PostModernism, Technology and Social Science

R.J. Anderson and W. W. Sharrock

TABLE OF CONTENTS

PREFACE

The questions we take up in these essays have part of their origin more than twenty five years ago when we were investigating ways of using Sociology as part of the design process for modern technologies especially computer applications and computational systems. Although there was enthusiasm on all sides, just exactly what Sociology could offer design was not all that clear. Even if we could have decided what might be offered, it was equally unclear how the exchange might be effected. Since that time, significant progress has been made in adapting some fieldwork techniques such as participant observation and some reporting methods such as ethnography to design need, but the alignment of sociological theory and design specification remains intractable. Nonetheless, throughout the intervening period, designers and especially members of the HCI research community have continued to advocate incorporation of forms of social and sociological theory into design but with very little substantive success.

As a result of our own experience and watching the efforts of others, we began to reflect on the relationships between disciplines and the possibility of a hybrid Engineering or Design Sociology. The work of developing those ideas is still uncompleted. The essays collected here focus solely on various aspects of the relationships among disciplines and some of the requirements for ensuring those relationships are well grounded. Whilst, much has been said about the virtues of multi-, inter- and, more recently, post-disciplinary research, it is our view that the transactions between disciplines which each of these mandates, will have to have a much firmer basis than simply the wish to borrow terms, concepts or approaches if any of them are to be fruitful and sustainable. In their different ways, these essays explore what such a basis might entail.

A second theme weaves its way through these essays; postmodernism and its consequences. Whilst it is certainly true that the high tide of postmodernism in the social and human sciences has ebbed, nonetheless issues and topics, and particularly ways of framing issues and topics, which were popularised when postmodernism was in full flood are still firmly embedded in social scientific analysis. Consequently, when others turn to the social sciences and especially to Sociology for

insights, often what they take up are characterisations of questions or problems issuing from the analysis of postmodernity. Overwhelmingly, such characterisations are far from helpful (or even plausible) with the problems alleged to have been discovered by postmodernist analysis scaled out of all proportion to their likely consequences.

The forms of opposition to modernity taken up the various positions discussed in these essays provides a third theme. Simple opposition to a way of thinking is not, of itself, sufficient to overcome the assumptions that underpin that way of thought. More often than not, those that seek to overthrow modes of theorising unwittingly adopt the self same pre-suppositions as part of their own arguments.

Three conjunctions in particular display the entanglement of postmodernism, oppositional critique and cross disciplinary relationships. They are: the ambitions of Actor Network Theory to set the social sciences and Philosophy on a new path; the promotion of Sociology as the means by which the assessment of moral or ethical value might be incorporated into the design of computational systems; and the implications of reflexivity for scientific (and sociological) method. A number of these essays take up the difficulties we see in all these proposals. Alongside them, we have put consideration of the incorporation of postmodernism within HCI and postmodernist theorising of the social significance of recent advances in digital technologies. Each illustrates the danger of enthusiasm and excitement regarding the 'radical implications' of some social theory blinding us to that theory's limitations and defects. Without careful appraisal of what a theory offers and how it might be put to advantage, we will, in all likelihood, end by violating Polonious' sage advice:

> *Neither a borrower nor a lender be;*
> *For loan oft loses both itself and friend,*
> *And borrowing dulls the edge of husbandry.*

We realise, of course, our occasionally severe criticisms of some the positions adopted by sociologists and some of the sociological accounts incorporated into neighbouring disciplines might give a the impression that we have a very negative view of sociology. In fact, this is not so. Where we do differ from many of our colleagues is in believing that although is it is usual for the discipline to present an external face which depicts coherence and unity, and to speak of 'the sociological approach' and even characterising the adoption of such an approach as 'a turn to the social', in reality there is neither deep nor extensive consensus on what Sociology is or might aspire to, nor on what doing it properly actually means. The many views on these matters all contest with one another. From the inside, Sociology appears to be divided into a variety of wholly heterogeneous positions which themselves feature a plurality of ways of defining their own identity (one has only to think of the enormous, and still expanding, number of 'Marxisms'). At all levels, these positions are commonly quite hostile towards and even dismissive of each other. What makes us different, perhaps even maverick, is our conviction that the diversified and hugely contentious nature of Sociology should be kept in the forefront of one's mind when considering claims made about the discipline and what it needs, and even more so when such claims also contain recommendations for what Sociology can offer others. For us, it seems that far too often sociologists confidently assert a *droit de seigneur* for their own chosen version of Sociology, thereby demonstrating no more confidence in as a unified and integrated whole than we do. The only difference between such views and our own is that whilst they hold than all but one or two sociological schemes contain fundamental and unresolved difficulties, we think they are all like this.

In many respects, the state of Sociology today is reminiscent of the description Thomas Kuhn gave of the early stages of some of the most prominent of the natural sciences. At the beginning, while there were areas of common interest, the whole field was divided among numerous, inconclusively disputing schools. Everyone's idea of how to advance the field was to tear down all existing schemes and start again. Kuhn did not suggest that this state of affairs was *necessary* for the sciences in question to mature in the way they have, and we do not imagine it must be necessary in Sociology either. We mention it only because Kuhn emphasises two features he thinks are typical of an immature discipline and which in the essays below we find also to be typical of Sociology. Because of the endless striving to start anew, the enthusiasts for any position have a tendency to exaggerate the extent that their doctrines are actually different from their predecessors and to understate the degree to which their proposals perpetuate rather than resolve the unresolved difficulties of those approaches they wish to overthrow. In as much as sociological schools tend to be overly indulgent to those who share their views and harshly uncharitable to those who don't, as we point out in Essay One *ambivalence* is almost the discipline's most characteristic feature. Moreover, this lack of charity extends to the views encapsulated by other scientific and scholarly disciplines as well as those more generally current in society.

In short, we suggest that it is almost always wise to take a cautious view of the advertising offered by sociological approaches on their own behalf. What you see is rarely what you get. Certainly, despite the self confidence of the assertions, it is by no means clear whether (a) there is a need to adopt any particular approach to obtain the effects it promises (equivalent effects are often on offer from other, no less sociological sources); and (b) the adoption of the approach in question (or any of its alternatives) can guarantee the realisation of its promised results.

So, Sociology is a domain of large ambitions and comparatively modest achievements with much of its work approaching more the promulgation of manifestos than it does the making and recording of solid research achievements. A dispassionate review of sociology's schemes would take them to be budgets of problems rather than catalogues of solutions. As a consequence, those who attempt to import Sociology into their own discipline are likely to find themselves embroiled in the inconclusive hostilities mentioned earlier rather than the recipients of robust and fully articulated solutions to the problems they hoped would be addressed.

Accepting and admitting all this does not imply hostility to Sociology. After all, the insistence of so many sociologists that hitherto all Sociology has been in error does not discourage them from continuing to practice Sociology or enthusiastically promoting its benefits to others. Neither does it faze us, having as we do only modest expectations for the discipline. The fact that the treatment of certain orders of problems has been somewhat clumsy or ill conceived does not mean we should not try to give them better and more thorough attention. However, what it does mean is that we should be sceptical of Sociology's ability to provide advice to others regarding the intellectual or even life worries that preoccupy them, when as its record all too clearly shows, it is hardly adept at dealing with its own.

Engagement with Sociology, then, requires an appetite for or at least tolerance of complicated, often confused, protracted, endlessly shifting and inconclusive controversies. Whatever might be said to the contrary, what is at stake is almost always *first principles*. Even when everyone within a particular school accepts the first principles have been agreed, as our earlier reference to manifesto production hints, there appear to be inevitable problems of follow-through. Sociologists, of course, recognise these difficulties and routinely use them to explain to each other why what was promised could not be delivered.

An image that we recurs throughout these essays is that of cross border trading between disciplines. Building on that image, we want to say the difficulties we refer to appear not in Sociology's marketing materials but in the small print of the contracts governing the exchange. Just as in other realms, anyone considering engaging in trade with Sociology should be sceptical about the claims made in the sales pitch and look at the small print to assure themselves they understand exactly what they are likely to be getting. This means having to work hard to unravel complex, tangled and uncertain issues. There is no doubt that such close inspection will often reveal that advances being claimed for one kind of Sociology over another come by begging questions rather than answering them. Certainly, the prospective importer of Sociology cannot depend on the promotional materials for a careful, accurate and balanced assessment of the similarities to and differences between its own and rivals' capabilities. Similarities, commonalities and overlaps are often hidden by the use of what on the surface appears to be distinctive terminologies. In our view, there is both more and less to be said about the various approaches to Sociology than they usually say about themselves. There is usually more that should be said than their detractors allow; and less than their enthusiasts would have us believe.

The almost congenital contentiousness of Sociology is a first feature which those coming to it should be warned about. A second is that, despite its diversity, in large part it remains very much *a heritage discipline*. More than half a century ago, impatient of Sociology's then current state, Robert Merton approvingly quoted Alfred North Whitehead's apothegm that a science that hesitates to forget its founders is lost. Merton wanted the discipline to move on from scholarly consideration of its founding fathers and to become an empirical, accumulating science. No doubt today Merton would be even more impatient since his call has hardly been heard let alone responded to. In the squabbles among sociologies, it is almost a *sine qua non* to locate the first principles for which one is arguing in the thought of one or other of the historic greats. Of course, the stock of any of the founding fathers rises and falls with fashion (early 20th century Pragmatism, for example, having had something of a revival of late). We take this broadly to imply, though we wouldn't want to over play it, that Sociology is perhaps best seen as *scholarly pursuit* immersed in permanently re-thinking its relationship to its intellectual heritage wearing the guise of a research discipline. It is only if one accepts what is, perhaps, the dominant myth of academic life namely that research matters more than scholarship that this could be construed as a fault or even failure.

Of course, the concern with heritage is but one aspect of the preoccupation with first principles. By and large, sociological approaches adopt the view that *if* first principles can be sorted out *then* any operational difficulties can be resolved later, which, of course, means that such difficulties get little or no, and certainly no sustained, attention. Not all that long ago, James A. Davis (1994) answered the question "What is wrong with Sociology" by contrasting two kinds of questions; ones which sociologists could answer relatively easily and ones they couldn't. The main difference between these two as far as he could tell was that sociologists were only interested in the latter. The result is that it is a characteristic of sociological research reports (one which, incidentally it shares with many other disciplines) almost always to summarise the outcome of research as the call for yet more research. Rarely, if at all, do researches suggest that some topic or issue has, to all reasonable intents and purposes, been disposed of. The engine of this reproductive research process is the problem of matching data to issue. Overwhelmingly, the data used in sociological research is indirect or second order with the phenomena which the data are actually data of standing proxy for the issue said to be under investigation. This situation is not new; nor has it recently been discovered. The same concerns have been raised again and again throughout the discipline's history. Sociologists very well understand the difficulty of tying data to phenomena. It is just that it has come to be viewed not as a problem to be solved but rather as a condition to be

lived with. The end result, and this is really Davis' point, is that empirical researches undertaken in Sociology are just as inconclusive as the disputes over first principles in whose service they have been constructed. Here is Davis' pithy analysis.

> *What is wrong is that Sociology is incoherent. It does not cohere ("to stick together; be united; hold fast, as parts of the same mass"). While each article/book/course may be well crafted, they have little or nothing to do with each other. They may share methods and even data sets (and grammatical voices so passive as to suggest a drug problem), but each is about a unique problem with a unique set of variables.*
>
> *Try this test: list the key concepts/variables in each article of in the last two or three issues of the American Journal of Sociology, American Sociological Review, or Social Forces. I expect the number of different variables will be at least 20 times the number of articles and few variables (save for a handful of demographics such age, sex and race) will turn up in more than one article.*
>
> *Another indicator: List the major subfields of sociology. Then try to arrange them in some pattern that has more intellectual bite than alphabetization. Hard, isn't it?*
>
> *Yet another: Why are there no conflicts over priority in Sociology? Because sociologists are nice? Nope. Because no two sociologists ever study the same thing so such conflicts are impossible (Davis, op cit, p 180).*

Naturally enough, Davis' diagnosis is based in own preferred style of Sociology, one whose versions are usually lumped together as 'Variable Analysis'. But his depiction is not *parti pris*. Like us, he finds Sociology to be diffuse, personalised and resource stretched. Sociology is deployed on just those problems which individual sociologists finds interesting or challenging with theories, methods and materials being combined in whatever ways the particular researchers thinks suits their purposes. Not only do sociologists study different things but the ways they do their sociologising are largely idiosyncratic too. Resources are not concentrated on a few key topics but spread thinly, with it being rare for more than one sociologist to work on a specific defined topic or, indeed, to work on it for any sustained period.

Add to all this the fact that data collection is time consuming affair. Replicated samples and data sets are difficult or too expensive to obtain and data cleansing and re-design in response to emerging issues impossible. The personal and financial costs of such re-work would be wholly out of kilter with the robustness of the data, being as it is the outcome of just one person's work. Even the largest teams consist in a small number of members with many of these being students for whom this will be the first time they have been asked to gather data 'for real'. Quite recently, many in British Sociology have been exercised (indeed, even in a panic) about sociology's ability to compete with the data collecting, collation and analysis capabilities of large 'data mining' companies (Savage and Burrows 2007). But this is to forget that the data sets used in the past by Sociology were often drawn from data bases created for administrative purposes and made available to the researchers without them having a clear understanding of the procedures that were used to collect and code the data and therefore of the relationships between such data and the phenomena the sociologists wished to study. This situation is only made worse when, in the face of the rising costs of large scale data gathering, researchers are pressured to re-purpose data

collected by others. Poor data with unknown provenance is fast becoming our research stock-in-trade.

Davis is unhappy because Sociology is inchoate, contentious, unfocussed, idiosyncratic, endlessly proliferating and not the integrated body of empirical, scientific knowledge servicing the engineering of social life he would like it to be. We don't have such visions for the discipline and so what he finds to be weaknesses, we find simply to be characteristic features. In our view, adding more resources and hiring more people to undertake Sociology will not change these things, only a scaling back of ambition and sustained attention to addressing their causes can do that. Sociologists know they face these problems; they are neither naïve nor secretive about them. Indeed, given that so much of the published sociological literature consists of texts about and commentaries on theory and method, they could hardly be. However, they are taken to be the inevitable requirements of doing sociological business-as-usual and so both largely ignored and relegated to the disciplinary small print. In the main, Sociology proceeds by using its own version of the hand waving "small matter of programming" injunction when considering how to overcome the conceptual and methodological difficulties which confront implementation. We accept it may be comforting for the anxious to be re-assured that a journey of a thousand steps begins with just one, the first step. But to complete the journey, 999 more steps need to be taken and having taken the first is no guarantee that all the rest (or even *any of* them) will be. It is easier, more entertaining and much, much more profitable to set forth a plan for an adventurous new approach to Sociology than to wrestle with the catalogue of stubborn, tedious and very, very tough problems which need to be dealt with in delivering any of the approaches we already have. Plaudits and career progression tend not to follow the deep, prolonged and largely anonymous work required to make any one Sociology actually work.

The modern idiom is to talk of 'the academic marketplace' and to think of different disciplines as sets of products or services. Hence the language of promotion and marketing is not thought to be as foreign nor as inappropriate as once it would have seemed. The notion of an economy of academic disciplines opens up the question of how far the products actually do confirm to the claims made about them; how far, to adopt the famous slogan, any one of them "does what it says on the tin". In most cases, and certainly those with which we deal in these essays, what is on offer is less a tin containing a product of predictable quality and more a can of unpredictable worms. For ourselves, we don't mind this. We like opening cans of worms and find them to be challenging, interesting and enjoyable. We even think rummaging in the small print has its own rewards. We are not saying that what Sociology offers is not serious, thought provoking or important, because it can be. Though not as often, perhaps, as its proselytes would have us believe. However, getting embroiled in a fracas over open-ended, hugely contentious and quite likely irresolvable disputes is not necessarily what those who get invited to take the turn to the social were necessarily looking for.

****

Over the years, a number of people have helped us to see more clearly how to formulate our thinking with regard to technology and the social sciences. We would particularly like to thank Sara Bly, John Seely Brown, Graham Button, Johan Dekleer, Hervé Gallaire, Christian Greiffenhagen, John Hughes, Marina Jirotka, Mike Lynch, Tom Moran, William Newman, Judy and Gary Olson, Dave Randall, Tom Rodden, Kjeld Schmidt and the late Mark Weiser. None of these will agree with everything we have said in these essays. Some may not agree with anything we have said. But all have helped in one way or another.

We would also like to that Derek Mcauley and the Horizon Digital Research Programme for its support.

ESSAY 1

COLLABORATION OR COLONISATION?

Introduction

Nearly half a century ago, Doris Wilkinson (1968) complained about what she called Sociology's "imperialistic disposition". By this she meant the diversification ("proliferation" was the word she actually used) of sociological ideas and approaches into a range of ancillary disciplines and the promotion of sociological perspectives alongside and within those of the home discipline or disciplines. The list which she produced to exemplify this expansion ran all the way from the study of adolescence to that of war on the one hand, and from Existentialism to Sociometry on the other. No doubt, if the exercise were to be repeated today, many more topics and "sociologies" could be added.[1]

In Wilkinson's view, the reasons for Sociology's disposition to invade neighbouring and not so neighbouring territory, are likely to be complex and multiple. Some derive from a sense of inferiority and insecurity vis a vis the natural and mathematical sciences as well as those social sciences which seem to have copied the natural sciences successfully. Some derive from the proselytising tendency which exposure to sociological ideas seems to generate. And some derive from an increasing awareness on the part of Governments and other agencies (and therefore the research funding agencies which they sponsor) that successful policy intervention and management in modern society requires a great deal more understanding of the social context within which such

[1] Whilst sociological approaches do move in and out of fashion, they are rarely decisively eliminated from the discipline's practice. As a consequence, many of those on Wilkinson's list will, in all likelihood, have some support today.

interventions are made than was (or is?) usually the case. This combination of forces had created an ideology which served to legitimate the territorial expansionism outlined above.

Wilkinson's worry about this imperialist expansion was, first, that the forms of sociologising carried out under this rubric would lack methodological rigour, open-mindedness and intellectual curiosity. Second, she was concerned that in becoming thinly spread and consequently diffuse, Sociology would lose a sense of its own core, both in terms of sharpness of concepts and of academically-driven values.

Sometime later, Phil Strong (1979) repeated the charge of sociological imperialism, this time in connection with the domain of medicine and health. Here by mounting a counter challenge to what it designated medical imperialism, Sociology had sought to reduce the predominance of medical definitions of illness in explanations of epidemiological and other health phenomena, and to complement, if not replace, them with sociological ones. Whereas Wilkinson saw expansionism as essentially an academic issue, Strong saw it as a professional one; or, rather, an extension of the urge to professionalise such issues in late Bourgeois Capitalism.

Another take on much the same phenomenon is offered by an equally venerable sociological analysis: Robert Merton's (1976) account of 'sociological ambivalence'. For Merton, ambivalence is a characteristic state of occupants of most social statuses. Very few statuses are governed by simple and homogeneous norms. The institutions which they comprise are typified by normative heterogeneity and hence the possibility of normative conflict. As a consequence, when faced with the need to undertake courses of action, members of these institutions routinely find themselves motivated to follow alternative, sometimes conflicting norms which would lead to different, perhaps diametrically opposing actions. In such situations, actors are pulled in different directions by the norms which appear to apply to them and by their own different feelings and emotions.

Merton analysed this ambivalence ('sociological' because the frameworks within which it is applied are sociological) in such diverse settings as science, modern organisations and medicine itself. Within science, Merton identified a range of patterns of differing norms. Here are just a few.

> *The scientist must be ready to make his new found knowledge available to his peers as soon as possible.*
> *But: He must avoid undue tendency to rush into print.*
>
> *The scientist should not allow himself to be victimized by intellectual fads.*
> *But: He must remain flexible, receptive to promising new ideas and avoid becoming ossified under the guise of responsibly maintaining intellectual traditions.....*
>
> *The scientist must not advance claims to new knowledge until they are beyond reasonable dispute.*
> *But: He should defend his new ideas and findings, no matter how great the opposition....(Merton 1976 p. 33)*

Surprisingly, Merton did not turn his sociological eye (at least in this respect) upon Sociology itself. Had he done so, we feel he would have identified a form of ambivalence which underlies the predisposition to imperialism which Wilkinson and Strong refer to. This ambivalence is expressed in the attitudes which Sociology adopts towards other disciplines (and, more often than not, which

one species of Sociology adopt toward another). Using the same format as Merton (which, incidentally, he borrowed from Robert Linton) this ambivalence could be stated as follows:

> *The sociologist should be open to the ideas of different disciplines and seek to promote interdisciplinary understanding.*
> *But: must not accede analytic priority to another discipline and must always promote the primordiality of sociological accounts.*

The consequence of this attitude is a form of sociological argumentation which seeks to appropriate and recast the topics which other disciplines have specified for themselves. The net result has been the continuous expansion of the scope of the discipline bemoaned by Wilkinson and Strong. Once an area comes to Sociology's attention, the predisposition is to substitute sociological accounts for those of the local home discipline. The result is a combination of colonisation and conversion.

Disciplinary Relationships

Colonisation and conversion are a pretty fragile basis from which to build a lasting relationship. Moreover, as with all imperialisms, they run the risk of loss of legitimation and subsequent overthrow. We would like to believe there are other ways of relating across disciplines we could aspire to, ones which are compatible with co-mingling, mutual interaction and exchange, but which do not assume that either Sociology or the relevant discipline are automatically *improved* thereby. A *modus operandi* which had these as its values would only be possible, though, if it is regulated by a set of prescriptions governing transactions between disciplines. Such prescriptions would specify what sorts of things (eg ideas, theories, concepts, methods etc) can be moved backwards and forwards (and therefore what can't) and the protocols for doing so. It is, perhaps, just because it lacks any sense of such prescriptions that a discipline such as Sociology where ideas seem to move freely in both directions, can engage in free ranging imperialism.

To begin with, we have to recognise that the willingness to trade in the concepts, theories or methods of other disciplinary practices varies greatly from discipline to discipline. Some disciplines vigilantly maintain the internal and external integrity of their borders, as for example with Mirowski and Nik-Khah's (2007) response to Michel Callon's (2007) proposal that sociologists might help themselves to part of what is currently Economics. With others, the policing is much more lax. As we say, Sociology is among the latter. But, even where there are open borders, surely it seems reasonable to expect some controls should be in place, if only to ensure that the trade is carried on in an orderly way? Of course, it goes without saying that given the borders we have in mind are academic, these controls should be shaped on a presumption of intellectual accountability for the exchange being promoted.

Intellectual accountability is another way of talking about rigour. Those who propose cross-border infiltration, trading or expansion are accountable for the rigour with which they justify the case they make. Such rigour might be directed to demonstrating a number of things, but most importantly, what ought to be secured first and foremost are:

The provision of as much clarity as possible regarding the symmetry of the context of use of the concept, method, theory or whatever in its home domain with that to which it is applied. Symmetry (or any other kind of mapping) must not simply be assumed. This you might think of this as a *presumption of caution*

The detailed explication of how and how far the concept, theory, method will need to be adjusted or adapted for the context of its new use. Any conceptual re-badging and localisation will be made set out and explained. You might think of this as a *presumption of transparency*.

The point here is to ensure care is taken when translating ways of framing problems into the domains of other disciplines. Such translation is often carried out by enthusiasts eager to promote the benefits they imagine themselves and their approach to be bringing. We have long felt that something like the Trades Description Act should apply to academic disciplines. There is no doubt such an innovation this would have a major impact on parts of Sociology.

This notion of accountability implies that those who are urged to consider and endorse some form of disciplinary exchange can assume that the approaches they are being offered observe the transparency and cautionary principles. Disciplines where the borders are rigorously policed would have strong accountability. On the other hand, in those academic principalities with relatively open or permeable borders, the transparency and cautionary principles would be less highly prized or exercised and accountability consequentially weak. In weakly accountable disciplines, ideas, methods, practices are drawn into academic practice with little or no systematic thinking through of their provenance nor of what adopting them might entail, thereby reducing the plausibility of the arguments put forth. It because Sociology is so weakly accountable in this sense that the imperialistic tendencies identified by Wilkinson and Strong are so suspect. However, it is not just that the discipline might muscle out alternatives, or lose its own core. An ever expanding Sociology will also be a Sociology not worth the having. It will be a discipline lacking any sense of intellectual rigour or respect for the rigour of others and perennially in pursuit of the new.

To illustrate what we mean by this, we will examine in detail a set of proposals which were recently made for the development of a new approach to the analysis of the domain of human-computer interaction (HCI) based upon a postmodernist social theory.

HCI and Cultural Theory

A number of prominent members of the HCI community (for example, Satchell (2006 & 2008), Sengers et al (2005), Sengers, McCarthy and Dourish (2006), DiSalvo (2009)) have advocated the deployment of concepts drawn from Critical Theory, Cultural Theory, or postmodernism more generally, within HCI.[2] The intention behind this advocacy is to broaden or deepen the range of aspects of the social context of use to which designers might attend. Whilst we find this intended outcome laudable, we have considerable reservations about its potential consequences should a weak form of accountability be used its justification. In our view, unless shaped carefully, postmodernism is likely to create more of disruption than a positive contribution; more distraction than focused attention.

The rest of this essay will explain the basis for this unwelcoming assessment. We will begin by summarising what those who argue for the use of postmodernist concepts believe will be made available thereby. Second, we will give some detail on exactly what postmodernism says about communication and computational technologies. This will be important to gain a sense of the

[2] We will use the catchall term "postmodernism" to encompass Cultural theory and Critical Theory since these two are usually set within this broad umbrella in the thinking which the advocates draw upon. The extent to which they are, in fact, simply versions of each other, we will leave to others to determine. All we would suggest at this point is that those within each camp would, in all likelihood, vehemently dispute amalgamation.

symmetry between postmodernism and HCI. The appreciation of the basis for these observations will require a little excavating of the recent history of certain kinds of European social philosophy and social theory. Having drawn out the conceptual background, we will then compare the uses which those who invoke postmodernist ideas have put those ideas with the requirements of that framework. The results of this analysis will allow us to take a view on how far these advocates are from satisfying the cautionary and transparency principles we have suggested. Finally, drawing on the description we have given of postmodernism and its roots, we will offer an assessment of the relative fit between HCI as a profession and an applied research discipline and postmodernism as a mode of social analysis.

What Does Postmodernism Offer?

Christine Satchell is very clear what she thinks can be gained by the introduction of postmodernist thinking. Here is her summary of the ideas being made available.

> *Cultural theory emerges from many different disciplines and philosophies including social theory, anthropology, Marxism, feminism and language theory. It produces a rich social commentary that positions phenomena in light of the complex conditions in which they are embedded. In doing so, new ways of thinking about culture and what our interactions with it means are uncovered (Satchel 2008 p. 1593).*

Further on in the same paper, she outlines exactly which elements of the above she has in mind.

> *On a more specific level, there is the use of the individual components of cultural theory within HCI such as Marxism, feminism, semiotics and hermeneutics...(ibid. p. 1594)*

For Sengers, the list is much the same.

> *Our perspective on reflection is grounded in critical theory, a Western tradition of critical reflection embodied in various intellectual strands including Marxism, feminism, racial and ethnic studies, media studies and psychoanalysis. (Sengers et al. 2005 p 50)*

She goes on to clarify what these approaches have in common.

> *Critical theory argues that our everyday values, practices, perspectives, and sense of agency and self are strongly shaped by forces and agendas of which we are normally unaware, such as the politics of race, gender, and economics. Critical reflection provides a means to gain some awareness of such forces as a first step toward possible change. (ibid, p.50)*

In her view, then, the implications for HCI are of vital import.

> *HCI as an intellectual field shapes what we as practitioners believe is technically feasible and desirable, while sometimes blinding us to other possibilities. Critical reflection on the limitations of the field's methods and metaphors can help us to see the world in a new way, identifying and weighing new technical possibilities.*

> *But given critical theory's emphasis on critical reflection as an essential tool to allow people to make conscious value choices in their attitudes and practices, the value of reflection for HCI goes beyond simply opening new options for designers. It can support new awareness and freedom for users as well. We believe that, for those concerned about the social implications of the technologies we build, reflection itself should be a core technology design outcome for HCI. That is to say, technology design practices should support both designers and users in ongoing critical reflection about technology and its relationship to human life. (ibid p 50 emphasis in original)*

What Satchell and Sengers see, then, is both the possibility of systematic reflection on what, for shorthand, we can call "designer practice" and, related to that, a powerful contribution to technologically-mediated interventions consequential upon design.

> *.....critical reflection is crucial to both individual freedom and our quality of life in society as a whole, since without it, we unthinkingly adopt attitudes, practices, values, and identities we might not consciously espouse. Additionally, reflection is not a purely cognitive activity, but is folded into all our ways of seeing and experiencing the world. Unconsciously held assumptions are not things we rationally know; they are part of our very identity and the ways we experience the world. Similarly, critical reflection does not just provide new facts; it opens opportunities to experience the world and oneself in a fundamentally different way. Even in mundane activities such as shaving one's legs, shopping for meat products, or navigating busy urban streets, critical awareness of feminism, factory farming, or racial issues alters our perception and interpretation of what is going on around us and the implications of our actions (Sengers op.cit,, emphasis in original)*

The means by which such reflection is standardised and systematised, or so we are told, is through the use of different "analytic frameworks".

> *We describe the organized ideas as 'frameworks' to be taken as generative themes and organizing questions as opposed to prescriptive directions or definitive classifications. The primary role of frameworks in this sense is not to uncover a ground truth about some phenomenon but to spark conversation about the organic development of a body of work. As such, the value or utility of the framework is in articulating a point of view that can be debated or engaged with, that opens up discussion and prompts new ways of thinking. (DiSalvo et al 2009, p 387)*

As we will see, this perspectivalism is a key part of the postmodernist analytic armoury, as is the notion of a 'conversation' across perspectives. Clearly, then, the frameworks and their components are being deployed not just for themselves but in so far as they inform, encourage, and facilitate challenge within the practice of design.

The offering of such rich descriptions and stimulation to reflection on presuppositions might be desirable, but the specification of the steps by which one moves from in-principled starting points to detailed, grounded and rigorous descriptions of phenomena that enable strong conclusions to be

drawn in specific cases needs to be set out as well. The test of a conceptual framework is the analytic results its enables not whether we feel good about the ways it allows us to talk about our phenomena.

Continually proposing fundamentally new beginnings is a characteristic of Sociology and the further line of justification suggested by DiSalvo et al, and Sengers, McCarthy & Dourish follows this pattern. Postmodernism is said to be offering a set of framing questions for the whole of design discipline of HCI rather than simply being a useful tool within any particular design activity. Their suggested framing questions are:

1. Values questions: discussion of the commitments designers do and should make in design.
2. Ontological questions: discussion of the character of research and practice in HCI.[3]
3. Questions concerning who are to be studied: in particular is user-centredness necessarily the leading term for design. How else might users feature in design?
4. Questions about the role of theory: what are the implications of adopting "theoretical lenses" from Cultural Theory, especially for the relationship between theory and practice.
5. Practical questions: how should the work be placed in relation to mainstream HCI?

What is on offer, then, is nothing short of a disciplinary shift to be achieved through the adoption of this set of framing questions. Naturally, the result of such a shift would hardly be HCI as we know it.

What is on view, then, are two very different visions of what could result from the introduction of postmodernist thought. We will term them 'strong' and 'weak' applications of postmodernism. The strong version proposes a set of questions shaped to ensure a wholesale shift in the form and practice of HCI as a design profession. The weak version offers a set of stimulus questions to provoke reflection on working assumptions, routines, models, definitions and so forth used in the practice of design. Whilst they are clearly related and have much in common, each merits separate treatment.

The weak version

There is something quite attractive and refreshing about the insistence that those who design and build artefacts, products, or technologies for others to use, should think long and carefully about what they are doing and seek to expose unrecognised predispositions and assumptions which might be embedded in the behaviours and patterns of use envisaged for the objects they design. In saying this, of course, no-one is saying anything as trite as ' designers should remember that not everyone is right handed', or 'designers should remember that some people are colour blind', or 'designers should remember that white middle class patterns of consumption are not universal', although these things should be remembered too. The insistence upon reflection seeks to delve much deeper. It wants to expose any unconscious reliance being placed upon the naturalness or presumed inevitability of particular forms of social organisation in the home, in the work-place, among friends and acquaintances, or in society in general.

Accepting that reflection is, or could be, a healthy practice, does not, of itself, commit us to accepting any particular basis for it. Nor does it indicate how extensive and unremitting, and thus *time consuming* such reflection is to be (if it is to be consistently applied then it will simply initiate an open ended regress which would be extraordinarily time consuming. How reflexive are the

[3] For ourselves, we would couch this as either a methodological or an epistemological question.

advocates of reflexivity prepared to be – will they follow Woolgar and Ashmore into perpetually chasing their tail, *shouldn't* they do so?). That is, it does not mandate the use of a definitive set of stimulus questions. Those who wish us to ask one set of questions (and, therefore, not other questions) have to demonstrate why these questions are the most pressing, the most relevant, the most interesting. In other words, they have to show that the questions pass muster for design.

There are probably just two explicatory routes open for such demonstration. Either the questions are of particular efficacy for design because they lead to improved or better (however you choose to measure that) designs; or *sui generis* they are *omni-relevant* and hence design too must address them; or, and this is perhaps more likely, that they are both.

Those who propose the weak version do not justify why the questions they seek to ask in design are, in fact, pertinent, let alone the most pertinent ones. Without such argument, all we are left with is the assertion that certain orders of question, about the politics of race, for example, or gender, age categories, experience and the like are self-evidently relevant no matter where or what the object under discussion might be. To repeat the quotation from Sengers we used above.

> *Even in mundane activities such as shaving one's legs, shopping for meat products, or navigating busy urban streets, critical awareness of feminism, factory farming, or racial issues alters our perception and interpretation of what is going on around us and the implications of our actions. (Sengers et al op. cit. p50).*

What is happening here is the importing (smuggling?) of a specific sociological theory4 into HCI. Accept this theory and certain questions becomee self evident and omni-relevant. Because no independent argument is made for the omni-relevance of such key issues, the weak version has to rely on the innovations it will enable in designed products for its justification. As we will see, this could well be a dicey strategy.

The strong version

The merit of strong postmodernism is that it does at least wear its expansionist or imperialist ambitions on its sleeve.

