
REPRESENTATIONS AND REALITY

INTRODUCTION

Science as an institution and science as a body of knowledge occupy important places in our society. The accumulated body of scientific findings and laws is taken to be as reliable an account of the world around us as it is possible for us to have. The method which science uses, namely the collection of and reasoned generalisation from evidence, has become the pre-eminent way of gaining an understanding of any phenomenon. This being the case, science has come to play a key role in framing many of the leading decisions affecting all our lives and is seen as the driving force behind technological progress. It is hardly surprising, then, that arguments which appear to question scientific method and the knowledge which it gives are met with bewilderment at best and, more often than not, hostility. In their turn, the rejoinders to such relativism or scepticism (depending on who is making the argument) are likely themselves to be responded to with vigour and assertiveness. To judge from the furore over Alan Sokal's hoax and its aftermath, for example, what we end up with is a cycle of escalating argumentativeness.¹²

The problem with this kind of debate is that because so much energy is expended in defending and attacking each other's positions and in aiming to push a position to the point of confrontation when it is not really necessary, the participants too often adopt forms of argument simply for their force rather than their cogency. The net result is that when all the sound and fury is over and the battleground clears, certain ways of characterising what is at stake seem to have become established even though they may be more than a little misconceived. Usually such characterisations pre-exist the hostilities, but in all the excitement, careful assessment of just what is at stake and to what extent the points made really do fit the arguments in train gets set aside in favour of their deployment as munitions. In our view, the debate over the 'radical critique' which Steve Woolgar (jointly and severally with his colleague Malcolm Ashmore) offered first of science and second of the sociology of science is an example of this. The various positions set out both on behalf of and rebutting this critique have now become an established part of the landscape of debate over the sociology of scientific knowledge. In this essay, we want to look at this controversy. In particular, we want to look at some of the pre-suppositions which secure the Woolgar/Ashmore position. By looking at what is probably its most accessible version of their argument, namely Woolgar's (1988) manifesto *Science, The Very Idea*, we will try to determine just what, if anything, might actually be at stake here. We choose this text for two reasons. First, the case is made clearly and concisely. These are virtues much to be valued. Second, because of its virtues, this account has become a favoured source for students and others coming new to the field. In several of the essays in this volume, we express concern that novices seem attracted to arguments which have an immediate or a surface

¹² See Sokal (1996), Labinger and Collins (2001) and Zammito (2004)

plausibility and an overtly critical edge. That they are attractive to new comers often seems to be the only demonstration needed for their arguments to be given credence.

HOW TO REPRESENT SCIENCE

In *Science, The Very Idea*, Woolgar sets out to counter what he believes are two misconceived ways of thinking about science. The first is what we can call the conventional view, allegedly held by science itself and by the world at large. On this view, science embodies a set of objective methods which allow us to accumulate truth about the physical, natural, social and psychological worlds we inhabit. The second view is the corrective to the conventional one and is offered by David Bloor and the 'The Strong Programme' in the Sociology of Science. This sees science's accumulated knowledge not as the disinterested application of rigour and reasoning but as the working out of social forces of various kinds. For the Strong Programme, these forces act as the causes of scientific knowledge. On the Strong Programme's view, science is not to be explained by reference to its own self declared method but by the forces which the Strong Programme identifies. As will become apparent, we are not convinced by Woolgar's objections to these views. However, just to be clear, this does not mean that we accept the broad positions which Woolgar means to attack either. We are not sympathetic to the campaign for realist and empiricist theories of scientific knowledge, nor (perish the thought!) are we trying to bolster David Bloor's ambitions for a causal account of scientific and other types of knowledge. Our point is simply that because it misfires at various points, Woolgar's case is not as convincing as it might have been.

The thrust Woolgar's case is this.

