ESSAY 4

THE SOCIOLOGISING OF PHILOSOPHY

INTRODUCTION

Actor Network Theory (ANT) is provocative, argumentative and ambitious. It also takes pleasure in its reflexive instincts and the exasperation which they sometimes generate. Moreover, not content to plough its furrow in the fertile fields of science and technology, it has, of late, begun to turn its attention to its mother discipline, Sociology, as well as to Philosophy. For ANT, much of the import of its own empirical studies is to be found with these disciplines and the relationships between them that. ANT wants to propose nothing less than complete re-design of Sociology and the extension of this new mode of sociologising into Philosophy. The outcome of such a re-design of the intellectual landscape is a body of studies and argument which appears to be a kind of spaghetti analysis.[20] Individual bits have a coherent, linear structure but when you put it all together, there is just a mass, a morass, of stuff to be dealt with. The structure disappears. One finds oneself confronted repeatedly with assertions, allegations and accounts which simply make no sense. For example, here is Bruno Latour on the state of social science.

> *(ANT) claims that since social accounts have failed on science so pitifully, it must have failed everywhere, science being special only in the sense that its practitioners did not let sociologists pass through their turf and destroy their objects with 'social explanations' without voicing their dissent loud and clear" (Latour 2005 p 101)*

And again this on the use of causal stories in social explanations:

> *If they don't literally replace some phenomenon by some social force, what do social explainers mean when they say that there is some force 'behind the illusory appearances' that constitutes the 'real stuff' out of which gods, arts, law, markets, psychology and beliefs are 'really' made? (Op. Cit. p 103)*

And finally, this on his own Damascene revelation.

> *...fisherman, oceanographers, satellites, and scallops might have some relations with one another, relations of such a sort that they make others do unexpected things.....Is there one element in this concatenation that can be designated as 'social'? No. Neither the functioning of satellites nor the*

[20] Spaghetti analysis is a kind of country cousin of the spaghetti code that software engineers talk about.

> *life habits of scallops would be clarified in any way by adding something social to the description. (Op. Cit. p 106-7)*

To use his own terminology, for Latour the social has disappeared from Sociology.

John Law demonstrates much the same sentiment although with contrasting tone.

> *These are the steps to follow if we are to attend well to practices, specificities, processes, and materialities. And they are also the steps that are needed if we are to undo the metaphysics of common sense realism. Is reality destiny? Common sense realism says yes. It suggests that while we may try to engineer the world and influence it, in the end the world is arranged in the way that it is: fixed more or less, definite more or less, and singular, coherent and outside practice. The move to performativity says no. It allows us to ask questions about realities that are simultaneously analytical and political. We may begin to ask how they are done. We may ask how they are contested.*
>
> *We may also ask how – and indeed whether – they might be done differently. In short, we open ourselves to the possibilities of an ontological politics. (Law 2009 pp12-3)*

So, on the one hand, ANT is engaged in an argument over the *bona fides* of the discipline from which it sprang and towards which it is now adopting its own analytic stance. On the other, we have an argument with the "mental model" (for want of a better term) by means of which its subjects constitute physical, psychological and social reality. If Sociology is about the social, then it has no topics. If, because of the grip that realism has in science and the authority which scientific accounts hold generally, ordinary people hold to a strong realist description of the world, then ANT has to disabuse them, or at least shake their faith.

Here indeed is imperialism of a very rapacious kind. Large issues are addressed and large claims made. The question for us is whether these claims actually hold water. We conclude they don't, and suggest this is because ANT systematically disregards the principles of caution and transparency we outlined in Essay One. For us, this lack of caution and transparency has meant that it is not so much where ANT started that is the source of the confusions we find it to be permeated with, but what happened after that. Although its initial position is a relatively secure and positive one, from the initial steps onwards, its foolhardy ambition and rush to conclusions causes things to go awry. Once it had wandered off course among the thickets and swamps of metaphysics and politics, there is no way back to where it started. All it appears it can do now is blunder around hoping to happen on a route back to safer and clearer ground. This strategy has yet to work. As can be seen from the kinds of comments with which we started, ANT has become more and more frenzied and frantic. In this essay, we will trace the path from ANT's beginnings to its current position. In so doing, we will underline those points at which caution and transparency might have offered alternative paths and more secure outcomes. This will lead to some recommendations about what might be done next.