> *Although HCI researchers and practitioners have engaged with critical reflection on their discipline for a long time now, HCI still lacks a systematic critical agenda. Most of the social and human sciences develop a systematic critical and integrative strand as part of their research, practice and educational activities ..... So, as well as amplifying previous calls for the need for a critical-reflective stance in HCI, the main aim of this workshop will be to attempt to develop a systematic agenda for a critically reflective HCI, taking what is currently a set of interesting issues explored at CHI conferences into a vibrant and coherent program. (Sengers, McCarthy & Dourish 2009, p. 1683)*

This is echoed by DiSalvo et al

[4] We prefer the term 'sociological' to 'social' here because, when teased apart, this theory turn out to be a particular causal story about how relationships, institutions, and processes are determined by certain forms of social structure.

> *We will now turn to considering how arts discourse can provide a useful resource to the field of HCI at a meta-level in terms of how we define ourselves as a field. As a 'meta' level, the field of HCI has been present throughout this paper thus far. To call for alternatives in design and assessment is to some extent touch on changes at a more fundamental level. However, the history of HCI, and indeed of any field, is also laden with examples of borrowing from fields outside its border in an instrumental fashion where new methods are imported but existing methodologies remain intact.... What must be articulated then is how arts discourse can enter the HCI community in a profound way. (DiSalvo et al 2004 p 392).*

What both programs will motivate, of course, is the set of re-positioning questions outlined earlier. Moreover, whilst both Sengers, McCarthy & Dourish and DiSalvo et al are explicit about the scope of their proposed programme, they are equally forthright about the challenges posed by the appropriation of conceptions from other disciplines.

> *These positions include phenomenology ..., critical theory..., the work of Bakhtin ..., and cultural-historical activity theory .... Each of these theoretical positions is dense with values and traditions accrued over their own equally contentious histories. However, as they are integrated into the work of other disciplines, there is a danger that their rich histories and the subtleties of their practices are lost. (Sengers, McCarthy & Dourish, op. cit., p. 1684)*

We could not agree more. Given the complexities of all the positions identified, let alone the interactions among them, it is a major challenge to get them framed correctly. Translation of approaches shaped for very different disciplines may not be a straightforward or, in the end, helpful matter. In addition, we would suggest there is a further and possibly more important consideration, namely how far the way that HCI is being encouraged to deploy these conceptions actually aligns with its own core theoretical frames of reference. A strong version of postmodernism is only as strong as the strength of (a) its demonstration that the translation is a helpful one; and (b) that there is a reasonable mapping between theoretical and professional questions in HCI and those of the contributing disciplinary matrix. The extent to which any modification on either side of the translation is taken to be significant is, of course, a matter for debate. To satisfy these constraints in the ways we have suggested, strong postmodernism will need to show:

1. That the concepts are consistent with or at least compatible with each other and that their deployment in HCI is consistent with use in the domain from which they are taken.
2. That the arguments in support of the modifications required to fit this domain (i.e. HCI) are persuasive.
3. That when deployed, they provide greater traction on problems faced and insights required rather than simply replacing current questions with new ones.

The third question is, of course, bears upon HCI as a design profession.

Both the weak and the strong versions are clear about their wish to draw on conceptual resources from postmodernist theory in the social sciences. However, neither actually lays out exactly what kind of social science theory, postmodernist thinking espouses, nor do they estimate the "goodness of fit" between that kind of social science theorising and the *modus operandi* of professional HCI. In

one sense, this is not surprising since postmodernism is explicitly non-programmatic in character and itself has left unresolved the question of what kind of opposition to illicit authority there can be within the postmodern condition. Nonetheless, if Sengers, McCarthy & Dourish want to postmodernism to change the *modus operandi* of HCI then we would have thought they would want to set out exactly what that might mean. In the next section, we sketch the issues which would have to be addressed in making such a case. We are helped in this task by the fact that a great deal of postmodernist thinking has been devoted to the topic of the social and societal implications of technology and especially communication and computational technologies. So, it is around that theme that we will arrange our summary.

Postmodernism and Technology

Perhaps the most well known, or at least widely read in HCI circles, researcher who has drawn upon postmodern social science is Sherry Turkle. In her classic, *Life on the Screen*, (Turkle 1995) Turkle draws upon a number of lines of analysis prominent in postmodernist thinking. Towards the end of her discussion, she sets two of these into quite sharp relief; the issue of virtuality and the reality of virtual worlds and the issue of identity in such worlds. She ends by summarising the challenge which she felt we, as a society, were set by the technologies.

> *People can get lost in virtual worlds. Some are tempted to think of life in cyberspace as insignificant, as escape or meaningless diversion. It is not. Our experiences there are serious play. We belittle them at our risk. We must understand the dynamics of virtual experience both to forsee who might be in danger and to put these experiences to best use. Without a deep understanding of the many selves we express in the virtual we cannot use our experiences there to enrich the real. (Turkle, 1995 pp 268-69).*

More recently she has returned to these issues. Her book *Simulation and its Discontents* (Turkle 2009) explores the concerns that practising scientists (physicists and biologists) and designers (architects) now have over the use of model-based simulations in their disciplines. In doing so, she compares the original introduction through Project Athena of computational technologies at MIT to the current position there and elsewhere. Nowadays it is impossible to conceive of the disciplines mentioned being pursued without computational tools. Her conclusion is that the original worries expressed over Project Athena by "conservatives" are being realised. Or, rather, the same worries are being raised by professionals, this time based upon their experience with teaching successive cohorts of students to use such technologies.

What were these fears? In brief, they are:

1. Sets of skills required to carry on bench science or studio design are being lost.
2. Sets of values associated with the engineering of the models and simulations are replacing the values associated with the specific disciplines themselves.

Turkle claims that the scientists and designers she interviewed believe the use of simulation as *the* way of practising science and design has led practitioners (and not just students) to become unable to determine the difference between the simulation and 'reality'. The distinction between the simulated and the real has been elided.

> *We have seen what simulation seems to want— through our immersion, to propose itself as proxy for the real. The architecture faculty who designed Project Athena's Garden dreamed of transparent understanding of design*

process; today scientists are reconciled to opacity and seeing only a CAVE's shadows. Over the past twenty years, simulation has introduced its dazzling environments and we have been witness to our own seduction.

When simulation pretends to the real, buildings look finished before they have been fully designed and scientists find no fault in "impossible" molecules that could only exist on a screen. Computer precision is wrongly taken for perfection. The fantasy, visceral in nature, is that computers serve as a guarantor, a "correction machine." (Turkle, 2009 p 80)

For us, the importance of Turkle's account lies primarily in the way that the cases she discusses appear to offer corroboration for the sociological analysis and prognostications expounded by postmodernist thinkers such as Jean Baudrillard. In her low key and decidedly undogmatic way, Turkle appears to confirm Baudrillard's claim that the introduction and widespread deployment of computational technologies, not just in science and other professional disciplines but in all walks of life, has undermined our grip on reality and inured us to a *hyperreality* in which, ironically, anything outside the realm of digital processing has been lost. This condition is the core of the consciousness required to enable the continuing reproduction of post capitalist modes of production which underpin consumer society. Although Baudrillard is mentioned only in passing in *Simulation and its Discontents*, it is clear from the title alone, never mind the analytic focus, that Baudrillard's post modernist sociological analysis has been an important inspiration for that book's approach.

There is a second reason for wanting to focus on Turkle. Her contribution, and particularly its tone, has been well received and widely endorsed. As a consequence, it has become a model for a style of analysis in HCI and elsewhere. And yet the unchallenging and easy to assimilate nature of Turkle's work should not lead us to adopt postmodernism inadvertently. Within the social and cultural sciences postmodernism is, in Douglas Adams' classic phrase, mostly harmless. It sits alongside other similarly apocalyptic narratives of recent and not so recent history and is treated as just another such. In the jejune areas?? of the social and cultural sciences where almost anything goes, then postmodernism is as good as anything else.[5] Not surprisingly, this sociological appreciation of the claims of postmodernism as a form of Sociology does not exist within the computational sciences. Although promoters of the framework may gesture at its potential role as one kind of resource which could be drawn in from the social sciences, they do not provide the balanced audit which one should be able to derive from Sociology itself. Such an audit would identify *the limits* to postmodernism as a way of doing social science and so draw out the analytic choices thereby being made.

We will try to rectify this omission. First We will explain the origins of postmodernism as an intellectual movement in the social sciences. Second, we will offer some considerations relevant to that history which bear upon its deployment as a form of social analysis within the computational sciences. Third, we will draw upon the *genealogy* offered and the *implications* sketched to project some potential difficulties which this line of thinking might pose computational disciplines and HCI in particular. Our conclusion will be that HCI should think very carefully before it tries to absorb

[5] This is a disciplinary statement not a political one. As a political matter, that is as a matter of authority, control, promotion, and publication, postmodernism with its emphasis on the importance of (gender, racial, and ethnic) *difference* and its promotion of minority culture(s) probably is the dominant modality.

postmodernist concepts and frameworks since their implications and consequences might not be what the profession is actually seeking or likely to be comfortable with.

Jean Baudrillard

Perhaps surprisingly, and certainly somewhat unlike their colleagues in Philosophy, sociologists are usually uncomfortable with the role of 'public intellectual'. This is not to say that they don't have opinions and views on public life which they are happy to publicise, but it is rare for a sociologist to become installed as a prominent media commentator on all aspects of social and cultural life. It is rare; but it does happen, especially in France. Towards the end of his life, Jean Baudrillard came to occupy just such a position among the French intelligentsia and, from that position, became well known and influential (well, certainly talked about a lot) in related fields in the Anglo-Saxon world. What made Baudrillard distinctive was not so much the baroque ways in which obviously complex phenomena were set out (all public intellectuals seem to revel in complicating the complex) as the tone which he came to adopt: a tone which was declamatory, aphoristic and increasingly millenarian. In the end, the style (almost) overwhelmed the thoughts which were being conveyed.

For example, three of his most widely read pieces on the conduct of the first Gulf war unrepentantly have the titles: "The Gulf War will not Take Place"; "The Gulf War is not Taking Place"; and "The Gulf War did not Take Place" (Baudrillard 1995). Elsewhere, in one of texts which is central to the development of his thought, he claims

> *"The Universe, and all of us, have entered live (sic) into simulation .....nihilism has been entirely realised no longer through destruction, but through simulation and deterrence." (Baudrillard 1994 p.159).*

Our target here is to explain how Baudrillard can come to this apparently bizarre conclusion. How can he deny the facticity of world events and argue that now we cannot tell the real from the non-real. We will do this by setting out the developing structure of thought underlying Baudrillard's pronouncements. This structure involves the rejection (or at least the serious revision) of two strands of social thought, Marxism and structuralism, which while prominent in French, and especially Parisian, social theory have not had a similar place in Anglo-Saxon social science. Thus, against Marx he wants to argue for a new theory of value based upon consumption not production. We live in an economy of mass consumption whose main engines include the communications industries. Against structuralism, he wants to argue for a change in the nature of signs and symbols and their relationship to that which they represent. It is the merging of these lines of thinking together with his penchant for the (over)dramatic which shapes Baudrillard's style. Finally, once we have a clear view of what Baudrillard and other post modernist thinkers are driving at, we will be able appraise postmodernism's relevance for systems design. Our strategy will be as follows. First, we will trace the logic that Baudrillard follows from a fairly conventional semiology of cultural forms to the extreme positions identified above. We will then locate that logic in a broader stream of thought concerned with the implications of techno-rationalist thinking and technology more generally which draws upon Heidegger on the one hand and Marx on the other. With this understanding in hand, we will be able to assess the extent to which the intent of postmodern social analysis, first, is symmetric with that of HCI and, second, what value it might offer.

From Interior Design to Hyperreality

It was Roland Barthes (1968, 1972) who applied Ferdinand de Saussure's structural analysis of language, and in particular the distinction between *la langue* (the language system) and *la parole* (speech) to cultural forms in general. Since the rationale for some of the key moves in

postmodernist theory involves modifying Saussure's ideas, it is worth laying out the detail of some of his thinking and in particular his views on the nature of 'the sign'. For Saussure, the sign was made up of two elements, one physical, the other mental. There was the element of sound (strictly, though this does not matter for this discussion, the sound image) and the element of thought, the idea associated with that sound. The sound counted as 'the signifier', the image associated with it, 'the signified'. Think, for example, of the spoken word 'tree' and the idea of a tree associated with it.

For Saussure, all languages are equivalently structured systems of signs or tokens. The character of each sign is arbitrary. There is no intrinsic reason why the sound continuum should be divided up into the units that are words, or why the continuum of thoughts should be divided into the signifieds (effectively meanings) associated with those sounds. Different languages, after all, have different sounds for the same thought (signified), and the words in different languages that are relatively closely related can nonetheless have somewhat different meanings. The nature of signifier and signified is therefore arbitrary, and there is nothing in the nature of the sound or the thought that explains their identity. The identity of the signifier and signified is to be understood as a product of their relationship to other signs, in fact to all the other signs in the language. A language is a closed system with fixed relations between the units making it up. Since the units in the system are all defined relative to each other, then what makes any one unit what it is, must be the ways in which it differs from or contrasts with other units. Language, then, is entirely a system of contrasts.

Saussure's approach to language was meant to break with those approaches to language (such as those of empiricists) which thought that the meaning of words was determined by their association with things outside language. In its simplest form, this is the idea that words are the names of or stand for things; that is, words have referents. Obviously, Saussure does not accept that view. For him, the nature of words is fixed within the language system. Of course, he does not deny that words can have referents, but given the nature of the language system, any connection between words and referents must be entirely conventional. What sign is used for what thing has nothing to do with the intrinsic nature of that thing. It is this core idea which helps explain why postmodernists find the idea of language being able to capture the intrinsic character of an external reality implausible. Noting the difference between signifier, signified and referent may also clarify the logic of postmodernists' subsequent dissent from Saussure's basic doctrines. Jacques Derrida, for example, rejects the idea that there is any need for a 'mental' element to the sign. Signs are just signifiers, they don't need a signified. He also rejects the idea that the interrelationships between signs determine any given sign's identity because, on his view, the actual relationships are too loose to do so. On the other hand, Jean Baudrillard thinks that the fact that representation is taking an increasingly digitised form does away with the distinction between signs and their referents. At the same time, whilst postmodernists are prepared to reconsider Saussure's fundamental idea, they are no less resolute in their insistence that language is an entirely conventional (and therefore cultural) affair. For them, this means that there can be nothing essential about any connection between signs and what they are used to talk about.

Barthes generalised Sausurre's approach from language to culture. Cultural artefacts could be analysed in terms of the structural rules by which signs and signifiers, denotation and connotation, were used. In so doing, Barthes opened up an avenue for structural anthropology based upon what came to be called semiology. Semiology aimed to be a general science of signs, not just of linguistic ones. Apart for its use in the analysis of ethnographic materials (for example Levi Strauss 1969 and 1988), possibly the most contentious use of semiology was in architecture and in particular the

critique of what was thought of as "modernist" urban planning. Design of urban spaces and buildings was found to follow a grammar of use and meaning. Barthes himself intended these schemes to be used to criticise all contemporary French bourgeois culture. Often this critique is couched in unwittingly romantic terms and hence slips into nostalgia.

> *'Most sociological explorations of mass culture, especially those undertaken within a Marxist or critical theory perspective tend to be elitist in their cultural and political assumptions' ... (Stauth & Turner 1988, p 509)*

Baudrillard picks up this debate and in his early writing, *The System of Objects*, applies it to the transformation of interior design in the mid 20th century. He bases his analysis in a contrast between the organisation of furniture and furnishings in a traditional French household with that of the contemporary era. This contrast is based upon the degree to which the traditional household design reflects the moral order of bourgeois capitalism. Contemporary design, or so Baudrillard asserts, has broken away from that order. Instead, objects, furnishings, furniture, space and colour are deployed to engineer what he calls a functional "atmosphere". The home is not just a place in which to live. It has become a deliberately designed expression of cultural meanings dictated by the rules of 'interior design'. Objects are now chosen both for the functionality they perform *and* the required repertoire of meaning designed into them (they are desired at least as much, or even more, because they have become signs as they are for their practical utility. Objects have the shapes and colours they have not because that is the best form to realist their practical use but because the "style" connotes the way of life with which the owner wishes to be associated.

Using this as his starting point, Baudrillard goes on to analyse the fashion for 'period' houses and 'distressed' furniture, the display of antiques, and, perhaps most interestingly from our perspective, the popularity of "automatism"; what he calls "gadgets" and "gismos". The more automated a machine is the more perfect it is taken as being. Function has been overtaken by automation and has led to a fascination with functional aberrations such as gadgets and gismos.

For Baudrillard, the driving force behind this transformation is the mass production of culturally high status objects (what he refers to as the move from "models" to "series"). Where once Chippendale chairs were made as unique pieces, now anyone can have a Chippendale 'lookalike'. The meaning of such objects is not given by its denotative function but by the style of life it connotes. The need to generate, facilitate, and extend new styles of life, has led to the need constantly to invent new modalities of consumption through fashion and advertising and to support them by new market arrangements such as financial credit and branding. This has created the remorseless demand for endless consumption. Here, Baudrillard applies the move which Saussure also used to ground his analysis of language, namely the detachment of the sign from its referents. Baudrillard claims that the modern system of production has dispensed with the convention that ties referents (the reality) to the sign. Objects, rather than being things that signs refer to, have become signs themselves. Objects are purchased for their meaning as signs of social prestige, of good taste, of high intelligence, of sexual attractiveness and so on. It is their meaning which is consumed. What drives mass production is the consumption of such signs. The relation between the economy and the cultural system found in (some versions of) Marx has been reversed. Consumption is no longer a means to live; it is a reason for living. Modern capitalist production is the production of consumable "sign-objects".

Baudrillard sees this transformation of capitalism as one in a number of historical stages through which the consumption of value has passed. There have been, he asserts

> *...a natural stage (use-value), a commodity stage (exchange-value), and a structural stage (sign-value).....The first of these stages had a natural referent, and use-value developed on the basis of natural use of the world. The second was founded on general equivalence, and value developed by reference to a logic of the commodity. The third is governed by a code, and value develops here by reference to a set of models. (1993 p 5)*

The transition between the second and third stage is what is described in *The System of Objects*. However, he now sees a further stage, the fractal stage where

> *....there is no point of reference at all, and value radiates in all directions, occupying all interstices, without reference to anything whatsoever, by virtue of pure contiguity.....Indeed, we should really no longer speak of 'value' at all, for this kind of propagation or chain reaction makes all valuation impossible....it is as impossible to make estimations between beautiful and ugly, true and false, or good and evil, as it is simultaneously to calculate a particle's speed and position (ibid p 81)*

This fractal stage is the one Baudrillard claims we are in now. The fractal stage passes from the basing of meaning and value in simulation to its basis in simulacra. Critical to this has been the deployment of computational and communicational technologies. Whereas in the third stage, objects reproduced (simulated) the real - the lookalike Chippendale simulates the real Chippendale, now the objects, products, services we consume are based in something that lacks reality, a simulacrum, they don't have an original to be copies of, but are, so to speak, (only) copies from the start. For Baudrillard, this was first typified by the creation of Disneyland as a tourist destination. Whilst Disneyland does reproduce aspects of American life in microcosm, the image it projects is not based on what America was ever really like. It is, to use his phrase, an "imaginary" which can be neither true nor false. Our experience of imaginaries is of the sign/symbol alone. As these experiences are described, referred to, and otherwise circulated through the media and elsewhere, the world of our experience becomes *hyperreal*. Further, more and more of our experience is itself mediated: mediated, that is, by computational and communication technologies. More and more, the media through which we experience reality shape, form, and produce the reality we experience. Increasingly, though the 'mediation' is displaced so that our experience of the digital 'media' does not connect us with something on the other side of them, but originates entirely within those media. This, or so it is asserted, is as true of the news and documentaries we watch as it is of adverts and movies; as true of the scientific simulations we run as of the massively multi-user on-line games we play or, to bring the examples up to date, the social networking sites we use and the 'friendships' we form thereby. Reality is rendered, shaped, formed, and controlled for us to experience. This is the context in which Baudrillard can say that the Gulf War did not happen. The war was fought on and through the media, by and with screen-based technologies. Despite the body bags, the burned out buildings and the displaced people, in essence it was an electronic not a real war. Bombs were not dropped on targets on the ground but targets on the navigator's screen. Rounds were not fired at Iraqi soldiers but at images on head-mounted displays, and so on. The Gulf War media consumers experienced was created in the media, it was not a mediated representation of something going on in the deserts of Kuwait.

This is where the *sotto voce* account of Turkle intersects with the frenetic hyperventilation of Baudrillard. Both see the ungluing of our fix on reality as experience is increasingly channelled through computationally driven media. We will no longer be able to tell simulacra from reality and

so be prey to the manipulation of our understanding of reality by the unscrupulous and/or the powerful. What the foreboding about the supposed loss of reality comes to, then, is a worry about the unavailability of 'authentic' experience and our increasing satisfaction with 'inauthentic' experience without awareness that it is inauthentic.

Before stepping back to locate this line of thinking, let us briefly summarise where we are. For Baudrillard:

3. We are in the midst of a transition to a new set of relationships between production and consumption. This new form is 'driven' by the universal adoption of information and communication technologies.
4. This transition is one to where "sign-value" is created and consumed.
5. As this new form of production/consumption comes to dominate, we are seeing the substitution of hyperreality for reality (or, perhaps better, hyperrealities for realities) as the basis of experience. Such hyperrealities are built upon simulations and simulacra developed though the increasing use of digital forms and the computational models underpinning information and communication technology. Progressive digitisation of the means of communication allows for increasing transformation from one digital form into another which in turn means that conventional social categories cannot maintain their application because digital transformations need not abide by them. Thus politics can be presented as entertainment, entertainment can become politics; news can become entertainment and so on.
6. At the level of socio-cultural consciousness, the outcome of this substitution is a loss of certainty resulting from the shifting of traditional distinctions and a descent into nihilism where nothing is absolute and knowledge only is a matter of convention. One can no longer separate truth and fiction. Ethical standards have become a matter of personal choice.

Martin Heidegger, Jacques Derrida and Jean-François Lyotard

Whilst the Baudrillardian version might appear overdrawn and certainly over-excited, it is closely related to a tendency in Philosophy which was being developed at much the same time: a tendency which sought to challenge the idea of securing knowledge through context-free position from which to survey philosophical problems. According to this line of thinking, we are inevitably embroiled in a context, a perspective, and hence de-contextualised knowledge, a perspective free of perspectives, is impossible. This tendency is taken to its extreme with postmodernism.

From 'Enframing' to the 'Deconstruction' of Knowledge

Although it comes at the cost of considerable simplification (but not we think *oversimplification*), Richard Rorty's separation of Western philosophy since the end of the High Renaissance into two broad groups is a useful place to start (Rorty 1980).[6] One, marching behind the banners of Descartes, Hume, and particularly Kant, sees the role of Philosophy to ground what John Locke called "right reason". That is, Philosophy's task is the determination and elucidation of the structure of thought. Thus it seeks to fix what the right relationships should be between subject and object; reality and appearance; the grounding of knowledge in truth; and so on. Its model is the mathematical sciences and its approach is to argue from secured proposition to secured

[6] We *know* this *is* a simplification and not everyone fits neatly into it. Some, like the Pragmatists, appear to straddle both (Peirce in one camp, James in the other), whilst the later Wittgenstein probably falls outside both.

proposition. The second marches behind the banners of Hegel, Schopenhauer, Dilthey, and Nietzsche. Here, the concern is with tracing the development of thought through history, not as an empirically based historiography, but as the expression of how we represent the world to ourselves. There can be no sense of defining "right reason" or of fixing the relationship between subjectivity and objectivity since the notion of reason and the definitions of the subject and the object are historically located. As such, the mathematical sciences are of interest first as one form of knowledge alongside other cultural forms, and second as an historically located species of knowledge which is itself in permanent flux. All forms of thinking, be they scientific or other emerge from earlier forms through an endless process of confrontation and fission. To reverse Marx's reversing of Hegel, the history of all hitherto existing societies has been the history of emerging Mind. The key element here is the way in which the notions of ideology and alienation have been shifted from the content of thought to the frameworks which shape the possibilities of thought. For postmodernists that makes any fundamental change in the modalities of thought that much more intractable, a consequence which give us pause if, as with Sengers, Mccarthy and Dourish (2006), we want to use postmodernism to drive changes in ways of professional thinking (one can't be a little bit postmodern???).

The philosophic framework underpinning postmodern analysis is firmly in Rorty's second camp and has its origins in a reading Jacques Derrida (1978, 1989) provided of the later writings of Martin Heidegger. Heidegger had begun his work by seeking to re-position Philosophy not in 'historicism' but directly in experience, and in particular in human direct and unmediated experience of the world. The primary apodictic categories which underpin thinking are, then *being and time*. Against not just Descartes, Kant, Hegel, and his own teacher Husserl, but the whole of Western Philosophy since Plato, Heidegger saw, not reflective thought, but unmediated experience as the primordial ground from which Philosophy had to begin. The essence of our being, *Being*, was to be found in unmediated experience when we are 'thrown into' and immersed in a world of objects and wholly engaged with them as, for example, when we 'thoughtlessly' ride a bicycle or hammer a nail. In this unmediated engagement, Being reveals itself to us as what Heidegger calls "presencing".

It was the basis of this break with previous Philosophy that Heidegger attempted to initiate in *Being and Time* (1962), a project that he continued to work on but was left unfinished. In the second half of his life, Heidegger became more and more concerned about the consequences of certain forms of thought were having for Being and for the forms of presencing that it was taking.[7] In particular, drawing upon the *schadenfreude* of 19th century Romanticism, he began to believe that the forms of rationality associated with modern technologies were perverting Being. The general name he gave to these rationalities has been translated as "En-framing" and modern technology is their primary expression. The essential characteristic of En-framing is that it "challenges forth" the world as "standing reserve". By these terms, Heidegger tries to capture, first, the way that instrumental rationalism (that is, the calculation of means-end relationships against a standard of efficiency) has permeated all social and cultural forms. We are en-framed by instrumental reason and cannot think outside it. For Heidegger, the nadir of En-framing is the professionalisation of Philosophy as a research career, an agenda of research projects rather than a personal journey of engagement and enlightenment on which, hopefully, the philosopher is accompanied by others.

[7] See Heidegger (1977) and Pattison (2000)

Modern technology is the highest expression of instrumental reason and treats the world as a set of resources to be exploited or deployed. Here, Heidegger takes determining the essence of technology as a central philosophical problem. In this he departs from the usual approach which focuses on the consequences of technology and technological change. The essence of modern technology is a stark contrast to earlier technologies (Heidegger is fond of contrasting the windmill and the turbine). In the windmill, the wind is used simply to move the sails and through mechanical energy directly turn the mill wheels. With the turbine, what is created is a product, electricity, which can then be transformed into a commodity. Modern technologies are both the consequence of En-framing and the means by which that outlook is promulgated. The danger which Heidegger sees consequent on the invasion of all spheres of life by En-framing has been and will be a loss of authentic experience — what Heidegger refers to as "homeliness". In common with 19th century Romanticism, he sees this as the loss of the rural way of life and community together with the craft knowledge they are associated with. Our homes now are not places where our horizons focus in to be with each other but where, because of our use of television and other modern media, we are elsewhere but together. Television allows us to be not as one at home but jointly on safari at a distance, separately watching sport rather than immersed in and at one with the crowd, and so on.

Heidegger, having started out attempting to re-ground Philosophy anew, ends with a romantic nostalgia for declining ways of life, a rejection of "modernist" ways of thinking, and a fear for the consequences of modern technologies. As we have seen, these are themes which were the object of Baudrillard's contumely. The bridge from Heidegger to Baudrillard is the philosophy of Jacques Derrida, and in particular the notion of the "deconstruction" of texts and, by extension, all cultural artefacts.

Derrida followed Heidegger in setting himself the task of developing a reflexive Philosophy; one which would ground itself by according due weight to the fact that Philosophy is a discipline of texts. Since Philosophy aims to uncover the foundational assumptions of particular forms of discourse (to use that term for the moment) then what Derrida wants to do is reveal the foundational assumptions of writing and reading built into Philosophy itself. The challenge he faces in *his* philosophical analyses, as he acknowledges, is to free himself of such textual assumptions.[8] He chooses to do this through a *deconstructive* reading of the classical canon from Plato to the modern day. For Derrida, the most important figures in this canon are Plato himself, Kant, Hegel, Nietzsche, Husserl, Heidegger and the structuralism of Saussure. In Derrida's view, each of these thinkers tried and failed to "close" philosophical thinking by overcoming the constraints of writing philosophically as these were set by the institutionalised practice of the Philosophy of their time. Such a closed account would be self-sealed and, thereby, provide an end to Philosophy. For Derrida, each failed because they failed to overcome the limits of metaphysics. The philosophies present themselves as achieving closure, but reflexive analysis of them as texts reveals the pre-suppositions on which they have been grounded. Heidegger's failure took the form of a continuing commitment to attempting to overcome writing through writing. That is, an attempt to end Philosophy through the provision of a text. However, since such texts are themselves cultural artefacts, they are systems of signs. In assuming some such system could, once and for all, depict how things are is to reproduce the Kantian fallacy. As a thoroughgoing materialist, Derrida dispenses with Saussure's separation of signifier and signified and accepts only that language consists of signifiers. There is no need to assume that a mental component, the signified, is needed

[8] There is a clear echo of Heidegger in this in that Heidegger constantly refers to the genealogy of our concepts in Latin and Greek and traces these links through to the modern idiom.

to fix the meaning of signs because one doesn't need to suppose that the meaning of signs *is* fixed. He also abandons the idea that meaning is definitively fixed by the internal relations of the language system. He insists there is 'play' (in the sense of loose fit) in the system, so that it cannot foreclose the possibilities of meaning. He uses this to point to the importance of '*differance*' underpinning the meaning of texts. Just as the meaning of a world is given by its place in the system of signs so the meaning of philosophical claims are given by their place in the system of the text, The philosophical implication of the fracture of sign from both signified and referent is that there can be no definitive, absolute relation of language to anything outside itself and so no truth: truth, that is, with a capital "T". In its place we have the possibility of infinitely many constellations created by the shifting relations of sign and signifier, symbol and referent, claim and truth. The meaning of texts cannot be uniquely fixed because there is plenty of 'play' in the meaning system. The idea of a final, definitive reading for any text, including those of philosophy, has to be given up. The challenge for any philosophical reading of a text, therefore, is to dispel the illusory desire for a final meaning and, instead, to deconstruct its claims to truth by revealing the mechanisms by which symbol and referent, concept and reality are glued together. but because, for the reasons just given, meaning cannot be fixed as tightly as Saussure imagined, efforts to set out a fully definite account of anything must fail, inevitably allowing in aporia concerning inconsistencies and ambiguities. For Derrida, there can be no first philosophy, no absolutely firm foundation of the sort for which Philosophy has always striven. All we can ever hope for is yet another text in the stream of texts. What he provides is the method for undermining predecessor texts by bringing out their indeterminacies.

From Heidegger, we have gathered the primordiality of experience and how that is being degraded in the modern world. From Derrida, we have a form of philosophical analysis which breaks the tie between appearance and reality, thought and object, and denies the possibility of absolute truths, the universality of meaning, and the end of Philosophy. There is just one more element to be added before the conceptual framework upon which postmodernism relies is complete. That is the analysis of the implications of the rapid development of information technologies for knowledge itself as a cultural form. These implications were spelled out by Jean-François Lyotard.

Lyotard (1984, 1991) begins from the presumption that we live in a post-industrial age and postmodernity is the culture associated with this age. For Lyotard, postmodernism adopts (in the phrase everyone quotes), "incredulity with regard to meta-narratives" both philosophical and scientific. All all-embracing intellectual schemes are just "discourses" carrying the illusion of being something more - a theme which resonates strongly with Derrida. For Lyotard, what characterises post-industrialism is the extension of economic value beyond mass production of goods to mass production of information (as with Buudrillard, culture has become the key commodity). This extension has occurred because of computational technologies. For Lyotard, the critical question is how knowledge is to be legitimated when, as information, it is disconnected from the social ties which used to legitimate it and is widely available from huge on-line stores of data (not his term) and promulgated through the universal availability of computational technology. Today, there is no privileged cadre of those who know, who have first hand access to knowledge and who can, therefore have the authority to assess claims to knowledge, which renders the status of knowledge uncertain. Moreover, developments both in the sciences themselves and in our understanding of the history of the sciences have led us away from the acceptance that science progresses towards certainty and "the Truth". Rather, what we see in science's history is the succession of alternative "paradigms". Science has lost its authority because it has associated itself with meta-narratives about the role of scientific knowledge in social progress and about the progress of science toward final understanding of everything which have become unconvincing because the promised the finish

never arrives. For Lyotard, because the institutionalised independence of science as the "owner" of truth has been lost, the authority of knowledge, and hence its claims to truth, will be given to those who own and control the information stores and the means of sharing that information. Their ownership of the means of information production will confer authority on the knowledge they disseminate. We will have no criteria for judging fact, truth, meaning other than those they give us. In essence, this is the same argument as that of Baudrillard and Turkle.

Summary

The dystopian view of modern computational and communication technologies which Baudrillard expounds may be on the extreme wing of postmodernism and cast to be deliberatively provocative and offensive, nonetheless it shares an intellectual heritage which is rooted in a long standing tradition within European Philosophy. Along with Lyotard, Baudrillard sees the legitimation of knowledge (our conception of factuality, reality and the real) as the crucial issue. For Baudrillard, legitimation is now achieved by founding reality in simulacra which are purveyed by computational and communication technologies. The issue of legitimation arises because, following the reinterpretation of the structuralist account of meaning and particularly its use by Derrida, the tie between sign and signifier has been broken. Philosophically, there is no place to ground meaning other than in the welter of language itself. And within language, nothing is fixed. Derrida arrived at this position from a consideration of the European philosophic tradition and especially Hegel, Husserl and Heidegger. The nostalgia which the last named felt for traditional ways of life increasingly threatened by the En-framing of modern technology resonates strongly with the vision which Baudrillard describes.

Postmodernism and HCI

With this understanding of the roots and the main influences on postmodernism, what now should we want to say about the proposal to draw it into HCI? Is the deployment of postmodernism in social science symmetric with how it might be used in HCI? Will the translation will be relatively unproblematic and, therefore, all the more likely to be successful? For us, the response to these questions must be very qualified. In the first place, neither the weak nor the strong versions of the proposal actually try to assess in detail just how and where postmodernism would fit within the professional and research models of HCI, though, to be fair, proponents of the strong version do acknowledge its particularities and peculiarities as a mode of theorising. Nonetheless, neither group of proponents offer any extended evidence for goodness of fit.

