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The Politics of Explanation: an Alternative

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From more ashmore the phoenix will rise.
(proverb from Yorkshire)

Reflexivity is necessarily at the heart of social studies of science because it is often argued that relativist sociologists are sawing the branch upon which they sit (Woolgar, 1982, 1983; Hollis and Lukes, 1982; Ashmore, 1985; Lawson, 1985). By making social explanations of the behaviour of natural scientists they make it impossible for their own explanations to be seriously believed by anyone. Their arguments in feeding back on themselves nullify their own claims. They are in effect self-contradictory, or at least entangled in a sort of aporia similar to the famous 'all Cretans are liars', aporia from which they cannot escape except by indefinite navel-gazing, dangerous solipsism, insanity and probably death.

The Accusation of Self-contradiction

Such a critique levelled at the work done during the last ten years in the social studies of science implies the following.

1 The strength of (natural) sciences comes not only from their somehow getting in contact with extra-human objects (no matter through how many mediations) but also from *not* being limited by the human, historical or local point of view; this ideal should be imitated by the social sciences and this is, historically, what they have tried to do. Thus, asking the social sciences to study the natural *sciences* – or, worse, asking them to study themselves – is a logical, moral, political and even aesthetic impossibility, since it means abandoning the only safeguard and source of certainty, which is offered by the non-local, non-historical, non-human contact with objects; the sky will fall on our heads if it is not firmly propped up by at least a few pillars much stronger than our weak forces or those of our contingent, local and historical societies.

2 *Self-contradiction* is so bad that if someone can be convicted of being self-contradictory this is the end of all his or her serious claims; the principle of non-contradiction is somehow necessary for all legitimate

explanations in both human and natural sciences as it is for daily life in general.

3   Providing a *social* explanation of knowledge (in the natural as well as in the human sciences) is, in effect, to nullify or at least to weaken the claims involved; if someone can be accused of being influenced by social factors, this means that he or she no longer needs to be taken seriously (Barnes and Bloor, 1982).

4   The *relativist* sociologist of science is supposed to offer social explanations of something, as this is what is expected of him or her; the 'something' comprises an infinitely long repertoire of objects which are admitted to be non-social; 'social' denotes a long but finite repertoire of elements that tie men and women together; a social explanation thus occurs when an element of the list of objects ('wrongly' thought by natural scientists to be 'only objective') is related to or replaced by one of the elements from the list of social factors; this relation is a one-to-many rather than a one-to-one type, that is, the *same* social factors are used to explain many natural or objective elements (see below).

5   Providing an *explanation* (in either natural or social sciences) is inherently good; thus accusing someone of providing no explanation puts an end to the dispute; the opponent is just story-telling and may be stopped by a simple question like 'so what?'; to answer the 'so what?' question entails proving that he or she is doing *more* than just telling stories, that he or she is really also offering some explanation.

6   A further point which is implicit in all the preceding: everyone is looking for who or what is *responsible* for some state of affairs; 'accusation' is thus implicated in all attempts at explaining something; the accusation made against relativists of being self-contradictory is a mirror-image of the accusation made by social scientists against natural scientists who are said 'wrongly' to believe that they are dealing 'only' with a repertoire of objective elements (see Figure 1).

In order to understand the importance and place of reflexivity in our field, and then to define our own policy of explanation, it is first necessary to criticize each of these common-sense arguments implied in the accusation of self-contradiction.

1   Relativist sociologists are not sawing the branch upon which they sit because they are not seated on it, and no one is or has ever been: the strength of any science, and indeed of any argumentation, has never come from non-local, non-human and non-historical allies; denying rationality does not mean that the sky is going to fall on our heads (as my Gallic ancestors used to believe), because the sky is supported by many other firmer pillars.

2   The principle of non-contradiction is far from the necessary condition of any explanation (be it in natural or in social sciences); on the contrary, the basis of anthropology of science, the principle of *translation* (Callon,

1986; Latour, 1987, 1988) makes the idea of non-contradiction a belated end-product of the practice of science.

3   Providing a social explanation of something has nothing to do with weakening or nullifying a position provided that. . .

4   . . .it is made clear that the notion of a 'social' explanation is entirely reshuffled; it must no longer be seen as a replacement of several elements in the infinite repertoire of natural objects by one or two factors taken from the list of social objects.

5   This in turn is possible only if we abandon the idea that offering an explanation is good for your health and inherently better than 'just story-telling'.

6   This in turn becomes possible only on the condition that the accusation process and the search for responsibility – which shapes the development of the social sciences – is brought to a stop.

The Politics of Explanation

To criticize the notions that form the basis of the accusation of being self-contradictory, we first have to define explanation. In its simplest form (see Figure 1) it means establishing some sort of relation between two lists, one comprising an inventory of elements to be explained (B) and the other a repertoire of elements said to provide the explanation (A).
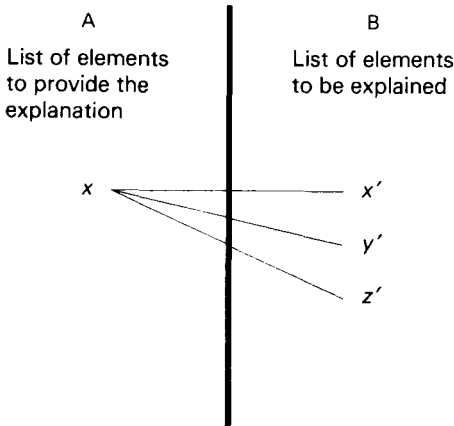

Figure 1   *The two repertoires:* explanans *and* ad explananda

It is generally admitted that if there is a one-to-one connection, nothing is explained since there are as many elements in one list as in the other. Thus, an explanation is said to be provided only when *more than one*

element in list B is related to one element of list A. In this general form the politics of explanation can be described like this: when you hold x element of A, you also hold the x', y' elements of B. It is in effect a general definition of power, power being understood in both its political and logical senses. The corollary of this 'holding of' several elements by one, is a general feeling of strength, economy and aesthetic satisfaction: the one element may 'replace', 'represent', 'stand for' all the others, which are in effect made secondary, deducible, subservient or negligible.