THE STARTING POINT

Science in general, and the natural, biological and mathematical sciences in particular, have a special status in our culture. Their propositions, especially their propositions about how the world is, are taken to be authoritative. Those of us outside science come across these propositions as summary pronouncements, often appearing as texts of various kinds. Scientists, however, experience science somewhat differently because they encounter it as a job of work. They encounter it from within; and from within, science appears to be a body of conventional processes, procedures, and techniques, including processes, procedures and techniques related

to publication.[21] For the practising scientist, it is these bodies of procedures, processes and techniques which secure the propositions of science.[22]

It is here that the sociological interest in science has its origins. Just how (and for some, why) do scientists go from one set of practices to another? Given also that since it is Sociology we are discussing, the conventional character of such scientific practices is taken to be *social*. For the sociologist, this means that what underpins the transformation from one practice to another must itself be social. To put is at its clearest, sociological interest is in the social character of the mapping between one scientific practice and another; between analysing data, say, and formulating a discovery; between running an experiment and separating data from noise; or between working out whether a phenomenon is a discovery or an artefact and, then, writing that discovery up.

To say that science rests upon social practices is neither a large claim nor of itself in any way a demeaning one, although because of the way its supposed consequences are drawn out, it is often thought to be. All it says is that science can be viewed as being carried on within a social milieux. The phrasing here is important. Describing science as resting on social practices is a *sociological* view. It adopts, to coin the phrase, *the sociological attitude*. Naturally, this implies that there are many other attitudes towards science which could equally well be adopted; the epistemological, the political, the ethical, and so on. These other *analytic attitudes* raise their own questions, some of which society at large may well take to be very important. Latour wants to contest the view of some sociologists that Sociology should have priority in studying science, but he does so only by promoting his own sociological story.

As we say, the conventionalised social practices of science are where Sociology, particularly the social study of science and technology, and ANT begin. Even in the initial steps of description and analysis, however, the authoritative nature of science takes on significance. If the practices (including the practices for mapping between practices) are construed to be social through and through, what might this mean for the status of the propositions made on the basis of those practices? This is a small step but it marks the beginning of a slippery slope. From a sociological interest in (a way of viewing) science, we have moved to an epistemological interest in evaluating the logic of science's results. If science's propositions are embedded in social practices, can they represent how the world is independent of such practices? And what does the answer to this question mean for the authority of science? Undermining the authority of science allows us to question the depictions it gives of the world. From here to the conclusion that ontology is a social construction (probably the central tenet of ANT) is but a short leap.

At this point further complications arise. The authority of science is secured by philosophical arguments. That is to say, the metaphysics and epistemology of science are held to be secure because they satisfy conditions set down by Philosophy. For ANT, though not for most other sociologists interested in science and technology, the sociological conclusion that scientific practices are social must weaken, if not actually destroy, such philosophical underpinnings. And, since these underpinnings are the paradigmatic outcomes of what, for short hand, is called 'modernism' in Philosophy, then modernism itself is threatened. Such a line of reasoning leads seemingly ineluctably to the proposal that modernism must be replaced by a philosophy which encompasses the sociological attitude. This is where ANT has ended up. What started as the application of a common *methodological stipulation* with regard to the study of science as a social institution has, step by step, led to a maze of epistemological and ontological quibble and debate, with Sociology *a la ANT* set against Philosophy *a la modernism*. The interest in science as a social institution has passed into the background.

MISSING YOUR WAY AT THE START

[21] One such set relates to the reformulation of what Abraham Kaplan (1998) called "Logic in Use" of activities into the "Re-constructed Logic" required for publication.

[22] From now on, we will summarise processes, procedures and techniques as "practices"

What lies behind this series of positions? Or, if you prefer, which wrong turning has led ANT up this garden path? When comparing conventional Sociology to ANT, Latour says the following:

> *Whereas, in the first approach, every activity—law, science, technology, religion, organization, politics, management — could be related to and explained by the same social aggregates behind all of them, in the second version there exists nothing behind those activities even though they might be linked in a way that does produce a society—or doesn't produce one. (Latour, 2005, p 8)*

ANT, then, rejects what it sees as the metaphysics of conventional Sociology, a metaphysics which stipulates that there is a class of "things" (social aggregates) which "explains" social institutions and practices. "Explain", here, is taken to mean provides a causal account for the phenomenon in question.

Let us unpick this a little further. In *The Rules of Sociological Method",* Durkheim says:

> *The first and most fundamental rule is: Consider social facts as things. (Durkheim 1964 p14)*

By way of explication of this rather bare statement, Durkheim offers the following.

> *We assert not that social facts are material things but they are things by the same right as material things, although they differ from them in type.....*
>
> *To treat facts of a certain order as things....is not to place them in a certain category of reality but to assume a certain mental attitude towards them on the principle that when approaching their study we are absolutely ignorant of their nature...(Op. Cit. p lxiii)*

Having defined social facts in this way, Durkheim famously summarises the category of social facts thus:

> *...it consists of ways of acting, thinking and feeling, external to the individual, and endowed with a power of coercion, by reason of which they control him.(Op. Cit. p3)*

For ANT, all sociology (including sociological studies of science and technology) is Durkheimian through and through.[23] Here, for example, are two summary statements from very different types of analyses in the general field. First, Brian Wynne on the theory of Ether in Physics:

> *...the present case appears to be one where the concepts and principles of a science were developed and sustained not only (or perhaps not even) for their technical value, but very much also for their social value. Scientific thought developed in particular ways related to its possible functioning in the general social context rather than the esoteric scientific context. (Wynne, 1982 p 228)*

Second, Jane Barker and Hazel Downing on the introduction of word processing in offices:

> *...traditional previously effective forms of control in the office which have their roots in patriarchy, are, within the present crisis in the accumulation*

[23] Being "Durkheimian" in this context should be taken to mean holding to Durkheim's metaphysics. For ANT, Marxist, Critical Theorist and Feminist analysis of science and technology are all Durkheimian in this sense.