1. Empirical and historical studies of scientific practice have shown that philosophical prescriptions for the logic of scientific discovery do not match the way science is actually carried out. Propositions, theories, and laws are taken to be veridical or factual descriptions of the way the world is but such factual descriptions are logically underdetermined. The only connection between the factual descriptions and the law-like generalisations is a wider social agreement about the relationship of representation to reality.
2. In like vein, research in the Social Studies of Science allows us to question two fundamental pillars of scientific method: formal logic as the guarantor of the move from premises to conclusions (statements about facts to truthful generalisations); and the distinction between representations and the reality which those generalisations are supposed to articulate.
3. For Woolgar, the consequence of such questioning will be twofold: first we will need to re-think the character of science and the institutional place it has in our society. Second, we will need to re-think the ways we study science. Since, the former will have to recognise the entanglement of the observer with the observed, so too will the latter. The sociology of science has no choice but to engage in radical reflexivity.

Science, The Very Idea undertakes the questioning identified above, but Woolgar offers no guides to how the re-constitution of science and its study might actually be carried out. Indeed, his analysis makes such programmatic impossible, for his is a *de-construction* par excellence. In fact, it is left to Bruno Latour and Actor Network Theory to try to find a way of bootstrapping reflexive social science. Alas, as we will see in the next two essays, this too has been a more-than-usually muddled exercise.

The problem of representation in science is Woolgar's central motif. He has two questions. First, how are the representations (theories, models, laws, etc) which science uses to describe the world, actually connected to and fixed by the world? Second and following from the first, how can we know if the means by which this fixing takes place is itself robust? Woolgar begins where the empirical studies in the sociology of knowledge end, namely with the conclusion that the descriptions usually offered fail to acknowledge the social character of science and hence the indexical, open and reflexive (in short, contextualised) character of its

representations. The meaning of any representation cannot be determined by its status as an 'objective truth' which corresponds to the facts independently of the context in which the representation is used. The indexical, open and reflexive character of meaning constitute what Woolgar calls science's "methodological horrors" and involve tensions which he does not think can possibly be resolved.

From this conclusion, Woolgar makes a further move. The representations which the sociology of knowledge wants to give of science are themselves subject to the same contextualisation. Reflexivity applies recursively. It is this recursion that Woolgar feels is his radical contribution (and which others have picked up). His argument applies as much to the sociology of science (and hence *mutatis mutandis* to Sociology in general) as to science itself. For many, the vortex of relativism looms at this point.

In *Science, The Very Idea*, then, Woolgar seeks to inaugurate a new view of science and Sociology. However, we feel Woolgar's treatment of both the questions he has identified is flawed. The consequence is that despite what he claims about it, when looked at in the cold light of day, Woolgar's critique makes little or no difference to the practice of science itself. In that respect, John McGowan's observation about post-modernism's problems in grounding an ethics applies equally well to Woolgar's account of science.

Unable to ground or construct an ethics within the terms of its critiques of foundationalism and of dominating humanistic values, postmodern politics is often reduced to the ironic, anarchistic effort to transform existing order by means of play, jouissance, or other textual strategies.... (McGowan 1991 p 28).

OBJECT AND REPRESENTATION

Woolgar begins by plunging into an age old debate about the relationship between the world and our accounts of it; that is, the linkage between word and object, sense and referent, propositions and the states of affairs they describe, representation and reality. The clearest part of his argument is the claim that in our thinking about these issues, we — that is his colleagues in the sociology of scientific knowledge, philosophers, and pretty much all of us who are conventionally respectful of science — have got things the wrong way around. To make this point, Woolgar focuses on the relationship between 'representations' and 'objects' which, in respect of science, he conceives as the relation between scientists' discoveries, their claims to have found something, on the one hand, and the things they claim to have found on the other. The former are 'representations' and the latter 'objects'. Woolgar intends that his point be taken as applying comprehensively to all our thinking about the relationships between objects and representations, not just in relation to science. Indeed, although his model was developed for understanding the process of scientific discovery, it is easy to see its extension to representation in general. It is a model not just of the constitution of course a scientifically discovered object but of all attempts to establish the antecedents of objects and things which thereby render them fixed (and objective) for a whole variety of purposes. For Woolgar, such attempts range from strategies of causal explanation to the practical character of perception and interpretation in general (Woolgar op cit, p.69). In setting out his argument in this way, Woolgar falls within a long sociological lineage which holds not only that our ordinary ways of thinking are misguided in one way or another, but also that these misguided ways of thinking are now so entrenched that it probably never occurs to us that we could think in any different way.