This simple definition allows us to measure the *power of an explanation*. An explanation becomes more powerful by relating more elements of B to a single element of A. This scale makes it possible to calibrate variations in explanatory power (see Figure 2).

The maximum on this scale is set when you can say that all the elements of B, including those which are *not yet* present in it, can be *deduced* from one element of A. In this case holding an element of A is holding *in potentia* the rest of the list. Traditionally, a mathematical demonstration is considered to offer this best form of explanation. The belief in the existence of a demonstration or deduction explains most of the enthusiasm characteristic of the classical age.

Scale of powerful explanations

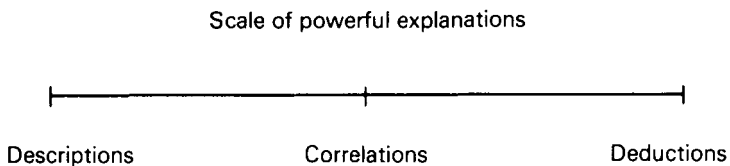Descriptions   Correlations   Deductions

Figure 2   *The scale of explanatory power*

The centre of this scale of explanatory power occurs when several elements of B are said to be often (always, frequently, significantly) related to one another. In this case no element of A can replace elements of B. What can be done, however, is to write down in list A the constant relation recognized among elements of B. Column A is thus made up of a list of constant correlations. The belief in the possibility of various structures, correlations, statistical laws does not trigger as much enthusiasm as the former belief in deduction but is better adapted to our sceptical age. What could be called a disappointed scientism runs through most of the natural sciences and all of the human sciences (apart from ethnography, exegesis and psychoanalysis).

The other extremity on the scale of explanatory power is when no deduction or correlation of any sort can be established. The list of elements in A is simply the repetition of several elements of B, arranged and summarized in such a way that, for a few practical ends, holding list A is provisionally equivalent to holding list B. This kind of explanation most

often has the literary character of a *story*. This is what is called a 'description', and is often associated with the work of historians.

This account of explanations is all very well but, apart from the sociological use of the word 'power', is still very much in keeping with classic discussions in epistemology. To go further and define our own policy of explanation we should understand why there are two lists in the first place. In other words, why should we want to explain anything? In what sort of peculiar situation is an explanation necessary and when is a powerful explanation seen as inherently better than a weak one?

A strong explanation becomes necessary when someone wishes to *act at a distance* (Latour, 1987). If you are in the setting x' you do not need to explain it – practice and weak accounts will be sufficient. If you are away from the setting and indifferent to it, you do not need to explain it either – practice in the new setting x will do. If you are away and simply remembering how it was when you were in setting x' you still do not need powerful explanations – story-telling will do the job much better. You start to need a stronger explanation when you are away and still wish to act on the setting x'. Why? Because you now have to be in two settings x and x' at once. You need to hold in the setting x some elements or features of x'. 'Information' is the word often used to describe all the elements of x' that can be mobilized, transferred, accumulated in x. Information is the go-between, the mediation, the translator, the *metaxu* that constantly oscillates between the presence of x' and its absence.

The Greek offer, as usual, the best mythology of the action at a distance, through the use of carefully designed forms. After all, Thales is credited with inventing geometry when, not wishing to climb on Kheops' pyramid, he 'simply' measured the shadows of a stick firmly stuck in the ground (Serres, 1983). His theorem (the epitome of powerful explanations) resulted in the possibility of holding all the pyramids (existing, to be built and never to be built) through the little calculations held in the hands. What we so much admire in the Greek miracle is a reversal of power relations: the weakest, that is, a tiny people holding only shadows and paper-forms become stronger than the ancient and powerful Egyptians with their heavy stone pyramids. Holding the forms is tantamount to holding, in addition, everything else. Platonism, through its many avatars, is the philosophy of this fantastic enthusiasm for a reversal of the order of priority between 'shadows' and 'things'. But the same process is at work for weaker explanations as well. Theorems are not the only technics that allow these moves. All sciences are defined first of all by the sort of elements they extract from the settings, and then mobilize, accumulate, combine and display: fossils, stuffed animals, photographs, trophies, questionnaires; everything which, in one way or another, solves the problem of action at a distance, fills the gap, through the production of information, between the presence of x' and its absence (Latour, 1986; Latour and de Noblet, 1985).

This definition of an explanation as a *measure of a distance between contexts* has three important consequences that I shall use later in the argument. First, it creates the very distinction between practice on the one hand, and knowledge on the other: practice becomes whatever people do in the setting acted upon; knowledge becomes whatever is mobilized in x to act upon the other setting. It also establishes the distinction between form and matter: form becomes whatever is transferable from x' to x; matter is whatever cannot stand the trip. Finally, it is also what produces the very separation between the 'outside world' and our 'interpretation of' what the world is like. Rationalists and relativists debate endlessly on whether or not our ideas have to be in correspondence or not with the world 'out there', without ever raising this simple question: how come that the world is '*out* there', in the first place, instead of being '*in* there'? The answer is simple enough. The problem of correspondence between the forms mobilized and the settings from which they have been extracted, becomes crucial only for those who want to act at a distance. If you are not at a distance, or do not wish to act upon other settings, the notion of correspondence vanishes, and so does the problem of the referent.

If you now wish, from the setting x, to hold not only x', but many other settings, x'' and so on . . ., you start to need more and more powerful explanations. This need does not arise from any psychological, political or metaphysical lust for power, it is simply the consequence of solving the practical problems of acting at a distance. Since the mobilized forms are not the settings themselves mobilized, it is perfectly possible for someone who holds the forms to hold nothing at all but shadows. Thus, something additional has to be done to (re)gain, in the setting x, the superiority which is lost by being away from x', x'', etcetera. In other words, the notion of a powerful explanation cannot be dissociated from the slow establishment of what I have called *centres of calculation* for acting at a distance. The two lists above and their various connections were an epistemological rendering of a very practical activity: *network-building*, that is, how we can tie as many settings as possible to as few elements as possible through as few intermediaries as possible (Latour, 1986, 1987).