> *process, becoming redundant. Microelectronically based equipment is seized by capitalists as a solution offering a new form of control which enables them to cheapen labour and intensify productivity...(and which) which embodies the social relations of capital's dominance over labour. (Barker & Downing 1985 p 162-3)*

ANT sees all such accounts as fundamentally flawed. There are two key reasons why.

5. If a scientific or technological phenomenon (theory, process, system) is caused by a set of social facts, and if the causal account is valid, it must be possible to re-write or substitute the phenomenon by the causal social facts. This, it is said, is how "cause" works in science. A good or valid causal account shows how some phenomenon can always be decomposed into its causal components. But if the phenomenon in view is a theory (of the Ether, say) or a technological system (word processing, say) and the explanatory account is in terms of ideology, economic relations, or political power, how can the association of the causes produce the phenomenon?[24] Ideologies, economic relations and political power do not (cannot) produce theories and systems. Of course, what this overlooks is that the explanations offered are explanations of why the innovations were introduced or taken up.
   For ANT, the upshot of this is that the Sociology of Science and, by extension, conventional Sociology rest on a mistake. Social forces/facts cannot provide causal explanations of anything because they are part of Durkheim's metaphysics and cannot simply be assumed to be deployable within any causal explanations sociologists might give (on the rare occasions they try to give real causal explanation, that is).
6. When the focus of investigation shifts away from the original phenomenon of interest (the theory of Ether, say) and is concentrated on the causal force (the ideology of late Victorian England say), that causal force turns out to be a mirage or an illusion; somewhat like the end of the rainbow, the closer you look, the more it seems to retreat. Moreover, it is only discernable in terms of other (observable) things (ways of talking, sales of books, debates over legislation, and so on).

For ANT, the combination of these characteristics turns the Sociology of Science and, again by extension, all conventional Sociology, into a kind of institutionalised confidence trick. Academics, Government, the general public, the media are brought to believe in the illusion and its consequences by the use of a particular *legerdemain* which arranges the ways how we see how things are.

Having come to this conclusion, ANT argues that there are only two possible courses of action possible if the Sociology of Science (and Sociology) are to be saved. They are:

7. In line with the usual scorched earth strategy, the Sociology of Science has to be re-constituted from the ground up to bring out the networks of associations which *do* produce the phenomena under discussion; that is, the networks of associated objects which do enable science and technology to happen. Descriptions of these networks must set social objects on a par with scientific objects in the ontology of the causal account. Social facts/objects do not lie behind scientific facts but alongside them.[25] The account must, therefore, show how the science and

[24] This conclusion involves a serious displacement of 'the phenomenon' under discussion, since sociological stories don't attempt to report what the components of a mechanical or software system are but, rather, how those things were come up with or why they were accepted. These explanations are cast in terms of components of the system can be identified as bearers of particular values and so forth. The phenomenon is the contrivance or acceptance of the mechanism, system, or other innovation.

[25] Here it is the double meaning of 'fact' that is being traded on, with 'fact' in one use affirming that something is known or established and in the other indexing the state of affairs which is known, established or actual.

technology developed through the association of the social and scientific (and material) objects; that is, through the practices of associating them. Nothing lies behind the practice of science (or technology, or anything else); it is practices all the way down (to quote John Law (2009) again).

8. If the Sociology of Science has to be re-constituted around practices of association, this must also hold true for the rest of Sociology. Only by focussing on practices of association by which its phenomena are produced, can Sociology (and *mutatis mutandis* the Sociology of Sociology) be weaned away from the confidence trick it currently depends upon.

However, once ANT begins along the path of re-constituting Sociology, it is not long before it has to confront the modernist (ie Rationalist) epistemology on which conventional Sociology (and science) is said to be premised. Rationalist epistemology underpins what earlier we called the Durkheimian approach. As a form of institutionalised reasoning, this Rationalism processes out the practices of associating by which knowledge is actually produced. To reform Sociology, or so the argument now goes, it is not enough to focus on practices. A whole new practice-based epistemology has to be constructed.

Once you step off the path of sociological analysis and into the mire of epistemology, it is rarely long before you are dragging yourself through the bogs and swamps of ontology and morality. What started out as a local disciplinary debate over explanations in the Sociology of Science turns into a confrontation with the intellectual frameworks which underpin science, technology, and the whole of our modern way of life.