So, on Woolgar's view, how do we think about 'object' and 'representation'? It is, he says, in terms of the two being connected in an order of precedence. 'Objects' come first, with 'representations' following from objects.¹³ What Woolgar wants us to understand by this suggestion is, for example, that we think the objects

¹³ Now it is very important to note that Woolgar offers no *evidence* for his assertions concerning how 'we' think about objects and representations. Nor do we have space here to explore the range and complexity of the ways we actually do think about what are, at least

that scientists discover exist before they are discovered. 'Discovery' is therefore the creation of a representation for what had previously been unknown and hence unrepresented. The usual assumption which embodies the relation of precedence between object and representation is that objects exist independently of any representation; that is, they exist whether or not we have any representation for them (i.e. know about them). Since this conception is so deeply rooted, the suggestion that there is an alternative to it will not only be difficult to credit, but also seriously disturbing. And yet, despite this, Woolgar does want us to accept that objects don't exist until they are represented.

In proposing his view, Woolgar is simply reversing the account of the relationship which he claims we hold. We should now insist that representations come before objects. Woolgar sets out to show this is so by examining how scientific investigations produce discoveries. However, his demonstration depends on the speedy insertion of a quite major and hugely controversial assumption, namely that 'epistemology' and 'ontology' are identical; that is, something's existing is the same as its being known to exist. In summarising the implication of studies of science whose conclusions he is in sympathy with, Woolgar proposes

....a major thrust of post-modern critiques of science is to suggest the essential equivalence of ontology and epistemology; how we know is what we know. (Woolgar, op. cit., p 54 emphasis in original)

Woolgar does not argue for this point, let alone demonstrate that it can be defended, but simply stipulates it and on its basis goes on to propose the "inversion" mentioned above. In doing so, he fixes the terms by which his examples are to be understood. In fact, Woolgar really has no need for examples to make his case since, as should now be clear, on his argument 'making a discovery' (as scientists and the rest of us are inclined to call some events in science) is just a matter of coming to know that something exists, and 'making a discovery' must, therefore, really only be a matter of creating representations. Given this, we seem forced to conclude that whatever is 'discovered' cannot have existed before its existence was made the subject of a representation.

Woolgar's argument has all the hallmarks of the 'give me a long enough lever and I can move the world' kind of logic. Once we accept a massive and hugely disputable assumption — that epistemology and ontology are the same — the rest follows. However, the force of the argument is much less compelling if one notices that what is being said about the ways scientific investigations themselves are organised does not differ all that much from what we might call 'the usual story', save that the usual story is retold in the language of 'representation' and 'object'.

The usual story holds that scientific discoveries often, even commonly, come at the end of investigations, not at their beginning. Investigations, at least paradigmatically, involve first, hypothesising some conceivable or possible phenomena, followed by further investigations to determine whether the hypothesis holds. Such hypotheses are commonly generated from existing science, (it being quite unremarkable to view research as part of a cyclical process of applying and revising existing science). Woolgar's suggested reversal of our conventional view of how objects and representations relate, doesn't affect the usual understanding of how scientists go about their investigations. In fact his presentation of what he calls the process of "splitting" presupposes it.

The term "splitting" is meant to suggest that in scientific investigations 'object' and 'representation' are originally one, but that over the course of the investigation they are separated from one another — the "splitting" — with the object being dissociated from its representation in a way which makes it seem that they were always separate. It is splitting which executes the reversal in the relation between representation and

as Woolgar treats with them, these rather loose conceptions. This being said, we shall set such caveats aside in order to pursue the main line of Woolgar's argument.

object. To begin with, the object depends on the representations (what Woolgar calls "documents") which represent it. As research proceeds, the object is made (out to be) independent of those documents, thus making it seem that the nature of the documents really depends upon the nature of the object documented.