This problem should not be seen as just a formal, technical, economical, aesthetic, or political endeavour. The same problems have to be solved no matter if one wishes to invent a new theorem, a telephone network, a trade route, an elegant theory, or an empire. More exactly, none of these separate endeavours is possible without simultaneously engaging in the others. This is why we had, in our studies of network-building, to replace distinct political, economic, technical and intellectual questions by one common task, namely how to build centres of calculation and extended networks (Hughes, 1983; Callon et al. 1986). The differences between realms of activity are less important than the possibility of them all conspiring to reverse the order of priority or the relations of strength between centres and periphery.

Such a point of view has one important consequence for the present argument. There is no reason to believe that there should be a list of social elements which can be related, in a one-to-many connection, to provide the explanation of some natural science elements. The possibility, indeed the very existence of two homogeneous lists, one of social elements, the other of non-social ones, is fanciful. What we see, on the contrary, is how settings strive to become centres by mobilizing everything at hand and tying their claims to as many resources as possible. Is this social or natural or technical? Good luck to the person who tries to carve out this kind of distinction from the imbroglio that holds together precisely because it associates as many heterogeneous elements as possible in one centre. To be sure, social factors are still there, but they are one of the things to be studied, not elements which allow us to understand. 'Social factors' are the particular product of professional social scientists striving to establish new types of calculations in their institutions. They define, in many various ways, what holds us all together. They call this 'society', and try to render their definitions indispensable to as many other people as possible, by insinuating themselves in as many other trades as possible (business, politics, academic life, journalism, the publishing industry, and so on). No matter how interesting their work, it is no different in form and purpose from that of all the others engaged in network-building. In other words, the social sciences are part of the problem, not of the solution. To expect an explanation of the natural sciences from them, is a bit like expecting the water distribution companies to 'explain' the telephone networks.

The main consequence of focusing on centres of calculation and on the many practical ways in which they extract and combine information, is a strong rejection of arguments in terms of cause and effect. No matter how much the notion of cause has been criticized (and now replaced by more modest variants such as correlation, correspondence, structure, pattern) there remains, in the back of our minds, the idea that a story is incomplete if it does not conclude with a set of words (or concepts, or sentences) endowed with the ability to trigger (generate, influence or produce) the events or occurrences which have been studied. All the debates about internal and external factors are based on the possibility of having something like 'factors' or 'determinants'. Even those who are sceptical about the possibility of finding a cause for phenomena (especially in the social sciences) do not dispute that some elements (*explanans*) should occupy the position of a cause while others (*ad explananda*) should play the role of effects. There is always something of a tribunal in these trials that settle responsibilities, decide on who will be accused and who will be innocent – the cause is always literally a cause; some of them like capitalism have even be turned into a '*cause célèbre*' (Girard, 1978).

The very existence of these two lists, or of these two sets, one of causes, the other of effects, becomes very doubtful in the work of building centres

of calculation is considered. Paradoxically, the cause appears as *the consequence* of expanding the networks and of reinforcing the centres. This is very beautifully expounded by Fernand Braudel (1985). You cannot explain the development of the world economy by invoking a force of some sort (for instance, capitalism) because this cause is itself helpless as long as centres do not exist which are able to capitalize, on a larger scale, on whatever is produced and sold. The heterogeneous association of many elements (which was supposed to be explained) is precisely what, *in the end*, gives strength to this capitalism which was supposed to offer an explanation. In more philosophical terms, it means that a cause (factor, determinant, pattern, or correlate) is the outcome of a trial of responsibility through which a few elements of the network are taken to be the impetus behind the whole business. It is, in practice, very much an election of representatives or, depending on the outcome, an accusation made against a scapegoat (Girard, 1978). The belief in causes and effect is always, in some sense, the admiration for a chain of command or the hatred of a mob looking for someone to stone.

Providing an explanation is, in a nutshell, working at empire-building; the more powerful an explanation, the larger the empire and the stronger the material in which it is built. What we admire in powerful theories we should also admire in freeways, multinational corporations, satellite networks, weapon systems, international banking and data banks. If we do not admire these achievements, there is no basis for using a double standard and letting the 'powerful theories' stand apart and alone be worshipped. What we mean by a 'powerful explanation' in the social sciences is most often an imitation of a simplified version of a combative interpretation of some hard sciences of the past when they were politically at their weakest and when they were dealing with their simplest objects (Shapin and Schaffer, 1985; Prigogine and Stengers: 1979)! Although such an explanation has been given of only a very few simple laws in some parts of mechanics and astronomy, philosophers of science have made of this *apax* a general rule.

Should We Explain Anything?

The issue is not to explain the natural sciences by using social sciences, nor to substitute 'centres of calculation' for 'capitalism', nor to replace the search for causes by empire-building. This would be to replace one scapegoat by another and again to leave our argument open to the accusation of being self-contradictory. As Girard (1978) suggests, the point is to modify the whole regime of accusation.

There are two ways of displaying powerful explanations, and thus two ways of solving the problem of the *distance* between the setting that offers the explanation and those that are explained. The first is common to all

disciplines: hold the elements of A and deduce – correlate, produce, predict, reorganize, comment or enlighten – as many elements of B as possible. The second has just been alluded to: *display the work* of extracting elements from B, the *work* of bringing it to A, the *work* of making up explanations inside A, the *work* of acting back on B from A. The first way tries to abolish distance, the second feeds on it. In the first, power is reinforced and the represented elements disappear in their represen- tatives. In the second, power is weakened and the initial elements are maintained in full view. The first is reductionist, because holding a single element of A is tantamount to holding all the elements of B. The second I call non-reductionist or 'irreductionist' because it *adds* the work of reduction to the rest, instead of subtracting the rest once the reduction has been achieved. The first creates a power, that is the impression that having one element involves having all the others 'in potentia'. The second creates what I call a gradient of force (Latour, 1988: part II). In other words, the first starts with *equivalences* without telling through which instruments and through which metrology these equivalences are obtained; the second starts from translations and tries to present the work of *rendering elements equivalent* by setting up new instruments and keeping long metrological chains in alignment. In still other words, the first tradition accuses and allocates responsibilities, while the second regards accusations as unfair because they always fall back on an innocent scapegoat. The first is on the side of the knowing, the second is also on the side of the known.