Second, and actually more important, any balanced review of the postmodernist theorising and analysis is likely to find innumerable points at which it and HCI will be out of kilter.[9] Any serious proposal for broadening the academic resources on which HCI can draw would have to address these. That is, it would have to offer ways in which such difficulties and infelicities could be overcome without overly distorting either the analytic ground of postmodernism or the professional endeavour of HCI. We think this is an extremely tall order and are not surprised that neither the strong nor the weak version has attempted it. In part this mismatch comes about because HCI is an *applied* discipline seeking to facilitate the improved design of computational artefacts. As we have seen, Sociology, and especially postmodernism, are congenitally *abstracted* (we won't say "pure"). Their motivation is reflection, analysis and commentary, not intervention. Reflection is about opening up possibilities, options and questions. Intervention is about reducing them. These

[9] This, of course, could be the point of the proposal. But if so, then it is not a serious contribution to the development of HCI as a discipline, merely a provocative jape.

different motivations lead to very different orientations and frameworks. Sociology is at home with the general and conceptual. Studies are undertaken and data collected in the service of refining general statements and conceptual distinctions. HCI is at home in the concrete and the specific. For it, studies and data serve to elucidate and refine specifications for particular designs.

Of itself, such a mismatch might be enough to prevent any serious attempt by HCI to absorb postmodernism.[10] This conclusion is reinforced by a number of other disjunctures which are equally telling. By way of final summary, we will draw attention to three potential fracture points which emerge from the account of postmodernism we have just given.

Problematising as a modus operandi.

The philosophic impulses of postmodernism are towards endless problematising. No issue, no standpoint, no pre-supposition can be taken to have been secured. It is always possible to adopt an alternative standpoint from which to offer critique or deconstruction. There are no secured foundations. In particular, this is true of the mode of reasoning being deployed. In postmodernist debates, mutual deconstruction is an honoured sport. Within HCI, the mode of reasoning is convergent and instrumental. It assumes that there are 'facts of the matter' which can be fixed and elaborated and the move from facts to generalisations made. In addition, it is assumed certain empirical methodologies enable such facts to be obtained. These encourage the idea that HCI could and should aspire to be cumulative and prescriptive (see for example Card and Newell (1985)). Ultimately, it is hoped this accumulation will result in standards which guarantee real improvement in designs. For postmodernism, no academic discipline can claim to be cumulative let alone prescriptive. Postmodernism insists that the appearance of development and accumulation is simply a reflection of cumulative institutional agreement about what is to count as facts and the prescriptions to be derived from them. What postmodernism seeks is restless, endless interrogation and problem creation. Introducing this mode of theorising into HCI is almost certain to create tension.[11]

A Preference for the conceptual ionosphere

HCI is directed to intervening in the world. It is concerned with our interaction with systems and devices and how such interaction might be best facilitated. Postmodernism is concerned with teasing out the significance that ways of thinking about such activity might have. As a consequence, its discussion tends to focus on what Ian Hacking calls "elevator words" (Hacking 1999) and to take place at the most abstracted and generalised level. Discussion is couched in terms of a rupture between experience and reality in general terms. The fact that most of us cannot tell handmade chocolate from the massed produced variety is presented as an elision

[10] This isn't the place to go into it, but strong circumstantial evidence (almost a trout in the milk) can be seen from the fact that despite a fairly long drawn out campaign that was met with considerable good will in HCI, attempts to introduce rigorous sociological thinking and ethnographic methods have pretty much failed. Sociological thinking and ethnographic methods have transmogrified into market research.

[11] One observation to be made is that although what was called "the turn to the social" in HCI was justified in terms of the possible consequences for design, the main consequence seems to have been the engendering of yet more academic debate, with the associated explication of approaches, controversies about where boundaries lie, debates with what are held to be 'rival' approaches, generalised critiques of design as an instance of 'modernist culture', and so on.

between representation and reality and the emergence of the hyperreal. Ordinary features of contemporary social life such watching sport or the news on television are promoted as having world historical, socio-cultural significance and as testimony to the power which computational technology and the broadcast media that deploy them now have. The consequence of this power is that we can no long tell the real from the virtual world (but in an important sense the lesson of postmodernism is that there is not and never has been any difference to tell – can we say postmodernism often chickens out on the implication that one thing is just as good as another??.

A second aspect of this conceptual ballooning is the result of failing to pay close attention to just what the word 'real' actually does in our ordinary language and hence in the language of HCI. As John Austin pointed out (Austin 1962), in many ways it functions as a substance hungry word (as an "adjuster word", as a "trouser word", as a "dimension word"). It is only in the peculiar discussions of philosophers and social theorists that ordinary objects are held to share the common property of 'being real' and hence, as a totality, to make up something called 'reality'. The assumption that objects do have this common property and do, in the aggregate, comprise reality is what allows the suggestion that virtuality has replaced reality. Outside this rarefied discourse, though, it looks more than a little fishy (as John Austin might have put it) to propose that because we can't tell cask ale from real ale, or a real news item from a spoof one, that somehow experience has become unglued from reality.

A Preference for Stretched Analogies

Whilst metaphors are widely used to guide design decisions in HCI, they are deliberately framed and restricted. The windows or desk top metaphors, for example, are used in very specific ways in very restricted domains. For postmodernism, imagery and analogy are the stuff of analysis and allowed complete free rein. This can be seen very clearly in the way Derrida's insistence that philosophising is a response to texts whose reading is from a point of view and in a context, has been taken up. Derrida insists texts are never closed for the reader but permanently open. The text, in that sense, is as much a reading as it is a writing. As a hermeneutic this is a perfectly understandable, although not exactly uncontentious. However, to generalise the notion of textual reading to the analysis of all cultural objects, so that all aspects of culture no matter what can be treated as set in and hence to be read off from their 'mode of discourse' is a particularly unconstrained way of speaking. To then suggest, as Lyotard does, that such models of discourse express the ideology and interests of particular social strata and particularly the dominant social stratum which, by definition, must control the means of (symbolic) production, places almost no bounds at all upon the use of the analogy.

The precision with which metaphors, analogies and other tropes are used in HCI contrasts starkly with the unbridled usage of postmodern theorising. Where, for one party, they function positively as analytic frames offering possible resources to be drawn upon in designing the use of a technology, for the other they are key tools in a rhetoric of revelation and serve to dramatise the usually negative inferences being drawn.

Conclusion

We admit that in selecting the proposal to introduce postmodernism into HCI we chose an example that was well fitted to the case we wanted to make. The wilder reaches of the former as just about as far as it is possible to get from the prosaic, engineering orientation of the latter. And the example does serve us well. Because it is so obviously ill thought out and because when thinking through what it would mean to do what it is proposed to do, so many fundamental difficulties are thrown up, it provides an excellent case to support the principles for managing disciplinary border

crossing we set out at the beginning. In both the strong and the weak versions of postmodernism in HCI, neither our cautionary nor our transparency principle were followed. Other examples of social science imperialism are not so clear cut nor so obviously misguided. The saving grace of the case of postmodernism and HCI is, in fact, that the proposal has met with few, if any, takers within the 'real world' of professional HCI. Perhaps that why, after a short period of enthusiastic promulgation, the proponents of both the strong and the weak versions appear to have dropped the idea altogether and have turned their attention to other matters.

ESSAY 2

REPRESENTATIONS AND REALITY

Introduction

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

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

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

How to Represent Science

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

The thrust ofWoolgar's case is this.

1. Empirical and historical studies of scientific practice have shown that philosophical prescriptions for the logic of scientific discovery do not match the way science is actually carried out. Propositions, theories, and laws are taken to be veridical or factual descriptions of the way the world is but such factual descriptions are logically underdetermined. The only connection between the factual descriptions and the law-like

generalisations is a wider social agreement about the relationship of representation to reality.

2. In like vein, research in the Social Studies of Science allows us to question two fundamental pillars of scientific method: formal logic as the guarantor of the move from premises to conclusions (statements about facts to truthful generalisations); and the distinction between representations and the reality which those generalisations are supposed to articulate.
3. For Woolgar, the consequence of such questioning will be twofold: first we will need to re-think the character of science and the institutional place it has in our society. Second, we will need to re-think the ways we study science. Since, the former will have to recognise the entanglement of the observer with the observed, so too will the latter. The sociology of science has no choice but to engage in radical reflexivity.

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

The problem of representation in science is Woolgar's central motif. He has two questions. First, how are the representations (theories, models, laws, etc) which science uses to describe the world, actually connected to and fixed by the world? Second and following from the first, how can we know if the means by which this fixing takes place is itself robust? Woolgar begins where the empirical studies in the sociology of knowledge end, namely with the conclusion that the descriptions usually offered fail to acknowledge the social character of science and hence the indexical, open and reflexive (in short, contextualised) character of its representations. The meaning of any representation cannot be determined by its status as an 'objective truth' which corresponds to the facts independently of the context in which the representation is used. The indexical, open and reflexive character of meaning constitute what Woolgar calls science's "methodological horrors" and involve tensions which he does not think can possibly be resolved.

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

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

> *Unable to ground or construct an ethics within the terms of its critiques of foundationalism and of dominating humanistic values, postmodern politics is often reduced to the ironic, anarchistic effort to transform existing order*

by means of play, jouissance, or other textual strategies.... (McGowan 1991 p 28).

Object and Representation

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

So, on Woolgar's view, how do we think about 'object' and 'representation'? It is, he says, in terms of the two being connected in an order of precedence. 'Objects' come first, with 'representations' following from objects.[13] What Woolgar wants us to understand by this suggestion is, for example, that we think the objects that scientists discover exist before they are discovered. 'Discovery' is therefore the creation of a representation for what had previously been unknown and hence unrepresented. The usual assumption which embodies the relation of precedence between object and representation is that objects exist independently of any representation; that is, they exist whether or not we have any representation for them (i.e. know about them). Since this conception is so deeply rooted, the suggestion that there is an alternative to it will not only be difficult to credit, but also seriously disturbing. And yet, despite this, Woolgar does want us to accept that objects don't exist until they are represented.

In proposing his view, Woolgar is simply reversing the account of the relationship which he claims we hold. We should now insist that representations come before objects. Woolgar sets out to show this is so by examining how scientific investigations produce discoveries. However, his demonstration depends on the speedy insertion of a quite major and hugely controversial

[13] Now it is very important to note that Woolgar offers no *evidence* for his assertions concerning how 'we' think about objects and representations. Nor do we have space here to explore the range and complexity of the ways we actually do think about what are, at least as Woolgar treats with them, these rather loose conceptions. This being said, we shall set such caveats aside in order to pursue the main line of Woolgar's argument.

assumption, namely that ‘epistemology’ and ‘ontology’ are identical; that is, something’s existing is the same as its being known to exist. In summarising the implication of studies of science whose conclusions he is in sympathy with, Woolgar proposes

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

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

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

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

The term "splitting" is meant to suggest that in scientific investigations ‘object’ and ‘representation’ are originally one, but that over the course of the investigation they are separated from one another — the "splitting" — with the object being dissociated from its representation in a way which makes it seem that they were always distinct. It is splitting which executes the reversal in the relation between representation and object. To begin with, the object depends on the representations (what Woolgar calls "documents") which represent it. As research proceeds, the object is made (out to be) independent of those documents, thus making it seem that the nature of the documents really depends upon the nature of the object documented.

> *In the first stage, the scientists have documents (traces); in the case of the discovery of pulsars these comprise the charts from the telescope recorder, but might also include other publications, papers, previous results, the telescopes themselves, other apparatus, what Hoyle says and so on. At stage (2), participants use (some of) these documents to project the existence of a particular object (in this case interference or an astrophysical phenomenon or whatever). Importantly, the object is created and*

> *constituted out of documents available to the researchers. At stage (3) the splitting occurs. Although the object was initially constituted in virtue of the documents (and more generally the social networks of which they are a part) it is now perceived as a separate entity, distinct from those documents. The object now has a life of its own. Indeed, it is just one short step from possessing an infinite history: it is about to acquire the status of antecedent. In stage 4, the relationship between the documents and the object is inverted. Whereas the object was constituted on the basis of the documents in step (2), it now seems as if the object (which was there all along) had given rise to the documents (Woolgar op.cit. p.68)*

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

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

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

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

Why do scientists need this suppression when the dependence of scientific investigation on what Woolgar labels steps 1-3 must be among the world's worst kept secrets? Allegedly it is to maintain

‘the ideology’ of objectivism and to conceal materials that would be directly subversive of it, namely evidence showing that the object originated in documents and that, therefore, the discovery was the scientists’ active creation.

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

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

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

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

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

Woolgar’s account is that science is justified by objectivism even though its practice manifestly contradicts objectivism. But then, if scientists do have an objectivist ideology (in the pejorative sense), it can only be functional if it somehow deals effectively with the fact that scientists own experience itself runs counter to the ideology. Unless we want to suggest that all scientists are riven by cognitive dissonance, there must be ways in which the encounter between ideology and contradictory experience are resolved in favour of the ideology to explain why objectivism remains a foundation of science. Whether participants are aware of the disparity between ideology and reality, they must act in ways that have the effect of preserving the appearance (for it is only that) of symmetry between ideology and actuality. It turns out, then, that Woolgar is presenting the

practice of science essentially in functional terms. Scientists' conduct on actual occasions somehow has to overcome a problem which is built into the practice in which they engage. Moreover, because their experience is rooted in the ideology, they are unaware that the functional problem is being resolved.

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

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

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

Conclusion

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

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

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

terms, of course, these philosophical issues are challenging and captivating. It is just that paying attention to conceptual arguments over the methodology of science and social science means *disattending* to the empirical issues regarding science which motivated the investigations in the first place. This, of course, explains why, when Sociology colonises other disciplines, they usually end by being populated with people arguing Sociology (which leads them into arguing philosophy).

ESSAY 3

ANT AND THE INVASION OF ECONOMICS

Introduction

It is little wonder that commentators on Actor Network Theory (ANT) are sometimes driven to distraction (see Amsterdamska 1990 for example). Even leaving aside the opaque nature of the claims being made (hypotheses? empirical generalisations? working assumptions? pre-suppositions?), there is so much to demur from, object to, have reservations about, say differently, correct, and downright disagree with, that it is hard, almost impossible, to know where to start. Take up any particular study, case, argument or point, and pretty soon one finds oneself so deep into a ramifying rabbit warren of verbose arguments, neologisms, excursi and character assassinations, that one is in danger, if not quite of losing the plot, then certainly being unable to recall, summarise and secure any logical accumulation that might be lurking there. Whole conceptual edifices get built but it becomes impossible to say how the structure is actually supposed to hang together.

This Joycean style has two consequences. First obscurity and baroque argument obscure the ambitious programme which ANT has set for itself. ANT does not want just to change the way science is studied and described, nor does it limit itself to orchestrating a revolution in Sociology. ANT's ambition is the overthrow of the predominant way in which *all* the social sciences *as well as* Philosophy are carried on *as well as* sorting out the relationship between 'science' and the wider 'society'. Second, the rhetoric makes a just evaluation of those ambitions, both the rationale for them and the strategy for delivering its aims, difficult. Pinning ANT down in order to see which of its claims are reasonable and whether what it is proposing is genuinely plausible is a Herculean labour.

Our response to what is mostly just ANT's stylistic conceit is a deliberately simplifying and containing strategy. In this chapter and the next, we offer cameo evaluations of ANT's programme; deliberately miniaturised versions of what reasonably full accounts might look like. This means that while we try to get round all the essential and important aspects, we will not try to set each of

these out in detail and fully justify our stance on them. Rather, we want to give a sense of what an overall evaluation (a) might look like and (b) might come to. In this essay we will work with just one example, namely Michel Callon's efforts in *What does it mean to say Economics is Performative?* (2007) to re-position ANT as the saviour of Economics (and also, perhaps, of the management of the global economy). In the next essay, we look at ANT's attempt to overthrow the dominant modes of conventional Sociology and Philosophy. Our aim is to show that where ANT is innovative, it is not Sociology and wrong; and where it is Sociology, it is largely uninteresting.

Approach

Because of this containing strategy, our account of ANT has a certain amount of edge. Given that ANT itself is nothing if not self-confident and assertive, we do not see this as likely to pose problems. Since the rhetoric ANT uses is one of its main ploys, we will start by picking out a number of common tropes generally used to organise ANT's positioning and arguments. These both shape topics for discussion and pre-dispose certain kinds of responses. Obviously, although the tropes are repeatedly used, the materials presented through them differ from case to case.

Quixotic Formulation of Positions

It is standard form for ANT to ground its argument as a response to and resolution of a proposed misapprehension, mistake, limitation or error committed by some other research tradition. In what is now almost a ritual, one finds the ANT rendition of the Sermon on the Mount. "You have heard it said....but I say unto you...." However, what we are supposed to have heard or have been taught before usually turns out to be more a figment of ANT's imagination than an expansive and documenteddescription of some actual position or claim. Callon, for example, starts from the quandary which Gerald Faulhaber and William Baumol (1988) (henceforth F&B) are supposed to be in as a consequence of their study of the innovativeness of Economics. This quandary is said to be how to account for the fact that Economics has not been all that successful at generating actual innovations in real economies either in terms of outcome or of process. As F&B say, the results are "mixed" at best; which is hardly surprising since most academic economists don't think that innovation, and particularly business innovation, is what they should be doing and don't therefore portray their work as having such potential.

Now, even if F&B are puzzled by the relative lack of interest in and application of the results of research in Economics, it is hard to see how their position could be described as a quandary. Much less is it clear how their matter of fact summary of their findings and their understated account of their conclusions could be regarded as being "tormented" (as Callon suggests). Certainly, F&B do not appear, first, to think that they are responsible for putting Economics to rights and, second, if such was their task, they show no sign of how they propose to do it. The only thing we can conclude is that Callon needs to turn their conclusions into a quandary, a problem, a challenge, a deeply puzzling state of affairs, because he wants to be able to assert that they do not understand their own results, *and he does*. On his account, the failure to understand their own results is what generates the quandary. It is also what enables him to propose his resolution.

The root of their misapprehension (or error if you prefer) is what Callon takes to be F&B's old-fashioned and limited linear model of innovation. Even then, elaborating an alternative, iterative model of innovation would not be enough to put things right. This is because Callon's F&B are also stymied by an epistemological dilemma. F&B say that they are surprised by their findings because they chose innovations which would improve the capacity of economic agents to succeed in the market. Callon turns this into "innovations that markets and agents should have invented and would eventually have invented on their own" (p. 313). Callon's rendering is an interesting gloss on what

F&B actually say, namely that markets get it right most of the time (eventually). Getting it right eventually is manifestly not the same as inventing the breakthroughs that economists might have made. All that it can possibly mean is that markets eradicate problems, eventually, and produce an outcome that is (more or less the same) as that which would have derived from following Economists' plans for the reformation of the economy had they made them.[14]

From here (though the point is made a few moments earlier to provide the context for the supposed quandry), it is but a short step for Callon to claim that if such innovations were to succeed, this would mean that economists would be capable of changing the behaviour of economic actors "from a distance". Further, since Economics is just like Physics, there would be nothing to stop anyone claiming that through their theories, physicists similarly can alter the laws governing planetary motion. This suggested inference, of course, trades upon an ambiguity in the notion of 'laws'; laws as the summary statements enunciated and laws as the patterns of activity which conform to the enunciations. However, while Physics can and occasionally does re-state the relevant laws, physicists have not (as yet) found ways to re-engineer the general pattern of planetary motion. Having reached his conclusion, Callon throws up his hands; surely claims to be able to change the laws of nature or the market are anathema in both Physics and Economics? A puzzle has been inflated into a quandary and from there into an epistemological infraction of the first magnitude.

Of course, the reason for all this is to allow Callon to roll out ANT as re-assurance for the economists. ANT does think that when their ideas are taken up and used in practice, physicists can change planetary motion and economists can act to change economic behaviour from a distance. The rest of *What does it mean...?* tries to show us why and how.

Close examination reveals two strategies generating this windmill for ANT to tilt at. The first which we have already described, consists in *gradual position morphing*. An argument, stance, outcome is moulded so that it can be subjected to the ANT treatment. In F&B's case, an interesting semi-professional puzzle as to why the results of economic research are not taken up in business much more frequently, becomes a protoypical exercise in erroneous economic history, then an avoidance of the supposed implications of their analysis, and finally a manifestly self-refuting exercise in epistemology. Had F&B had the insight, foresight, courage to adopt the ANT point of view, all these troubles would just evaporate.

The second strategy is interwoven with the first and consists in *contentious comparison*. On this occasion, it is the comparison of Economics with Physics. Elsewhere in the paper, other equally contentious comparisons abound. This comparison is first introduced as an implication of the F&B puzzle about the take up of research in Economics and the predominance of the view that economists are describers of patterns of economic relations not innovators in business practice. Since physicists might also refer to themselves as describers, Callon feels free to ask that if the supposition that economists can intervene in the market is accepted, "Wouldn't this be tantamount to claiming that physics and physicists are able to influence the laws governing the course of the planets?" (p313). So we have the tendentious identification of a social science (Economics) with a natural science (Physics) simply on the basis of what each might say about its attitude to its phenomena (that is, that they are describers of it). No attempt is made to indicate just how and

[14] It was Linblom & Cohen (1979) who pointed out that most policy oriented research simply fails to appreciate the extent to which social problems either solve themselves or cease to be important enough to warrant solving.

why the phenomena under study in these disciplines can and should be treated as being isomorphic. That one is *social* and the other *natural*, is reduced to a matter of mere labelling. It marks no real difference (or at least, if it does mark a difference that F&B might see as significant, that difference is not even acknowledged, let alone respected). Developing ingenious applications of the 'laws' of Economics does not change those 'laws'. All that happens is that, rather like Engineering does for Physics, the phenomena to which the laws apply are re-arranged somewhat. Of course, for ANT, that difference does indeed mark no difference. As Callon asserts later (p 315), it is his thesis that the natural, life, and social sciences all "contribute toward enacting the realities they describe". This pronouncement is licensed by wholesale identification of the disciplines. But why F&B should be burdened with ANT's categorisations is left completely unexplained.

Of course, as soon as one begins to ask about the degree of isomorphism, the rug is pulled from under both strategies. What does "distance" mean in each case, for example? In Physics, action at a physical distance (that is without apparent causal intermediation) remains a troubling puzzle. For Economics, it is social distance between specialisms in the division of labour. However, for all the social sciences, premised as they are on *interpretive social action*, it is quite reasonable to say that behaviour can be changed across *social distance*, and frequently is; for example by policy makers, managers and others in authority. Second, the behaviour of material things is affected through the implicit or explicit use of the laws of Physics, not by changing those laws. When Physicists change their minds about how to frame their laws they do not thereby themselves physically rearrange the phenomena which the laws are designed to describe. Re-framing the law of gravity does not manipulate the spatial relations between planets.

Methodological Monomania

The analytic disciplines, be they scientific or social scientific, take their departure from what Alfred Schutz called "the play of possibilities". No matter how elaborated and detailed, no single description of any phenomenon can capture everything which can possibly be said about it. Each analytic discipline takes up a particular array of ways of constituting phenomena in order to explore just how that constitution could provide for such a description. The constitution of phenomena is facilitated by the relevance structures which the analytic discipline brings to bear. It is their differing relevance structures (and the constitution of phenomena that is derived from them) which accounts for the difference between Economics and Physics, for example. For social sciences such as Economics, the cornerstone of the structure of relevance is *social action*; that is action oriented to others and based upon interpretation of meaning. For Physics, the cornerstone is the constitution of matter based upon causal conjunction. Across the social sciences, there are very different ways in which questions concerning social action are themselves constituted and pursued. These are expressed in the various forms of Sociology, Politics and Economics encompassed within those disciplines. For ANT, however, there is just one single master question which all social science disciplines should pursue, namely the exhaustive description of the circumstances which make social action possible. Even where the disciplines *say* they are interested in other issues and problems, ANT insists that they *must* answer its master question. Not surprisingly, most disciplines fall short of having an account of the circumstances of social action which meets with ANT approval.

F&B set out to ask about the take up of innovation in economic research. They wonder why it doesn't happen very much. For Callon, the only possible way of responding to this is to re-state the question as a request for the delineation of *all* the circumstances which would need to be in place for such take up to be possible. Such circumstances must be defined to include the individuals concerned, the social and economic arrangements that are in place, the policy frameworks that

govern them, and the material conditions through and under which they operate. This, and only this, counts as a description of innovation (or, perhaps, the lack of innovation) as social action. All of the participants in the social, economic, policy and material environment are *actants* whose contribution to the action must be described and the causal stories told of how their contributions enabled, facilitated, or performed the action. This is analytic monomania. ANT has the hammer of *performative agencements* and everything and everyone is to be treated as performative *enacting* nails.

We have already glimpsed this monomania in the treatment of F&B's research and the transformation of the question they were interested in. It is equally clearly on view in Callon's account of the Norwegian fishermen (pp 336-8). Here what could be a perfectly normal narrative of how a group of fishermen came to understand and take advantage of a policy change in EU regulations by changing their methods of fish farming, is re-written, first, as the "ontological mutation" of fish into "cyborg fish" and, second, the transformation of fishermen into *economic men* as modelled by Economics. No other account will do. Both fish and fishermen (among others) must be seen as actants in the *agencement* that resulted.

Surreptitious Positivism.

The monomania we have just described is but one symptom of the way ANT reproduces some of the preconceptions and problematics of positivism. Another is the fascination with the problem of representation and the consequent commitment to a singular, universal descriptive format. Only when we have provided an exhaustive description of the material and other conditions of the *agencement* will we have a secure way of hooking our representation onto reality. This may not be quite the reductionism and assumption of a unity of method of the Logical Positivists (not even Callon manages to say that everything can be reduced to descriptions given by Physics or that only experiments and quasi-experiments yield valid descriptions) but nonetheless it is built around the core positivistic conundrum of how to secure the veracity of descriptions. If descriptions are relative to context, what secures their truth? And what prevents just any description from being as good as any other? In trying to answer that conundrum, Callon confuses the notion of description as an achievement and description as the name of a form of utterance. Not all descriptions are rivals and whether one description is better than another will depend on what the description is to be used for and when. It follows that no description, not even the exhaustive description of the conditions of the *agencement,* can offer the last word, the complete account of some social phenomenon.

In place of positivism's reductionism and unity of method, we get performativity secured by a tacit constancy hypothesis. Those descriptions which lay out the performativity of a practice (be it Physics, Economics or fishing) are the ones which have fixed the linkage between how things are (ontological reality in ANT speak) and the practical reasoning being carried on in Physics, Economics or fishing. Thus translation into the terms of performativity provides a unified description of reality. The world is many ways, but there is just one way to describe it; positivism in a nutshell.

Argument through Forced and False Dichotomies

The core thesis offered in support of the performativity of Economics is a distinction which Callon makes between what he calls "confined economists" and "economists in the Wild" (the latter term being an unacknowledged borrowing from Ed Hutchins' (1995) programmatic approach to the study of distributed cognition). Confined economists are academic economists who research and teach the professional academic discipline of Economics. Economists in the wild are those who investigate

and theorise economic activity as part of the work they do as participants in the economy.[15] In *What do we Mean....?,* Economics is taken to be market, and particularly financial market, behaviour. Those people who organise supply of goods and services, set prices or regulate, record and administer economic transactions are among Callon's economic *actants*. Those who go shopping, pay their pension contributions and so on are all ignored. The leading example of economists in the wild are the Chartists; that cadre of investment analysts who track the moment by moment movements of financial assets and instruments and make their investment decisions upon the trends that emerge from such data. However, Callon insists anyone who offers an account, an explanation, a prediction of what some set of markets might do qualifies as an economist in the wild. The Economics they do is vernacular economics. Economics, then, comes in two flavours: professional and vernacular. How these two relate is left unexplained.

On just what is this dichotomy built? Obviously no-one will deny that there is a difference between the formal or quasi-formal explanations of economic phenomena offered by professional Economics and the explanations to be found in the professional practice of any other occupation. One set is derived from very specific (if much argued over) formal premises about rational choice, value and markets. The other derives either from post hoc rationalisations or commonsense theories about the way the economy works. In principle, as a working theory neither is better or worse, though neither will work very well as a theory in the domain(s) of the other (as F&B demonstrated). But a difference is not automatically a dichotomy. Certainly the practical reasoning that goes on in both sets of Economics is not directed either to achieving a common outcome or departing from common assumptions. Of course economists and practical people engaged in trying get something done (make money from the financial markets, re-frame commodity price structures, manage resources) both talk about economic activity, but they do different kinds of things based upon what they say (write books and papers or make investments and policies). Setting these up in contradistinction forces the putative difference into a dichotomy and predisposes the line of thinking that somehow they are 'really' just the same. Once we get to that point, it is no step at all to stipulate that "Economics" should be expanded to include both these very different forms. This, in turn, licenses the bald claim that "Economics contributes to the construction of the reality it describes". But of course, the "Economics" that claim covers is *both* professional and vernacular economics. The version which is doing the reality constructing through interventions based on its theories, is of course the latter. The baldness of the claim (and its point, presumably) is that it appears to apply equally to the former. Since no analytic grounds are offered for proposing the unification of this dichotomy of theorisations, we are left to conclude its basis is rhetorical. Setting up the dichotomy is meant to challenge conventional professional Economics and its resolution allows ANT to show how radical that challenge is.

Of course, if, as in this case, the dichotomy is clearly forced and false, and based in a stipulation, no matter how radical the proposal, it solves nothing.

Conceptual Mistreatment

If concepts had the equivalent of the UN Convention of Human Rights, then ANT would be very vulnerable to prosecution for gratuitous mistreatment, especially of concepts associated with domains far from those which it is usually associated with. In *What do we mean...?* the most conspicuous examples of conceptual maltreatment are found in the discussions of "the pragmatic" and "semiotic" turns in social science, and Robert Merton's concept "the self fulfilling prophecy".

[15] Where economists in Government fit in all this, we are not sure.

In the first case, 'pragmatic' and 'semiotic' are used to describe bundles of concepts which depend upon a notion of performativity. Performativity is a way of resolving "the paradox" that language can be used both to describe the world and to perform (social) actions in the world. This contrast is, of course, as forced and false as that between the types of Economics discussed above. The path Callon takes out of the paradox skates over the philosophical curriculum of ancient Greece[16] and Port Royale Logic to the introduction of pragmatics in Linguistics. The latter is construed as being concerned with the context of language use rather than its formal structures. The conclusion Callon derives from this tour is that we can adopt either a minimalist position and sit pragmatics alongside syntax and semantics as mutually exclusive but complementary accounts of language, or a maximalist position "and argue that nothing in linguistic phenomena can escape pragmatics" (p 317). Why we must be driven to this particular (dichotomous, of course) decision is not explained.

Into this somewhat odd construal of the history of language studies, Callon throws John Austin and his notion of "performative utterances" (Austin 1962). This is the bridge from the original paradox, and its consequential dichotomous form, to performativity. Austin is held to demonstrate "that only the maximalist position is defendable". Quite what Austin might have said about this suggestion we can only guess. It is true that Austin did talk about performative utterances, and it is also true that, in his own unique way, he compared his interest in language use as akin to botany; that is the classification of types of such utterances. His point, though, was, first, to make clear to philosophers that language use was not just representational (i.e. comprised of statements (true or false) describing states of affairs). Language was equally about action; doing things with words, as he put it. Second, just as there are felicity conditions for the truthfulness of statements, there are felicity conditions for the effectiveness of performatives. Austin was as interested in squibs, misfires and other mishaps as he was in what might be thought of as correct performance. What he did not imply, and would probably blanche at being taken to imply, is the suggestion that for constative utterances "the object is in the outside world", whilst performative utterances "cause the reality that they describe to exist" (p. 317), not least because the point about 'performatives' for Austin was that they didn't describe. They just perform the action they nominate. Austin was more than a little chary of any discussion that invoked "reality" in this globalised way.[17] The forcing of language to be both inside and outside "the world" as a preface to claiming that Austin came to the conclusion "there is no language; there are only acts of language" (p 318) is a complete mangling of the concept of performative utterance. It appears to be needed simply so that it can be used to resolve the outside/inside choice by denying the (false) distinction on which it is based.

If this does not amount to gross mistreatment of a concept, it is hard to see what would. And if that is not enough to warrant this judgement, hooking Austin, by implication, to the claim that "Scientific theories, models and statements are not constative; they are perfomative, that is, actively engaged in the constitution of the reality they describe" certainly is. In saying "I name this ship Britannia" or "Dissolve baking powder in vinegar", one is certainly doing things with words; naming a ship or suggesting some kitchen chemistry. What Austin would not have ever wanted to say is that what they were doing was constituting reality, not least because whilst one does successfully name the ship, the other does not dissolve anything in anything. Austin subsequently

[16] Interestingly missing out Grammar from the discussion of Logic and Rhetoric. Should we see this as what Callon himself would undoubtedly call a motivated absence?

[17] In setting the distinction up in this way, Callon scrambles it. Because performatives do *not* describe anything, he has actually re-defined them as constatives.

did treat all sorts of linguistic doings as doing social actions, but this was not to show the linguistic constitution of reality but to specify the different forces which utterances can have. Austin had nothing against ‘constatives’ as such, only against philosophers’ traditional and exclusive preoccupation with them to the exclusion of other linguistic forms such as performatives. He did not try to insist that all utterances are ‘performatives' rather than ‘constatives’ but, when he expanded on the idea of performatives, took the view that constative utterances also have performative force. Whatever else performative utterances might do for the notion of performativity (which is not much, we should think), they do not provide a bridge from language use to ANT's predilection for metaphysics.

If the treatment handed out to performative utterances is unfair, that handed to the notion of self fulfilling prophecy is even worse. The term (and its twin, the self defeating or "suicidal" prophecy) has its origin in Robert Merton's classic essay (Merton 1948) elaborating on W.I. Thomas' prior apothegm "If men define situations as real, they are real in their consequences". Although Merton illustrates the concept with the example of a run on a bank, the vast bulk of his essay is given over to an examination and explanation of the persistence of ethnic and racial prejudice in the USA. This is held to be the process of "moral alchemy" whereby "in-group virtues" of the dominant group in society "become outgroup vices" (p. 198). Here is an exemplary summary of what Merton means by this.