In the first stage, the scientists have documents (traces); in the case of the discovery of pulsars these comprise the charts from the telescope recorder, but might also include other publications, papers, previous results, the telescopes themselves, other apparatus, what Hoyle says and so on. At stage (2), participants use (some of) these documents to project the existence of a particular object (in this case interference or an astrophysical phenomenon or whatever). Importantly, the object is created and constituted out of documents available to the researchers. At stage (3) the splitting occurs. Although the object was initially constituted in virtue of the documents (and more generally the social networks of which they are a part) it is now perceived as a separate entity, distinct from those documents. The object now has a life of its own. Indeed, it is just one short step from possessing an infinite history: it is about to acquire the status of antecedent. In stage 4, the relationship between the documents and the object is inverted. Whereas the object was constituted on the basis of the documents in step (2), it now seems as if the object (which was there all along) had given rise to the documents (Woolgar op.cit. p.68)

What Woolgar is seizing on here is the different ways that scientists speak about their phenomena at different points in their investigations. But his description of scientists "project(ing) the existence of a particular object" is readily recognisable as what in the conventional account is called 'hypothesis formation'. Whatever 'splitting' of representation and object actually occurs, takes place with regard to the scientists' understanding their phenomenon and hence results in different ways in which 'the object' is represented. From Woolgar's abstract presentation, it seems the transition is from a time when 'the object' is speculatively proposed (the possibility of its existence being derived from and justified by available science) to the time at which the object's existence is regarded by the scientists as confirmed (or not) by investigations testing the hypothesis. It is surely not surprising that the kinds of things scientists say to each other during their investigations are apt to change as the investigations go on. It is particularly unsurprising that the qualifications which mark early mention of an object as hypothetical tend to be dropped as evidence for the object's existence accumulates.

For Woolgar, a central element in this 'inversion' is how scientists represent their own contribution to discovery. They move from presenting themselves as active investigators to portraying themselves as (only) passive perceivers. This change is captured in changes in the kinds of representations scientists generate throughout their work. However, Woolgar does not explore them as changes in modes of expression appropriate to the different kinds of work that the scientists are doing. Rather he sees them as involving the (retrospective) substitution of one kind of expression for another.

In the first phase, scientists express themselves in a way which signals their active involvement in their scientific work. In the latter phases, they present themselves as having only a passive involvement in discovery. Woolgar proposes that the latter form has retroactive force. That is, it is meant (forgive the expression) to represent the scientists' participation as though it had always been (merely) passive. Given this is the intent, measures are taken to ensure that all evidence of a more active involvement is suppressed. Here is Woolgar's stage 5:

Step (5) is crucial. In order to maintain the inverted relationship of step (4) it is important to play down or minimize all reports which draw attention to the earlier steps (1), (2) and (3). Step (5) thus comprises the backgrounding of all component parts of the process. Step (5) rewrites history so as to give the discovered object its ontological foundation (Woolgar op cit. p. 68)

Why do scientists need this suppression when the dependence of scientific investigation on what Woolgar labels steps 1-3 must be among the world's worst kept secrets? Allegedly it is to maintain 'the ideology' of objectivism and to conceal materials that would be directly subversive of it, namely evidence showing that the object originated in documents and that, therefore, the discovery was the scientists' active creation.

What is not clear in all of this is how Woolgar wants us to conceive scientists' relationship to the 'objectivist' understanding that he ascribes to them. It is the scientists themselves who do the 'the splitting' and carry out the 'inversion'. Even though their own actions demonstrate (conclusively, in Woolgar's view) the error of objectivism, scientists insist, in the end, on presenting things in objectivist terms. They disregard their own experience to maintain the ideology. Surely, he implies, that means we must accept that the objectivist ideology is rightly attributed to them?

Well, not really. On Woolgar's construal, they seem to be no more than part-time objectivists. For Woolgar, the precedence of object over representation is the hallmark of objectivism, yet scientists seem to be operating quite contentedly on the basis that their representations precede the object — in which case, objectivist assumptions are hardly indispensable to the work of scientific discovery. Can we say, then, the ideology of objectivism is truly theirs?

It is easy to conceive a line of thought in which it is, but it is not one which does Woolgar's position much good. We could accept that scientists are whole-hearted objectivists and do conduct their work on the basis of its assumptions. But wouldn't that require the assumption that objects pre-exist enquiry to run from the start of their investigations? In turn, wouldn't they therefore conceive of their representations as a means of searching for and finding such objects which, in turn, can only be found if they are there to be found? This is presumably why they don't talk about finding 'an object' in their documents, because their understanding is that any such object is 'out there' and that their inquiry must itself point in the same direction?