This distinction makes clear that relativist sociologists are far from trying to emulate the natural sciences they study – an emulation which, as we saw earlier, provides the basis for the accusation of being self- contradictory. Social studies of science are not an inferior breed of science, unable to offer explanations as powerful as those of the natural sciences. No science, be it natural or social, has ever offered a powerful explanation of that sort. As well as they can, they all strive to tie their claims in as many ways as possible to a sufficient number of elements to establish an effective two-way connection with the settings on which they want to act from a distance. In this game, the social sciences fare no worse than many natural sciences.

The aporia from which I started is thus completely modified: the first question is no longer 'Since you do not believe that science comprises of non-human, non-historial, non-contingent elements, how can you, relativist sociologists of science claim to explain anything?' No explanation, no matter how abstract the science, no matter how powerful the regime, has ever consisted of anything more than a disproportionate amount of heterogeneous, historical, contingent elements. *We do not deprive ourselves of allies* when we show that these are the only allies that have ever been on the side of the hard natural sciences. We, the soft and critical sciences,

have the same type of resources, although, I admit, *less of them*. The second question is no longer 'How can you, without contradiction, appeal to social factors in order to explain the development of natural sciences?' Social sciences are not a reservoir of notions and entities from which we would draw our resources. They are part and parcel of the very activity we want to study, part of our problem, not of our solution. In the course of our work we have irreversibly altered the meaning of the word social. Thus, to the seemingly tricky question: 'What is left for a relativist to explain' the only possible answer is: 'Everything'.

This reformulation, however, does not bring the issue of self-contradiction to an end. We now have to raise a new question, a moral quandary much more difficult than the simple trap inside which rationalists tried to corner us. (It is also much more difficult because we raise it against ourselves.) If the work of explaining something is that of empire-building, *should we* explain something? Do we really want to participate in network-building? Do we want to add yet another discipline and profession to the many that we study? Do we wish to offer more powerful explanations, that is, to transfer the power relations from the setting studied to the centre of calculation studying them? Do we lust for power and recognition? Do we want to imitate the ethos and styles of science? Do we want to dominate the natural scientists by evaluating, explaining and judging their behaviour? (This is not a spurious question since, in at least a few sub-disciplines like science policy, evaluation or management of R&D, we have some of the means to produce evaluations and judgements, and we can therefore be dangerous.)

The answer is a qualified *no*. In other words, now that I have freed my enquiry from the false accusation of being self-contradictory, I have immediately to limit its newly-obtained freedom and turn this time not against the rationalists – who are no longer any match for us – but against my own trade. The ideal of an explanation is not to be reached, not only because it is unreachable, but *because it is not a desirable goal anyway*.

Figure 3 may help us to understand this new quandary. The disciplines we study are taken to be either really fictitious or really scientific; our own explanation of their development may also be taken as really ficti-tious or as really scientific. The four boxes obtained represent four un-acceptable ways of continuing our social studies of science. Boxes 2 and 3 are rivals in arrogance, the first because it dismisses all attempts at studying science as being unscientific and the second because it grants itself privileges it denies to the sciences it studies. Box 1 drowns everything, including oneself, in a cynical and derisive regression. Box 4, on the contrary, extends a pompous, uncritical and scientific belief in science to everything, even going so far as to generate the monster known as science of science!

Disciplines studied

		Fictitious	Scientific
		1	**2**
	Fictitious	Coherent; anarchist version; our own account is as fictitious and no more serious	Incoherent; our own account is ridiculous and does not threaten science (version often held by scientists)
		3	**4**
	Scientific	Incoherent; our own account is privileged since social science is firmer; asymmetric sociologism	Coherent; scientistic version; no threat to science

Our own explanation

Figure 3 *Four unacceptable ways of doing social studies of science*

Reflexivity, Yes, but Which Sort?

Let us square the circle: we are looking for an explanation of the natural sciences quite different from what is usually called a scientific explanation; we strongly reject the helping hands offered us by the social sciences; on the contrary, we consider them all part of the networks we want to explain; we try to establish a space which is neither above nor inside those networks; we want to describe and expose the politics of explanation, but without replication and without adding another discipline to the plethora already striving for existence; we want to be at once *more* scientific than the sciences – since we try to escape from their struggles – and much *less* scientific – since we do not wish to fight with their weapons. Our quandary is similar to that of a non-violent pacifist who still wishes to be 'stronger' than a violent militarist. We are looking for weaker, rather than stronger, explanations, but we still would like these weak accounts to defeat the strong ones. . .

This problem is so difficult because it entails simultaneously resolving three paradoxes. The first paradox is common to all forms of writing: how to be at once here (in a setting *x*) and there (in another setting *y*); the second is common to all sciences: how to be at once here (in *x*), there (in *y*) and *in between* managing the network that ties the two together; the third is common to all texts that try to escape the alternative between

fiction and science: how to steer a course between being believed *too much* by the readers and *not enough*. Resolving the first paradox would enable us to write stories; resolving the second would make us remain scientists; resolving the third would enable us to write *reflexive* accounts. I use 'reflexive' to denote any text that takes into account its own production and which, by doing so, claims to undo the deleterious effects upon its readers of being believed too little or too much. Resolving all the three paradoxes simultaneously would mean that we could write texts which would at once be craftily written, scrupulously true, which would not make the readers believe that what is reported is exact and which would still be interesting. Such texts would in effect abolish the distinction between science and fiction.