> *Thus, if the dominant in-group believes that Negroes are inferior, and sees to it that funds for education are not "wasted on these incompetents" and then proclaims as final evidence of this inferiority that Negroes have proportionately "only" one-fifth as many college graduates as whites, one can scarcely be amazed by this transparent bit of social legerdemain. Having seen the rabbit carefully though not too adroitly placed in the hat, we can only look askance at the triumphant air with which it is finally produced. (In fact, it is a little embarrassing to note that a larger proportion of Negro than of white high school graduates go on to college; obviously, the Negroes who are hardy enough to scale the high walls of discrimination represent an even more highly selected group than the run-of-the-high-school white population. (Merton 1948 p 200)*

Compare this to the summary of Callon gives of a self fulfilling prophecy explanation of the way economic theory works to produce that which it predicts.

> *Those who support the thesis of the self-fulfilling prophecy.....explain that if an economic model or formula can act as a convention (by nature arbitrary), it is because its object is human beings, whose actions and behaviours depend entirely on their beliefs and the meanings that they attribute to the social world surrounding them. (Callon Op. Cit. p 322)*

A little later, this becomes:

> *Whereas the notion of self fulfilling prophecy explains success or failure in terms of beliefs only, that of performativity goes beyond human minds and deploys all the materialities comprising the sociotechnical agencements that constitute the world in which these agents are plunged... (Op. Cit. p 323)*

What, for Merton, was a process whereby attitudes and beliefs were expressed through institutionalised patterns of action such as the deployment of resources, the organisation of schools, the quality of housing available to different groups etc etc is now caricatured as a statement about beliefs alone. The subtle working through of courses of action and *their* consequences is turned into a crude (and bizarre) strawman for ANT and performativity to tilt at. For Merton (and others who have used the notion of self fulfilling prophecy to describe the inertia of certain kinds of institutionalised behaviour as well as the crowd psychology that produces runs on banks), it is precisely that the beliefs by themselves are *not* enough. For the belief to be real in its consequences it needs the institutional arrangements to be in place: in the case of bank runs, the positive gearing of loans to deposits; the low ratio of cash to assets; as well as the physical properties of cash management. They know that a run on the bank can only occur with these in place. But these do not trigger the run; and it is what triggers the run and reinforces it which is of interest to them. Once again, ANT's monomania comes to the fore.

Tone Deafness and Colour Blindness

Given what we have just said, you would be forgiven for thinking ANT is tone deaf and colour blind to analytic differences and nuances. And you would be right. But the insensitivity to the subtleties of ordinary social life is probably even more telling and important. The misconstrual of other people's concepts and theories is as nothing to the re-working of ordinary experience and its casting into ANT jargon. In *What does it mean...?* Callon accomplishes the extraordinary trick of managing to do both at the same time. The prime example of this is to be found on pp 328 - 330 where Erving Goffman's (1969) dramaturgy is used as the departure point for an account of embodied interaction and then the failure of research in Economics to be taken up by business and commerce.

Callon begins with a quotation from Annemarie Mol which asserts that Goffman's *Presentation of Self* proposes that "(people) present not so much themselves but a self, a persona, a mask. They act as if they were on stage. They perform." Building on this, Callon says:

> *"We thus dissociate that which happens backstage and concerns psychology from that which happens frontstage and concerns sociology - the personal identity on the one hand and the public identity on the other." (pp 328-9)*[18]

Now Goffman was a racy writer and *Presentation* is full of colourful descriptions, quotations and examples. But he was also a careful thinker and an equally careful observer of social life with *his sociologising* certainly including what goes on 'off stage', much of that being understood in terms of its functions for maintaining the features of the public performance. What *Presentation* sets out to do is to explore the notion of role *as a metaphor* using conceptual props drawn from drama. He is not saying that people behave *as if* they were teachers, doctors, engineers, academics, mothers or whatever. They are those things. Of course, once we are sensitised by concepts drawn from drama we can, as Goffman does, provide startling and insightful descriptions of the social organisation of face to face interaction. Instead of just focusing on the performance centre stage, we can notice all of the backstage work together with the props and other materials that support it, the ways the effects are produced and the repertoire of skills the actors can draw on to be convincing. The reality of the performance and how we are convinced (taken in) by it can be

[18] En passant, another conceptual scrambling. Goffman's interest was sociological. He investigated the social organisation of both front and back stage. Thus to allocate personal identity to Psychology and public identity to Sociology is just what Goffman would not and did not do!

construed as the result of dramaturgical work. None of this says anything about how "in reality" social life must be nor that it is an endless game of charades. Neither does it commit us to the ontological demarcation of what in this game is psychological (in the head?) and what is not.[19] The multiplicity of metaphors that Goffman applied to the interaction order indicates that he was not searching for a single correct description but was viewing that phenomenon from different angles so see what features they made visible.

The insensitivity does not stop there. Following Mol's lead, Callon rejects any account of identity (who we are in any encounter) which does not place equal (?) emphasis both on social and psychological features *and* on the *materialities* of the "sociotechnical *agencement"*. Only such an account secures the reality given in the description. Once again we have the monomania of singular description. Goffman is to be dismissed first because he is thought to be arguing that identity is fixed by performance; and second because he was not providing ANT's dualistic account of what actually does fix identity. The latter is true but unfair; the former just plain wrong.

Even the most superficial reading of Goffman would show the deep concern he has with the ways that social actors can be described as endlessly producing and re-producing their array of complementary and discordant roles; and that they do that as much in circumstances thrust upon them as by the free rein of their own choice. What Goffman is interested in and sensitises us to is the repertoire of skills we as ordinary actors all have in continuously and relatively seamlessly moving through our social lives. For him, (and for us all, we would argue) our experience of ordinary social life cannot be reduced to the "constant struggle" that Callon tells us it is or must be (Callon p 329). Moreover, even though we find the social world is ordinarily experienced as a smooth flow of action, we know, and Goffman's analyses are exquisite, that sometimes things don't fit. We just get it wrong, or do things that are out of place, or misread situations, or whatever. We know that without the surrounding circumstances in place, some activities simply can't happen. To understand the mosaic of daily life, either as ordinary actors or as sociologists, we don't need talk about the materialities of sociotechnical agencements. Adopting this vocabulary adds nothing to and takes nothing away from the insights which Goffman and others have given us concerning the institutional and interactional character of daily life. All it does do is totally obscure them behind a cumbersome and superfluous jargon.

Repackaging as Revolution

It is not unusual in Sociology for the proponents of novel approaches to want to draw a sharp dividing line between what they propose and all that has gone before. However, the scorched earth policy which ANT takes to all previous sociological thinking (and the other social sciences more generally) makes one want to ask not so much what has been immolated, as what has been put in its place? If we give up on all that has gone before, what do we get? In our view, very little; or rather very little that we really didn't have before. The central motif of ANT and performativity turns out to be a re-visiting of the age old issue of individualism and the ascription of actions to individuals. Moreover, in *What do we mean....*, the treatment of this question, once it is stripped of the carapace of jargon, turns out to be a series of very familiar and somewhat tired moves whilst the stalking horse for the account of individual action is that old chestnut, the inadequacy of *Homo Economicus* as an explanation of social action.

[19] We touch on the issue of description and sociological re-description later in this essay .

The individualism which Callon targets is one which conceives the individual as bereft of all relational characteristics, not to mention all social relations; one which sociologists can easily be persuaded must be wrong because people have both those characteristics and those relationships. In that respect, Callon's argument about Economics initially follows a standard sociological form. The economic actor, *Homo Economicus*, is misconceived because it is such a denuded portrayal that we cannot imagine anyone of whom it would be an accurate picture. However, as Callon deploys it, the advantage of performativity is that this objection can be moderated somewhat. It is sometimes possible for economists to produce conditions under which people actually begin to resemble these models. Since such conditions might brought about by people conforming to the precepts of the theory or model, economists could be said to be making their models true by realising them in actuality.

More than 60 years ago, Gilbert Ryle (1954) pointed out the widespread confusions that exist over the nature of *Homo Economicus*. One of them is the stock sociological complaint about its inadequacy. The premise of the complaint is Economics and Sociology offer rival descriptions of one and the same individual; descriptions which are in conflict. The description provided by Economics gives a very different portrayal of the individual from that of Sociology. It seems that we cannot hold both. One must be sacrificed for the other. Thus the denuded one must be replaced by the 'fuller', more 'complete, even more 'realistic' description' that Sociology gives.

The delusion that these two descriptions are rivals is almost entirely due to the supposition that they are each descriptions of the same individual. But as Ryle points out, economic theory says nothing about particular individuals. It does not identify them or detail their attributes. It does not, for example provide descriptions of Ryle's brother in anything like the way that Ryle himself might —invoking such as listing his brother's age, occupation, residence, baldness and so on. The generalities of Economics cover, or apply to, Ryle's brother (and everyone else) in certain respects only. It has no need of or use for the sort of information about Ryle's brother that the police, say, might see as relevant to their very different purposes. Critics of *Homo Economicus* might think that they are being critical of a scientistic simplification, but in many ways their very production of the problem is itself the product of the retention of the same suppositions; namely, that science is a kind of master scheme to which all forms of description must be reconciled and reduced.

Against the dogma of the master scheme, Ryle suggests two strands of argument. He argues, first, that this is to read the contents of scientific works as if they were exclusive; as if, that is, a science's failure to mention things equates with a denial that those things exist. For Ryle, scientific schemes might better be understood as inclusive in that they do not feel the need to mention all the innumerable things to which they might rightly be applied. Absence of a mention of certain characteristics says nothing about the possession of those characteristics in actual cases, only about their irrelevance to the (scientific or economic) purpose in hand. Because of the way it has resolved the problematic possibilities of scientific description, Physics has nothing to say about the coroner's classification of a falling human body, but only about its rate of acceleration, the speed at which it will arrive on the ground and the force that will be delivered by the crash. It makes no difference to Physics whether the body fell, was pushed or jumped. It will accommodate all kinds of motion regardless of how they came about.

For Ryle, it follows that *Homo Economicus* is not an exhaustive view of the characteristics of individuals. If it were, it would be obviously false. Should economists be trying to promote such a position, one could only wonder how they imagine anyone would accept a view insists for example, that individuals have no families, friends, loyalties, politics, etc. However, since Economics is only

interested in individuals insofar as they are engaged in what Ryle terms 'marketing behaviour', buying and selling things, characteristics not specifically implicated in those activities are simply irrelevant to the economist's central interests.

Of course, care is needed here. Someone, a child, for instance, can instantiate the economist's basic model by deciding whether to spend the whole of their pocket money on ice cream or to save some to buy a comic later. We do not have to import model based decision programs into the commercial and financial world for the description to be convincing. The framing of this instance is enough. The child does not need to buy food, clothing and other items since it will doubtless have these bought for it. The priority of ice cream and comics have to do with the child's tender age. Characteristics like the age of the child do not particularly matter to the economist, and insofar as they do, they are simply the givens of the situation in which the economist is interested, namely the optimal satisfying of preferences in the context of limited resources. The interest is in finding a general solution to the problem of how anyone can trade off utilities in arriving at a purchasing decision. The only factors which matter in our case are the preferences for ice cream and comics, the price of these and the amount of the pocket money. It does not matter how the preferences were acquired, how the level of pocket money is determined, and so on. The economic modeller doesn't care, either, about the moral quality of those preferences, which, again, does not deny that they may differ significantly. Of course, we can accept there are determinate answers to such questions. It is just not the job of economic models of rational decisions to answer them.

It would seem, then, that far from being quite unprecedented for someone to satisfy the economists' model, it happens almost everywhere. Notwithstanding whether the model is actually useful, it appears that is it is complied with every time someone makes a decision about how to deploy limited resources to satisfy their preferences. There is no need to construe the model as portraying individuals as universal and relentless maximisers simply because it applies when and insofar as they are engaged in 'marketing behaviour'. The preferences which can be realised through marketing may be relatively few, and certainly are not necessarily the most important that people may have. It is, then, simply a misunderstanding to suggest that the model of economic rationalisation used by neoclassical Economics is bidding for Sociology's territory. Equally, it would be just as great a misunderstanding to suppose as, as some have done, that simply because rational decision models can be used to describe some aspects of some examples of social behaviour, they offer a universal model for the sociological description of social action. Construing all social life as the application of maximising strategies for choosing over preference hierarchies is just one-eyed.

ANT's second theme, namely the ascription of actions or individuation of action, is related to the issue of description and explanation. As with the rest of ANT, Callon insists that the only adequate explanation of social phenomena is to conceive them distributed networks of actants. This is a counter-individualist position in that, at least on Callon's conception of it, individualism must attempt to treat the doings of actors (and it is really *all* about actors, not the other kinds of actants involved in the doings of actors) as if they were entirely free (for want of a better word) of 'external' dependencies. We have just shown that, for even the most elementary of economic models, this supposition is false. Callon's efforts to counter individualist accounts of action provides nothing new at all to Sociology. In fact, all ANT does, and then in a clumsy and undiscriminating way, is address Sociology's central problem, namely the individuation of action. Callon asks: what is the source of the action? Rather than initiating some new and radical strategies for sociologists, what we get a rather poorly formed version of an old question. In response to Callon, one wants to ask: whose action are we are talking about? After all, it is not as if we can identify an action independently of determining who is undertaking it. Whether we can or

need to ask about the source of an action depends very much upon how we identify the action in the first place. Callon offers 'piloting' as an illustrative example. If we are asking in a general way about who is piloting a plane, we can answer variously with 'the aircrew', 'the pilot', 'the co-pilot', 'the autopilot' or, if all four engines abruptly cut out at thirty thousand feet, 'no one'.

For Callon, this would be inadequate as a basis for a sociological description because it leaves out the *sociotechnical agencement*, the surrounding panoply of material, social and other conditions. This is surely just as misguided as his account of Economics is. The fact that things don't get mentioned is taken as tantamount to obscuring them rather than presupposing them, whereas many things are not mentioned because they are simply presupposed for the purpose of giving a description. Callon's example of 'piloting' presupposes that it is aircraft and not ships which are being piloted. It also presupposes readers' familiarity with the piloting aircraft e.g. that a pilot is dependent on a vehicle to pilot, does not carry out keeping the aircraft in the air by personally providing the energy for lift, controls its movements through the use of an instrument panel and not through direct mind-emanating contact with each of the 747's over 6 million parts to keep them flying in formation, and so on). Callon's question belongs to that class of sociological questions which seem as if they are challenging us to rethink our usual conceptions when, in reality, they indispensably trade on those very conceptions. Everyone knows flying large commercial passenger aircraft is very much an organised activity. It involves all sorts of complex dependencies, teamwork and so forth (unlike, say, flying a microlite). But the fact that the pilot's work is assisted does not imply for one moment that one cannot (a) determine what part in the interdependent and collaborative activities involved in the flying of modern commercial jets is played specifically by the pilot; nor (b) that one cannot construct a model of the pilot's decision making without having to supply the wiring diagram for the many miles of cable that run through a modern aircraft.

Two further muddles which follow from the above can now be brought out. Collective and individual action are not a contrast pair. The fact that there are collective achievements does not militate against the individuation of the contributions to such collective achievements. Thus, the fact that the army is victorious does not prevent identification of someone's contribution to the victory through the feeding of the combat troops, nor as the piloting example suggests, of treating some contributions as more directly related or more crucial to the collective result.

Second, we have insisted that the request for an ascription of an action is often provided by the specification of that action. If one wants to model the pilot's decision making then the question must be how to specify the pilot's doings (which can feature the questions 'what is the pilot doing *as opposed to the co-pilot*?' 'what is the pilot doing *as opposed to what is done automatically by the control systems*?' and so on). Answers to these questions will provide input for a model of the pilot's decision making (since, after all, they will determine how much and which parts of the pilot's doings are actually decision making). The pilot isn't making decisions about the route and direction of the flight but is carrying out the scheduling requirements of airlines and airports as stated in the flight plan. The pilot isn't choosing directions and headings, he is under the direction of air traffic control systems and so on. An understanding of these clearly would provide assumptions for modelling the decisions that airline pilots actually make,

Conclusion - The Irony of Performativity

The central tenet of ANT is that conventional Sociology (though actually the real target is the Sociology of Scientific Knowledge) is committed to an ontology which privileges the social above all else. And, to be fair, within Sociology generally, and certainly that particular sub-thread, the tendency to descriptive hierarchism is pretty rife. However, surely the most effective way of

countering this tendency would be a firmer and clearer constitution of the distinctiveness of Sociology not an insistence that all previous accounts must be thrown over in favour of a new version which has to be amalgamated with those of other disciplines?

For ANT, the commitment to the explanatory priority of the social, means that Sociology creates a world, an ontological structure, which is blind to the contribution the non-social makes to the genesis and shaping of social action. Conventional Sociology's world is held to be both partial and distorting. It is, therefore, deeply ironic to find that, in all its accounts of how conventional Sociology should be superseded, we are presented with what are themselves partial and distorted versions of hackneyed issues. As we have seen with *What does it mean...?* ANT proceeds by creating a motley of imaginary targets, cyclopean epistemologies, forced dichotomies, mashed concepts, bizarre accounts of ordinary experience, and ill informed re-workings of old issues. These constitute the world in which performativity is to be located, explained and justified.

Having set out to confront what it saw as distortion, ANT ends in a characterisation of social life and social action which is nothing but a weak parody of just the kinds of accounts it wants to overthrow. Not only does performativity provide us with an underpowered version of familiar Sociology, when stripped of jargon the supposed insights and explanations are trite. Sociology has always been interested in the ways "Men make history, but in circumstances thrust upon them" (as Marx and Engels put it). Generations of sociologists have toiled to elucidate just how and when and where and under what circumstances. ANT's vocabulary of *actant*, *agencement* and the like adds nothing but obfustication to this tradition. In the end, as a contribution to Sociology, Actor Network Theory is deeply uninteresting.

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*

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

> *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 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 run throughout it, 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 presented as such. 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 nouveau 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 takes one to the top 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

[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"

represent how the world is independently of such practices? And what does the answer to this question mean for the authority of science? Suspending 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 specified in Philosophy of Science. 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 commonplace *methodological stipulation* with regard to the study of science as a social institution has, step by step, led into 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 disappeared into the background.

Missing Your Way at the Start

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 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.

1. 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,

[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.

[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 that 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.

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).

2. 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:

1. 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 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).
2. 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

[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.

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?'

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.

---

[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 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 invariably *causal* (and hence law-like or proto law-like) they are commonly *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

[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).

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 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

[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.

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 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

[29] This is a familiar ANT strategy.

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 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*

[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)

> *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 – 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.

THE NEMESIS OF METHOD

Introduction

One way of describing postmodernism is as a campaign to re-balance intellectualised privileging. That is, postmodernist interventions in any domain always question the privileging of one side of an intellectually specified dichotomy. Thus, we have attacks on the privileging of text over speech, writer over reader, reflection over action, theory over practice, reason over emotion, the objective over the subjective, and, of course, Philosophy over morality (or ethics or politics, depending on exactly who is undertaking the campaign).

In this essay, we take up one such attempted re-balancing, or rather *set* of re-balancings; between disinterested observation and engaged intervention, decontextualised and contextualised understanding, what once used to be called emic and etic descriptions, and perhaps most of all, between what nowadays are most often described as grounded and abstracted theory. The potential outcome of this re-balancing is, of course, to be a levelling of descriptions. None is prior or more fundamental than any other. In the domain within which this particular debate occurred, however, namely qualitative approaches in Anthropology and Sociology, it has had one further consequence. Because of those disciplines' proclivity for what Clifford Geertz (1988) once called "moral hypochondria", a new meta-research task has been ordained. This is the requirement to engage in methodological reflexivity. Such reflexivity is now (at least in some places and for some discourses) the hallmark of authentic social science.

Of course, postmodernism as a mode of thinking was not the prime mover here. A distaste for scientistic forms of cultural analysis had long been a strong theme in qualitative social science. This was married to a disenchantment with the proffered alternatives to surveys and statistical modelling. The collecting and classifying of objects, events, rituals, indeed all cultural forms, (alternative techniques appearing to have been derived from natural history rather than natural science) failed to provide insight into the subjective experience of members of different cultures. Programmatic summaries by senior figures such as Clifford Geertz set out the frameworks for a

new departure centred on what was called "the interpretation of cultures" (Geertz 1993). Such interpretive stances explicitly and determinedly rooted themselves is the presumption that as social actors we are all "suspended in webs of meaning" (to quote Geertz yet again). The purpose of ethnography as the method for the interpretation of culture was, as far as practically possible, to capture and represent "the native's point of view".

Reflexivity as Method

It was into the junction of these points of view, those of the academic, disinterested researcher and the practical, engaged social actor, that postmodernism drove its wedge. This was first framed in terms of the impossibility of bridging, or otherwise overcoming, the social, political, economic, in short *cultural* gap between the researcher and the researched. Despite the researcher's best endeavours, without such bridging it was inevitable any rendering of the setting would be in terms which were alien to it. The researcher's point of view would be privileged over that of the researched. Furthermore, in as much as members of the researched society became engaged in the professional activity of social science, and therefore learned to see their culture in terms framed by that discipline, it would be alienating as well. What the first challenge seized upon, then, was what was interpretive social science's entanglement in the transition from colonial to post-colonial power structures.

However, a second challenge was also posed. This was regarding the consequences of introducing the metaphorical elision between interactional and textual understanding which interpretive social science was promoting. Interpreting the activities of a culture was said to be like deciphering a palimpsest or translating an ancient text. Reading culture was an exercise in hermeneutics. But, just as it had with literary criticism, postmodernism questioned the possibility of such readings. It questioned both whether the conception of pre-given text to be read did not privilege the writer over the reader and whether the notion of a ground truth to be captured and represented in any account was coherent. As Stephen Tyler put it:

> *A post modern ethnography is a co-operatively evolved text consisting of fragments of discourse intended to evoke in the minds of both reader and writer an emergent fantasy of a possible world of commonsense reality, and thus to provoke an aesthetic integration that will have a therapeutic effect. (Tylor 1986 p125)*

By extending it to the gap between the observer and the observed in locales which were far more familiar, the caesura between reader and writer to which Tylor points was turned back on itself. Postmodernist arguments in Queer Theory, Feminism and Sociology of Ethnicity, to name but a few, questioned the authenticity of accounts of forms of sexuality, gendered lives, and minority cultures in *our* culture where the biographical disjuncture of the researcher and the researched might not have been so obvious.

Once the genie of biographical justificationism was out of the bottle, the scramble to assert personal grounds for authenticity got underway:

> *In my recent interview work with Black gay men living with HIV/AIDS .....I seek to help these men tell their stories; stories that traverse the boundaries between death and life, between loss and gain, between fear and a powerfully embraced self-determination; between acceptance and regret, and between regret and a righteous transcendence into self knowing ..... And once again, although I claim a particular membership as a Black*

> *gay man, but one NOT living with HIV/AIDS, I claim full membership in this community of men as a mourning subject—as a man who has lost a biological brother and a host of cultural brothers to AIDS. I seek to include voices of Black gay men living with HIV in the discussions of AIDS, discussions that are shrouded in secrecy in the Black community and racially erased from the public discourse of AIDS. And maybe this is not "real ethnography"* per se, *per se now as a noun that names the particularity of a thing. Though my intensions are not just to capture some aspect of their oral history—but to contextualize lived experience within a cultural community (gay life, the era of HIV/AIDS, and the politics of activism) that is both about the intersectionality of Blackness, gayness and the category of man that makes salient these characteristics, but also in a larger culture of discussion and silence around the particularity of their predicament. (Alexander 2011 p99)*

As Alexander goes on to set out, the mode of research reporting that is now required is one which begins and ends in the particulars of the researcher's biography.

> *So I do not do "traditional ethnography" per se.* Per se *in this sense as an adjective signifying the oxymoronic relationship of not really, but really—for I am engaged in a radical revisioning of relationships and spaces of possibility in human social engagement—in the classroom, in the Black community, in academia, in the embodied presence of being a Black gay man, and other locations that I find actual and mythic representations of my body circulating and preceding my arrival in time and space—feeling and resisting the reflective appraisals of others. So I unapologetically locate myself in my fields of study. (Alexander Op. Cit. p100)*

This "location" results in a new, hybrid method which Alexander calls "auto/ethnography".

> *My own approach to reflexivity in ethnography allows a space of opening, a space of reconciliation between objective facts and emotional response to critically reflected upon experience, on what we know and how we came to know it. And this same reflexive component in auto/ethnography, both written and performed, helps to provide a template on which the auditors of these texts might follow suit; applying the method (or approach) to significant aspects of their own cultural experiences and ways of making sense of experience. (Alexander Op. Cit. p 101)*

For many in the discipline, reflexive auto/ethnography induces vertigo. Finding a secure place from which to say anything definitive about the domain under study becomes almost all consuming. Here is a not untypical rendering.

> *It is the task of each researcher, based on their research aims, values and the logic of the methodology involved, to decide how best to exploit the reflexive potential of their research. Each researcher will choose their path - a perilous path, one which will inevitably involve navigating both pleasures and hazards of the marshy swamp. For all the difficulties inherent in the task, to avoid reflexive analysis altogether is likely to compromise the research. The swamp beast still needs to be confronted as MacMillan's ..... reflexive poem captures so eloquently:*

> *Reflexivity, like hypnotherapy, has various levels.*
> *Some dabble near the surface,*
> *dipping into reflexive moments, flirting with the images evoked in the reflection,*
> *before returning to the safety of the mundane.*
> *Others attempt to confront the fear of the monster lurking in the abyss*
> *by descending into the deeper realms of reflexivity. It is those who confront the beast*
> *who will truly know what is there, in the dark beyond . . .(Finlay 2002 (b) p 227)*

In a related piece, the same author describes the attraction of the method by using a term which is redolent of the dynamic which this line of thinking was serving.

> *"Coming out" through reflexive analysis is ultimately a political act. Done well, it has the potential to enliven, teach, and spur readers toward a more radical consciousness. Voicing the unspoken can empower both researcher and participant. As more researchers grasp the nettle, the research in the future can move in new, creative directions. Are we ready to embrace the challenge? (Finlay 2002 (a), 543-4)*

The mandarin detachment of Malinowski and Evans Pritchard has morphed into political activism. Answering Howard Becker's rhetorical question 'Whose Side Are We On?' (Becker 1970) is now the first task in formulating a research programme.

Stepping Back

What seems to be happening here is the substitution of one form of legitimation or grounding of social science description by another. The levelling of the metaphysical antinomy of 'objectivity' and 'subjectivity' has not led to tolerance for both but rather the privileging of subjectivity instead. Since there can be no externalist 'view from nowhere', all views from somewhere are subjective, partial and political. Without countervailing acknowledgement and acceptance of such bias, the accounts given by researchers cannot be other than exploitative and oppressive; providing accounts of *their* views of the world in terms of *our* views of the world. The task of postmodern interpretive social science, therefore, is to challenge that exploitation and oppression and to reveal them for what they are.

This is worth teasing apart a little, if only because, as we will see in our discussion of Cultural Sociology[31], this line of reasoning is very attractive to students and tends to engender a fair degree of intellectual giddiness among the unwary.

Perspectivism rests on an argument with four distinct parts, none of which is, *ab initio,* implausible.

1. A succession of scholars, most notably Thomas Kuhn and those working in the Sociology of Science, have demonstrated that the institutionalised ideology of scientific objectivity and progress is not an accurate representation of the way science is carried on. It follows that attempts to base social science method and approach on that ideology are at best misguided.

[31] See Essay 6

2. Interpretive social science deliberately eschews attempts to copy the physical and natural sciences both in its definition of its topic (subjective meaning and experience) and its definition of its method (qualitative analysis).
3. Analysis of meaning in Linguistics, Philosophy and Hermeneutics stresses that meaning (of words, sentences, assertions, propositions, and texts) is reflexive on the context of interpretation. Meaning is as much found as left.
4. To access the meaning of cultural forms, researchers have to immerse themselves in and, at least in significant part, come to share the culture they study. This cultural sharing resolves the reflexivity of meaning.

As we say these are not unreasonable positions to adopt. But then we get a major *non sequitur*.

5. Because the researcher does not actually share the culture (does not go completely native, so to speak), in the end, no full, authentic, fully reflexive understanding is possible. The outsider view will always predominate and this will be a partial, distorting and biased rendering of the insider perspective.

However, this last assertion does not follow from propositions 1-4 above. Or rather, it only appears to follow if we privilege as the basis for social scientific description what we will call 'the native's point of view' over the researcher's. This reverse privileging rests upon two further mistakes.

First, underpinning the inference is the assumption that there is a single, definitive, universal fact of the matter description which is 'the native's point of view'. This is no more than the reintroduction of the positivist Holy Grail, albeit in a new form. What we end up with is not 'scientistic positivism' but a 'subjectivist' one. Moreover, if we hold, as John Law (2009) does, that it is interpretations all the way down, the choice of interpretations becomes the problem. On what grounds do we come to the conclusion that the native's account of what he is doing is a better, more insightful, more fertile a basis for social science reporting than the interpretations of the social scientist? We hardly want to say that the native is engaged in doing social science, do we? What he or she is doing is answering questions and performing routine activities as part of their practical now-being-observed-by-the-social-scientist daily lives.

Second, the argument ignores the findings of those parts of Linguistics and the social sciences which have studied meaning and its resolution in everyday language use. These studies point to an array of practical methods which those co-present in some setting call upon to manage and determine meaning in context. In as much as they sustain interaction at all (and Geertz's (1993) descriptions of the difficulties and fragilities of so doing are revealing), fieldworkers and their informants achieve a here and now, in this situation and setting, common sense understanding of what is going on. And, as all fieldworkers know, this working, local knowledge (different from but bearing some family resemblance to the native's working, local knowledge) is all that they have to go on to form their descriptions. What this illuminates is not the political nature of the bridge thrown over the cultural gap between researcher and researched but its *practical* nature. Doing the work of fieldwork investigation is just another domain of practical action, and managed in and through the methods and practices which all social actors have for providing and assessing what are subjective and what are objective accounts and descriptions. The objectivity and subjectivity of accounts and descriptions found within social activities are produced through deployment of and adherence to such conventionalised and commonsense practices.

Accepting all this does not mean that fieldworkers (*and* their informants) cannot be crass, boorish, prone to misjudgement, misunderstanding and distortion. But it does not make fieldwork

inevitably so. Neither does it imply that making sense of some unfamiliar culture is not hard; that somehow it is easy just to slip in and immerse oneself. All fieldworkers know the uncertainty, disorientation and sense of being at a loss what to do which they feel on entering the field. (The further the cultural distance the research site is from the researcher's home base, the greater these feelings). But, as feelings, these are no different to those felt by any novice, newcomer, stranger. Moreover, they are addressed and resolved in much the same way.

Two things follow from what we have just said. First, reflexivity is not a special problem for fieldwork in Qualitative Sociology. It is everyone's problem. Second, its resolution is not a matter of imposing distortion, bias and politics, but instead a matter of the practical management first of social interaction in the setting, and second the construction of sociological accounts of what was encountered there. It becomes, that is, an issue of the practical management of routine sociologising.

Prioritising the Subjective

Earlier we attributed the move from objectivism to perspectivism in the social sciences to a predisposition to moral hypochondria. And in part that is so. Disciplines so determined to be on the side of right are likely to be vulnerable to claims from others to the moral high ground. But it was not all this. There was, in addition, something in the very logic of the way that the turn to subjectivity was introduced into the disciplines which contributed as well. Well over half a century ago, a paper which deserves much greater celebration and prominence than it has had, Egon Bittner identified this logic. In *Objectivity and Realism in Sociology,* Bittner (1973) expressed the fear that rejection of the operationalisation of meaning and proceduralising of formal reason, ideas which were in place and dominant in American Sociology in the immediate post-Second World War period would lead to another, equally elusive and illusory aspiration, namely that of authentic descriptions grounded in subjective experience. For Bittner, the substitution of subjectivity for objectivity was unnecessary. In addition, forcing a choice would, in all likelihood, lead to the consequence that the 'subjectivity' which became prized would actually be that of the sociological researcher. As we have just seen, this is exactly what has happened.

The search for 'authenticity' was the primary driver for the proposed shift to the subjective, where authenticity was assured by immersion in and engagement with the setting under view. The aim was the presentation of social reality as seen from within rather than from without. In Bittner's view, this would become an objective which, in practice, would be frustrated by the fact that the researcher would enter the field bearing a burden of preconceptions drawn from Sociology. The end result would inevitably be creation of a substantial disparity between the experience of inhabitants of the social setting and that of the visiting researcher would become integral to the methodology's practice. In this way, the search for authenticity, though inspired by Phenomenology, would become a distortion, or even abortion, of the phenomenological project.

For Bittner, rushing to embrace the fullest form of subjectivity would be likely only to bring its own troubles. First, there is the risk that what will dominate investigative interests are the enthusiasms and/or preconceptions of the investigator. Even if this is avoided, the desire to present an account of reality from the point of view of the actor must "return", as Bittner puts it, to an "objectiveness" but one that this time is grounded in intuitions gained through 'being there'. But this warrant, this being there, can only come at a cost.

> *The greater the effort to enhance the adequacy of observation on counts such as acceptance, transfer of trust, subtlety, perspicacity, open-*

> *mindedness, patience and scope, the less likely that serious, searching questions will be asked about that which has come to view by means of all this loving care.....It is not whether he observes well or poorly that matters but the circumstance of his being an outside observer with all the consequences issuing from it (Bittner 1973 p.119)*

This unease was justified. As we have seen, reflexive ethnography commits the very mistake Bittner points to, namely of assuming that because of the intervention of the sociologist as an observer of the social setting and the social and cultural distance between the sociological observer and the members of the society under study, reflection on the researcher's own experience *vis a vis* that setting must be a central and critical concern when describing social life in some setting. To use the image that is most often deployed when explaining why this must be so, without an understanding of the lens through which the observations are focused, there is no possibility of compensating for any partiality or distortion of the sociality under view.

This mistake underlies the conundrum of how the researcher is to offer an analysis which both respects the view of social reality as seen from within and is recognisably and properly sociological. How can you be both inside and outside at the same time? How can you capture and represent *their* interpretations within the framework of *your* interpretation? How do you treat their point of view with respect without sliding into cultural relativism or an interminable regression, or by distorting their point of view through your own presuppositions?

Bittner acutely foresaw that attempts to correct positivism's misrepresentation of society in the name of subjectivity-as-experience would induce comparable, though substantively different, distortions. Positivist objectivism sought to access social reality through faithfulness to methods designed to depersonalise inquiry. The inversion of that position envisages access to social reality through the personalising of inquiry, through faithfulness to the subject. Neither approach encapsulated what Bittner considered the genuine, phenomenologically appropriate orientation of *faithfulness to the object*, which in this instance would be to social reality as experienced from within its midst. For Bittner, 'objective' and 'subjective' are not to be counterposed and polarised and so we are not forced to choose between them. Rather, the challenge is to achieve greater clarity about their relationship; that is, the proclaimed objectivity of social reality as it is present in social settings and intelligible to those who inhabit those settings.