Woolgar agrees the idea of a reversal is controversial and something to strain at. But, as we have just seen, there is no compelling need to try to swallow it. If 'inversion' is as counter-intuitive as Woolgar portrays it, then citing a few uncontroversial features of the standard scientific investigation process provides only the flimsiest reasons to think it is needed. Looked at independently of the way Woolgar characterises them, the features he cites do not count decisively against an 'objectivist' conception and can as easily be construed, rather, as showing that the scientists' moves can be understood in 'objectivist' terms.

Notice, we're not offering a defence of 'objectivism' here. All we are trying to point out is that the features of the investigative process that 'splitting' is meant to capture are just as compatible with understanding the process as being conducted on objectivist assumptions throughout as with seeing it being a denial of them. For us, the real issues do not involve electing between that and its obverse but largely arise from a lack of effective co-ordination between the points of view of participant and analyst in Woolgar's narrative.

Woolgar's account is that science is justified by objectivism but in practice contradicts it. But then, if scientists do have an objectivist ideology (in the pejorative sense), it can only be functional if it somehow deals effectively with the fact that scientists own experience itself runs counter to the ideology. Unless we want to suggest that all scientists are riven by cognitive dissonance, there must be ways in which the encounter between ideology and contradictory experience are resolved in favour of the ideology to explain why

objectivism remains a foundation of science. Whether participants are aware of the disparity between ideology and reality, they must act in ways that have the effect of preserving the appearance (for it is only that) of symmetry between ideology and actuality. It turns out, then, that Woolgar is presenting the practice of science essentially in functional terms. Scientists' conduct on actual occasions somehow has to overcome a problem which is built into the practice in which they engage. Moreover, because their experience is rooted in the ideology, they are unaware that the functional problem is being resolved.

Woolgar ends up treating any instance of scientific investigation as being subject to the same general problem. It has to be organised in some way to preserve objectivist assumptions. If it were not, things would develop so that the incompatibility between ideology and actual experience would be apparent. Realisation of this incompatibility would jeopardise the ideology. Scientists themselves, of course, simply presuppose the ideology, and so do not face the need to sustain the general supposition that objects pre-exist their discovery. All they need to do is apply that — unquestioned — assumption to the issue of whether *this* object exists.

Woolgar's way of presenting the scientists' situation understates the extent to which their understanding of the prior existence of particular objects and sets of objects depends upon the framework that their science provides and not on generalised 'objectivist' assumptions. This is especially so in regard to the timescales within which they understand the genre of phenomena they are investigating. Their science entitles them to take-for-granted the prior existence of innumerable objects, and, from the start, to conceive 'the object' of their discovering work to be one which, if it is confirmed to exist, will have a history antecedent to the point at which the investigation into it began. After all, the remoteness of astronomical bodies is expressed in light years, a combination of distance and time, and assigns pulsars, for example, an age of several million millennia which far exceeds that of modern scientific inquiries. Similarly, archaeological investigations of dinosaur bones takes-for-granted their historic character, dinosaurs being understood as extinct even before there were human beings, let alone scientific investigators.

We're not hostile to the idea that, *for the purposes of* the sociology of scientific knowledge, the study of scientists' investigations should commonly avoid presupposing the veracity or 'factuality' of the discoveries issuing from the investigations under study. However, this is no more than a modest methodological precaution occasioned by the thought that hindsight is not always a benefit, and is required to effect a careful alignment between the understandings of those being investigated and the social scientists investigating them. *If* one is interested — which, for various reasons, sociologists very well might be — in examining the real-time step-by-step development and progression of scientific investigations, then the eventual outcome, being currently unknown, cannot shape analytic decisions about how the steps of the investigation are to be sequenced. Knowing how things turned out can result in reading into situations which preceded their formation, understandings that developed later. Doing this will inevitably distort the portrayal of how the scientists' understandings unfolded. *In that sense*, 'the object' (that is, whatever is eventually discovered) can be left out without significant loss to the sociological story.