In order to develop this alternative policy of explanation let us assess the advantages and disadvantages of reflexivity. To do so I shall distinguish two kinds of reflexivity which correspond to the kind of deleterious effects writers wish to overcome. Meta-reflexivity is the term for the attempt to avoid a text being believed by its readers, and infra-reflexivity the attempt to avoid a text *not* being believed by its readers.

Meta-reflexivity

For many writers the main deleterious effect of a text is to be naively believed by readers. Readers have this bad habit, they say, of being immediately taken-in by any story and being led to believe that there is something 'out there' which is the referent of the text and which is in correspondence with the text. Many ancient and modern writers, wishing to point the attention of the reader away from the referent have tried to add reflexive elements that operate in the manner of so many caveats: do not believe me, something else is at stake which is more important.

When they try to shift the attention of the reader to the *text* these writers are called 'deconstructionists' and are often associated with Derrida. When writers try to sway the attention of the reader to the very activity of believing and making sense of something, they are sometimes called 'ethnomethodologists' and have Garfinkel as symbol. The deconstructionists try to write texts in such a way that they neither refer to anything nor give the impression of presenting or representing anything. Ethnomethodologists aim at just the opposite, they write texts that, although by necessity distant from the setting they describe, aim to give the impression of being still present out there in the lived world of their subjects, without deformation or transport. The stylistic goal is similar in both cases: render the text unreadable so that the usual two-way link between the account and the referent be interrupted and suspended.

In spite of their claims to novelty and post-modernism, these writers too often forget that a third way of creating reflexive texts has been practised for centuries by writers who try to direct attention to the reader

himself, to his own life and fate. This redirection is obtained by many religious texts, especially the New Testament, and implies a radical rejection of the whole business of explanation (Péguy, 1914; Latour, 1975). For instance, the story of the empty tomb in the Gospel of St Mark (Mark 16) is not to be read as information about the distant empirical tomb in the outskirts of Jerusalem sometime around Easter, AD 30, but about the reader of the Gospel and the kind of signs he needs in order to understand for himself that Jesus is alive, that he has risen from the dead. The silly empirical question of the women 'Who will roll the stone away from the entrance of the tomb?' (Mark 16:5) is replaced by the angel's admonishment, 'He has risen, he is not here. See the places where they laid him.' The good reader of such a text is not the one who asks the silly question 'What *really* happened there? Would I find traces of the empty tomb if I were to go to that place in Jerusalem and dig the ground?', but the one who asks the question: 'What is happening to me, now, hearing the angel's voice? Jesus is not really there, out there, any more. This is, indeed, what the angel means. Stops asking silly questions. He has risen. He lives now.' And in the process, the reader becomes the writer or the commentator, or the preacher of *another* text that transforms, translates, embroiders and adds to the unbroken chain of commentaries. For hundreds of years (until the seventeenth century) every effort had been made to make a normal 'linear' matter-of-factual reading of the Bible impossible. Not surprisingly, when scientifically-trained exegets started to read the Bible in their new way most of the stories fell apart.

In terms of reflexivity, translation, cunning and cleverness, I am not convinced that the post-modern deconstructionists are any match for the Evangelists and Fathers of the Church. In comparison they play with very few tools. Their meta-reflexivity is obtained by *adding* specific parts about the way texts or discourses should or should not be written (as I am doing now). This is what is usually called methodology. In the end the only way of writing a text that does not run the risk of being naively believed is to write methodologically. The dire result of such a tack is visible in the prose of Derrida and Garfinkel. If the prose was just unreadable, not much harm would be done. But there is something worse in it; worse, that is, from their own reflexive point of view. Deconstructionists and ethnomethodologists consider that if enough methodological precautions are taken, then better texts (better, that is, in the sense of texts which solve the absence–presence quandary) can be written. Derrida really believes that by all his tricks, cunning and entrapments, the texts he writes are more deconstructed than the column of a *New York Times* journalist writing about the latest plane crash. Some of the followers of Garfinkel really believe that once all the methodological precautions have been carried out, the lived-in world of the competent members can be presented truer to life than in the gloss of a classical sociologist such as Merton. Derrida

believes that a text can escape from the fate of presence, whereas Garfinkel seems to believe that a report may eventually escape from the fate of absence. Beneath these opposite reflexive claims there is a naive and irrepressible belief in the possibility of writing truer texts. Ashmore's marvellously funny PhD thesis (1985), ridicules all these claims to meta-reflexivity by pushing them to their ultimate dead ends. As we could say in French: 'Plus réflexif que moi, tu meurs.'

Meta-reflexivity is based on the idea that the most deleterious effect of a text is to be naively believed by the reader as in some way relating to a referent out there. Reflexivity is supposed to counteract this effect by rendering the text unfit for normal consumption (which often means unreadable). This accepts as given that the readers are naive believers, that there is such a thing as normal consumption, that people easily believe what they read, finally, that believing is always to relate an account to some referent 'out there'. This is a very naive set of beliefs in the naive beliefs of readers. I suspect this post-modern view of what it is to be modern is the result of a naive and uncritical version of what it is to offer a scientific explanation of something. Our experience in studying the scientific literature makes me seriously doubt these four assumptions (Callon et al., 1986). Readers seem to be much more devious, much harder to take in, much cleverer at deconstruction, much faster in fiction-making than is assumed by those writers who, with some arrogance, believe that others believe. Here, too, 'we need to play down the exoticism of the other'. Scientific texts prepare themselves against a much more likely outcome: that of *not* being believed by their readers, or worse, that of not *interesting* anyone.