The error Bittner is pointing to consists in the mistaken assumption that because experience is primeordial within social life, it must have primacy for sociological descriptions. Whereas, of course, the aim of inquiry conducted in this way is not to seek to persuade anyone that social reality is really only the subject's motile artefact any more than it is to demonstrate that determination of the real structures of social life is obstructed by layers of subjective misconstrual. As Bittner saw it, the need was to do justice to the patent *and overwhelmingly unquestioned* objectivity that social structures do have in our daily lives. In this, Bittner was drawing upon Schutz' characterisation of the natural attitude of everyday life

> *By the everyday world is to be understood that province of reality which the wide awake and normal adult simply takes for granted in the attitude of commonsense. By this taken-for-grantedness, we designate everything which we experience as unquestionable; every state of affairs is for us unproblematic until further notice (Schutz & Luckmann 1974 pp. 3-4)*

As we have noted, Bittner is clear that Phenomenology (together with its troublesome step-child Heideggerian Existentialism) was the inspiration behind the turn to the subjective. However, the elaboration of what this entails led to the situation described earlier. The phenomenological project was built on the assumption that the life world, the world of everyday social life, is available to observation and understanding *prior to* the production of any scientific or analytic scheme for its further examination. Moreover, phenomenological inquiry is needed for the clarification required as a propadeutic to the 'understanding' of social reality through the adoption of scientific (or at least theoretical and methodological) principles. Among other things, this inquiry would set standards to fix what 'understanding' was to be. Phenomenological investigation is, then, *prior to* understanding of social life through the adoption and operationalisation of some set of methods and/or principles. The risk for any objectivist approach is that, without such secure determination of correct standards and ways of understanding, social reality will remain unknown. For Phenomenology, on the other hand, if social inquiry is stipulated to be *apriori,* then sight might well be lost of the social reality that is the site and setting of the inquirer's own inquiry. An approach to the study of social life chosen *apriori,* might simply fail to recognise that the understanding of social reality is present in social settings and available to those resident there. It is on this understanding that the affairs of everyday social life actually run. As the struggles with it make abundantly clear, the conundrum of reflexivity confronts us only because of the assumption that the social researcher is seeking a special, primordial understanding of social reality. Bittner diagnosed the importance and consequences of this assumption well before reflexivity became the *topic du jour* that it is now.

Drawing It All Together

We have been examining just one strand of the debate over reflexivity in social science. One which might fall under the 'positional' rather than the 'textual' categorisation Doug Macbeth (2001) uses. We have suggested that the search for some special form of methodological reflexivity as part of a distinct attitude towards research engagement which will overcome the privileging of outsider, analytic, objectivist views is both naive and misguided. There is no coherence to the claim that some set of interpretations and descriptions is reflexive and some other is not. All interpretation and description is reflexive, including those in ordinary talk or in formal institutionalised settings such as science. It makes no sense to define some forms of Sociology as being reflexive and non-reflexive and set them against each other, just as it makes no sense to contrast science and commonsense as unreflexive and reflexive. All forms of commonsense reasoning rest upon reflexivity and its management.

This conclusion has two implications. First, it opens up the activity of practical sociologising as a topic for enquiry. We can turn to sociological reasoning as instances of the management and resolution of the reflexivity which is essential to all practical activity. What is 'the work' of field work? What is 'the work of participant observation' and how can the 'members' methods' that comprise this work be best described? To do this is to do no more than take an analytic interest in the mundane reasoning of Qualitative Sociology. Of course, as Michael Lynch (2000) suggests in the summary of his mammoth cataloguing of reflexivity, such descriptions are likely to be of little or no interest to fieldworkers, pointing as they will to the mundane, taken for granted, culturally invisible, ordinary features of sociological life. Such descriptions will be "essentially uninteresting" and as such all the more testimony to the power to sustain social life of the phenomena they depict.

The second implication is that in levelling down reflexive ethnography from the privileged position it is given, its potential to be revelatory, politically radical and enlightening will have to be

set aside. If ethnography can have no special claim to reflexivity then it loses its aura. With that loss would also go any hopes to use it as the vehicle for political action. As Lynch says, such action and the ....."(h)opes for enlightenment and political emancipation (which it carries) would then return to the streets where they belong" (2000 p. 48).

THE ENDLESS PURSUIT OF THE NEXT NEW THING

Introduction

A main theme of the essays in this volume is the unfortunate impact which postmodernism has had upon social science. The 'radical' re-thinking which this socio-philosophical approach demands has caused no end of muddled theorising. Nowhere is this more apparent than in those parts of Sociology which take an interest in the particular cultural forms of contemporary western society. As we have seen (Essay 1), the roots of post-modernist social analysis lie in part in the analysis of cultural artefacts to be found in modern homes. These *anthropological* (in the best sense of the word) studies have been taken as licence to extend postmodernist critiques to every facet of our lives and thus to create a whole domain of investigation aptly called *Cultural Sociology*.[32] The pervasiveness of post-modernism within the human sciences and the common theme of critique which is thereby espoused has resulted in a particular and peculiar fusion of social analysis and cultural commentary. From the global dominance of a small number of (often American) brands, to the obsession with health and fitness, to the awfulness of reality tv or the fashion for tattoos, whatever broadsheet Sunday Supplements are preoccupied with today is likely to be the next, new topic for Cultural Sociology. All the above have been grist for its deconstructive mill; each in turn analysed as but the latest instance of the insidious reproduction of post-capitalist consumer or power relations (or both). And so it is with the latest topics taken up: Web 2.0 and the services and applications it has spawned, as well as the fast emerging phenomenon of pervasive computing itself (i.e. the so-called 'internet of things'). Luminaries such as Scott Lash, are currently forming a bandwagon to roll out the definitive sociological account of the place and significance of digital technologies in modern society.

Now, at one level, this should not unduly perturb us. There is nothing new in forms of Sociology drawing their research directions from the concerns of the 'educated elite'. Second, as we have

[32] Of course, the social sciences have always been centrally concerned with culture. However Cultural Sociology embodies a distinctive take on what previously had been the province of the Sociology of Culture. See Nathalie Heinich (2010) for a particular view of this distinction.

mentioned before, the capacity of sociological theorising to shape phenomena to fit its predispositions is almost its hallmark. Nothing would surprise (indeed startle) us more than to come across a study which definitively concluded that its favoured concepts were of little or no value in explicating its chosen topic.

At another level, though, apart from the banner headlines, breathless fervour and Henny Penny warnings of imminent disaster in which the analyses are couched, there is something distinctive about Cultural Sociology. In its pursuit of novelty and relevance, Cultural Sociology represents a new modality. With its concern with consumer goods and consumer patterns and its predisposition to follow fashion, Cultural Sociology is itself taking on the characteristics of the cultural form it critiques. It provides easy to assimilate, pre-digested, boil in the bag sociological explanations which are fitted to the contemporary world of its analysis.

Because it is the stick that broke the back of our fortitude, we will use the discussion of recent digital technologies as the stalking horse for our examination. Quite unashamedly, we will cast it as a model for all Cultural Studies. By looking at this example in depth, we will try to bring out just how much:

1. The claims that are made about the phenomena studied are wild exaggerations based upon misapprehensions. These exaggerations arise because of the need to force the phenomena into the straightjacket provided by the concepts ready to hand;
2. The concepts which are deployed not only homogenise analyses so that they are easily understood but indistinguishable, they also direct attention away from those difficult to summarise and analyse features which make phenomena novel or distinctive and about which social science might, indeed, have some interesting things to say;
3. The mode of analysing (deliberately?) substitutes stipulation for discovery and in so doing violates its own methodological grounding. The result is a bundle of lightweight casual, mock-causal stories masquerading as explanation.

Since recent innovations in digital technologies are the ground against which this assessment will take place and because we find the portrayal offered by Cultural Sociology so inadequate, we will begin by briefly summarising (in as non-technical a fashion as possible) just what these technologies are and what makes them interesting and innovative *as technologies*.

Web 2.x, the Cloud and The Internet of Things

The pace of change in the world of digital technology is truly astonishing. One has only to reflect that just 15 years ago, the internet was unheard of outside academia and R&D labs and mobile telephones were almost the size and weight of bricks.

At the heart of these changes are three common trends:

1. The astonishing durability of Moore's Law;
2. The extraordinary resilience of communications networks;
3. The universalising of the web browser as user interface.

Moore's Law is the well known prediction that the number of transistors on a chip doubles approximately every two years. What has been powering this has been relentless improvement in chip fabrication methods and technology. The net result has been a continuous logarithmic increase in the power of chips together with an associated scale reduction in their size and cost. As a

consequence, the size of the devices they power has also shrunk whilst, at the same time, delivering an increase in the computing power they can call upon. Today's mobile phones computationally are more powerful and flexible than the desktop pc of the 1990s.

The internet was designed to be a resilient communications network. It was funded by DARPA to provide a way of coping with any possible targeted missile attack. At its heart are two elements: the address system which provides an identifier (the IP address) for every device connected to the network; and the communications protocol (TCP/IP) for managing the communications. TCP/IP relies on multiple redundancy in communications paths. As the internet has grown, the protocol has shown a remarkable capacity to respond to scale. Equally, successive versions have allowed the expansion of the address system. The latest version (IPv6) which is now being rolled out will, in the words of our ex-colleague Craig Mudge, allow "everybody's toaster to have an IP address". That is, with IPv6, the number of 'devices' which can have internet addresses (and hence be simultaneously available on the internet) will become effectively limitless.

The scaling up of the internet has in turn been driven by the growth in the world wide web and, in particular, the way that the web browser has become the favoured user interface for almost every application. What this did first was to allow individuals and organisations to publish (static) web pages on web sites. The web of sites could be searched by search engines and pages read through browsers. With Web 2.0, pages have become active and able to incorporate multiple media. The use of Java, XML and Flash technologies alongside the traditional HTML have enabled pages to be interactive and linked. It is Web 2.0 that has allowed the explosion of blogs, podcasts, social networking sites, as well as growth of shared repositories such as Flickr and so on.

More recently, two further developments have introduced yet more possibilities; 'cloud computing' and 'the internet of things'. In one sense the cloud has been with us ever since we had networked distributed computing. For example, projects such as SETI used the spare cycles donated by their owners of machines connected to the internet to undertake the computation tasks involved in the search for extraterrestrial life. Recently, though, new businesses have sprung up offering to provide data management services by hosting data across the internet. This has two key advantages. Customers no longer have to provide space and management resources for their data. This is proving a valuable financial saving for businesses and a relief from what was often poorly managed by individuals. Second, because data is stored in multiple places across the internet, data access and protection from loss is 'guaranteed'. The consequence of all this is that data repositories, both corporate and individual, are now migrating to the cloud.

Mobile telephony has ridden Moore's Law and mobile phones have moved to become internet devices. This has further reinforced the scale/power/cost trend but at the same time expanded the services into location-awareness and image capture. Almost anything (in fact just about anything) can now have "embedded computing" and with it the capacity to know where it is, capture information through sensors, and communicate this information across the internet. This trend has been called 'pervasive' or 'ubiquitous' computing and the extension of connected devices has been dubbed 'the internet of things'. The race is now on to provide valued services which take advantage of the fact that not only can the data generated by pervasive computing be linked and integrated but also that integrated data can be further linked and integrated with other public or 'open' data sources. The range of these services and how valuable they might actually be is unclear just now, as are the business models for them. Nor is it clear exactly how the balance between deep personalisation and robust privacy protection will be set.

Fears, Fantasies and Assorted Hobgoblins

C. Wright Mills taught us that the sociological imagination is a wonderful thing. Unfortunately, at times, it can also run away with us. This is precisely what has happened to Michael Beer with regard to the implications and hence significance ofthe technologies we have just outlined (Beer 2009). For Beer, these technologies have crept up on us (as we will see, quite who this 'us' is deserves some elucidation) unbeknownst and now have an insidious, pervasive and menacing presence in our lives. The menace comes equally as much from the fact we don't notice them, or even know they are there, as from the power they might exercise over us and the way they could channel our experience and behaviour.

The Technological Unconscious

What has aroused Beer's concern is the fact that digital technologies are everywhere, always on, and interact with each other in ways we cannot see and do not understand. All this information gathering, communication and re-purposing has crept up on us and we have not grasped its significance. We don't understand how it works and we don't understand what it is doing for and to us. Except, of course, it hasn't crept up on us and its significance is very little different to the significance of other more traditional technologies with which we are very familiar.

To get a flavour of the hyperbole that Beer (and others) deploys, let us offer you a lengthy quotation.

> *Creating insights where software and the web are so much a part of how we live is inevitably fraught with difficulty. Not only do we have this problem of familiarity but, as already mentioned, these communications technologies often operate at the level of the 'technological unconscious' (Thrift, 2005). In other words, they operate in unseen and unknown ways. Unsurprisingly then, researching these systems is highly problematic.....*
>
> *.....What we have are forms of power that are reactive, concealed, and which are shaped on the ground at the multifarious points of communication. However, it is possible to begin in this article to make some connections between Web 2.0 and post-hegemonic power that might be useful in shaping an agenda for research into participatory web cultures - an approach, that is, that takes account of the way Web 2.0 interweaves with the 'technological unconscious'. (Beer 2009 pp 995-6)*

Two different orders of claim are being made here. First these technologies are so different that that standard concepts of social science (well, Sociology (well really Sociology of Culture)) cannot cope with them and so new concepts such as those of post-hegemonic power will have to be forged. Second, the reality of this difference is to be found in the way the technologies appear in our everyday lives. They are everywhere, so much so that we don't notice them. And they are concealed, so much so that we don't understand them. It is the fact of this dangerous insidiousness that is pressing the need for conceptual innovation in Sociology.

Both these claims are more than a little forced. Just how different are these technologies *as technologies* in our everyday lives? Do we need new concepts? and if so are the ones Beer prefers (i.e. Scott Lash's neologisms of 'post hegemonic power' and 'algorithmic rules') actually fit for purpose? To begin with though, how different is all this?

What's new about ubiquitous but hidden technology?

Beer and the authorities he cites are not actually saying that technologies which use embedded computation are hidden. After all laptops, i-phones and i-pads, cctv cameras, smart cards, satnavs and the rest are evident all around us. What they are saying is that the *software* they use is not visible. We can't see it working. Or, at least, we can't see it working without going to a lot of trouble and learning to program the relevant application programming interface (API). This is, of course, true; true but irrelevant. It is equally true that you can't see what is happening in the innards of your car engine or fridge (two equally ubiquitous devices) without going to a lot of trouble and certainly learning some considerable engineering skills. The workings of the valves, pistons, con-rods, crankshafts (not to mention the engine management system) are all hidden away, and it is just as well too. Moreover, and this is even more important, as long as the car works, most of us are not interested in what is going on 'under the bonnet'. And when it doesn't work, we are usually wise enough to let someone do the investigating who does know what is going on and does have the knowledge, tools and equipment to take the engine apart. The fact that 'the inner workings' of the technology are not visible and immediately understandable is, then, an absolutely familiar part of (most) widely used technologies.

The implication that Beer et al want us to draw from the ubiquity of this new technology is, of course, that we take it for granted at our own peril. But when he first started talking about "ubiquitous computing", the late Mark Weiser (Weiser 1991) was actually trying to de-mystify computation by suggesting we should regard it more like a utility (electricity or water) than a complex and arcane technology. Weiser's prediction (which, as we can all see, is coming to pass) was that computation would be available everywhere. Devices would simply plug in or tap in. Beer does not seem to find the ubiquity of electricity or water a challenge either to Sociology or society. Why should he think utility computation is? After all, the ubiquity of computing is in large measure a function of the ubiquity of electricity.

Of course, for Beer, it is not just that the technology is everywhere and hidden, but that most of us who use it, don't understand it and the uses to which it is being put. We not only can't see how it works, we couldn't understand it if we could. Software is mysterious and its language arcane. Ordinary people can't read or speak software. (Scott Lash (Lash 2001) actually talks about software as a kind of technological writing. We will come to that later). Again, this is true, but irrelevant. Most people can't speak or write the sub-atomic physics which is needed to understand electricity either but they have very little problem plugging in their CD or turning on the lights. At a push, we would guess that, if they have a theory of electricity at all, most people conceive it as some sort of fluid-like stuff flowing through the cables. And, no doubt, if asked most people would offer a similarly simplified but not wholly ludicrous account of how software works. It is a straightforward fact of ordinary life that what we might think of as commonsense theories of complex technologies rarely capture the deep understanding that science and engineering might give us. Nonetheless, we are not terrified of, oppressed by, or totally at a loss how to deal with them.

The insistent stress which they put on the novelty and distinctiveness of computation, serves to allow Beer and his colleagues to manufacture an array technological hobgoblins with which to frighten themselves, Sociology, and the rest of us. But these are childish fears. In and of themselves, the technologies are as hidden, as pervasive, and as embedded in our everyday lives as electricity, clockwork or the wheel. (How many people shopping in Tesco's could actually tell you how a wheel works or how they ride a bicycle?)

Sleepwalking our way to dystopia

Of course, the hidden character of the technology isn't really what Beer objects to. It is the nefarious uses to which it is being or could be put. We can't see the technologies and we can't see what's being done with them. The two prime domains in which such 'hidden use' seem to arouse most suspicion, search engine recommender systems and linked data bases of public and personal data, nicely illustrate two characteristic features of Cultural Sociology; the generalisation from a small number of celebrated cases and the tendency to take the research agenda from the mass media. In the case of search engines, browser providers such as Google (and it seems that Google has now inherited the mantle of *The Evil Empire* from Microsoft) use the data about our search patterns they have collected to interpret over the structured sorting of web pages provided by Google's proprietary algorithms. This enables the search engine to 'serve' pages which it believes best fit what we are looking for. There are two supposedly hidden dimensions here. First, in the background, the browser is tracking our search patterns and recording them. It is this tracking which allows the widely circulating allegation that Google (or whoever) knows more about us than we know ourselves. An allegation reinforced by constant repeating of the apocryphal Scott MacNealy apothegm.[33] Second, because the sorting algorithms are confidential, we do not know the basis on which pages are indexed and weighted. Hence we cannot say, for example, how far commercial considerations influence the selection. Because Google will not reveal its algorithm (not surprising, since it is the core of its business model), the inference is drawn that commercial considerations must predominate. The net result is the suspicion that without our knowledge or agreement, Google is deliberately using its knowledge of us to direct our attention to some pages rather than others, and is doing so for commercial reasons.[34]

The second example is the increasing use of linked personal data to provide personalised product and service recommendations as well as other forms of marketing. Using private data such as loyalty, credit card or other retail data together with public, open data such as residential data from the electoral roll, census data and the like, data companies 'sort' individuals into 'retail' or 'purchasing types'. Populations of these types can be purchased and used for targeted marketing. The data that the Government, stores and credit card companies collect can be garnered, integrated and structured to enable bundles of relevant offerings to be made to us, some of which may be 'driven' by a knowledge not just of our preferences but of our precise location. As with the gathering of web pages, this collection and integration is held to be going on in the background and apparently without our knowledge or approval.

The claim of ignorance here is implausible though. Discussions of the media and especially of new media in the press and elsewhere constantly make reference to the fact that Google sets a price for page ranking, that pages can be 'written' to ensure high page ranking, and that loyalty card and other data can be bought from the organisations that collect it. Moreover, we all know that the junk mail we receive through the post and email is a consequence of using that data to try to work out what types of products we might be interested in. So the claim that 'we' (ie the general populace) don't know this is happening is more than a little patronising and/or misleading. We do know, and most of the time it doesn't worry us. The pages we are served provide the answers we were looking for (mostly). The marketing materials, adverts etc we are sent cover the range of products we are most likely to want to buy and we might (occasionally) even find to be useful.

---

[33] "You have no privacy. Get over it!"

[34] We return the Google and its algorithms in Essay 7.

Moreover, and this is the rub, the sociologists who are warning us about these dangers get their understanding of the importance and scale of the issues for much the same sources as the rest of us.

To say the least, the claim that we are unaware of the practices being used and the reasons for them is stretched. This is not to say we can detail just how such technologies and their related practices work (why on earth should we expect to be able to? And if we could, would it make any difference?). As long as they work for us, we are prepared to take them for granted.

The worm i' the bud

For Beer, this concealment masks something else — namely the real significance of the changes that the technology is thought to be bringing. This is because it is insisted that computation just *is* different. What is being invoked is the claim (myth might be better) that computation changes everything. There is an irony here. In recycling the view that computation changes everything, Beer et al are swallowing whole the hokum peddled by gurus, consultants and other snake oil salesmen in the 1990s about how computation, the internet, something, was going to change the world. It would, they wanted us to believe, create a 'frictionless, knowledge-based economy'. Now, while Beer et al don't really believe that has happened or soon will (who does?), they do believe that Web 2.0 and the other associated technologies are somehow changing the world. In fact, it is just these changes, they claim, that Sociology is increasingly finding it impossible to cope with.

At the risk of using a broad and somewhat crude distinction, it is reasonable to think that the changes Beer has in mind, if they are occurring, should be visible either in the way that people routinely behave (they just are doing different things) or in the social processes that are institutionalised in society (society is organised differently); or both. At the risk of setting off methodological hares, we might say that both are *empirical* questions. That is to say, to find out if the changes are happening, one reasonable strategy might be just to go and look at what is going on and see what has changed and how far.

The work that has been done on both questions is pretty conclusive. Although texting, twitter, the forming 'friendships' on Facebook and other social networking sites, storage in the cloud, use of location-based personalised services and so on and so on are recently learned sets of behaviours, as patterns of behaviour they serve very traditional functions. Texting, twitter and Facebook are forms of communication; digital versions of sharing, chatter and gossip. True the 'followers' and 'friends' might be both known and not-known; true the vocabulary might be distinctive (when isn't the argot of the young distinctive?), but as patterns of behaviour, they do no more (and no less) than the sharing, chatter and gossip networks of the past. The work that has been done indicates that the ways the sites and applications are used is remarkably like that of the structure of other social networks. In similar fashion, the on-line world of gaming (another domain held to be socially degenerative in some way) turns out to be very much like the off-line world of many other leisure pursuits. Sure the means are new and the vocabulary too, but in the end Facebook is not a lot more than the Hula-Hoop of the early 21st century. Web 2.0 technologies tell us far more about the constancy of human nature and the predisposition to conform, follow the crowd, and prefer to be insiders rather than outsiders, than it does about how behaviour, let alone human nature, is changing.

When we turn to the cloud, to location-based services, the internet of things, the one thing we do not see is some entirely new set of business practices. Far from it! The business models, pricing

models and marketing models are all very familiar ones (for example, pay-per-use, service drag, outsourcing cost by free riding on the communications network or crowd-sourcing software).

Surprisingly, or perhaps not so surprisingly, just going to look never seems to be the obvious thing to do for the protagonists of Cultural Sociology. For them, the issues are not actually empirical but conceptual. Here is Beer again.

> *The prominent new media theorist Scott Lash (2007b) has recently spoken of what he describes as a 'new new media ontology'. This is a term designed to capture a shift toward forms of living in which information becomes active in shaping lifestyles and environments. What is useful about this slightly oblique terminology is that it can be used to group together a range of emergent work in the social sciences, and particularly in urban studies, that picks up on the technological challenges to human agency offered by the decision-making powers of established and emergent software algorithms. (Beer 2009 p 988)*

Beer goes on to quote Lash as follows.

> *What may be happening in the information order is such a collapse of ontology and epistemology. Ontology itself is increasingly epistemological. And of course the notion of information implies this. What else could informational being be? But equally epistemological or modes of knowing are increasingly also modes of being. Being always necessarily shifts over into modes of classification. (Lash 2006: 581)*

So it is not a matter of different things being done but rather of our need to talk about them differently. We have now to talk like this:

> *… the 'stuff' that makes up the social and urban fabric has changed - it is no longer just about emergent properties that derive from a complex of social associations and interactions. These associations and interactions are now not only mediated by software and code they are becoming constituted by it. (Lash 2007 emphasis in original)*

Just in case we missed it, Beer glosses Lash like this:

> *The shift that Lash is intimating, and which is being picked up on across a variety of contemporary new media work ..... is toward information becoming a part of how we live, a part of our being, a part of how we do things, the way we are treated, the things we encounter, our way of life. The result is that information is not only about how we understand the world, it is also active in constructing it. (Beer Op Cit. p987-8)*

All this, though, simply creates a puzzle. Beer began (and indeed ends) by saying we know absolutely nothing about Web 2.0 technologies and what they are doing to us. Our methods and research strategies are inadequate to address them. And yet what we are offered looks remarkably like an empirical generalisation about the real-world effects of these technologies culled from a body of investigations. It looks like a conclusion. But of course it isn't. It is a stipulation. We are not being told that Lash, Beer et al have studied these technologies and found them to be having this or that effect. Rather, Beer is telling us to see them this way and then go out and carry out the

studies he recommends to show just how they are like that.[35] It is this stipulative character which then sets the negative tone for the accounts that are to be offered. Because they constitute our worlds and are ubiquitous but hidden, they must be controlling. That is, they must be an exercise of power, a new kind of power, one that Lash calls post-hegemonic. What remains unclear in all of this is just what the shift from seeing technologies as tools for assisting our understanding of the world to seeing them as playing an active role in the construction of that world actually is.

New Concepts for Old Problems

As a body of sociological work, Cultural Sociology rests upon Critical Theory. Although by no means integrated, homogeneous or even harmonious, Critical Theory does acknowledge the same lines of descent; on the one hand, from the Frankfurt School (and particularly Adorno, Marcuse, Horkheimer, Benjamin and latterly Habermas) and on the other from Michel Foucault.[36] A third thread concerns the importance of postmodernism as a social process, but this is by no means as universally acknowledged. For the Frankfurt School, the key question was why, despite the fact that exploitation and domination were evident all around, the exploitative character of modern capitalism had not led to its overthrow. Their answer was that the ideology of capitalism is now part of the core beliefs of our society, shared by all and continuously reinforced through political processes such as elections and parliaments as well as cultural processes such as forms of art and music, mass media and so on. Even forms of academic philosophy (especially Heidegger's philosophy) were subjected to this critique. Power relations, then, are immanent in cultural forms.

For Foucault, what was even more important was that the immanence of power lay in the social institutions within which we operate; the structuring of knowledge within these institutions produces and reproduces the distribution of power. For The Frankfurt School power is expressed through what we believe; for Foucault, power is expressed in all forms of knowledge. Both views adopt the notion of 'hegemony' from Gramsci (Gramsci 1996). Hegemony is the power to control by enabling the subjected group to control themselves. For Cultural Sociology, the motif of hegemony motivates all explanations and descriptions. Accounts of ways of life in the modern city, fashion, youth culture, consumption, brands and marketing, leisure activities and travel, etc etc are cast as depictions of the operation of hegemony. Each is an instantiation of how we, the masses, are voluntarily brought to subject ourselves to the demands of capitalism for ever new mechanisms for expropriating value.

For Cultural Sociology, power is not just an another analytic category: it is the universal explanans. Power is not *found* in cultural objects and processes, it constitutes them. The trick is to show the hegemonic nature of this power.

Knowledge, understanding and the conceptual Three Card trick

In his account of the significance of digital technology and its relationship to post-hegemonic power, Scott Lash (2001) begins with what is a commonplace in the Sociology of Knowledge. At particular junctures, often related to specific scientific or technological breakthroughs, particular images seem to grip the public imagination. Thus from the 16th to the 19th century, accounts

[35] As we will see, the list of studies he proposes even if they are a little uninspiring, are not that bad,.

[36] See Jay (1996) for the early history of the Frankfurt School and McCarthy (1990) for Foucault and The Frankfurt School

invoking mechanical mechanisms and relationships were routinely used to describe non-mechanical phenomena. The success of Physics in explaining (or seeming to explain) the physical world led in the social and related sciences to reasoning by analogy with physical accounts. In the 19th century, following Darwin, biological images came to predominate and the forces at work in social life were expressed through evolutionary metaphors. For Lash, Sociology from its modern formation well into the 20th century, deployed this biological formulation (for example, Durkheim's contrast of 'mechanical' and 'organic' solidarity; Parson's use of systems theory).

Into this mix, Lash now drops a *philosophical* distinction between ways of understanding the material and social world around us. This is between a concern with what he calls the 'logical meaning' of things and their 'experiential meaning'. He calls this the contrast between epistemological and ontological understanding. These two forms of philosophical understanding are then mapped onto how 'we' (presumably 'we' the general populace) understand things. Rather than understanding things from 'above' society (the epistemological stance), we now understand things from within social life (the ontological stance).

> *Through no longer being above things but in the world with things, we come to grips, not with epistemology and appearances, but with deeper ontological concerns. (Lash Op. Cit. p 107)*

This is a deeply odd conclusion to arrive at. Up to this point, Lash's argument has been about the ways Sociology represents the world and our ways of thinking about it. Its concern is entirely intra-sociological, if we might be allowed to use that term. And, as we say, it is not an unusual argument. But now these sociological conceptions are read (or imposed, if you prefer) onto ordinary life. So it is not sociologists who have changed their ways of looking at the social world but we, the ordinary members of society. We have moved from an outside-in modality of understanding to an internal one. It is we who have moved from a concern with appearances to a concern for experience. Thus, from a *conceptual* argument, we jump to an *empirical* conclusion. From an argument about how social theories have talked about social life and ways of thinking differently, we are offered a conclusion about how ordinary members of society understand these things. And with that conclusion in hand, Lash turns to the significance of modern digital technology.

It is worth dwelling on the move that Lash makes here because it underpins both his conclusions about modern technologies as well as the uses which Michael Beer makes of his ideas. Whilst not uncontentious, Lash's potted history of social thought is a relatively familiar one (it has the same themes as those of Latour, for example). The thread running through it, of course, is the philosophical frame within which Sociology was located. Mechanical and organic forms of social theorising are to be seen as 'positivist' and concerned with the grounding knowledge in the relationship between appearance and reality. It was the introduction of a phenomenological frame into social theory that introduced the concern to ground knowledge in experience. The themes and contrasts that Lash runs are all forms of philosophising. That is, they are formal ways of reflecting upon how we should conceive the world around us. Neither positivist nor phenomenological philosophies say anything about how ordinary people either do or should reason about the world. They are about how Philosophy should be done. They are about *formal* not *commonsense* understandings of the world and certainly neither provides an empirically based description of how commonsense understandings are arrived at. What Lash does is convert formal, philosophical accounts of understanding into purportedly empirical descriptions of commonsense understanding, and then on the basis of that conversion, proceeds to draw conclusions about the general significance of modern digital technology.

Hegemonic power and the power of the algorithm

Lash sets things up by proposing that 'in technological forms of life, we make sense of the world through technological systems' (p. 107). He explains what he means by this as follows:

> *I operate as a man-machine interface - i.e. as a technological form of natural life - because I must necessarily navigate through technological forms of social life. As technological nature, I must navigate technological culture. And technological culture is constitutively culture at a distance. Forms of life become forms of life-at-a-distance. I cannot navigate these distances, I cannot achieve sociality apart from my machine interface. I cannot achieve sociality in the absence of technological systems, apart from my interface with communication and transportation machines. (Lash Op. Cit. p 107-8)*

Now Lash is not describing some idiosyncratic way which he just happens to have adapted to modern social life. He is proposing what it is like for all of us. But what on earth is all this supposed to mean? That we can no longer socialise with family and friends over dinner or in the pub? That we cannot go about the daily tasks of bringing up children, running household errands and doing domestic chores, buying goods and services without operating through some 'machine interface'? Really? Of course we know that email, mobile phones, internet shopping, social networking sites and the rest are important features of our lives, but no-one wants to propose (do they?) that we only live our lives through such applications? Of course, Lash doesn't really think this. His rhetoric is meant to elide the ways that social science has construed social life and how, as a consequence, we must accept social life is lived.

This working back from sociological theorisation to empirical description occurs in all three of the core characteristics which Lash attributes to technological forms of life: *flattening*, *non-linearity*, and *lifting out*. In each case, Lash reviews the commentaries theorists have offered about social life and treats them as empirical descriptions of the character of social life and social institutions. The net result is the usual smorgasbord of topics which Cultural Sociology has taken for itself: mass media, brands, trademarks, intellectual property, e-commerce and so on, all reinvented as modes of a technological form of life.

It is within this context that Lash introduces power. In a technological form of life

> *...Power works less through linearity and the reflective argument of discourse (or for that matter the linearity of ideology), than through the immediacy of information, of communications (Lash 2001 p.117)*

And if power works in this way, then it can no longer be conceived to be hegemonic. Once again we get a reading of social life through the lens of Cultural Theory played back to us as an empirical description. Cultural Theory has only partly grappled with the issues. For Lash

> *...At stake here is not just technological forms of life (Lash, 2002), in which forms of social life are technologically mediated. At stake is the technologization of life itself, the mediatization of life itself. Once we make the step from computing or technology to media, the question of content also comes to take centre stage, as does that of communication. When media are ubiquitous, interfaces are everywhere. The actual becomes an interface. People and other interfaces are connected by protocols that*

> *connect an ever-greater variety of interfaces with one another. It is such protocols that make communication possible. (Lash 2007 p. 70)*

Since we are now talking about digital media, it is digital coding, programming and algorithms, that become the object of attention. With his usual penchant for mixing up concepts, Lash adds the mathematical concept of an algorithm to the legal concepts of regulative and stipulative rules. It is through the algorithmic, generative rules which are the mechanism through which modern capitalism exercises non-hegemonic power. For it is the operation of the algorithms underlying digital technology and modern media which create what the late Roger Silverstone (2007) calls a 'doubling' effect. Because experience is held to be increasingly mediated though digital technologies and their media alone, these systems both present the material and social worlds to us *and* construct their facticity for us. For Lash, constructing the facticity of the world is non-hegemonic power. Here is his (revealing) summary.