As we have said, there is no obvious and easy way out of the swamp ANT is now in. To change the metaphor, no straightforward and comfortable way of rolling up the magic carpet. The whole endeavour is too path dependent. What is needed is to go right back to the very first steps that ANT took and see if there is some other way to go and if there is, to look where it would lead.

How Durkheimian is Durkheimian Sociology?

We have called conventional Sociology "Durkheimian" as a shorthand for a methodology that seeks to explain patterns of social activities in terms of underlying social forces or facts.[26] And it is true that conventional Sociology often talks about itself in Durkheimian ways. However, just how Durkeheimian is it really? More pertinent, perhaps, just how Durkheimian is the Sociology of Science? This is an important question because if it should turn out that the Sociology of Science and conventional Sociology are not that Durkheimian in the first place, then ANT's rejection misses its target and harmlessly passes them by.

Before answering this question though, we should clarify what we are trying to do (or rather, not do). In talking about Durkheimian Sociology, we are not suggesting that this is the Sociology which Durkheim carried out or wished to carry out. What the actual sociologist Durkheim did or did not do is not germane here. Further, in seeking to explain how and why ANT has misunderstood Durkehimian Sociology, we are not thereby seeking to defend that form of Sociology. We come neither to praise Durkheimian Sociology nor to bury it. Our task is simply to ask whether ANT has got the Sociology of Science and conventional Sociology right.

The critical terms are the words " could be related to and explained by..." in the quotation from Latour we cited above. Durkheimianism thinks these relationships are law-like regularities of association between activities and underlying social facts and that the explanations of them are causal. The question is, simply (or perhaps not so simply): 'What would Durkheimian Sociology have to do to do that?' and 'Is that what Durkheimian Sociology actually does?'

[26] We might as well get one thing straight right now. By 'methodology' we do *not* mean an investigative technique (say questionnaire surveys, participant observation, cohort studies or the like) nor a loose limbed theoretical outlook (Grounded Theory, say or Exchange Theory). We mean a tightly coupled theoretical framework, research issues, investigative technique, and mode of analysis and presentation. A methodology is the whole package, not some subset. See Felix Kaufman (1944).

The first question first. In her book *The Dappled World*, Nancy Carwright (1999) argues that explanations in terms of causal laws in the sciences (mostly but not always Physics) work only in very constrained conditions. They do so only when scientists can formulate and construct a working *nomological machine*. This is what she means by that term.

> *The starting point for my view is the observation that no matter how we choose our (properties to be investigated), the kinds of associations required are hard to come by, and the cases when we feel most secure about them tend to be just the cases where we understand the arrangement of the capacities that give rise to them. The point is that our knowledge about those capacities and how they operate in given circumstances is not itself a catalogue of modalised regularity claims. It follows as a corollary...that laws of nature (in this necessary regular association sense of 'law') hold only ceteris paribus - they hold only relative to successful repeated operation of a nomological machine.*
> *What is a nomological machine? It is a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws. (Cartwright 1999 pp 49-50)*

The essence of a nomological machine is its *constrained description*. The initial conditions and the outcomes are constrained to be singular in their relations (x's behaviour or action causes y and given the set up of the machine is the only cause of y). There is a detailed analysis which tracks how x's behaviour or action produced y *and how it will do so in all similar circumstances*. As Cartwright goes on to say, the mathematical models which scientists trade in are, by and large, blueprints for nomological machines of varying degrees of robustness.[27]

Durkheimian Sociology might like to think that it constructs nomological machines, but is that really what it does? If we look at the Sociology of Science and the explanations it provides we can quickly see that they are not like this at all. Rather than being *causal* (and hence law-like or proto law-like) they are *functional* in form. Rather than providing detailed descriptions of the mechanisms (the causal story) by which some cause produces some effect, they offer accounts of the *significance* of that effect for the social formation in question. What is called "the causal chain" usually amounts to no more than the identification of formal parallels between the phenomenon under investigation and broad social doctrines and the invitation to accept that "It is no coincidence that...." This can be seen quite readily if we refer back to the two examples we used earlier.

Brian Wynne's account of the trajectory of 'Ether Science' places it firmly in the context of the "struggle" over the professionalisation of science and the academy. On the one hand, we have the proponents of a utilitarian, empiricist, naturalism seeking to form a professional scientific community. All the key figures here were members of the emerging *bourgeois* middle class created by the industrial revolution. On the other hand, we have the defenders of a holistic, 'spiritually' directed conception of the universe, albeit one which had to be couched in the dominant experimentalist idiom. The latter (and certainly all those located at the theory's intellectual centre in Cambridge) were either members of or closely associated with the traditional upper class elite. Wynne traces through these associations both in terms of kinship and friendship groups but also in terms

[27] The central point Cartwright makes is that these nomological machines only work in very, very constrained circumstances where the operation of the machine is "shielded" from all other influences (that is, they hold *ceteris paribus*). Since, nature, the world, reality, never is *ceteris paribus*, the laws of science describe only a small part of our world. While important for Cartwright's case, this argument is not germane to us just now. For Cartwright's analysis of cause in the Social Sciences see Cartwright (2007).

of membership of the Society for Psychical Research. On Wynne's account, the metaphysics which underlay the search for psychical phenomena was all of a piece with those of Ether Science.