But the most bizarre belief involved in meta-reflexivity comes when you study 'self-reference'. Woolgar (this volume, Chapter 2) for instance, assumes that an ethnographic text by Malinowski that talks about the way ethnography is produced is more reflexive than an ethnographic text about, say, the Balinese. Moreover, Malinowski's reflexivity could be, for Woolgar, a naive way of telling us a true story of how ethnography is reported. So he devises, along the same line, a truly third degree reflexive text that shows how Malinowski naively believed that being reflexive (in the second degree) he could escape from the accusation of being a naive story-teller. But Woolgar does not want us to believe that this third degree would be 'truly' reflexive, so he is very happy to imagine many other rungs on this Jacob's ladder — the top of which does indeed disappear in the sky but fails to promise an endless fecundity. Unfortunately, no amount of degrees, layers and Hofstadter's tricks, will make a very simple semiotic argument go away. A text about Malinowski's way of writing about the Balinese is no more and no less reflexive than Malinowski's text about the Balinese and this is no less and no more reflexive than what the Balinese themselves say; and Woolgar's nth degree account of the

whole thing is no more and no less reflexive than any of the others in the chain. Why can't they be ordered in a pile of reflexive layers? Because they are all texts or stories bearing on *something else*. There is no way to order texts in layers because they are all equal. Texts, so to speak, live in a democracy, as far as semiotics is concerned. The whole vertigo of self-reference stems from the very naive belief that the *same* actor appears in both the first (down below) and last text (up there). Conversely, reflexivists believe that when the text does *not* have the author as one of its characters it is *less* reflexive than when it does, as if these were not, in semiotic terms, two similar ways of building the enunciation (Greimas and Courtes, 1983; Bastide, 1985). Semiotically, the role played by the Balinese in the first text is exactly that of Woolgar in his fifth degree account. Instead of riding piggy-back on one another, the accounts simply stand side by side.

When Woolgar shows a photograph of himself writing a caption for this same photograph in an article about a book on the observation of observers, he seems to suggest that he is several loops of reflexivity above a 'naive' and 'unproblematic' photograph of a naked native. Semiotically he has not moved an inch; the two pictures, side by side, just show different actors and things. Woolgar's picture fails to abolish the problem of distance in the slightest. This also means that the original picture is in no way more naive or less reflexive than his. The pictures are on equal grounds, since they both show things at a distance and they both play with this distance. There is no difference between showing a woman planting rice in a paddy-field, and a sociologist writing a caption for his own photograph. The first is no simpler than the second, any more than 'once upon a time' is more unproblematic than 'this is the first sentence of the story'. The surrealists delighted in little tricks like 'ceci n'est pas une pipe' and such aporia, not because these broke away from common sense, as was claimed, but because they believed that common sense was naively believing. If Woolgar is right, then 'playing down the exoticism of the other' (this volume) means we have to get rid of all these loops, not because they are useless, but because everyone else makes use of them as well.

Infra-reflexivity
Meta-reflexivity is counter-productive since it makes texts less interesting, less rich and less believable; like all others, these texts already suffer from being uninteresting, poor, disputable or discredited! To think that social studies of science can benefit from this form of reflexivity is, in my view, a suicidal attitude, similar (in spite of the contrary impression one might have) to the older idea that a sociological account full of statistics and methodological commitments can defend itself better than a 'plain' journalistic account.

Fortunately, there is another direction which allows us to maintain the

necessary reflexivity without whirling helplessly in our efforts to outdo and outwit each other in proving that the other is a naive believer. I call this other tack infra-reflexivity because instead of writing about how (not) to write, it just writes. 'Just'? Well, not exactly. Let us detail this sounder policy of explanation.

A deflation in methodology. Since no amount of methodology will ever bring a text closer to the distant setting about which it writes, no amount of ignorance of deconstruction will take a text farther away from it either. I much prefer reading the *New York Times* to Derrida, and between Garfinkel and Merton I would hesitate for more than a minute (and probably would pick up the latest copy of the *New Scientist* instead – see why below). If many critieria, other than the extent of its meta-reflexivity, define the quality of a text, why not do away with all the paraphernalia of methodological precautions altogether? If meta-reflexivity is marked by an inflation of methods, infra-reflexivity is characterized by their *deflation*. Instead of piling layer upon layer of self-consciousness to no avail, why not have just one layer, the story, and obtain the necessary amount of reflexivity from somewhere else? After all, journalists, poets and novelists are not naive make-believe constructionists. They are much more subtle, devious and clever than self-conscious methodologists. They did not have to wait for post-modern writing to tell stories; they are as self-conscious as those who naively believe they are *more* self-conscious. Instead of saying that precautions should be taken either to recover the lived world of the competent member or to render the text unusable for make-believe consumption, just offer the lived world and write. Isn't this what novelists have done for three centuries?

Replacing methodology by style. This is especially clear if the main problem for any text is, as I claim, not to be too much but to be *too little* believed. What is merely signalled by methodological warnings should be done by style. (Amateurs of self-reference will have noted with delight that these last two paragraphs are self-contradictory: I am glad to offer them this delight.) All the literary resources that can be mustered to render an account lively, interesting, perceptive, suggestive and so on have to be present. Probably a better model is offered by English and French eighteenth-century philosophers and natural scientists, than by nineteenth- and twentieth-century German or French writers. It is true that to use the resources offered by such authors it means abandoning the naive idea that there is such a thing as post-modernism and such a thing as modernism (Latour, 1988: part II). This means abandoning the cherished idea (cherished in academic circles) that other people, before or elsewhere or down there below, believe in things and behave without consciousness. 'Forgive them, Father, for they know not what they do.'

Self-exemplification instead of self-reference. A much simpler way of obtaining the necessary reflexivity is to stick to principles of analysis which are self-exemplifying. This means that no privilege is asked for the account at hand. This main difference between meta- and infra-reflexivity is an ethical as well as a methodological or a stylistic commitment. Deep in the minds of (meta) reflexive writers there is the possiblility of reaching a meta-language, in terms of which all the infra-languages could be evaluated. Their passion for a meta-meta-level that would judge all the others (or render them harmless) is the best indication of this. The reductionism of this position suggests that reflexive writers share the belief in the possiblity of a final level. Infra-reflexivity goes against this common belief in asking no privilege for the account at hand. When I portray scientific literature as in risk of not being believed and as bracing itself against such an outcome by mustering all possible allies at hand (Latour, 1987), I do not require for this account any more than this very process: my own text is in your hands and lives or dies through what you will do to it. In my efforts to forestall certain outcomes and encourage others, I too muster all available allies, all linguistic possibilities (if only, God – or Mammon – willing, I could write in my own mother tongue!).