> *Why is this second-wave cultural studies, this post-hegemonic cultural studies so suggestive to today's generation of students? Because it speaks to the world that they encounter. If the empirical is informational, then it is already knowledge: it is already transcendental. If the empirical we encounter is mediatized, then it is already transcendental.....Second-wave cultural studies and its transcendental empiricism speaks to us because we encounter a world of transcendental-empiricals. Thus science encounters a world of bio-media. And art deals in (video and information) materials that are already mediated in addressing a transcendental that is equally mediatized. And in all these cases it is a question of ontology and epistemology. Being is mediatized, as is knowledge. And the two stand less in a relation of radical separation than of fusion. (Lash 2007 p74)*

In the end, the justification for the use of a conceptual pot-pourri buttressed by logical leaps is not that we get firmer sociological ground on which to understand the social world and its institutions. It rather that our students find it attractive and that 'it speaks' to their experience. It is this justification which led to our earlier suggestion that Cultural Theory is taking on the characteristics of the consumer society it so criticises. Sociological theories are to be prized as consumer objects which our students like and which re-describe in high-sounding terms what they already know.

Back to Michael Beer

Beer roundly castigates Sociology in general for failing to address modern digital technologies. Taking Lash's account of technological forms of life and post-hegemonic power as his departure point, he suggests a research programme which operates at a number of different levels.

7. Which organisations own, control and create the applications and services that deploy these technologies? What are their business models? How successful are they? What are the supply chains that get set up to support the applications? This you might think of as the Management Science of the modern web.
8. What are the product architectures, the data architectures and data flows that are deployed in these applications? What information is harvested, re-purposed and re-deployed, and how is this done? To continue our analogy, this you might think of as the Socio-Technical Systems Theory of the modern web.

9. Finally, how do the technologies and their interrelationships appear and get used in daily life? What do people do with them? What do they share (and not share)? How do they tailor and adapt the technologies? How do they manage to traverse the line between the real and the virtual (assuming there is a distinction to be held and a line to be crossed)? What do these technologies mean in their daily lives? And so on. Think of this as the Social Anthropology of the modern web

As Beer clearly recognises, these questions do not circumscribe the whole gamut of sociological research interests. But he sees it as a start. The only trouble is for at least the last 25 years, sociologists have been taking a keen interest in just these questions and in many, many more. It is true that they have not paid the same degree of attention to every part of the programme Beer outlines. By and large they have been more interested in 3 and its affiliates than the others. But it would not be right to say that since nothing has been going on, a start must be made. One has only to explore fields such as HCI and CSCW, Business Studies and so on to see just how much has been achieved. Moreover, and this is the irony, a great deal of the work that has been done has called directly upon the theoretical frameworks developed in Cultural Sociology. As we saw in Essay 1, whole conferences and journal issues have been given over to debates on the relevance of (some version of) Heidegger, Bahktin, Foucault, Baudrillard and Bourdieu. The analytic skein of culture, brands and consumption spun by Cultural Sociology has been invoked and used in account after account, paper after paper. Beer might not like what is being done with the impedimenta of Cultural Sociology but it is a bit much to ignore the fact that it is actively going on.

In turning to Web 2.0 technologies, Beer repeats the canard that the core technologies are hidden, beyond reach, and, most importantly, inscrutable to extant social science methods. This is despite the fact that, as we shall see in Essay 8, many investigators have provided detailed accounts of how they work. However, why *a technology* should automatically be open to social science scrutiny (as opposed the technology's socially organised characteristics and consequences) remains a mystery. As we will see with Winner's account of bridges and nuclear power in the next Essay, it is not necessary to have a detailed understanding of the technology to understand its consequences (even if you do get them wrong).

What Beer sees happening with Web 2.0 technologies and especially the profiling processes they facilitate, is a supposed transfer of agency to the software itself. This is because:

i. The profiles created are used to generate social sorting;
ii. This sorting allows personalised recommendations.

The system 'knows' lots about us and on the basis of its knowledge, goes on to facilitate data 'finding' its way to us. Second, the extensive detail provided to these sites by users actually allows the "doubling"' process to occur.

Taking the issue of agency first. Talk about software making decisions is, of course, just a *façon de parler*. What the algorithms do is manage logic gates to allow the processing of data according to the models and procedures which have been built into them. The methods for doing this are those chosen by the software designers. The program may indeed 'learn' as it does this processing but such learning is, once again, simply the application of criteria of goodness of fit between different representations of data which have been provided in the design and as such part of the functionality of the 'system'. Although the decisions they make may not have been predicted or even predictable, programs have no agency, no motivation, no intentionality, and no understanding of what they are doing. When done informally by software designers and commentators, talking as

if the software acts to make decisions is a relatively harmless trope. Used as an analytic social science description is, at best, a serious misapprehension.

As for the sorting itself, this is very little different (except of course for the scale and range of detail) from the hand crafted analyses that social scientists, marketing analysts and others have traditionally used to summarise the heaps of data they have collected from focus groups, questionnaires, customer feedback and so forth. Principal components analysis is used to partition the data into a few relatively manageable clumps. The integrity of the clumps is tested for cohesiveness and differences between the clumps are tested for their 'relative distance' from one another. The profiling techniques of web 2.0 technologies are simply larger scale versions of these processes. No-one has gone around bemoaning the use of profiling in targeted snail mailing (except to complain about the volume of junk mail they get), or the way traditional retailers track and manage their customer relations. The scale of data collected through the internet makes no essential difference.[37]

The second issue, that of 'doubling', is held to be a consequence of the role which digital media have in the transition from modern industrial society to what is called "post-modern" society. This is quintessentially one from urbanised to globalised social formations. In the emerging digital economy, not only will the products and services offered be developed globally, the markets for them will be global too. The digital economy is, then, globalised through and through. The challenge in the globalising of political culture through the digital economy is one of possible fragmentation. Modernism is held to have a homogenous social imaginary (to use Charles Taylor's (2004) term). However, it is by no means clear that postmodern society will share a universalised political culture based on that of western societies. Across the globe, political cultures could well be aligning around multiple and complex lines; ethnic, religious, regional, as well as around nation states. In addition, even within western societies, the supposed coherence of the modernist political culture may be under threat.[38]

Reflecting on these processes, commentators such as Roger Silverstone (2007), have begun to talk of *mediapolis* as the locale of the diverse political cultures in the global public realm. In *mediapolis*, our experience of the world is channelled through a merged set of communications media rather than through local communities and the institutional intermediaries of modernist society. *Mediapolis* is what Taylor calls an "open access society". In this context, it becomes important to know how likely it is that the common platform of communications technologies will shape either a shared or a diversified political culture. Will that platform predispose integration or even further fragmentation as, paradoxically, global society emerges as a constellation of minority communities?

Two concepts are held to be central to answering these questions: *the space of appearances* and *doubling*. The former refers to the physical (and increasingly digital) and cultural space of experience. Things and people appear for us in this space. Where this space is rooted purely in physical space, the directly personal, known and co-present (within local communities in mostly pre-modern societies), the culture that we are immersed in provides ways for us to understand and respond to what we experience. In the globalised *mediapolis*, such people and events may be

[37] We return to the issue of the supposed troubling nature of digital technologies in the next essay.

[38] See the Postscript to this volume

physically and culturally far from us and encountered virtually and asynchronously. As a consequence, the assumption of a common basis for understanding, interpreting and responding to events may no longer hold, indeed may no longer be an assumption which *anyone* actually holds..

The world of experience within *mediapolis* will be both a familiar and an unfamiliar one. Currently, when we encounter the unfamiliar, the strange, we can call on the resources of our common culture to translate the unfamiliar into the familiar. To illustrate this, Silverstone recounts an anecdote based upon an interview broadcast on the BBC where an Afghani blacksmith suggested that the Americans were bombing his country because Al Queda had killed many Americans and their donkeys and had destroyed some of their castles. Quaint though this might be, it does bring out the proposed general problem and its consequent strategy of translation.[39] For analysts such as Silverstone, through the ways that they routinely present events to us, communications technologies predispose certain orders of translation and familiarisation. The unfamiliar is shaped to become the familiar. This familiarisation comes, however, at the price of a loss of appreciation, and possibly respect, for difference. The result is likely to be increased fragmentation and social distance.[40]

For these theorists, then, the communications media through which we increasingly encounter other cultures, people and events have a *double role*. They both present events and people as 'facts in the world' and, through the ways these facts are shaped and contextualised, they construct their 'factuality'; that is, what we take them to mean. Without such doubling, the globalised world would appear irredeemably strange, perplexing and dangerous. Within traditional, localised communities, continuity of co-presence, the fact that all live in a common welter of daily life, underpins a commonality of outlook which in turn enforces the boundaries of normative order. For Silverstone, this is what secured the Greek *polis* and its democratic structure. As we move away from experience rooted in co-presence towards mediated experience of globalised digital worlds, the commonality of outlook dissolves and hence normative boundaries are at risk.[41] For Silverstone, through their doubling, the media have the power to create a new normative order for globalised experience, one which might be either universal and integrative or local and fragmented. This power to close the political choices on offer is what, in his eyes, makes the media irredeemably moral.

For "doubling" both to occur and be the threat that Beer and Silverstone believe it is, one has first to presume that the user (notice the type) of these technologies lives outside what we currently take to be normal social relations. Not just some but *all* their experience is channelled through the technology. Maybe a social recluse might live like this, but it is hard to imagine a *whole* society like this. Moreover, given what we said earlier about Facebook etc, even when these forms of networking are large scale, the social forms that appear are the familiar ones from the our co-present experience. We are enmeshed in networks of social relations that flow in, through and around technologies of all kinds. And our commonsense cultures and shared expectations serve as

[39] of course, its quaintness is the reason why Silverstone uses it. One may well doubt it describes the kind of account those who share a digital culture would use.

[40] Once again notice how we have moved from an anecdote to a generalisation about the monolithic and homogeneous nature of the media and global culture.

[41] This amounts to no more than a modern rendering of Durkheim's classic counterposing of 'organic' and 'mechanical' solidarity.

grounding for our understanding of social life. To imagine a society made up of the kind of atomised and anomic individuals envisaged by Beer et al is simply scaremongering (of the kind sociologists used to go in for in the early days of television). Our understanding of the world around us is grounded in the culture we share and so is our use of the technologies we deploy.

Conclusion

Within Cultural Sociology, the influence of postmodernism has replaced the old certainties of investigation and analysis with the new certainties of stipulation and critique. The critique takes the form of a supposed revelatory discourse which re-run the stories publicised in the mass media and in which the enmeshing of cultural forms in power relations is the explanans rather than the explanandum. This hyperbole offers descriptions of social activities which bear almost no relation to the ways that, as ordinary members of society, we experience and carry them out. The case of Web 2.0 technologies is but the latest example of this, but a particularly telling one. These technologies are the technologies of the young; and so our sociological account of them must be shaped to fit their preconceptions and predispositions. Cultural Sociology is the Sociology of and for Generation X. The irony is that is seeking to be so *a la mode* and relevant, Cultural Sociology has itself become a packaged, post-modern product which justifies its place in the marketplace of ideas by its attractiveness and familiarity to its consumers. Academic rigour, structured thinking, close reasoning have been dispensed with and replaced by the pursuit of the new and the whim of fashion

THE POLITICS OF ARTEFACTS

Prologue

Over the past 40 years or so, Sociology and the disciplines and professions concerned with digital technologies have enjoyed a somewhat complex relationship. If we disregard their burgeoning role in the practice of Sociology itself (and this story does deserve to be told) and admittedly at the risk of some simplification, broadly we can discern two streams. As technological innovations, digital technologies have been the object of much fascination and study. The approaches adopted and the interests taken have been drawn from the body of work associated with the Sociology of Science and Technology.[42] Separate but related to this stream has been a concern with the design, development, implementation and roll-out of products utilising digital technology. This latter interest has its origins in work such as Enid Mumford's ETHICS approach (Mumford 1995) to participative design[43] as well as later ethnographic studies of the workplace such as those of Lucy Suchman and her colleagues (Suchman 1987). Fields such as Computer Support for Co-operative Work and Human Computer Interaction have both tried to incorporate sociological findings in design.

Whilst the studies *of* technology and the studies *for* design have intersected at many points, of late a new nexus seems to be emerging. This is around the political and ethical implications of

[42] Though some exceptions, as such they have largely avoided the confusions and muddles we have discussed in other essays in this book.

[43] Enid Mumford's legacy appears to be largely lost to-day. Her work is very rarely referred to. This is a pity as her application of the Tavistock Institution's theorising of organisations did actually grapple with (and to some extent resolve) many of the problems which preoccupy contemporary debates.

digital technologies on our ordinary lives.[44] Whereas studies of technology identified the social forces which shape the ways digital technologies emerge and progress to product and the studies for design sought ways to improve the effectiveness of the product within the context of use, this new concern seeks to use findings of studies of technology to influence design itself. It seeks not just the practice of Sociology as a complement to design but also the incorporation of sociologising in design. Because of their understanding of the social and ethical consequences of innovation, it is argued that sociologists should be brought in as members of design teams in order to help designers avoid designing technologies which will have deleterious consequences.

In this essay and the next, we consider this proposal. Whilst we understand what motivates it, we are less than convinced that the case for it is made, or that it is a viable recommendation. Neither of which, of course, should be taken to imply that we do not think digital technologies set ethical conundrums. They clearly do. Our fear is that some of the approaches to these problems, and especially Disclosive Ethics, may create more problems than they solve. In Essay 8, we explain why we have come to this view by considering the arguments of two major advocates of Disclosive Ethics, Philip Brey and Lucas Introna. Whilst there are many similarities between their positions, they differ in quite fundamental ways. Both are heavily dependent on arguments made in Langdon Winner's paper *Do Artefacts Have Politics?* (Winner 1985) and so in the current essay we consider that contribution in some depth. To begin with, though, since Winner, Brey and Introna want to call up sociological or sociological-like arguments in support of the cases they make, we will set out some considerations with regard to the practice of sociological theory and research which we feel bear upon the whole debate. These considerations will surface again and again in our discussion of Winner, Brey and Introna.

A Word to the Wise

Digital technologies are now critical to our way of life. Not only are they to be found everywhere, they are in everything. Their ubiquity and pervasiveness was the characteristic which encouraged some to describe modern life as an information society (Castells 2001, Baudrillard 1995). With this dependence has come concern (van Den Hoven & Weckert 2008). Just what are these technologies being used for? And are such uses always ones which we are content to tolerate? These are important questions. Questions of what is right and wrong in the use and application of digital technologies are ones which we as a society must address. Moreover, as sociologists, we are pleased to see sociological findings and arguments valued enough to be drawn into these debates by philosophers and others who seek to resolve the dilemmas and conundrums they set.

However, this use of Sociology has to be a careful one. Sociological accounts and research can appear disarmingly simple to the unwary when beneath the surface all sorts of complications and difficulties lie. Such complications and difficulties make up *the culture* of the discipline, if you like; what is known about but rarely commented on in the hurly burly of practising it.

Our purpose is to surface some of this culture so those who want to call upon Sociology as part of the engagement with the political and ethical issues set by digital technologies, can do so confident

[44] One aspect of this has been the introduction of the topic of digital technologies into Cultural Sociology. We discussed this in the previous essay. What we focus on here is the exporting of Sociology into design as the basis for political and ethical judgements. In that sense, our interests here are more akin to those we expressed with regard to ANT and Economics than they are with the ones we expressed concerning Cultural Sociology.

they know just what they are relying upon; just what Sociology can and will give them, and what it will not. Our hope is that Cervantes' adage will once again prove sound : "Good wits jump; a word to the wise is enough".

The Unresolved Nature of Core Problems

Whilst it is true that there are many unresolved problems in the Natural and Mathematical Sciences, for the most part those who work in the respective fields know what it would take to resolve them. It is just that the theory has not been well enough developed, the experiment cannot be designed, or the data is not amenable to capture with the instruments we have at the moment. With the social sciences, and Sociology in particular, the situation is quite different. Despite endless re-workings of the same materials, we appear no closer to knowing just what would resolve a number of core problems to the satisfaction of all. From where we stand now, the problems look less unresolved than unresolvable.

Which problems do we have in mind? We will nominate just three, all of which bear in one way or another upon social and ethical implications of digital technologies.

1. Technological Determinism: Is the proposal that technological development is the prime mover in socio-economic change viable? Associated with this is another oft debated question. If technological determinism is not a defensible position, does that mean Marx's account of the evolution of Capitalism is flawed because it is technological determinist?
   The positions on these two are myriad with much debating art going into teasing out highly nuanced distinctions which can be defended. So we find scholars such as G. A. Cohen (1979) staunchly claiming both that Marx *was* a technological determinist and that *there is nothing wrong with that*. Others such as Andrew Feenberg (2002) want to save Marx from his interpreters and, by judicious adjustment, construct a different Marxian and critical theory of technology development. Feenberg's account draws upon approaches to socio-economic change which posit a mutual interdependence between technology form and social structures and which, therefore, reject technological determinism.
   Despite all the discussion, we are no closer to a firm consensus first, on whether technological determinism is a viable sociological account, and second even whether Marx was a technological determinist, not least because it is by no means clear or agreed just what technological determinism is committed to.
2. The origins of Possessive Individualism. It is generally accepted that early modern society emerged first in late medieval England. It is here that the combination of legal, political, social and economic structures which transformed itself into modern society can first be discerned. This transformation is usually held to be from a peasant-based feudal society to a class-based capitalist society; a change labelled 'The Great Transition'. At the heart of this transition is the replacement of a value system which gave priority to collective and communal bonds by one which stressed the rights and obligations of the individual: in particular the rights of the individual to alienate, hold and dispose of property. Possessive Individualism characterises modernity (MacPherson 1962).
   The debate over the origins of this value system continues to this day. Are they to be found in radical religious views which culminated in Puritanism (e.g. Weber 1930)? Are they to be found in the forces driving the increased commercialisation of agriculture needed to feed a burgeoning population (e.g. Homans 1930)? Or, is it perhaps the case

that, as Alan Macfarlane (1978) has argued, if a peasant society is to be characterised by the absence of Possessive Individualism, England never was a peasant society and so, at least in the leading case, we should stop talking of a major transition. As much historical evidence as is heaped up on one side of this multi-sided debate is matched by the evidence heaped up on other sides. Indeed, *the same* evidence often is offered to support diametrically opposed arguments. Finally, when invoking these arguments, we should remember that much of the evidence usually offered bears only marginally on the cases that are being made.

3. Nature and Nurture: Although few to-day are prepared to argue that 'biology is destiny' (or the inverse), nonetheless the precise weighting to be given to inherited traits as compared to culturally learned behaviour in the explanation of particular aspects of social life remains unresolved (Pinker 2004). Despite all the studies of educational attainment, criminal behaviour, attitudes to work , voting patterns, and so on, we are still no closer to saying how much of any of these can be accounted for by genetic and natural forces and how much by social and cultural ones. In fact, one characteristic feature of these discussions is that the protagonists start from the position where they *know* what the answer is and so their reading of the evidence is in service of their pre-supposition.[45]

What does all this mean? Well, apart from indicating that for anyone to be able to claim literacy in the field, serious training in Sociology should include examination of the original classical as well as contemporary explanations of these issues, it means that in order to pursue their research, researchers have to take positions. One simply cannot make any headway if one is always stymied at the start by indecision over how to frame the research question. Obviously it is good for students and novice researchers to understand that there are many different ways the question can be taken, but to do any work, you have to take the question some particular way. This is not a deliberate denial of the debates, rather the practical requirement of doing practical work. So, very often it will appear from the way the research is framed and how the facts of the matter are presented that some debate, issue, bone of contention has been resolved. But this is usually simply a device for curtailing argument in order to get on with presenting the research. Things appear to be settled and agreed, when in reality either the debate still rages or it has cooled and been left unresolved. All that has happened is that the researcher concerned has simply made his or her own mind up and proceeded from there. The implication of all this is that we should be just a little suspicious of claims about what Sociology has or has not done as well as about what prior forms of Sociology could and could not do.

In Sociology, many, perhaps all, of the central and critical issues around which the discipline is gathered remain highly contested. As a consequence, the unwary, coming across sociological accounts of phenomena which catch their interest, need to be more than a little cautious with confident appearing assertions of fact and settled agreement.

[45] Arthur Fine (1990) offers a measured account of the difficulties facing both sides in trying to demonstrate their case though causal analysis. Leyland Gerson Neuberg (1989) offers a more formal analysis of the fundamental methodological problems in trying to resolve such issues by means of randomised, double blind experiments

The Point of Sociology

The second fundamental issue concerns what might be thought of as the point of Sociology. Of course, everyone says that their aim is to explain the nature of social life, but what is meant by that takes two very different forms. One the one hand, scholars seek to provide narratives laying out, or explicating, the *causal ordering* of pre-conditions and effects which lead to particular social forms and patterns of behaviour. The debates in socio-economic history on the Great Transition are a prototypical example of this, as are the various debates over patterns of educational attainment, voting patterns, relative social mobility and the like.

On the other hand, much of Sociology (and perhaps all of popular contemporary theory) seeks to explain *the meaning* or *significance* of such patterns and outcomes. Why are they important and what role do they play in the overall structure of society? Thus, what is being explained are the reasons why such patterns persist and their relationships to other, often overdetermining structures.

This difference is crucial for how evidence is marshalled and deployed. The approaches differ in fundamental ways though both see the deployment of evidence as critical. Whereas the colligation of examples, conditions and effects can be thought of as loosely conforming to an inductive approach to explanation, the examination of the meaning of social patterns proceeds on a *post hoc, propter hoc* basis, with the analyst knowing in advance what the account of the phenomenon is to be. What is arrived at by this approach, then, is repetitive *confirmation* of an antecedent theoretical position rather than cumulative *discovery* of social patterns. Remember, it was common reliance on this pattern of inference in the human sciences around the 1920's that motivated Popper's falsificationist programme. The theoretical positions may be up to date but the methodological problem is a hallowed one.

When considering and evaluating a proposition such as "artefacts have politics" (the central assertion in discussions of ethics and information technology), it is crucial to be clear first whether this statement is the summary of a discovery process or the reiteration of an antecedent position, and second what evidence is being used to support it and how. Sorting these matters out is, we will accept, sometimes extremely difficult, especially for those who are not necessarily *au fait* with the intricacies of sociological reasoning. This situation is not helped when those concerned to elucidate the significance of a social practice talk as if they are describing its causes. As we have argued in previous essays, this is precisely the case with those who want to draw on Critical Theory or "post-humanist" versions of Actor Network Theory to offer accounts of digital technologies (for example Hofkirchner (2007), Latour (2002)).

The Centrality of Leading Examples

Perhaps because so much sociology is learned through absorption of textbook expositions rather than engagement with original studies, summaries of positions and arguments in the research literature tend to depend on a few, often only one or two and generally the same one or two, leading exemples. Apart from the medieval English peasantry and its role in The Great Transition (see above), one has only to think of the role of Shawnee linguistic forms in justifying the Sapir-Whorf hypothesis, or the stories told to Margaret Mead by Samoan adolescent girls with regard to theories of the cultural relativity of adolescence.

Equally importantly, many times the claims made on behalf of the examples are made *as if* there is no doubt that the research did demonstrate the case being made. But, as is brought out *par excellence* by Augustine Brannigan (2004), such a presumption may be mistaken. Often the research

record contains long standing and serious reservations about the initial and influential findings though these are not so well publicised. Such reservations which mostly start from the fact that the evidence provided by the original example was not as conclusive as it was initially made out to be.[46]

Finally, the lack of attention to original studies often means that the use of the arguments that these cases are said to support resembles Chinese Whispers, to use Joerges (1999) phrase. Exemplary cases become enshrined without any calibration of what is now being claimed on their behalf against what the original example claimed to show. As a consequence, all too often invocational drift takes place, with the original example and the current claim bearing only a distant resemblance to each other.

In short, it is wise to be careful when using what appear to be standard examples drawn from the sociological research literature, particularly if they have iconic status. All too often, the example will not bear close inspection, let alone the weight it is being asked to support.

The Predominance of Functional Analysis

As with the rest of Sociology, the majority of sociological studies of science and technology seek to expose the significance of scientific and technological practice (e.g. Pickering 1995. Barnes & Edge 1982). In this context, exposing significance is usually taken to mean showing that things are not necessarily to be taken as they are described by those engaged in those pursuits. In particular, emphasis is often placed upon the importance of social interests and other factors in determining research outcomes. So, for example, social forces are said to determine the evolution of scientific theory and the dynamics of technological innovation.

The argument used in such studies is *functional* in form. Functional arguments have some distinctive properties which it is as well to bear in mind. We will summarise just four.

1. For some set of circumstances or outcomes to be a functional consequence of a social practice, the circumstances must be *both logically and empirically distinct* from the practice. If there is no logical distinction then the practice and consequences are identical. If there is no empirical distinction, there are no grounds on which to base a description of how the practice gives rise to the consequence.
2. Evidence is central to the *plausibility structure* of all sociological accounts. Evidence which underpins the strength of a functional analysis demonstrates the degree of close coupling between the practice and the consequence on the one hand, and the consequence and overall system stability on the other. What makes a functional description convincing is, quite simply, the strength of the story that such evidence tells. This means that, unlike Achilles' view of Logic in Lewis Carroll's parable, a functional analysis cannot take you by the throat and force acceptance. No matter how convincing the description, there is always another way the story can be told, another narrative which can be played out, another interpretation constructed. This, more or less, is the central reason why sociological debates seem so non-convergent.
3. It is *an axiom* of functional analysis that all social systems self regulate to achieve cohesion, integration and stability. Even in the face of deep structural and disintegrative forces, adaptive processes are at work. Key among these are shared

[46] See Lucy (1997) on Sapir-Whorf and Derek Freeman (1983) on Mead. We have already discussed the debates over the Great Transition.

> systems of norms and values. The introduction of new social practices, for example social networking via the World Wide Web or the use of CCTV, may well bring changes in norms and values but these changes will take place in the context of overall stability.[47] Furthermore, as they become "institutionalised" or embedded as social practices, they too will contribute to overall adaption but under somewhat changed conditions which come about partly a consequence of themselves. In this way, changes in norms and values are, or can be, functional consequences of new social practices.

In many ways, one could argue that Sociology's main contribution to the study of social life has been functional analysis. Certainly, anything passing for Grand Theory tends to be functional in form not least because genuine specification of causal relations is almost infeasible in Sociology. What this means is that any research endeavour operating within a standard or classical theoretical tradition, even one that seeks to integrate whatever modish oppositions happen to be *au courant*, is likely to be offering support for functional explanations. There is, of course, nothing wrong with this except in so far as the explanation does not made a clear distinction between its own operative procedure (namely rationalising back from outcomes) and that more usual in causal analysis. Making such a distinction is critical lest the former be taken for the latter and hence functional descriptions of a social practice be construed as causal accounts of how things are. Such a misapprehension, or so we would argue, is central to some prominent discussions of the impact and importance of digital technologies.

Let us now return to our original purpose. In setting out the considerations above, we do not mean to belittle or disparage Sociology, especially to those who would like to use its findings to help understand the ethical implications of digital technologies. We believe sociological analysis should be insightful and could have lots to offer such debates. However, to repeat our introductory caveat: sociological investigations and their findings can look disarmingly straightforward when, in fact, they are not. It is, therefore, important to understand the trailing clouds of open questions, analytic stipulations, and methodological pre-suppositions with which they come. For if one does not, there is a serious chance that the Sociology taken up will be felt to be more secure and resilient than it actually is. In all probability, the resulting philosophical, ethical, professional and policy structures will be like the house of the biblical foolish man — built on sand with all of the unhappy consequences that followed. Using leading discussions of the ethical implications of new technology, in this and the next essay we illustrate just how this can happen.

The Generation of an Urban Myth[48]

In debates over the ethical and political implications of new technologies, Langdon Winner's (1985) discussion of the bridges of New York and the evidence offered for the conclusion that artefacts have politics has an iconic status. The case he makes is taken to be a conclusive demonstration that artefacts do have politics. It functions almost as a totem in every subsequent discussion in that it is repeated or referenced without reflection.

---

[47] Thos who think this is denied by Marxist Sociology might want to reflect on Marx's analysis of The German Ideology (Marx & Engels 1938) and Marcuse's (1964) notion of "repressive tolerance" or by Cohen (op cit) and Jon Elster (1986) both of whom argue that Marx offered functionalist arguments. The cases made by Cohen and Elster are, of course, disputed, but that is our point. Everything is Sociology is disputed.

[48] In using this phrase we are following Steve Woolgar's lead. See Woolgar & Cooper (1999)

However, we feel, that a number of serious questions should be asked before accepting the argument Winner makes. These questions derive from the style of sociological analysis Winner relies on. Our aim is not to de-bunk Winner (well, not entirely!) but to raise issues which those who wish to use Sociology to aid careful consideration of the ethical, political and other value dimensions of technology, especially digital technologies, might like to reflect on. If the Winner case is not as secure as is usually thought, what does this imply for arguments about ethics and technology premised on it?

To begin with, both as a way of ensuring a common base and to assist those who might not be so familiar with Winner's paper, we will summarise his key arguments.

The Politics of Artefacts

The objective Winner sets himself is to find a path between accounts of technology which insist it is neutral with regard to the effects it has and accounts which insist, on the contrary, that such effects are determined by the social and economic environment within which the technological innovation is placed. The former want to say that it is how technology is used which gives it whatever political form it might have.[49] The latter want to say that, no matter how distinctive the technology is, its political form will reflect the dominant social forces.[50] In other words, both positions treat the effects of technology as a consequence of something other than the technology itself – the intentions of its users, the power of the hegemony. In opposition to both these views, Winner argues that the technologies do have effects in their own right: they have those effects because of the way *they are* and not simply because of the environment that surrounds them. He identifies two types of "politics" technologies can have. The first he calls "forms of order"; the second he calls "inherently political", by which he means they predispose certain patterns of power and authority.

The design of the New York bridges is of the first kind. According to Winner, Robert Moses who was responsible for the design of public works for New York at the time, deliberately set the height of bridges on the parkways he was planning so as to prohibit buses (the predominant form of transport for the low income, mainly black population) using them. This had the effect of preventing this section of the community from accessing the beaches of Long Island, and in particular, Jones Beach which was popular with the white middle classes. The social consequence of a particular technological solution was to reinforce class and ethnic discrimination and inequality.

For the second kind of politics, Winner suggests that choosing to develop and deploy certain kinds of technology is to choose a "a particular form of political life" (p 31). What he means by this is that consequences are not specific outcomes but rather the tenor, form of authority and decision-making they require. The example he chooses is nuclear power but he believes the phenomenon is more general and perhaps even typical of advanced industrial nations, characterised as they are by large-scale institutions and organisational processes. The centralised decision making processes (usually described as 'command and control') which modern organisations display are, or so Winner

[49] Although Winner identifies this as a commonly held position, he identifies no one with it. The only case we can find of anyone actually defending such instrumentalism is the attribution of it to David Sarnoff by Marshall Mcluhan (Mcluhan. 1964, p 2)

[50] This is by far the most common sociological position. See the other papers in Mackenzie & Wacjman (1985)

thinks, the antithesis of open democracy. It is not surprising, therefore, that the arrangements surrounding the deployment of nuclear power have been structured in this command and control form. To put it more crudely, but we feel not unfairly, for Winner, nuclear power is predisposed to authoritarianism. And we can expect the erosion of civil liberties and other features of authoritarianism in the name of exigency to follow where the management of nuclear power is concerned.

The two types of political effects which Winner distinguishes are very different. As a consequence, we will take each in turn.

Technologies as Forms of Order

The central question is whether Winner's account of the New York bridges actually demonstrates that they are indeed political. Let us grant, for the moment, that Winner is right and that Robert Moses did deliberately fix the bridge height to prevent sections of the community accessing Jones Beach and other parts of Long Island.[51] That act of setting the bridge height with the intention of excluding certain groups is, clearly, 'political' in some recognisable sense. Moses wants to maintain some kind of social divisions. But how does that motivation on the part of Moses get transferred to the bridges? Since (we assume) no-one is going to argue that the bridges want to exclude certain groups from Long Island, all we can say is that Moses was using the bridges to achieve *his* ends. The ends, and let us agree, again for the moment, they are political, are Robert Moses' not the bridges'. That might seem blindingly obvious but it implies that the bridges cannot be viewed as *political themselves*. The bridges are neither acting nor refraining from acting to achieve any ends. They are, at least as Winner tells the story, the instruments of Robert Moses' politics; a familiar enough tale told about technology. But, and this is the critical point, this account of technology is precisely one of the standard positions from which Winner wants to distinguish his own. As such, Winner's account fails to deliver his own objective and show that technology (the bridges)have an intrinsic politics.

So much for Winner's objective. What about Robert Moses' objective? Does Winner show that, through the bridges, Moses succeeded in reinforcing segregation and social class? Or simply that such an outcome was his intention? In this respect, the bridges example is subtly different from the second example Winner cites in this part of his discussion, namely the introduction of a particular type of casting machine in the McCormick factories in Chicago. This was done as a deliberate and successful strategy to replace skilled and highly unionised labour force with unskilled and non-unionised one. There appears to be no question but that this strategy worked. After some time, unionised labour had disappeared from the McCormick work force. The union records are evidence that this happened. This is not surprising since all the enablers for it were within the control of the factory owners.

With the bridges, though, what evidence is there to show the purported strategy worked? We might say that that it stands to reason that it would, but its standing to reason is not any kind of *evidence* that secures the case. The difference between the two examples turns on the scope and location of the consequence and the evidence for it. The machine tool case is limited to a factory and the composition of its labour force, for which evidence is relatively easy to obtain. The bridges case is a diffuse social consequence affecting a large, perhaps the major part, of the local population. No doubt the claim might have been secured had it been buttressed by studies of the

[51] As Woolgar and Cooper (1999) make clear, this is not necessarily a secure assumption.

use of the highway and beaches and the effects of the parkways as 'pinch points' in accessing Long Island. This would require comparison of the parkways as access routes against other access routes that remained available. It would also require comparison of the use the beaches by different communities and the likely reasons for it.

None of this is offered. Instead, Winner suggests that the designer of the bridges wanted them to have a particular social effect; to operate as a form of order. But unless we are using the term 'political' as a way of judging artefacts independent of their effects, without an examination of their efficacy in achieving the intended outcome, this can hardly be said to be a demonstration that they are *political in their effects* and hence political at all. The most one can say, it seems, is that Winner alleges the bridges are the instruments of Robert Moses' political preferences.

So, the argument that the bridges were political is, to say the least, weak. What about the suggestion that they were deliberately designed to have the effects which Winner attributes to them? In a detailed review of the whole example, Bernward Joerges (1999) offers a number of considerations well worth noting.