In summarising his account, Wynne explicitly rejects what he calls "one way traffic" in terms of determinations. Rather he looks for explanation through "symmetrical interaction" (Wynne 1982 p 225). What he means by symmetrical interaction is what sociologists usually talk of as "functional fit".

> *Features of the general context influenced the cognitive content of late Victorian Physics in important and systematic ways. (Wynne Op Cit p 226)*

This influence can be seen in the ways that the concept of ether was transformed; how that concept was fitted into a broader moral discourse which rejected naturalism; and finally how views of matter, force and other central concepts had to be re-shaped because of the reality of ether. What we have here is not a causal story rooted in a worked through (or even embryonic) nomological machine. It is rather a description of the association of two modes of thought with their constituencies (the symmetric interaction) and the significance of that association as a microcosm of the emerging *bourgeois'* struggle for ideological dominance in post-industrial England.

Functional fit as an explanatory device is even more apparent in Barker and Downing's analysis. Here the frame of analysis is the crisis of capital accumulation in late Capitalism and the introduction of automation with its consequential de-skilling. This de-skilling allows an increase in the expropriation of labour value through routinisation and productivity. As Barker and Downing argue, this expropriation takes place in a context where labour relations might be typified by what are known as "rituals of resistance"; that is, ways in which predominantly female secretaries 'manage', 'control', 'undermine' the (patriarchal) power of their (male) principals. These rituals of resistance are, from a managerial perspective, causes or consequences of inefficiency and loss of productivity.

Barker and Downing draw attention to the managerialist ideology within which the value of word processors was located and to the likely de-skilling (loss of shorthand, for example) and fragmentation of labour relations (typists will no longer work for a principal or a section but have work allocated on the basis of availability). No doubt this will also lead to standardisation (of document production) which is another (desired) characteristic of automated systems By de-skilling document production, it will be possible to reduce costs and increase productivity.

Once again, the logic of the account bears no relation to a nomological machine. The *ideology* of automation fits the requirements of late capitalism. In any particular case or firm, there is no traceable path from the crisis of capital through to the introduction of word processors. Instead, we have "symmetrical interaction" (to borrow Wynne's phrase). The word processor and its impact on office labour relations, represents an instance of how the crisis of Capitalism is to be described. Together they gain their significance simply in virtue of the fact that they are such an instance. They fit together; and the fitting goes both ways.

Whatever else one might want to say about these two examples of the development of science and technological innovation, they are fairly representative of the approach taken by the Sociology of Science, at least in its early to middle period. It was this style that ANT reacted against. However, if they are fair representatives, then it follows that the Sociology of Science does not actually produce nomological machines. It also follows that it is not Durkheimian in the sense we mean, even if it did mostly talk about itself as if it were. ANT might want to reject the Sociology of Science, but can hardly do so on the grounds that it produced plausible causal accounts that were in fact confidence tricks, if it was not producing causal accounts (plausible or otherwise) in the first place. In as much as ANT defines itself in opposition to the Sociology of Science's Durkheimianism, that self definition appears to rest on a mistake.

Thus ANT takes the first wrong turn.

AGENCY AND PRACTICE

If ANT rejects Durkheimian Sociology, what does it propose to put in its place? The answer is deceptively simple to summarise. ANT replaces causal descriptions with descriptions of the ways that social actors create scientific theory and technological innovation. It describes the practice (or practices) of science and technology.[28] However, in formulating these descriptions, ANT makes three moves which are crucial. First, the category 'social actor' is extended beyond the human domain to include the material. The constituents of the social world and the material world both have the capacity to act (or *agency*). This is how Andrew Pickering describes what is meant by this.

> *The world, I want to say, is continually doing things, things that bear upon us not as observation statements upon disembodied intellects but as forces upon material beings. Think of the weather. Winds, storms, droughts, floods, heat and cold - all of these engage with our bodies as well as our minds, often in life threatening ways. (Pickering 1995 p 6)*

Second, there is no attempt to account for one element in the category (material objects, say) in terms of the other (social objects). ANT descriptions are committed to analytic levelling. Third, the descriptions draw out the interrelationships among actors as they move together to form networks. Such interrelationships are transacted through *mediators*. Acting together through mediators, actors-in-networks create scientific and technological innovation and development. The modes of acting together are either the domain's *practice* or are constituted by its *practices*.