Writing non-scientific texts. We each use a touch-stone to evaluate analyses of science in the making. Marxists, for instance, say that we need a powerful alternative theory that allows us to reject entirely the existing sciences; this is what reflexivity means to them. The meta-reflexivists say we need to stop saying anything about the world or about the way sciences talk about the world. All factions require *specific words, signs* and genres to decide whether or not an analysis is acceptable (see the disclaimer at the end of this paper and the editor's final word). For instance, Marxists (and other social scientists) accept the findings of a case study only if it shows the larger framework within which the case study is situated and by which it is in the last instance determined. Without the *words* 'larger framework' (or some equivalent), self-righteous readers consider that the 'case study' is misconceived, perhaps even dangerous since it leads to the 'pit-falls of empiricism' (Russell, 1986). A reflexivist of the Woolgar denomination considers a text naive if it describes only how life goes on in a biological laboratory (Woolgar, 1982). Without the presence in the story of the *character* 'author telling the story', the text is considered dangerously close to following the well-trodden path of scientism. Such reactions imply that these writers are fascinated by the presence or absence of certain words as a tool for evaluating texts. They suppose that by including characters like 'the framework' or 'the author', they can escape the terrible fate of being just a story, just another story. They would like to force us to limit our repertoire of literary tricks because they think their stories are somehow more than just a story. In effect they reject the semiotic turn.

The same puritanism is evident in attitudes as diverse as those of ethno-methodologists and deconstructionists. The latter scream if a text just tells a story because it might persuade the reader that the fiction is somehow true. The former scream if a text just tells a story about someone because, by representing them as what the text is 'about', it betrays those members truly responsible for the text. They too would like their texts to escape the terrible fate of being just another story about absent things. They too reject the semiotic turn.

All texts need somehow to solve the problem of being about absent elements (whatever the various reasons for this absence), that is, of being written in A about B. In most cases, a text tries to establish safe, two-way connections between A and B and to present (or represent) in A as many elements as possible of B. This is done so as to forget what happens in B as quickly as possible; for all practical purposes, B is now 'in A'. To fight this scientistic way of delegating representants and forgetting about the distance, the other solution consists in *interrupting* the two-way connection. The text becomes unreadable. This solution may be implemented in either A or B. Phenomenologists and ethnomethodologists solve the absence problem and try to stay in the setting B. The deconstructionists and reflexivists try to solve the presence problem by staying in A, without saying anything about anything.

The irony comes from the following observation. Each of these three solutions implies that the complete network must be *constantly maintained, surveyed and kept up*. No matter how scientific you are, you need constantly to move back and forth betweeen A and B, from the knower to the known, always afraid of being interrupted, unfaithful or wrong. No matter how ethnomethodological you are, you still write books in A about mathematicians (Livingston, 1986) or about biologists (Lynch, 1985) in B, but these are neither mathematical nor biological books. No matter how devious you are at writing about nothing, readers still assume you are at least saying something, about deconstruction. I do not point this out to ridicule these endeavours or to show, as reflexivists might, that they are all self-contradictory. The point is that they are all doing each other's job. Since everyone is in any case moving back and forth between A and B, and is worried about how to establish (or not to establish) the ties and how to represent (or not to represent) one setting in another, why don't we take this activity as the name of our game: displaying the knower and the known and the work needed to interrupt or create connections between A and B? I take this as the *non-scientific* way of studying both natural and social sciences; I also take it as a possible definition of infra-reflexivity.

The consequence of this position is that many more *marks* of a good story become available compared to the few insisted upon by the reflexivists. Since no amount of reflexivity, methodology, deconstruction, seriousness or statistics will turn our stories into non-stories, there is no reason for our

field to imitate those few genres that have gained hegemony in recent time. To the few wooden tongues developed in academic journals, we should add the many genres and styles of narration invented by novelists, journalists, artists, cartoonists, scientists and philosophers. The reflexive character of our domain will be recognized in the future by the multiplicity of genres, not by the tedious presence of 'reflexive loops'. As Chairman Mao said 'Let a hundred flowers bloom...' (although he had them cut rather short afterwards).

On the side of the known. The reflexivists spend an enormous amount of energy on the side of the knowing, and almost none on the side of the known. They think that any attempt to get at the things themselves is proof of naive empiricism. Even those from Yorkshire who claim to use literary tools to pursue social sciences (Ashmore, 1985; Mulkay, 1985) do so only to expose reflexive claims, never to talk *about* something. Talking 'about something' is anathema to every one of them. This horror, the fear of contamination with empiricism is amusing, because it is exactly the counter-part of the empiricist position. They all think that objects, things-in-themselves, are somehow out of reach. As if any access to the world was for ever in the hands of the empiricist programme. As if the world in which we live was the property of scientistic accounts of science. Reflexivists fully endorse the scientistic agenda when they believe there is no other way out of empiricism than language, words and self-reference. This hidden Kantism is unnecessary. There is another way: that of the world, not the word.