1. The whole story (Joerges calls it "a parable") depends upon two interviewees whom Winner quotes from Robert Caro's biography of Moses. No other corroborative evidence is cited or seems to exist. These interviewees *attribute* the discriminatory motive as their rationalisation of the designs that Moses authorised. Ultimately, the 'factuality' of Moses intending to use the bridges to reinforce segregation rests upon the surmises of these two people. These surmises are the only evidence offered.
2. As against these inferences, Joerges points out that the design of the parkways was part of a regional transport plan. They were to provide a rapid, free flowing access route to particular destinations. However, *they were not the only routes*. The supposed excluded groups could and did get to Long Island by other means. That they didn't go to Jones Beach, or didn't go in numbers, can quite easily be understood in terms of what we know about US society at that time. This was a society riven by social, especially ethnic, divisions. Why would such groups have wanted to go to places where they would not be welcomed and in all likelihood (very) actively discouraged from staying? It does not take much reflection or insight to light upon plenty of *contextual reasons* why the beaches of Long Island may have been used the way Winner suggests they were.
3. Moses may or may not have held much the same beliefs as his white middle class peers about race, class or anything else. In all probability he did, but we don't actually know. His beliefs only matter if the choice of bridge height can unequivocally be seen to have been motivated by political ends. Joerges produces (p.10) two quite straightforward reasons of his own for why the bridges are so low; reasons, he says, which have been suggested to him by US civil engineers. First, commercial traffic, therefore including buses, was prohibited from using the parkways. This was not a local policy but a national one. There was, therefore, no design requirement to raise the bridges to accommodate buses. Given the very good transport system in the region, the buses had alternative routes available to them and did not need to use the parkways. In addition, without a design requirement, the increased cost of building to a greater height would have been unjustifiable.
   In other words, there were simple project engineering reasons why the bridges are so low.
4. To the above, Joerges adds his own speculation. Moses had two lifelong pre-occupations; the need to accommodate the motor car within urban design; and the

> value of environmentally sensitive design. In prioritising motor car traffic, Moses was following the first. In keeping the bridges as low as possible in order to meld into the urban landscape, he was following the second. This might be a speculation of Joerges part, but it is no less a speculation than the opinions offered by the two interviewees Caro quotes and Winner takes as evidence for the discriminatory nature of the bridge design.

It would seem, then, all the components of Winner's argument that the bridges of New York have politics are less than secure. First, as told by Winner, the story of the bridges is actually one of their instrumental use by Moses rather than of their having their own politics. Second, no evidence is provided that the design of the bridges did have any additional divisive effects. The segregation of beaches on Long Island was at least as likely to be the result of endemic cultural and socio-political forces. Third, no evidence is offered to show they were actually designed to have such an outcome, and plenty of reasonable grounds for suggesting they would have been designed the way they were no matter who was responsible for them. In sum, the whole story is little more than an urban myth.

But what of Winner's other argument, namely that technologies are inherently political?

Inherently Political Technologies

With his second category of technological politics Winner might have inaugurated an empirical research programme. No doubt in some circumstances and for some forms of technology, his view would be vindicated. Once we have an array of studies which demonstrate that as particular technologies evolve within our society, their associated decision processes anneal into authoritarian or other modes, then we will have evidence that, at least *for those cases and under those circumstances*, technologies have politics. However, making this case cannot be done by starting from the presumption that all technologies must have a politics, finding rationalisations for why that claim can be sustained in particular cases, *and then* claiming this proves technologies in general have politics. And yet that is exactly how, at least in the case of nuclear power, Winner proceeds.

What Winner is struggling with here, of course, is the wish to broaden the conception of the political, and hence the study of politics, beyond formal political institutions, but without at the same time fragmenting the sense of the political which we associate with those institutions. Within the formal institutions, we know what we mean when we say that every action and every feature "is political". Winner wants to take this understanding and attach it to things outside formal political processes. For social scientists, both formal politics and non-formal politics constitute "the political culture". Social science theory says that both are held together and glued to each other by the centripetal forces of dominant interests. The politics of technologies is just one aspect of this. Seen from this vantage point, Winner's view is all of a piece with standard functionalist descriptions of political life offered by sociologists as varied as Peter Worsley and Steven Lukes. Worsley (1973) points out that formal political activity is but a sub-set of the political activities in a society. Whenever we try to ensure others act in ways that make ends we value more likely, we act politically. These actions can be within formal governmental structures, formal and informal organisations (both of which constitute what Worsley calls Politics 2) or within the flow of ordinary personal interaction (what Worsley calls Politics 1). In like manner, Lukes (1974) wants broaden the conception of power, and therefore the exercise of power, to the variety of ways in which political agendas and values are implicitly prioritised and legitimated within society. This is what he calls "the third dimension" of power. Both are offering what are clearly *functional* views of politics

utilising *post hoc propter hoc* arguments: views which assume there are political consequences to every aspects of our lives and, moreover, that such consequences serve to reinforce current forms of domination.

In sum, any innovation or novel social practice will be introduced into a context of institutions and practices, what we can call the local and global social structure. As it gains in popularity and use, it helps to re-shape and hence change this structure. To be successful, though, the innovation must both sustain and amend the extant value system. It is this which makes it political. But because this applies across the whole of social life, as Worsley says, everything is political. To say that technologies are political is to say no more than under some very specific sociological description or interpretation, technologies play a role in the political culture. That is, they are political by definition.

Are Technologies Political?

What, then, are we to make of Langdon Winner's famous assertion of that technologies have politics? We have seen that the first sense he attaches to this claim has a very shaky basis. The core example, the design of the bridges of New York's parkways, is a tale that has grown in the telling and re-telling. As Bernward Joerges puts it, its current status owes more to the practice of Chinese Whispers in research referencing than anything else. In the second case, what we have is the replay of standard functionalist political sociology to demonstrate the adaptive processes of the political culture. In other words, if we take the assertion to be an empirical generalisation, either it is unsubstantiated (in the first case) or it is old hat (in the second).

Of course, it might be that that the assertion was never meant to be an empirical generalisation at all, but rather a call to arms. In his review, Joerges is clear that he thinks this really what is going on. In an interview with Joerges which he quotes (p 15), Winner states quite categorically:

> *I am not interested in theories, I am interested in moral issues. My point is not explanatory, it is about political choices.*

In other words, Winner's intent is to start a political argument about the place and power of technology in society and how that can be changed, not to provide a disinterested evidenced-based analysis of either the social causes and effects or functional consequences of technological innovation.

That being the case, the bridges of New York do turn out to have a politics, but this time they are Langdon Winner's politics not Robert Moses', let alone their own. The bridges have a place as a rhetorical device in the morality tale Winner wants us to be convinced by. Winner wants us to believe that technologies have politics so that we will be more likely to support the case for change in the power relations surrounding them. In making the argument, he doesn't start with artefacts and find politics: he starts with politics and, after searching around for a clinching case, ends up with the artefacts he describes.

Why Does All This Matter?

First, of course, it matters to get the record straight. Whatever one might think of mid-20th century New York urban planning, it is important to ensure that those who were involved in it are not pilloried; or at least, not pilloried for the wrong reason. Second, it also matters because Langdon Winner's paper and the New York bridges example, in particular, have such wide currency. It has become an article of faith that the story as re-told by Winner is a fair and accurate account

of what went on. Each telling of the tale reaffirms this and thus stands as proof of the justification of Winner's assertion. Finally, and probably most importantly, the case has become the anchor bolt both for further studies and for the analyses based upon them. These analyses have led to calls for interventions within design processes both at the curricular and at the operational level. Such interventions are proposed in order to ensure that the case of the New York bridges could not be repeated. Political and moral philosophers, professional engineers and others have joined together to try to formulate programmes of study and professional practice precisely to ensure that, as far as is possible, artefacts can be shorn of their politics. But, if the key case that all these arguments rely on, cannot be sustained, what then?

Langdon Winner has created a chimera out of the bridges of New York and, unsuspecting of its mythical status, many others have found it elsewhere too. In our view it is time we stopped pursuing this imaginary *bête noir* and began instead to think carefully, systematically and thoroughly about the social, political and ethical challenges which technological innovation actually does set us. In particular, since we might well be satisfied that some artefacts do have politics in the sense that Winner suggests, by so doing we might be able to determine *which* artefacts have *what* politics. Using a broad definition of politics might allow us to say that all artefacts have politics, but that in itself hardly in itself advances the capacity to determine the politics a given artefact actually has.

ESSAY 8

DISCLOSIVE ETHICS

Introduction

Philip Brey (2000, 2010) believes we underestimate the effects of technology in general and information technology in particular. Computer systems and software have consequences which, by and large, we have failed to notice; consequences which are embedded in the technology and

which carry important ethical implications. Along with other commentators, Brey suggests we need to adopt a combination of Value Sensitive Design and Disclosive Ethics. This combined approach would, or so it is hoped, reveal commitments we are unaware of and hence avoid some of the more deleterious effects of information technology. Lucas Introna has used Brey's work as well as that of Bruno Latour to develop an account of the deeply political character of digital technologies (Introna 2007)

We have reservations about the arguments which Brey and Introna offer. We also believe there are plenty of practical reasons why the approach Brey suggests wouldn't work. However, these and like views seem to be gaining currency within the research communities concerned with the human and social aspects of computing. Efforts are underway to promote them more broadly (Cummings 2006, Manders-Huit 2011). Although in some ways we find this popularity unsurprising — many colleagues engaged in research within the HCI and CSCW communities seem particularly drawn to what appear to be new and controversial ideas coming from the social sciences — it *is* a little puzzling. As far as we can tell, the analysis on which Disclosive Ethics rests is a species of very conventional (not to say traditional) Sociology. Moreover, the arguments for an ethics based on that analysis appear to be grounded in a common fallacy, one often associated with the adoption of social science findings by other disciplines. The possibility that a particular sociological description of some phenomenon can be provided does not mean that this is the sociological description which must be used.[52] Both Brey and Introna base the need for Disclosive Ethics on this fallacy. The result is that we are left in the uncomfortable position of being asked to adopt a particular approach to ethical judgments simply because a sociological analysis intimates we should.

In this essay we will set out our reservations. We will focus first on Brey's (2010) extended account of his position and the general analytic scheme he suggests for identifying what he calls "embedded consequences". Having set out our views on Brey's position, we will turn to Introna's use of Brey's scheme. In both cases, we will mostly be concerned with their general character and the ways the scheme is used to demonstrate the ethical import of specific information technologies. Having set out what might be thought of as "disciplinary issues", we will move on to a summary of more practical matters and propose there are good reasons to suspect the proposals Brey and Introna make will not work, or not in the way which they suggest they should.

Before we start, though, we want to make two important points. We are not here setting out to challenge or otherwise dispute the style of sociological analysis on which Brey relies. Posing questions of it should not be read as a subterfuge for arguing for some other form of Sociology. We simply ask: Will this Sociology do what Brey wants? Second, we are not denying that some systems and software do indeed pose ethical challenges which we should all reflect on. As will become clear, we are less than convinced that such challenges should be said to be embedded in the technologies themselves.

Embedded Consequences

Brey wants to distinguish those cases where the use of a particular system or piece of technology may turn out to have ethical implications from those where those implications are embedded in the technology itself. Both are said to be consequences of the technology. For Brey, the use of geo-locational applications to stalk people for example, whilst definitely an ethical matter, is not an

[52] If it were not so cruel, you might label this a form of the naturalistic fallacy; one peculiar to cross border trading in the social sciences.

embedded consequence. The ethical challenge comes, rather, from the use of the technology by particular individuals. In his view, almost all discussion of the ethical challenges of information technology is of this kind. Debate on such issues is valuable and important, but not what he has his eye on.

Brey wants to focus on consequences which derive from the way the technology has been designed or from the way it plays in the network of infrastructure and correlated systems within which it is deployed. In this he follows Langdon Winner (1985) and proposes that such consequences may be intentionally or unintentionally built-in. He goes on to assert they may also be obvious and generally recognised or unrecognised and hence unacknowledged. These distinctions which form the core of his analysis, are set out in the table below. Three of the four examples in the table are ours.

	Intended	Unintended
Recognised	Traffic Light Management	SMS & Texting
Unrecognised	Quality of Service	ATMs

Taking each cell in turn. The software that manages traffic light systems uses allocation rules to produce orderly traffic flow. These might simply be constant or variable time allocation or, as is often the case with lights used when road works are in place, by relative volume of traffic as sensed by motion sensing cameras. An orderly flow is intended, and it is obvious when it is produced. The case of SMS and texting is a little different. The Short Messaging System (SMS) was provided by mobile telephone companies to enable engineers to communicate with each other and for the companies, themselves, to be able to send messages to their customers. However, once the facility began to be used outside the engineering environment, it rapidly took off and became the mainstay of growth in mobile phone use. Whilst this was not what the engineers intended, the creation of a product and its market, "texting", was obvious.[53]

Quality of Service management is necessary in any environment where demand can outstrip supply on a moment by moment basis. In managing access to broadband, for example, suppliers have introduced functionality which enables them to tune the access individual users receive. This tuning can be on the value of the account, the type of application being used, or a number of other parameters. Only very occasionally will users understand the effects of such management, usually experienced though the slowdown of delivery of video, for instance, or the inability to access certain other bandwidth hungry applications. Quality of Service management allows broadband companies to conserve their resources and limit their investment in bandwidth. Thus, they manage their costs and increase their profits.

With ATMs (this example is Introna's & Whittaker's (2005)), the story is slightly different. ATMs are designed to be High Street located dispensers of cash. They are, therefore, designed for the mass of the population. However, because they are designed for the majority, they assume certain physical and psychological capacities: the ability to see and to read; the ability to manipulate a keyboard; the ability to understand pin numbers and remember them; and so on. When the deployment of ATMs coincides with the closure of High Street banks themselves, those who cannot

[53] See Taylor & Vincent (2005) for the history of SMS

operate ATMs lose ready access to cash. This combination of circumstances leads to discrimination against them by the retail banking sector. Such discrimination, of course, violates the social value of equal treatment for all. For Brey, and here again he follows Winner, of all of the types of consequences, it is unintended and unrecognised consequences and their correlated values which are the most important.

First, let us look at the distinction between recognised and unrecognised unintended social consequences. For sociologists, this is a very familiar distinction, one which is insisted on by innumerable sociologists. For example, it is the fulcrum around which Robert Merton's (1949) classic paradigm for functional analysis turned. Merton termed the two kinds of consequences "manifest" and "latent" functions. The paradigm set out how, for any designated system of action (social institution, society, sub-culture, social practice), the consequences of courses of action could be shown to be either functional or dysfunctional; that is, they could either contribute to the adaptation of the encompassing system or to its disruption. Tracing through the latent functional and dysfunctional consequences of social phenomena became the central motif of much sociological thinking and analysis.[54] Certainly, in one way or another, it underlies the sociology of science and technology[55] and as our sketches indicate could easily be applied to software and computer systems. As can be seen, the snippets we have given are functional in form.

Anyone unfamiliar with the history of sociological disputation (which runs wide and deep) might wonder why this matters. We are not, here, engaging in arguments for and against functionalist arguments, but attempting to draw the attention of those not versed in the debates over functionalism to the unresolved and highly contested form of that analysis. When argument forms openly designate themselves as functionalist, they are apt to be discounted by many sociologists simply on the grounds that functionalist arguments are held to be inherently conservative in character. In many cases, however, functionalist forms are not openly identified as such, often because, as with Brey, those who put them do not recognise their functionalist character. Both those who advocate and those who dismiss functionalist analysis recognise that there are real difficulties in setting functionalist theories out in a valid form and in securing those functionalist schemes as empirically sound descriptions of their designated phenomena. Brey offers no solutions to these problems and simply begs the question with respect to the very serious difficulties inherent in the type of approach he has taken.

Because his analytic structure is so clearly functionalist in form, it is open to a number of the considerations which we outlined in Essay 7. In fact, Brey's framework displays most, if not all, of them. As a consequence, the grounding for the requirement to introduce Disclosive Ethics turns out to be extremely weak. In the rest of this section, we will outline why.

---

[54] This is not the place to elaborate on the advantages and disadvantages of using terms like "perspective" to characterise different sociologies. Nonetheless, as we explain in the previous essay, we would want to say that those perspectives which give primacy to the explanation of social structures and processes at the global level are all, inevitably, functional in form.

[55] Given Merton's role in the development of the history and sociology of technology, where he has become the leading example of what more recent sociologies of science and technology are *not* doing. Brey's failure to acknowledge the style of sociology his own analysis deploys is more than a little ironic.

On Brey's account, the embeddedness of consequences in an artefact depends on the extent to which such an artefact can have autonomous effects and the relative specificity of its context of use. A bridge, for example has more autonomous consequences than a power drill because the drill depends upon an operator for its use.[56] Furthermore, because the bridge is fixed in its location and has a single purpose, very particular consequences can be said to be built into it. The traffic flows it allows determine certain outcomes.

The question to be asked here is just what we mean by saying something is built into the bridge. Obviously there are the physical characteristics. These constitute the bridge; they are the bridge. Because it has a certain set of physical characteristics, it enables some kinds of traffic (private cars, say, but not buses) to pass underneath. Let us allow, for the moment, the suggestion that because only some people own cars, the bridge can be said to discriminate against non-car owners who use buses. But do we want to say that this discrimination is built-into the bridge in precisely the same way that the physical features are? If so, isn't it part of the constitution of the bridge as well? If we want to say that this is so, that discrimination is part of the constitution of the bridge, then there is no logical distinction between the bridge and its consequences. Brey has taken over this weak argument and further weakened it.

Of course, saying that the discrimination is built in could really be just a way of reinforcing the dramatic tie between the bridge's physical characteristics and its social consequences. In this way it would be, so to speak, a rhetorical flourish which helps to add force to the story being told. Such a device might be necessary because the lack of close coupling between the elements in the story. With Winner, we go from physical characteristics to traffic flows to social demographics to discrimination without securing any of the steps on the way. Certainly no evidence is offered by Brey or Winner to show that the suggested consequences did actually come about.[57] Without the assertion that the social consequences are built-in, the functional account loses its plausibility. As a result, it can only be secured only by connections which have an "It stands to reason...." ring to them.

So, what are we left with? The argument that consequences are built into artefacts falls on two grounds. First, it collapses the logical distinction functional analysis requires between a social phenomenon and its consequence. This distinction is needed for a consequence to be a function of the social phenomenon. Second, the collapse of the analytic space compounds the failure to offer evidence that the consequence did indeed come about. Here, in adopting his example, Brey simply incorporates Winner's lacuna. Finally, given the weakness of the functional case, all that is left to support the contention is an appeal to intuition.

From Embedded Consequences to Embedded Values

We turn now to the way Brey moves from consequences to values. Brey begins by suggesting that values are often realised only in part. For example, the value "freedom" is only realised in full if everyone in the world is "completely free". Given the "constraints and limitations", as he calls them, which keep people from being "completely free", freedom is only realised "to a degree" (all quotations from p 46.). What are these constraints and limitations that limit us simply because we

[56] This is not a chance example. Bridges, it will be remembered, were at the core of Winner's argument.

[57] To be fair, Winner does provide some sort of case. But, ironically, it is (a) not the case he actually wants to make and, as we have said, is (b) very weak.

live in a social world? Limitations to do what, and where, and under what other circumstances? What would we be free to do but for the fact we live in a social world? What rights others have over us and what obligations we owe to others has been the central motif of ethical theory. Trying to define and understand them has been a key debate in philosophical ethics (see for example Nagel 1991). Brey simply glides over this debate confident in the assumption that we can say *tout court* that values such as freedom are always realised only in part. Another way of looking at this is to say that Brey simply mischaracterises the actual value of 'freedom' as it is held at least in western society, where 'freedom' is actually treated as a qualified value, one to be redefined relative to and not pre-emptive over other values. As we will see, Brey himself doesn't want a company such as Google to be free to exploit its advantage as maximally and persistently as it might like.

Having made this claim, Brey goes on to suggest that the partial realisation of a value and, in his terms, its unjust limitation, can be the consequence of a piece of technology. Where this occurs "systematically" (and again we get no help in understanding what that might mean), the limitation on full realisation of the value can be taken to be an embedded consequence. In other words, in such circumstances the realisation of partial value outcomes can be the manifest or latent functions of technology.

There is a small but important point to be considered here. The consequences which Brey cites are all construed negatively. Presumably, though, embedded vales need not always be negative. To turn to the bridges of New York example again. Had they been built 2 metres taller presumably the fact that they allowed a wider variety of traffic and hence a greater array of social groups to use the highway, would be regarded as a positive consequence. Improving integration would, we assume, be a positive value outcome. Or, to take another example: computer controlled milking parlours. This technology has significantly reduced the financial and administrative burden on farms, especially small farms, as well as allowing improved animal welfare. What otherwise would have been unprofitable or marginally profitable farms have been able to say in production. In turn, this has kept families in the countryside and so allowed schools and other services to be maintained. Where this has not happened, rural areas have been "cored out" and so lost their sense of community spirit. Milking parlour software, or so it might be argued, has had the consequence of modifying and preserving a way of life.

What are we to say now? The introduction of the milking systems, like the improved power of tractors, has reduced the need for farm labour and introduced mass production techniques into dairying. For many this can only be a bad thing. But it has kept farms running and this presumably is a good thing. How are we now to decide whether the identified embedded consequences sums positively or negatively?[58] A significant part of the problem is actually the use of a kind of circular logic. Analysts such as Brey turn to Sociology in the hope it will provide strong (perhaps even scientific) evidence that will support, if not vindicate, their ideological, political and/or moral preferences. And yet, it turns out that they depend on their original preferences to decide how the sums are to be totalled.

[58] There is yet another irony here. If, as many want to do, you turn to Heidegger for guidance on how to make these determinations, you are likely to end in an impasse. Heidegger's (1977) rejection of the "enframing" character of modern technology reaches its apotheosis in computer-controlled mass production. At the same time, that rejection is rooted in prioritising the (agricultural) community and its way of life.

Our purpose in labouring the lack of clarity is to emphasise just how loose, and hence slippery, the steps in the analysis are and to emphasise that an argument which is essentially a preaching to the ethically converted, shows no awareness of the fact that valuations in a society are not necessarily uniform and consequently offers no means of resolving these differences into unified judgements. Neither firm conceptual connections nor strong empirical evidence are provided. But the analytic security of each step in the description is what gives functional analysis its plausibility. Brey simply waves his hands at how these connections are to be made. To understand just what could be meant by the suggestion that technology can have value outcomes, we will look at an example which Brey cites, where a clear set of ethical outcomes generated by a specific technology is said to have been demonstrated. The case is that of web search engines as described by Introna and Nissenbaum (2000). This example has another useful feature. Like Winner's bridges, it has become totemic in the literature.

The web, as we all know, is gargantuan. The task for search engines such as Google is to reduce the search space of web pages to manageable proportions. This is done by a combination of page indexing and page ranking. The ranking of indexed pages enables them to be presented to the searcher according to some order of relevance. The search engine, then, reduces the number of pages to be sorted and then sorts them by relevance criteria. At its core, the algorithm Google uses defines relevance in terms of importance with the number of pages linked to a particular page being taken as a proxy measure of importance. Once a page has been defined as important those pages linked to it both derive importance from it and serve to bolster its importance (a kind of increasing marginal return to page rank power). Thus, what emerges is a highly configured landscape with a (relatively) few high ranked pages and a (relatively) large number of low ranked ones.

The key term here is "at its core". In addition to this page link criterion, Google uses other criteria which, it says, underpin its competitive advantage. For this reason, it will not divulge them. These other criteria working together with page linking and ranking enable Google to provide the service it does and hence attract the users it does. In turn, this allows it to charge a premium for advertising and other services. In business-speak, the search algorithm is the heart of Google's business model.

How do matters of ethics enter all this? Well actually for Introna and Nissenbaum, they don't. Following Winner's lead, they want to talk about the politics of technology not its ethics. This is not a trivial point. The relationships between certain orders of valued ends (e.g. freedom, to use Brey's example once again) and certain political means (for example, representative democracy) have long been the subject of "moral" debate. That the practice of politics is entangled with the advocacy of values is undeniable. What is very deniable is that there is agreement on a clear and well understood way of demarcating them. In citing the search engine example, Brey provides no indication of how to translate Introna and Nissenbaum's "politics" into his "ethics", unless we are to assume that all politics *is* ethics.[59]

Introna and Nissenbaum believe the internet and the web are likely to follow the same development path as the mass media and commercial broadcasting. Relying on McChesney's (1997) review of the media, they propose a scenario where commercial interests could become "woven in to the very fiber" (p169) of the internet. If this happens, what was a public good will have been

[59] This assumption, though easy to make, would be difficult to sustain. See Nagel (1991) and Taylor (1995)

suborned by vested, private interests. Further, since, they assert, web search engines have biases built into them, we can already see the beginnings of this happening.[60]

It is Google's refusal to publish its algorithm in full which is central here. Introna and Nissenbaum accept that search engines must operate in much the way Google's does. However, how they work should be transparent to the user. At present, any user of Google cannot know how the search space has been configured nor how the relevant results have been compiled. As a consequence, users cannot tell if anything *they might have felt to be important* has been left out or lowly weighted. The withholding of information about the weighting algorithm reduces transparency and so is a deliberate, political act. Lack of transparency sets the reciprocal challenge to the writers of web pages. They cannot know how to get their pages weighted highly by Google. Although there are rules of thumb for the design of web pages, where to put important information, how to tag, and so on, everyone who is designing a web page is, in fact, second guessing the algorithm.[61]

Introna and Nissenbaum recognise that there are many different search engines. 'Why should we be worried about "bias" if we have choice?', one might ask. Won't a market for information access emerge which will provide everyone with what they need? For Introna and Nissenbaum, the acceptance of the web as a market (or market of markets) is just as bad as accepting the bias of search engines. However, their position can't be that search engines aren't meeting our needs, only that we cannot tell if they are meeting them as well as they might. To operate efficiently and effectively, markets have requirements for free flow of information that are not and will not be provided by the web.[62] And anyway, are markets the right way to distribute access to a public good? Should not public goods (access to full information) be open and available to all?[63]

The combination of a lack of transparency and the use of market mechanisms to provide access to a public good is what, in the end, makes web search engines political. This political character is achieved at the cost of a partial realisation of a value namely freedom (to access information). Without transparency, we cannot know if the algorithms used by Google and others are sacrificing the interests of the majority in retaining open access to information to the commercial interests of a minority. And, whilst we cannot know this, given the history of broadcasting and the mass media we ought to assume not only that it is happening but that the process will accelerate. As this happens, or so the thinking goes, what had been an innovation shaped as a public good to provide open and democratic information access to all will be controlled by sets of vested interests. It will become anti-democratic. Anti-democratic social institutions are, or so it is implied, unethical.

---

[60] We have already discussed this use of "built into". Its use has become the leading term in discussions of technology and ethics.

[61] We will simply note in passing that *if* everyone knew how to get a high ranking by satisfying the algorithm, the need to provide an ordered listing would simply mean the ranking was achieved in some other way.

[62] This is an interesting failure to distinguish the presuppositions of an (or *the*) pure economic model of free markets with the conditions within actual markets. Even economists don't think that real markets are like pure markets in all respects (Kuorikoski, Lehtinen & Machionni 2010).

[63] Of course, in many respects the internet is *not* a market, since many of its services are provided for free.

A number of points should be noticed here. First, the analysis slides around between the various types of consequence used by Brey. Because Google won't publicise all the criteria and how they are weighted, we cannot know if, for commercial reasons, the algorithm is deliberately directed to select certain pages and not others, although, given the commercial environment that Google operates in, Introna and Nissenbaum suggest we would be wise *to suspect* that it is. Such deliberate intervention in the operation of the algorithm would be of the same instrumental type as Langdon Winner ends up describing for the bridges of New York but is, of course, not the kind of *unrecognised* consequence that Brey is hunting down. This evidence would then secure the steps from social practice to consequence. However, Introna and Nissenbaum do not even produce the most minimal evidence to support their claim. They show neither that the "bias" (as they call it) is present and intended nor that it actually does serve dominant commercial interests. What earlier we called Winner's lacuna, is repeated here too. Without such evidence, the claim is no more than vague hypothesis (at best) or an allegation (at worst).

The second point to note is the lack of clarity in the concepts such as 'freedom' and 'democracy' that Brey deploys. We have already pointed out that 'freedom' describes a very constrained value in our society. Our freedoms are limited by the freedoms of others. In addition, the notion of 'free access' is distinctively ambiguous. Free access to information meaning access open to all does not imply that access should be free of cost. There are similar confusions in the way in which interests and democracy are connected. The difficulty of determining what *exactly* are the configurations of Google's (or any commercial organisation's) interests or those of any users means that what we are offered are stereotypes of 'commercial interests' and 'democratic interests'. The contrast between them relies on a preaching-to-the-converted sense of ‘anti-democratic’, whereas, for large tracts of society, the fact that, for example, corporate organisations are not run democratically does not mean that they are undemocratic, let alone anti-democractic. Such simplistic preconceptions of values run up against the undetermined character of values accepted in society.

The second point concerns materiality. Even if the technology was biased and did work in the way claimed, would that matter? Or rather, would it matter for us, *the users of Google*? After all, if Google allows us to harvest relevant and useful information from the universe of web pages, does it matter to most of us most of the time that some particular pages are not selected? If Google does what we want it to do, is it material that the results might be marginally effected by commercial considerations? As users, we, and we suspect others, would think not.

It might matter, of course, if the world wide web and other services provided by the internet were public goods. But are they? We can see that the original DARPAnet could be said to be a public good, provided as it was from Governmental funds, as presumably was the original DNS service. Equally, the hypertext protocol could be said to be a public good originally released as it was by Berners-Lee and CERN for the benefit of the (scientific) public. However, we find it hard to see the web of published documents as a public good, provided and funded as it is by a wide variety of individuals, groups and organisations. Search engines were one of the first services to be provided for that web of documents. In many ways the old metaphor of an information superhighway probably remains the best way of thinking about all this, with the internet infrastructure a public good, like the road system, around which anyone can set themselves up as a site. The parallel then becomes between search engines and wayfinding services such as maps and navigation systems. No-one thinks these are or always should be public goods. Moreover, they can be at vastly different scales and for very different purposes. No one supposes that there should be a single integrated map which showed everything (what on earth could that be?).

It seems, then, that both sides of the Introna and Nissenbaum argument fail. The arguments about the politics of a lack of transparency turn out to be assertions with no evidential base. Those about the suborning of a public good are, at best, muddled. All we are left with, then, is a commonplace but weak analysis of possible latent functions to act as a bridge from consequences to values

In the end, then, we are no further on in securing the move we need. Neither Brey nor Introna and Nissenbaum actually make the case for the embedding of values as latent functions in information technology. Yet, without accomplishing this, the need for a Disclosive Ethics is left hanging in mid air. Of course, such an ethics might be a good thing to have and highly necessary. But the arguments considered so far don't seem to give us reassurance on either count.

Disclosive Ethics

If we were to need a Disclosive Ethics, what would that be? For Brey, the thing to underscore is that the consequences of technology are often unrecognised. He calls such consequences "morally opaque" (Brey 2010 p51). Consequently, the task of a Disclosive Ethics is as follows:

> *Many computer-related practices that are morally opaque are so because they depend on operations of computer systems that are value-laden without it being known. Many morally opaque practices, though not all, are the result of undisclosed embedded values and norms in computer technology. A large part of the work in disclosive computer ethics, therefore, focuses on the identification and moral evaluation of such embedded values. (Brey op cit pp 51-2)*

This will be done, he suggests, by looking first at how some system jibes with a given ethical principal such as the preservation of privacy. Next, as and when the introduction of the technology coincides with apparent changes in values, tensions between ethical principles and the priority ordering among them are taken up. Finally, at what he calls the application level, the outcomes of the previous two levels of reflection are applied to technical decisions. Furthermore,

> *Disclosive Ethics should not just be multi-level, ideally it should also be a* ***multi-disciplinary*** *endeavour, involving ethicists, computer scientists and social scientists. The disclosure level, particularly, is best approached in a multi-disciplinary fashion because research at this level often requires considerable knowledge of the technological aspects of the system or practice that is studied and may also require expertise in social science for the analysis of the way in which the functioning of systems is dependent on human actions, rules and institutions. Ideally, research at the disclosure level, and perhaps also at the application level, is best approached as a cooperative venture between computer scientists, social scientists and philosophers. If this cannot be attained, it should at least be carried out by researchers with an adequate interdisciplinary background. (Brey op cit. p.53 emphasis in original)*

This raises a host of issues, some of which are disciplinary and some not. We'll start with two disciplinary ones.

Disclosive Ethics is required because of the moral opacity of systems. We ordinary citizens cannot tell if they are discriminatory, biased, anti-democratic or whatever. Sociological analysis will tell us which is what and why. But the logic of moral terms, including second order moral terms such as

these, (which is what ethics is about, after all) cannot be fixed by reference to some set of descriptions of how things are. If that were possible, ethics would be a lot less challenging and intractable than it is. Calling a set of software discriminatory, biased or whatever is not a morally neutral description, but rather judgement of how it is to be viewed. Because there can be no recourse to "the facts" outside of the description given, moral judgements cannot be secured by those judgements alone. We have to consider the details of particular cases and instances. Not every selective process is discriminatory or biased. And a system which, on some occasions is, indeed, biased, may not be on others. It is all very well to say that Disclosive Ethics will provide this critical reflection. But how good and how thorough will this reflection be if the value determination is already made?

This worry relates to a second concern. We think it was best summarised by R.M Hare (1986) as follows, though the example he was considering was slavery.

> *Nearly everybody would agree that slavery is wrong; and I can say this perhaps with greater feeling than most, having in a manner of speaking been a slave. However, there are dangers in taking for granted that something is wrong; for we may then assume that it is obvious that it is wrong and indeed obvious why it is wrong; and this leads to a prevalence of very bad arguments with quite silly conclusions, all based on the so-called absolute value of human freedom. If we could see more clearly what is valuable about freedom, and why it is valuable, then we might be protected against the rhetoric of those who, the moment anything happens which is disadvantageous or distasteful to them, start complaining loudly about some supposed infringement of their liberty, without telling us why it is wrong that they should be prevented from doing what they should like to do. It may well be wrong in such cases; but until we have some way of judging when it is and when it is not, we shall be at the mercy of every kind of demagogy. (Hare op cit p. 165)*

Mutatis mutandis the same holds for Disclosive Ethics. Until those who hold that software systems violate their rights to free access to knowledge (or are discriminatory, or invasive, or whatever) can demonstrate, first, that the systems have they effects they claim and second tell us why it is wrong that they do so, we will, as Hare says, be at the mercy of any demagogue who opposes technological innovation.