To demonstrate what this looks like in an actual case, we could pick any one from the vast array of ANT studies (from scallops to hinges, from salmon to electric cars). We will use Pickering's (1995) account of the development of the Bubble Chamber which, for a while, became the key tool in elementary particle physics. We use it simply because it is foursquare in classic Sociology of Science territory and because Pickering deliberately simplifies the description of the science carried out so that the main themes he wants to emphasise can easily be discerned.

The interrelationships among the actors in Pickering's descriptions are characterised as resistance and accommodation. Human actors and material actors resist and accommodate one another in and through the practice of science. This 'dialectic' of resistance and accommodation, Pickering calls *the mangle of practice*.

The story of the Bubble Chamber although technically complex is quite simple to tell. Donald Glaser, then a relatively junior member of the scientific community, set himself the task of resolving a major issue in Particle Physics, namely the development of a process to capture "strange particles". Over the next few years, Glaser tried many different approaches and set ups without overwhelming success. Once he published his initial results and conjectured why it might be so difficult to achieve his goal, other scientists began to join in. Eventually, having changed his theoretical framework and adopted a different style of technology development, Glaser was successful in developing a working and effective chamber. This is how Pickering summarises the story.

> *My suggestion is that we should understand the history of the bubble chamber as a more-or-less violent tuning process involving the continual reconfiguration of material setups in the pursuit of an intended capture of*

[28] This difference matters a great deal to ANT members. Pickering, as we will see insists he is interested in the practice of science. Law, on the other hand, stresses professional and organisational practices.

> *material agency. This process was itself organised as a dance of human and material agency. (Picking 1995 p 51)*

This 'tuning' consisted in a serial process of Glaser adjusting and revising his rigs and then stepping back to watch what would happen, what the rig would do. Thus both Glaser and the material world (of rigs, elementary particles, etc) both acted upon each other in the 'dance of agency'.

When reading Pickering's description of Glaser's travails as he struggled to build the bubble chamber, it all seems very familiar. Or, at least, the process looks familiar even if the technical details are arcane. Anyone who has every tried to build anything from scratch, be it a go-kart or a racing car, knows the process of trial and error, test, fail, re-test and re-design. Pickering couches this familiar process in the vocabulary of practice, agency, perfomativity and dance. Why? Why choose that way of talking? What is this vocabulary doing for him? Interestingly, although this interactional dance is held to be going on, Pickering does not treat Glaser's acting upon his equipment and the consequent states of the equipment as formally identical. His account presupposes that Glaser is doing things to the equipment but not that the equipment is doing things to him.

The first answer seems to be that the vocabulary has been chosen for its startle effect.[29] Generating a startle effect is a standard pedagogic technique in social science (and especially entry-level) courses. Take a general process or institution with which the group is relatively familiar and cast it in some odd way. Modern medical practice seen as divination and witchcraft is a popular one. The purpose is not to get novices to stop going to the doctor for their ailments but to ask them to look with fresh eyes at how the social institution of medicine is organised. The scientific authority of medicine is, thereby, set aside for the moment. By talking of agency, dances and the like, Pickering is trying to achieve the same end. He wants us to set aside both the scientifically authorised accounts of what is going on and the sociologically authorised versions too; the latter being the accounts that the rest of Sociology of Science produces.

The reason he wants these versions set aside is that he wants to enrol us on his side in a metaphysical argument with both science and the Sociology of Science. The metaphysics he wishes to defend is one is premised in ontological plasticity. According to this view how the world is a product of how we construct it. For the Sociology of Science, the process of science is one of social shaping. Within science the usual view is one of the discovery of a pre-given, passive, there to be discovered world. Pickering wants to argue with both. The Sociology of Science is wrong in seeking to reduce the metaphysics of science to social forces. Science is wrong in not providing for the agency of the material world.

It is not so much the argument with Sociology of Science (after all it is just another collection of sociologists) as the argument with science that is important. For Pickering to be right, science (Glaser in the bubble chamber example) has to be wrong. The descriptions science gives of the nature of the discovery or invention are faulty. ANT, it seems, then, is predicated on arguing with (some of) its data. This is where ANT's second wrong turn is made.

This odd stance is not unique to Pickering. Here is a passage from John Law. It is from a discussion of what he calls "collateral realities". For Law, the appreciation of collateral realities contrasts with how ordinary members of our society view the world; what he calls 'Euro-American commonsense realism'.

> *So what is 'Euro-American common-sense realism'? There are whole libraries on this, but here is a gesture. First it tells us – it assumes – that there is* ***a reality out there****. Second it tells us that whatever is out there is largely* ***independent of*** *our actions. (A qualification: it is obvious that our actions sometimes influence reality). Third, it tells us that whatever is out*

[29] This is a familiar ANT strategy.

