALMERS, 2015 P.18)*

Problematisation by substitution results in a denial of the philosophical premises on which technoscience predicates its account and the deployment of alternative premises which predicate sociology’s account. Where technoscience points to deductive logic using the axiomatic methods of mathematics, intersubjective concordance as the grounds of objectivity and the obdurate causal reality of the physical world, sociology points to social convention, collective norms and institutionalised values. In philosophy, Chalmers says, the result of premise deniability is sophisticated disagreement. In sociology, alas, the sophistication is mostly missing though the disagreement equally non-convergent.

So, although radical reflexivity has proved and, no doubt, autoethnography will prove of little lasting significance for sociology, they do exhibit something which is of general import. Actually, they share more than one thing. In their desire to kick over the traces in sociology, they adopt precisely the same structure of reasoning that most of the rest of the discipline does. In their desperation to be different, they look, feel and sound more and more like the very discipline they are rejecting. However, there is yet one more convolution in this story. The purpose of the substitution is to replace premises

which need to be contested. What gets put in place are sociology's premises but these turn out to be based on precisely the same foundational metaphysics, epistemology and, as we have seen, epistemic values that underpinned the premises being rejected. All that changes is the rhetoric. Now, if they wanted to be really different, radical reflexivity and autoethnography would have to change those premises. But *that*, as they say, would be horse of a very different colour.

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