CONCLUSION

The promotion when undertaking social science investigations of science, of a somewhat modest *sociological* point about the lack of necessity of presupposing the epistemological status of a particular scientific discovery, into a call for a 'radical' and general inversion of relations between object and representation provides yet another illustration of sociology's imperialist inclinations and its readiness to promote ideas that are defined relative to its needs as if they were appropriate for other disciplines regardless of what *their* purposes might be.

Though this one is perhaps more (and more deliberately) provocative than most sociological proposals for a deep, if not fundamental, revision in our general ways of thinking required to accommodate a more 'sociological' understanding of things, Woolgar's efforts are representative of sociology's tendencies toward

imperialistic revisionism and of the difficulties which consequently attend any reasoned evaluation of the claims made in support of such proposals. The problems do not start with the alternative to our established ways of thinking. They begin with the depiction of what our 'usual ways of thinking' are alleged to be. Woolgar is trying to change our idea of 'discovery', any discovery not just scientific ones. But what does he say our idea of discovery is? He treats it as if it were a simple idea. Making discovery is a matter of unexpectedly stumbling on something, with the object so discovered simply presenting itself to us. This is what grounds the notion of our 'passivity' within the process of discovery. Related to both is the notion of discovery as a 'point event', something that is entirely completed in a moment. This might be how we think about one *kind* of discovery (apart from Archimedes' eponymous 'Eureka' moment, perhaps the most famous examples concern Newton's apocryphal apple and Fleming's petri dish), but surely we don't think all discoveries (including complex ones like the hugely publicised and decades long saga of the quest for the elusive Higgs boson) are like this? No doubt serendipity plays its part in the research process, as it does in many of the things we do, but given these discoveries require prolonged and massive preparation, we hardly want to say we simply stumbled over them. Again, even though discoveries may sometimes, or in some respects, be point events, we do not suppose that, even in such cases, the point event is all there is to them. The views that Woolgar offers as challenging conventional views of discoveries turn out to be quite compatible with an understanding of the (diversity of) form(s) that such discovery can take. The invidious contrast is between his own views and a simplistic idea of discovery which is taken to be exhaustive of what that could possibly be. Earlier, we made much the same point about the way that a quite standard understanding of the structure of scientific investigations underpins Woolgar's story about 'splitting'. If we question that we hold the initial misconceptions that Woolgar assumes we do, and if we accept that Woolgar's own account is dependent on a quite conventional version of scientific discovery, then the need to problematise how the 'order of precedence' between object and representations in scientific discoveries is achieved simply dissolves.

The urge to invert the order of precedence of objects and representations comes about, not because it is necessary to understand the fairly banal facts about scientific discoveries, but from incorporating the rather large assumption about the identity of epistemology and ontology. The inversion is actually required in order to find a way of accommodating factual observations made within sociological studies to that assumption. In the end, very little consequential difference is made to understanding the process by which scientific investigations result in discoveries. Actually, the discussion of the scientific examples is really a red herring, obscuring as it does the fact that acceptance of the assumption is a high price indeed to pay for not very much change in understanding of the sequencing of scientific investigations. What is subversive and drastic in Woolgar's argument, then, is the identification of epistemology with ontology. This security of this proposal can hardly be accepted solely on the basis of Woolgar's confident *en passant* assertion of it. Equally, we can hardly grant it to be intelligible simply on the basis of his speedy assurance that it is so. If Woolgar wants to be taken seriously, he has to satisfy us that the equating of epistemology with ontology can be sustained. But, of course, that would immediately take us into highly contested territory with extensive literatures both supporting and combating such an idea. The need to slog one's way through the intricacies of philosophical debate is hardly what someone who was looking to sociology for insight into how science proceeds, was expecting. On their own terms, of course, these philosophical issues are challenging and captivating. It is just that paying attention to conceptual arguments over the methodology of science and social science means *disattending* to the empirical issues regarding science which motivated the investigations in the first place. This, of course, explains why, when Sociology colonises other disciplines, they usually end by being populated with people arguing Sociology.