> *there substantially **precedes** our actions or attempts to know it. Fourth, it assumes that whatever is out there is **definite** in form. Fifth, it takes it for granted that there is a single reality, that it is **singular**. And sixth, probably (perhaps less certainly) it assumes this reality to be **coherent**.*
>
> *We may debate the specificities, but if we take performativity seriously then most of these assumptions need to be undone. Only a stripped-down version of the first (call this 'primitive out-thereness') remains. If we think performatively, then reality is not assumed to be independent, priori, definite, singular or coherent. Rather the logic is turned upside down. If reality **appears** (as it usually does) to be independent, prior, definite, singular or coherent then this is because it is being **done** that way. Indeed these attributes or assumptions become examples, amongst others, of collateral realities. (Law 2009 p1 emphasis in original)*

Just in case we should think that this is meant as a purely investigative stipulation, a way of teasing out aspects or phenomena worthy of interest, Law goes on as follows:

> *But what is it, 'to do'? Where are the collateral realities being done? The response is that they are done in **practices**. Practices enact realities including collateral realities. This means that if we want to understand how realities are done or to explore their politics, then we have to attend carefully to practices and ask how they work. Libraries have been written on this topic too, so I simply offer another gesture. For my purposes, practices are **detectable and somewhat ordered sets of material-semiotic relations**. To study practices is therefore to undertake the analytical and empirical task of exploring possible patterns of relations, and how it is that these get assembled in particular locations. It is to treat the real as whatever it is that is being assembled, materially and semiotically in a scene of analytical interest. Realities, objects, subjects, materials and meanings, whatever form they take these are all explored as an effect of the relations that are assembling and doing them. Practices then, **are** assemblages of relations. Those assemblages **do** realities. Realities, including the incidental collateral realities, **are inseparable from the patterning juxtapositions of practices**.*
>
> *There is an immediate methodological consequence. We need to proceed **empirically**. If we are to do philosophy, metaphysics, politics, or explore the character of knowledge, we cannot do this in the abstract. We cannot work 'in general', because there is no 'in general'. All there is are: specific sites and their practices, and then the specificities of those practices. So philosophy becomes empirical (Law, Op. Cit. 1-2 emphasis in original).*

Notice the by now very familiar moves. An interest in the social organisation (or assembly) of science (or policy development in Law's case) posits a particular ontological structure for the social world; one of human and material agency and performativity. But this structure requires a different metaphysics to motivate it to those of both science and ordinary understanding. This new metaphysics, the world of collateral realities, must therefore replace the world of scientific and commonsense realism and the *analytic practices* which sustain them. We must move to a new set of analytic practices, most critically one in which Philosophy (or at least that body of modern Philosophy which underpins both science and commonsense) becomes empirical. The argument is now with "modernism" itself.

Thus a third wrong turning is taken.

IN WAND'RING MAZES LOST[30]

We have argued that slippery step by slippery step the conceptual development of ANT (its logical path, so to speak) involves a slide into using Sociology to do Philosophy. What began as a reasonable set of sociological presuppositions and pre-occupations has evolved into a cross-disciplinary imperialistic campaign. We have tried to show this journey through (some of) ANT's writings. Quite recently, in a broad ranging autobiographical piece (Latour 2010), Bruno Latour reveals that this has been his objective, if not since the beginning, certainly for some considerable time. He marks the realisation that ANT has to confront "the moderns" (as he calls them) as coinciding with his becoming unwittingly embroiled in the "science wars" of the 1970s. However, it was only later, after further field studies, that he was able to formulate just what the objective should be.

> *What was clear to me, at least, was that the two master narratives of 'nature' and 'society' with which modernism had built what I called its Constitution, have always been only the most superficial part of what had happened to them. Something else had happened that required a double-edged critique of Nature and Society. For criticizing the latter, I had to delve into social theory and to propose, with Michel Callon, under the horrible name of actor network theory, an alternative possibility – which, I later discovered, had actually been entertained by Gabriel Tarde at the beginning of sociology. For the former, that is nature, the task was much more complicated, since it meant a rethinking of much philosophy, and, as I discovered completely by surprise, of politics as well. (Latour 2010 p 603)*

In just what does this re-thinking consist? Here is Latour's summary.