Tracy Kidder's (1981) marvellous, 'unreflexive' – 'merely' journalistic – account of the building of the Eagle computer, tells us more about reflexivity than piles of Hofstadter's tricks, because it is the computer *itself*, yes, the *thing* itself that appears as a reflexive, sentient and historical event. Callon's (1986) account of the scallops' harsh life deep in St Brieuc's bay in Brittany is completely unreflexive. It talks about scallops, fishermen and scientists, not about social scientists and self-reference. But are these scallops the same as those portrayed in oceanographers' struggles? No. No more than Kidder's computer in any way resembles the black-boxes displayed in computer showrooms. Are these objects in the same way as an empiricist would like them to be? Not a bit. They are freed, active and anthropological *projects*, full of life, and ready to take place in a dramatic story. I claim that there is more reflexivity in one account that makes the world alive than in one hundred self-reference loops that return the boring thinking mind to the stage. Infra-reflexivity is the programme followed by Serres (1983) that pushes the knower off-stage. Down with Kant! Down with the Critique! Let us go back to the world, still unknown and despised. If you sneer at this claim and say 'this is going back to realism', yes it is. A little relativism takes one away from realism; a lot brings one back.

Throw-away explanations. The belief in the existence of a framework inside which events are inserted in order to be explained is the hallmark of non-reflexive social sciences. This is the basis of the process of *denunciation* that allows social scientists to allocate responsibilities, to accuse, expose, unveil or to prove innocent (Boltanski, 1984). No one should conclude from this that reflexive studies of science should either say nothing or be limited to case studies (or to historians' narrative genres). Actors have the peculiar ability to tie together many heterogenous elements. They have very strong ideas about what framework is, who is responsible, what counts as an explanation and who is innocent. Once rid of the general framework, we are not back at the micro-level; we are instead introduced to a world in which actors have only *relative* size and are fighting hard to vary the size of everyone else (Callon and Latour, 1981). In order to do this, they need to recruit as many heterogeneous allies as possible. The stylistic conclusion is that we have to write stories that do not start with a framework but that end up with local and provisional variations of scale. The achievement of such stories is a new relationship between historical detail and the grand picture. Since the latter is produced by the former, the reader will always want *more details*, not less, and will never wish to leave details in favour of getting at the general trend. This also means that stories which ignore cause and effect, responsibilities and accusations, will be unfit for the normal mode of denunciation, exposition and unveiling. Our way of being reflexive will be to render our texts unfit for the deadly proof race over who is right. The paradox is that we shall always look for weak explanations rather than for general stronger ones. Every time we deal with a new topic, with a new field, with a new object, the explanation should be wholly different. Instead of explaining everything with the same cause and framework, and instead of abstaining from explanation in fear of breaking the reflexive game, we shall provide a one-off explanation, using a tailor-made cause. I am all for throw-away causes and for one-off explanations.

Cross-over instead of meta-language. One other way of displaying infra-reflexivity is by avoiding building a meta-language. If the ideal of an explanation, as I showed at the beginning, is to subsume the thing to be explained under a new account, this subsumption is precisely what we, social students of science, should abstain from. It would be absurd to develop a meta-language of say, two hundred specific words, and then to test whether such a vocabulary is able to replace the hundreds of thousands of terms and practices of the sciences we study. The worst outcome would be to be successful at this little game, thereby substituting the boring rote of the sociologists' repertoire for the rich work of the natural sciences. Lynch (1985) has provided us with the ultimate critique of such an ambition. There is no sociology to be done, he argues, other than the technical work

of the scientists themselves. They already have their sociology. Our work is to extract it. Unfortunately, Lynch failed to present an elegant solution to the problem because he was still embarrassed by Garfinkel's hopeless rejection of the semiotic turn. One possible formulation of Lynch's marvellous insight is that we strive for *equality* with the discipline we study. Instead of explaining it we want to *cross-over* it – as in a genetic cross-over. We want to learn our sociology from the scientists and we want to teach the scientists their science from our own sociology. This programme seems ambitious, even arrogant, but it simply means equal status for those who explain and those who are explained. This is 'affirmative action' extended to the social sciences; they have suffered discrimination for so long and they should not dream of an impossible revenge by trying to *dominate* the sciences through the use of a metalanguage.

Hybridization instead of disciplinary boundaries. Displaying the work of achieving an explanation is possible only if the display is not restricted to one location. If the work in our domain ends up generating a specific, distinct field of scholarship – defined as such perhaps in curricula – it means all our work has been by definition unreflexive. The criterion of our reflexivity is our ability to have our work distributed among the networks. This may be achieved by co-authorship with scientists (to abolish the meta-linguistic attitude of observers observing observers), but also by blurring the distinction between the study of science and the production of other sciences (thus showing how hegemony can be practically challenged). This in turn requires the ability to get out of academic circles and to tie our work to the many current struggles to resist being known, explained, studied, mobilized or represented. The shibboleth of reflexivity is not 'Do you include the author in your study?' but 'Can you make good your promise not to remain within the academic boundary?' Our domain will eventually be judged by its capacity to invent a technical and scientific democracy by showing how this relation between representant and represented can be altered. To propose an alternative policy of explanation is necessarily to define some new politics.

Conclusion

I have very briefly outlined some of the politics of explanation. I have freed our enterprise from the simple-minded argument levelled against us (by rationalists) that it is self-contradictory. I then tackled the crucial problem raised by reflexive writers such as Woolgar: we cannot innocently develop still another social science. Although these writers have rightly recognized the importance of the problem, their solution, meta-reflexivity, is too narrow and in the end sterile. I have argued for an alternative, infra-reflexivity, and sketched a few of its possible definitions. The

reflexive trend is inescapable: otherwise our field would indeed be self-contradictory, not in the sense propounded by the rationalists as a way of trying to get rid of us, but in our own sense. The worst outcome would be to get rid of ourselves by imitating the sciences and attempting to offer stronger explanations of their development. If the proposed alternative – the search for non-scientific and weaker explanations – seems daunting, let us remember that the sciences are still young and so are we – provided the rhetorical style of some star warrior does not bring the whole story to an abrupt end.

Disclaimer

This is not a self-exemplifying text. My subtle referee asked me to explain why. I have no answer except this: 'Why does this generation ask for a miraculous sign? I tell you the truth, *no sign will be given to it*' (Mark 8:12).

Note

Knowledge and Reflexivity

New Frontiers in the Sociology
of Knowledge

edited by
Steve Woolgar

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