> *The modernist parenthesis, opened at the time of Locke, begun with a new role given to primary qualities (the stuff out of which the objective world is made) and to the secondary qualities (the subjective values that the mind adds to it – 'psychic additions' is Whitehead's term for it). This distribution of roles has become, over the three centuries of modernism, such an entrenched prejudice that every single official category depends on it and, most of all, the sacrosanct distinction between facts and values. And yet, it is a recent and a very baroque invention that takes not a very long empirical inquiry to contest. If, in the eyes of Whitehead, William James had put an end to the modernist parenthesis (to what he calls the 'Bifurcation of Nature' (Whitehead, 1920), it is because James had made a shambles of the distinction between primary and secondary qualities. Relations are not what is added to a world of meaningless matters of fact, but what are empirically given in the world of experience. 'Nature' might be made of primary qualities, but not the pluriverse, to use James' term for a world freed from being defined by only one mode (James, 1996 [1909]). To be sure, in 'nature', it is very difficult to give an ontological status to all the other entities on my list*

[30] Others apart sat on a hill retired,
In thoughts more elevate, and reasoned high
Of Providence, foreknowledge, will, and fate,
Fixed fate, free will, foreknowledge absolute,
And found no end, in wand'ring mazes lost.
(Milton, Paradise Lost Book II)

> *– they are to be treated at best as 'language games', at worst as pure fantasy – but in the pluriverse there is plenty of room for other modes of existence, each with its own key. (Latour Op. Cit. p 604)*

Putting the very broad brush summary of philosophical debate to one side, we want to draw attention to just one (telling) phrase. This is the observation that the universal distinctions between primary and secondary qualities, or between facts and values, take "not a very long empirical inquiry to contest". This contesting amounts to no more than the proposition that what Latour defines as a corner stone of modernism, was the invention of a particular culture at a particular point in its history. This finding is an outcome of what Latour calls 'The Anthropology of Modes of Existence', and is what he claims what his life's work really amounts to.

Our question about this proposition is simple one: *what difference does this 'finding' make to Philosophy?* Of course, like everyone else philosophers are narcissistic to some extent and interested in the history of their own discipline and the relationship of modes of inquiry to social contexts. But Philosophy is not about its own historiography. What philosophers are concerned with is the *logic* of arguments and the structural coherence and consistency of programmes of argument. To show that modernism is flawed *as Philosophy*, one has to show that its logic is flawed, not that it was created in a specific set of circumstances by a specific set of people. Of course one can point out that they *chose* certain presuppositions and suggest reasons (both laudable and otherwise) why these pre-suppositions were attractive to them. But to defeat the Philosophy *as Philosophy*, it is necessary to show that the arguments built from these pre-suppositions are inconsistent, incoherent or otherwise violate the conventions of philosophical argumentation. Contesting the pre-suppositions by pointing out they could have been different (and perhaps should have been) is as helpful to philosophising as the apocryphal Irishman's response to the request for directions ('If I was going there, I wouldn't start from here').

This is the fourth wrong move. In formulating his critique in the way he has, Latour passes modernist philosophy by. It has no philosophical interest in his points; and his points can get no grip on its concerns. The result is not so much a dialogue of the deaf as a one-sided argument. The outcome is that ANT finds itself stumbling around seizing on almost any issue that comes to hand to shout its objections. Such megaphone debating is hardly likely to be effective, especially if one's antagonist isn't listening.

To (Re-)Begin at the Beginning

We have said that ANT begins in the right place with the right problem. It is just that everything goes awry after that. What is this place? And what is this problem? The place is the adoption of the *sociological attitude* towards science. When we say this is *the right place* we do not mean it is the only place from which to start, simply that adopting the sociological attitude towards social phenomena is the right place to start in Sociology. This might sound odd, not to say a truism, but, all too often what purports to be sociological analysis starts from an entirely different place, mostly a political or ideological one. That is, the sociologising begins (and aims to end) with an account of just why and how some set of social practices are exploitative, undemocratic, repressive, or whatever. ANT did not do this, and so we say it starts in the right place.

It also has the right problem. What we mean here is that it wants to start as an *empirical* discipline and *observe* the institution of science. It wants to take the practice(s) of science as its topic. Its accounts of how science is, are to be based on direct observation of what scientists do, not on *post-hoc* reconstructions by either the scientists themselves or by others of what must have happened or could have happened. This means ANT wants to root its studies in the actual places where science gets done; laboratories, field sites, and so on. Of course, in as much as certain orders of *post-hoc* reconstruction are essential to science's practice, the practices of producing these re-constructions will themselves be part of ANT's topic matter.

Finally, this focus in right place and on the right problem are to be couched in *sociological descriptions* of the ordinary character of science's daily work. The purpose is to describe the social practice of science and not to explain it, let alone explain it away.

A great deal of excellent work adopting these principles has been carried out in science and related areas (Lynch (1997), and Livingston (1986) are foremost examples). It shows it can be done. It also shows that such work does not have to follow the path that ANT has followed (or slid down). None of these studies ends up arguing with the science they study, or the scientists and their defenders/promoters. None of these studies see themselves on a crusade to correct the (wilful?) ignorance of their data. ANT only arrives at these positions because it throws caution to the winds and seeks to substitute sociological modes of analysis for scientific and philosophical ones. When both science and philosophy fail to be impressed or even interested in this substitution, all Latour and ANT can do is raise the pitch and volume of their imprecations. This is what leads to frenzied proclamations with which we began this essay.