Third, there is Brey's overly sanguine view of the state of the disciplines of Ethics and Sociology. Wittgenstein once described Philosophy as a "motley". Had he been talking about Ethics, he might well have called it a "mêlée". Nothing is settled. Almost every position on any topic is under siege from some other position. There are certainly no agreed approaches which can be used to determine the moral value of consequences and actions. Utilitarianism vies with Deontology whilst Virtue Ethics has recently gone through a renaissance. Moreover, there are even highly charged debates over what the proper basis and scope of ethics should be.[64] How is all of this to be reduced to clear maxims and rules of thumb for use by engineers and designers without resorting to a vacuous and principle-free pick 'n' mix approach? Appealing to ethical commitment to values which are generally accepted such as freedom or democracy is no substitute for laying out a methodology for ethical choice.

[64] See Putnam (2004), MacIntyre (1990)

Add to this the general condition of Sociology with its unresolved debates, non-converging arguments and the fact that any functional analysis of some institution or social practice can be countered by an alternative equally functional account, often with diametrically opposed conclusions and contested links between phenomena, evidence and conclusion. Ethics and Sociology are not pacific fields of endeavour. Dispute, even on the most fundamental concepts, is endemic. Moreover, such dispute is not between positions which are secure and unambiguous and those which are not. *All* positions are loose, ambiguous and unsystematic. Erasing or short circuiting these debates to try to provide the kind of advice which designers and engineers will see as useful and would value is, as we well know, rightly only going to be dismissed by sociological (and philosophical) colleagues as trivialising, or worse.

Politics, Ethics & Metaphysics

The idea that the means of generating economic value serves particular social interests is an old one. Whilst its most familiar formulation might be in Marx and Englel's famous apothegm about the hand mill and the steam mill, the proposal can already be seen in the political beliefs of Gerard Winstanley and the Diggers and probably has its roots in the medieval radicalism of both Britain and Europe. More recently, it has been a constant theme in almost all accounts of the development of modern technologies. It is not surprising, then, that as we have moved from breathless hyperbole about an imminent "information society" (Castells 2010) to more measured accounts of the role and consequences of information and communication technologies in contemporary society, the argument has been pressed there too. As we discussed in the previous essay, a key contribution to this discussion was Langdon Winner's (1985) discussion of the "politics of artefacts". Whilst Winner was not discussing information technologies, nor indeed technologies which could be said to be the prime means of generating economic value, nonetheless his arguments have become the bedrock on which on which most discussions of modern technology, especially information technology, are built. Having swallowed Winner's argument, such discussions go on in an unreflecting way under the assumption that all that is needed is yet more demonstration of its importance and further description of how political consequences manifest themselves.

We have argued that Winner's case is not very convincing. We see it more as a tract than an analysis, its aim being to raise an issue rather than demonstrate an argument. As a consequence, we find ourselves demurring from proposals that take its descriptions as their departure point, believing as we do that it stretches its claims beyond the evidence that supports them. What Winner does not do, and despite all that has been said about his paper perhaps he never intended to do, is provide *the* conclusive demonstration at all technologies are inherently political. However, because this is what he has been taken to have accomplished, unintentionally or not, Winner has acted as a Pied Piper. Researcher after researcher has set off to follow where they thought Winner was leading, only to end up in some very strange places indeed.

In this section, we will take all this up in relation to the work of Lucas Introna.[65] We will show how, because of his reliance on Winner and the weaknesses of the approach he uses, Introna has to resort to 'empirical stretch' to secure his conclusions. We will then turn to the discussion of how the implication of Introna's analysis should be taken up by software engineering. Here we will be

[65] The papers we will concentrate on are Introna and Wood (2004) and Introna (2007). Other similar and closely related examples are Introna & Whittaker (2005) and Introna and Nissenbaum (2000)

concerned mainly with the cogency of arguments made by Bruno Latour since they provide the rationale for Introna's suggestions.

The Politics of CCTV

Even without the occasional public revelations of how CCTV sometimes gets used, knowing what we do about the agencies which use face recognition technologies in public places, we are likely to be more than a little sympathetic to the allegation that they could be politically biased. Study after study has shown the widespread use of stereotypes in policing and security matters. However, this is not the argument presented by Introna and Wood (2004). Rather, following his related study (Introna and Nissenbaum 2000) of search engines, the suggestion is, first, that the operation of the relevant algorithms are not open to scrutiny, and second, the technology is a 'silent' one. We do not know when our images are being captured and processed. Because we don't know it is happening, and even if we did the process is inherently unscrutinisable, there is the possibility of bias and hence what is termed "micro politics". As we will see, the fact of this 'micro-politics' is not a discovery made by investigation and gathering evidence. It is a stipulation. Because of the very broad definition of 'politics' that is used, for any technology and its alternatives, there *must* be micro-politics in play.

Following the line of argument set out in the earlier study of search engines, the need for scrutiny is held to come from the technical requirement to reduce the information space before comparisons of face patterns can be made. Digital CCTV images are huge. Megapixel colour cameras are now widely deployed. Given the way colour is represented in image processing, this means each CCTV image could be as large as 8 million bits. Face recognition software tries to identify a face pattern within those 8 million bits in real time and then to compare it to a database of stored images or templates. Introna and Wood report that to prioritise speed of analysis and comparison, the face pattern may be encoded in as little as 84 bytes. The information reduction and determination of the array of pixels which constitutes a 'face' is done by complex statistical analysis. Based on these procedures, patterns within the image can be associated with patterns in the database. Once identified, the 'face' can be compared to the relevant database.

Information reduction combined with other features of the process lead to what Introna and Wood call "bias" in the system. Such bias leads to 'micro-political' consequences. First, the algorithmic processes *are* statistical and therefore some level of (randomised) error is to be expected. Second, the comparison to the images in the database is only as robust as the robustness of the images there. Lack of representativeness in the sample population or poor quality comparator images will all affect the robustness of the association. The possible implication of these is summarised by Introna and Wood as follows:

> *To conclude this discussion we can imagine a very plausible scenario where we have a large database, less than ideal image due to factors such as variable illumination, outdoor conditions, poor camera angle, etc, and the probe image is relatively old, a year or two. Under these conditions the probability to be recognized is very low, unless one sets the false accept rate to a much higher level, which means than there is a risk that a high number of individual may be subjected to scrutiny for the sake of a few potential identifications. What will be the implications of this for practice? (Introna and Wood, op cit p 189).*

Such implications are defined as unanticipated and unacknowledged consequences following from possible combinations of circumstances: the suppliers may oversell the robustness of the technology; the operators may not understand the system's limitations; the socio-political environment may encourage a tendency to accept false positives, and so on.

> *One might imagine that in an environment where there is an acute sense of vulnerability it would not be unreasonable to store these false positives in a database 'just in case'. These false positive may then become targets for further scrutiny. Why? Just because they have features that make them more distinctive. We are not saying that this will happen. We are merely trying to indicate how seemingly trivial 'technical issues' can add up to political ideologies at the expense of some for the sake of others.(Introna and Wood o[cit p193)*

Before we move on to consider where, on the basis of this case, Introna wants to take the argument, we ought to make a couple of points. First, it is clear that no-one is saying that the bias of these systems is the result of what elsewhere has been called 'institutionalised racism' or the like. It is not the effect on the use of the technology of a cultural milieu that Intona and Wood are describing. It is, rather, the effect of the technology within a complex socio-political and cultural milieu which is micro-political. This is, of course, directly in line with Winner's argument.

Second, even though CCTV is highly visible, the "silentness" of the technology is of vital import. However, for ethical evaluation, the relative harm that derives from this silentness has to be calibrated against the perceived level of harm from the actions that the technology is designed to prevent. We can, indeed, imagine circumstances where such capture would certainly be felt to be unnecessarily invasive. However, we can equally well imagine cases where it would be accepted as proportionate, pre-emptive action. Yet again, it all depends on the circumstances. That the technology is silent is not, by itself, either politically or ethically significant.

Third, we can always imagine, as Introna and Wood do, scenarios where over-zealous selling, poor professional practice, and so on lead to misuse of the technology. But imagining scenarios is not describing actual instances and gathering evidence of actual cases. Once again, as with Winner, the lack of evidence from actual technologies in actual circumstances that the consequences were being realised, means all we are left is little more than scaremongering.

It is our contention that Introna and Wood end up in this position because they have taken Winner's account to be both a factual and generalisable description rather than either a political argument or the sketching of a potential research programme. For them, Winner describes not just what will happen but what must inevitably happen. Consequently, Introna and Wood see their task to be the teasing apart of the processes by which what is termed an "unauthored strategy" (or more familiarly a "hidden hand") works to achieve this general outcome. This unauthored strategy serves to ensure that than technological innovation will fit and reinforce the existing dominant socio-political and moral order. Through its micro-political enmeshing with existing institutionalised norms, values and practices, technology has the latent moral and political function of reinforcing the *status quo (*which conclusion is not the result of tracing a succession of causal connections between operating cameras and stable properties of the system but, once again, a purely functional derivation. This enmeshing provides information technology with its significance. Information technology is now shaping how we experience the world. It is to the analysis of significance rather than the causal description of effects to which this whole approach is given

over. The finding that technologies have politics is only important because of the significance which can thereby be given to them.

The Significance of Information Technology

For Introna (2007), information technology has the critical role in modern society. There is a duality to this. As a technology, it has become the pre-eminent source of images, metaphors and ways of thinking which re-affirm what Charles Taylor calls the "social imaginary" of modern life (Taylor 2004). This imaginary is instrumental rationality or what Ellul (1964) called "technique". Indeed, by and large, rationality is defined only in means/end terms. The instrumental outlook, what Borgman (2008) terms "the device paradigm" moulds — or "En Frames" to use the Heideggerian expression — the way we think. The second aspect comes from the pervasiveness of this technology. Unlike, say, the internal combustion engine or electric power, information technology is not just to be found everywhere; it is in everything. It is all pervading. It is shaping our experience and thus our conception of who and what we are. To paraphrase Orwell: all technologies determine our social imaginary, but information technology is more determining that others.

Introna starts from Latour's (2002) assertion that instrumental rationalism is predicated on the separation of means from ends. In its view, technologies are seen only as means and often described in tool-like terms. As tools they are morally and politically neutral. It is those that design, build, deploy and use technologies who make them a means for good or harm. However, because of their significance for the shaping of our ways of thinking about and experiencing the world around us, such technologies must be drawn into moral discourse. This will only be possible if the disjuncture between neutral means and valued ends can be overcome. To do this, Introna invokes Latour's ontology of "folding" as the means by which this can be done.

As we saw in Essay 4, Latour thinks the classic binary distinctions which underpin modernism (fact v value; subject v object; representation v reality, and so on) are all suspect. They are based upon a metaphysics which, first, divides the word into human and non-human entities; and second, prioritises the former over the latter. This metaphysics permeates our imaginary and forces us to think that there can be no logical bridge between the two categories. However, from his studies of science and technology, Latour wants to argue that such binaries are unsustainable. Facts are enmeshed in values; representations are all the reality we have. Moreover, the ontology of subject and object blinds us to the moral agency that objects (especially technologies) can have.[66] Objects, tools, technologies are not just used in ways that have moral consequences, they are immersed in and constitutive of moral choices. Studies from as varied cases as nuclear power, electric cars, the development of vaccines, and many more have shown that science and technology develop within and as part of a moral order. To overcome our blindness to this, Latour proposes we should adopt a unified ontology of mutual relationships. Material culture including technology is not over against human social life but deeply entangled in and with it. Folded withIn these entanglements are time, space and human socio-cultural history. They are rolled up in them in a similar fashion to the higher order dimensions of String Theory. It is because of these foldings, these hidden but surveyable dimensions, that the duality of subjectivity and objectivity fails as the basis of ontology and so as the buttress for the doctrine of the moral neutrality of technology.

[66] In many ways, this can be seen as providing a moral twist to the standard actor-network theory argument that technology has material agency. see Pickering (1995)

Given the style and level of analysis Latour provides, it is hard to know how exactly to take this set of arguments. On the one hand, he appears to want to confront the whole of mainstream Philosophy by denying its basic categories and reducing all discussion of its paired concepts to uniform dichotomies. He is doing this, not on the basis of detailed and rigorous conceptual analysis, but rather through a dazzling fire hose of empirical studies of science and technology, metaphors, and images. This is more Finnegan's Wake than *Principia Mathematica*. However, since the sociologising that Latour invokes to support the dissolution of the distinctions is premised upon the methodological assumption that, for the purposes of giving a *sociological* description, we can suspend the distinction between representation and reality, fact and value (social constructivism *Is a methodological assumption*), philosophically we are getting no more out of the analysis than Latour has already put in.

Second, what does this flattening of ontology actually mean? If cars, hammers, power stations, software are to be viewed as actants standing alongside humans in webs of relationships, human and non-human, what does this imply for moral (and legal) *theory*? Is he really proposing that we should scrutinise the conduct of these technologies the way we scrutinise the conduct of people and hold them accountable in the same ways? Can our concepts of responsibility, blame and approbation be extended to objects and technologies?

Third, if the bridge from studies of technology to metaphysics is secured by the former's refusal to truck with modernist dichotomies, how are we to take the results of these studies and their success in Sociology, studies of science, and now studies of ethics and technology? Are they of the same order (subject to the same strictures) as the studies they treat as their topics? Are they, too, no more than exercises in invention which are to be judged through the influence of relationships, interests, and ultimately power and (moral) domination?[67]

Of course, as we saw in regard to Winner's claim about the politics of artefacts, what is under discussion might not be a proposition at all. It could be a (post-modern) joke, a rhetorical ploy pushing to the extreme a sociological account of technologies and tools. As such, it would be a trope, no more, intended to be left behind, rather as Hume advised all philosophising should, when moving from the study to the world of practical affairs.

Introna tries to resolve these difficulties by aligning Latour's claims with the view that Heidegger (1962) took on the constitution of a first philosophy. The primordial ground of such a philosophy must be our immersion in and experience of the world not our abstraction from and reflection on it. For Heidegger, the latter is not just Descartes' error, but the wrong turn taken by all western philosophy since the pre-Socratics. Following Latour, Introna suggests, the unified ontology is available through our experience of objects and technologies in the world around us. The foldings inherent within the latter are available to us as the "affordances" of this or that tool, this or that device. Such affordances are not added to the technology but "there" to be "grasped" by us in our ways of using them. Affordances constitute the possibilities and potentials of technology which are realised in use.[68] So for us, a mobile phone has a variety of uses. We can communicate with friends

[67] This, it will be remembered, was Woolgar's conclusion. See Essay 2.

[68] We will just note, in passing, that Gibson's (1977) notion of "affordance" is based in and motivated by a very un-Heideggerian psychology. Unless Latour and Introna are using the term metaphorically, the introduction of "affordances" can only make the theorising of this ontology less consistent not more.

and relatives, store images on it, use it to manage our calendars, and so on. For a society with no concept of wireless communications and "mobile apps", the phone can be no more than a paper weight. The culture of use is folded within the phone and enables our use of it.[69]

Introna takes one further step, though, and suggests that it is this constitutive character of technologies which defines their political nature. This enables him to expand the space of politicised decisions integral to technologies. As well as design and operative decisions, decisions over markets and users, decisions over implementation, roll-out and deployment, decisions about sales strategies and market entry, customer engagement, product quality, product maintenance and support, and of course product end of life, all have to be closed out and made under real conditions of time, budget and practicality. Each decision achieves closure on an issue: the inclusion and exclusion of particular outcomes and possibilities. For Introna, the cumulative effects within any socio-technical environment of such decisions and their consequences must be seen to be political in that they determine the meanings that the technology (the affordances, opportunities, possibilities) convey. In an echo of Cultural Theory's characterisation (see Essay 6), this determination, he says, is "hegemonic ".

> *It is this ongoing, and often implicit, operation of hegemonisation - of inclusion and exclusion - inherent in all political sites which is the concern of a Disclosive Ethics. (2007 p 15)*

Hegemonisation is outcome/consequence of all the design, implementation and roll-out decisions associated with ATMs, web search engines, face recognition systems, and so on. The ethical and political challenge is to reveal this hegemony and open up the possibility of its reversal. This is the role conceived for Disclosive Ethics.

There are several things to say about all this. First, despite expansion of the conceptual apparatus to include elements of post-Gramscian political theory, the structure of the analysis remains steadfastly functional in form. An unauthored strategy, a hidden hand guided by the interests of the dominant, ensures that the nexus of human/technological relationships reproduces hegemonisation. But this is not a *finding* of this way of looking at technology. It is a motivating assumption. Hegemonisation is seen to be a functionally adaptive process whereby outcomes are rationalised in terms of dominant interests.

Second, and this is critical, to make the whole approach tractable, Introna, like Latour, turns the metaphysics, i.e the dissolution of the dichotomy between human agents and material culture, into a methodological assumption. For the purposes of carrying out his (sociological) studies, he proposes to treat human agents and material culture as similar orders of (moral) being. This licenses his description of them and their consequences. But it does no more than that. This methodological move is to be justified by the evidence it makes available to us and the insightfulness, rigour, interest, fertility, or novelty of the *sociological* findings it enables, not by the popularity or radicalness of ethical (or metaphysical) stance we might choose to draw from it. To justify that ethical stance, we need to show first when, where and how hegemonisation is taking place (the close coupling of practice and function), and second why it is wrong (the ethical evaluation). For that, we would want a very different order of justification and a very different

[69] The sheer banality of this observation seems lost on Introna. Everyone knows this including those nations which have moved directly to using mobile phones because they lack the infrastructure of landlines.

kind of argument; one which looked at detail of instances and cases. Without such an argument, the turn to Disclosive Ethics remains unjustified. There would be no more reason to hold with Introna that information technologies pose a critically important threat to the openness of our society than there would be to agree with Dr Pangloss that all is for the best in this best of all possible worlds.

This takes us to a third and very familiar point. In none of the cases, examples, specimens that Introna discusses, is there any *evidence* for his argument. As a result the steps in the analyses become very loosely connected, something, again, that Introna shares with Winner. Furthermore, there is a deep paradox to be found here. The philosophic premise which is supposed to underpin modernity, the duality of fact and value, of how things are and how we see them, is precisely the premise which Latour wants overthrown. We cannot hold the distinction between representation and reality, fact and value. Using his conceptual apparatus of foldings and hegemonisation, Introna builds a picture of how information technology systems can be seen. This picture prioritises the politico-ethical consequences they can be described as having. But, if Disclosive Ethics is to work, it has to be possible to reach conclusive judgments and finalise re-designed technologies, the picture of how things are must be *independent of how we are choosing to see them*. The task of assembling the detail of any one design process is immense and no indication is given either of how we could collect that information nor what redesigning the design process itself would entail. This leaves Disclosive Ethics as a vague portmanteau of theories and nominated but unexamined empirical domains. They are selling an idea, not establishing an effective approach.

Getting down to the practicalities

We finish with some practical concerns. The first relates to how real life commercial software (and other) projects are lived.[70] Overriding everything else is the fact that there is never enough resource to implement the signed off specification document against the deadlines set. Some things have to go simply to stay on track. In addition, the schedules of technology development are unremittingly wicked. From the start, no-one expects to adhere to all of them. Delays are endemic and overruns normal. Yet some deadlines cannot be moved and so the project must be squeezed down to fit the available resource (time, money, manpower) to get it 'out the door' when it was committed for. And then there is the value engineering. Project costs never undershoot estimates and so the cost component of the eventual price has to be engineered down.

Into this fraught environment, Brey is proposing to introduce sets of professionals who, as we have continually suggested, are trained not to pursue convergent thinking; whose disciplines are in an open-loop, open-argument state and the members of which suffer from what Kenneth Burke called a peculiar occupational psychosis, supposing that all life is, or should be, lived as if it were a university seminar. For project delivery, decisions need to be made and secured; designs have to be agreed and frozen. Specification and decision drift are *the* banes of the project manager. As a consequence, the average software project is the last place to conduct open-ended non-converging philosophical and sociological discussions about the significance, likely consequences, and ethics of design; and software project teams are the least likely people to stand for them.

[70] These observations draw upon our own and others' fieldwork in commercial software and development environments (e,g. Sharrock & Anderson 1996, Button & Sharrock 1994, Rooksby et al 2009)

The suggestion they should join such teams assumes sociologists, by dint of their sociologising, can foresee the likely consequences of some innovation. In fact, of course, they cannot; or at least no better than anyone else. What Sociology provides them with is a template, a structure, for relating consequences to institutional practices once those consequences are to hand. Engineers and designers might be as well turning to crystal balls or Tarot cards get the answers they need as they would to hiring sociologists.

There is more! We were part of the effort which opened up the promise of ethnographic fieldwork for software development. We still believe that it has a lot to offer. However, in the intervening years, we have watched as what can only be regarded as old fashioned customer relationship management has masqueraded as ethnography. As the consultants have moved in, the canons of fieldwork rigour have been eroded. Now, it seems, any kind of conversation with any kind of user/manager/customer can be called ethnography. The net result is a Gresham's law for research where good ethnographic work is being driven from the technology development environment. We have little doubt that if Brey were to be successful and persuade software managers to employ sociologists and philosophers to undertake the assessment of Disclosive Ethics on project teams, the consultant companies will be very fast second movers. Inevitably, as untrained and unskilled people start to ply their trade, the same dispiriting descent into banality will occur.

Conclusion

Philip Brey is motivated by good intentions. He wants to make sure that sufficient attention is paid during the design process to ensure that the chances of ethical breaches in the use of information technology are minimised. This is very laudable. However, the approach he takes is unconvincing in three major ways. It utilises a form of sociological analysis which was not designed to provide the sort of accounts he needs for ethical analysis. Second, he offers no clear way of reasoning from the accounts it does give to judgements concerning ethical outcomes. Third, if, as Brey intends it should, his approach were to be introduced into the development environment, it is likely to be either disregarded or damaging. Disclosive Ethics offers no panacea for the ethical challenges of information and other new technologies. Mandating it as part of the design process will only cause more problems than it can hope to solve. Because of the generalised and abstracted nature of its reasoning, it ends up advocating that general measures of (quite drastic) reconfiguration of the social order are required to counter what are, at best, only *occasional* features of the socio-technical order

That information technologies can be put to worrying uses is not in dispute. There are enough instances in the research literature and elsewhere to put that claim beyond debate. Addressing this issue is a matter of careful design and equally careful regulation of the uses and contexts of such technologies. However, to say this is not to say, thereby, that information technologies are *necessarily* ethical or *inherently* political. This claim is universal in its quantification. All technologies are political and ethical in their outcomes. The problem is that the claim can only be secured within a functional analysis, one where the purpose is to demonstrate what one means by inevitability or necessity of outcome in this regard. To put it starkly: functional analysis looks backwards to the analytic presuppositions not forward to the facts. And yet, if one wants to intervene in the world, to impose extra strictures on systems designers and developers, to demand existing systems be re-configured and re-built, then one has to offer an account that is rooted in how things actually are. It is because they really, really are political and ethical, that these systems must be changed. Functional analysis does not tell you how things really, really are, except under the auspices of functional assumptions (to use the phrase). Faced with this impasse, Introna uses Latour's convoluted ontologising to try to unify the material and cultural worlds in a single

moral frame; such a frame is how the world is. But the unified ontology is itself built on the overthrow of such distinctions. It proposes that these distinctions are nothing but ways in which we construct and enforce an account of reality. At that point, the whole project falls apart and Introna is left with nothing but exhortation and expostulation.

POSTSCRIPT

A POSTSCRIPT ON POST-DISCIPLINARITY

Given postmodernism's determination to break out from the confines of rationalism, in the social sciences it is not surprising that the rejection of the notion of intellectual or academic disciplines and the wish to move to post-disciplinary approaches to topics and problems has been a somewhat tardy afterthought. John Urry (2000 & 2007) has argued the need for a post-disciplinary approach to new forms of sociality emerging in contemporary life, whilst Bob Jessop and Ngai-Ling Sum (2001) suggest changes in the nature of global political order will require new post-disciplinary analysis.[71] Others have observed that as a result of new areas of study such as tourism, complete disciplines themselves need to be re-constituted (Coles, Hall and Duval 2006). The arguments offered for this re-ordering are broadly two-fold. First, there are claims that the context in which the current organisation and professionalisation of knowledge was developed no longer holds and as a consequence academic disciplines have become blinkered and narrow as well as inclined to invasion and colonisation. Possibly the clearest statement of this position is Andrew Sayer's manifesto *Long Live Postdisciplinary Studies* (Sayer 2003). Second, there are claims made on the basis of ANT's deconstruction of the metaphysics of science and the social sciences to the effect that the ontologies enshrined in academic disciplines are no longer defensible. This line of thinking has been forcefully promoted by John Law (2004). Although both sets of arguments have common features, they are actually quite different. As a consequence, using Sayer and Law as our guides, we will briefly review each in turn.

[71] Actually, in the Jessop and Sum case it is not clear whether it is a new or an old approach which is wanted. The authors advocate both!

Imperialist Parochialism

Andrew Sayer sees disciplines as enforcing a narrowing and over simplifying of topics, issues and problems which, as a result, they are then unable to see beyond. The 'economic', the 'sociological', the 'psychological' perspective (itself a much abused term) is promulgated as the only way to understand whatever the matter at hand. Pointless arguments, then ensue as to which view should be held. Additionally, Sayer sees disciplines as endemically imperialist, seeking to move into and occupy whole domains which other disciplines view as their own. And to be fair, as will have been clear from many of the essays in this volume, we have some sympathy with Sayer's allegation about disciplinary imperialism, at least in so far as it applies to some parts of Sociology. This, we think, is the first thing to say about the whole argument. *Some* parts of *some* disciplines are *sometimes* imperialistic. However, universalising the claim actually robs it of its cogency. A discipline such as Sociology that has open borders and actively seeks to expand them should not be taken as the model for all disciplines. The peculiarities of Sociology and other social sciences (and especially the family squabbles over who has rights over what domain) should not be imposed on disciplines as diverse as English Literature, Theology, Archaeology, Botany, Synthetic Biology, Astrophysics, or even Physics.

The second thing to say is that the current disciplinary division of a labour is, of course, a product of particular historical circumstances. The German model developed by von Humbolt and taken up in the latter part of the 19th century in the USA which defined the mission of the university as combination of teaching and research and which sub-divided the domain of inquiry into the famous (infamous) binary of *Naturwissenschaft* and *Geisteswissenschaft* together with the myriad of professionalised sub-domains subsequently spawned, is obviously not the only way that academic (and intellectual) life could be organised. But although it is an historically contingent model for organising universities, their teaching and research, it is nonetheless the model we have and it does have some advantages. The point is not to become fetishistic about any of the structures of the standard model, but to think with them and beyond them. The main advantages that the current, or indeed any model, brings are first that it gathers together ways of defining and analysing problems and topics which have a family resemblance to each other. They share some things but are not identical. These common features make it easier to engage with and learn from the analytic work carried on in sibling and neighbouring domains. Second, and strongly related, academic life is, at root, collegial. Although there are lonely scholars cloistered in their studies, for the most part academic life is carried out as a social activity. Disciplines make it easy to find and relate to like minded others, which of course is not to say that all members of a discipline are equally like minded! Disciplines are useful congeries of like-minded academics.

The third thing to say about Sayer's critique is that the conventional frames of reference (we will avoid the much overused term 'paradigm') which typify academic disciplines are precisely that, conventional. Consequently, they serve some topics and issues and ways of developing research well, and others not so well. It may be a matter of institutionalised academic politics and an over conformist culture that certain ways of carrying on research within a discipline predominate but such social or sociological facts do not have epistemological consequences, merely important practical ones. The extent to which views that are not in current favour are 'read out' of the discipline is, naturally, a matter of intellectual concern. But nothing epistemological hangs on it.

So, we would propose a little more circumspection in respect of Sayer's claim of disciplinary imperialism and dominance,. Pariochialism and imperialism are, of course, evident, and are simply a reflection of the need for focus and the institutionalised politics of academic life. But having said that, the structure of disciplines we know and get along with (if not love) serves us as well as any

other. As far as the claim that because the world is much more complex than any discipline can conceive therefore any structuring of knowledge is inimical to full understanding is concerned, we would want to ask exactly what is being suggested here? That line of reasoning seems to be premised in a conception of how the world must be conceived so that we can fully understand it. This is precisely where John Law begins, so we will now turn to the case that he makes.

A Mess of Method

In *After Method* (Law 2004), John Law deploys ANT's reflexive approach on the social sciences and on their research methods in particular. Not surprisingly, he finds these methods to be riven with philosophical realism and its associated metaphysics. On the basis of insights he draws from the development of the notions of perfomativity and enactment, he insists that method in the social sciences must be re-grounded. Unfortunately, he is unable to say precisely how this might be done. Here part of his concluding paragraph.

> *What does this mean in practice? The answer is that I do not know. But one thing is indeed clear. In the longer run it is no longer obvious that the disciplines and research fields of science and social science are appropriate in their present form. It is no longer obvious that a division of labour is desirable, a division of labour that rests on the parcelling out of truth to different specialists who are then divested of the need to practice other goods. (Law 2004 p. 156)*

Instead of providing the necessary re-grounding, Law offers an array of images and metaphors as stimuli in the search for connections across the multiple realities which science and social science will inevitably now have to deal. Such realities are the product of the processes of *difference* and *multiplicity* revealed as a result of construing method as performative. Differing methods and their practices lead to different realities. Since there a multiplicity of methods then there is a multiplicity of realities. In the glossary he provides for what might well be unfamiliar terms for the casual reader, Law defines these crucial ideas as follows:

> ***Difference,*** *problem of: the simultaneous existence of different objects that are said to be the same. This arises, as Annamarie Mol shows, if objects are enacted in practices, and those practices are different, then so too are the objects they produce, even if the practices in question are said to relate to, or be aspects of the same object. Problems of co-ordination then arise in the relations between practices/objects.....*
>
> ***Multiplicity****: like difference, the simultaneous enactment of objects in different practices, where those objects are said to be the same. Hence the claim that there are many realities rather than one. This arises because practices are endlessly variable and differ from one another....(Law op. cit. pp159 and 162)*

These definitions reveal the heart of Law's confusion and what has led to his bewilderment as to what to do about disciplines. Notice the important phrase '...are said to be the same'. That is, two things are called by the same name or said to be the same in some particular context. So your motor mower has a choke to enable smooth starting and running. Your dog has a choke collar because he is inclined to pull when out for a walk. Both are 'chokes' and function to cut off air supply when required. But that they have the same name does not mean they are the same thing, or are different things in 'different realities'. In using the term choke to describe both objects, we

have a standard for comparison based upon what we are using the description for. Of course, Law and Annamarie Mol are not thinking about carburettors and dogs. They are more interested in cases such as the ordinary pub table as described by the asset register in the accounts of the brewery that owns the pub, the table as an array of force vectors in Physics, the table as a composite of different materials with different chemical properties, or the table as you or we might describe it. In the asset register of the brewery, the table is an 'accountant's object' (Anderson, Hughes and Sharrock 1989). It has its existence, properties and role within the scheme as defined by the chart of accounts. In the pub, the table is an ordinary object on which we might put glasses, laptops, keys and, if badly behaved, our feet. You can't put your feet on the table in the asset register, or on an array of vectors. Thus, on the Law/Mol line of reasoning, if they are all the same table, they have to be the same thing in different realities. The table is the same and different and exists in all of them.

The accountant's world, the physicist's, the chemist's and the ordinary world are different and we can, for the purposes for sociological analysis call them different realities if we like. But this is not to propose there are multiple tables in these multiple worlds. There is just one table and that is being described in differing ways for different purposes. Gilbert Ryle makes the point with his usual crystalline clarity.

> *(The) branches of inquiry are not giving rival answers to the same questions about the same world; nor are they giving separate answers to the same questions about rival worlds; they are giving their own answers to different questions about the same world. Just as physics is neither the foe nor the handmaiden of geometry, so history, jurisprudence and literary studies are neither hostile nor ancillary to laboratory sciences. Their categories, that is, their questions, methods and canons are different. (Ryle 1971 p 195)*

The Law/Mol error is to confuse meaning which is tied to convention and practice with ontology. Rather than feel the need to push a sociological insight about the conventional nature of meaning as a substitute for a philosophical basis to ontology, we can use the conventional nature of meaning in the social world to ask how we construct and organise these 'multiple realities'. The point that Ryle makes aligns closely with what Alfred Schutz (1962) called "structures of relevance". Differing disciplines have different structures of relevance. Structures of relevance circumscribe alternative finite provinces of meaning and their presuppositions. Law's bewilderment over disciplines arises in large measure because, despite his opposition to realism especially in its positivist incarnation, he has not divested himself of the fundamental assumption of positivistic realism; namely that it must in principle be possible to correlate differing descriptions of 'the same object'. For positivism, this correlation was through reductive re-description ultimately into the terms of Physics. For Law it is elaborative concatenation of manifold descriptions, with such descriptive forms all somehow being pinned or woven together. It is only when you think you have got to have a unified description that the social fact of multiple realities becomes an ontological problem.

There is much to improve in the contemporary organisational infrastructure of contemporary intellectual life. Much could be done to ease the inertia of stale ideas and the friction of traditional practices. But none of these things require us to abandon the notion of academic disciplines *tout court* nor to jettison the forms of method that have served us reasonably well till now. The only reason to do either (or both) would be if we felt had found a different way of demarcating forms of knowledge that would serve us better. Gilbert Ryle and Alfred Schutz tell us that thinking we are

driven to do this for ontological reasons is misconceived. Andrew Sayer and John Law simply show how right they were.

BIBLIOGRAPHY

Alexander, B.	2011	Standing in the wake: a critical auto/ethnographic exercise on reflexivity in three movements. *Cultural Studies <= > Critical Methodologies* vol 11 (2), p 98 - 107.
Amsterdamska, O.	1990	Surely you are joking Monsieur Latour!'. *Technology and Human Values* vol 15 (4) pp 495-504
Anderson, R, J. Hughes and W. Sharrock	1989	*Working for Profit*. Aldershot, Avebury.
Austin, J	1962	*Sense and Sensibilia*. Oxford. Oxford University Press
Austin, J.	1962	*How To Do Things With Words*. Oxford. Oxford University Press
Barker, J. & Downing, H.	1985	Word processing and the transformation of patriarchal relations of control in the office' in D. MacKenzie & J. Wajcman, *The Social Shaping of Technology* Open University Press, Milton Keynes, pp147-164
Barnes, B & Edge, D.	1982	*Science in Context*. Cambridge Mass. MIT Press
Barthes, R.	1968	*Elements of Semiology*. Hill and Wang, New York
Barthes, R.	1972	*Mythologies*. Hill and Wang, New York
Baudrillard, J	1993	*The Transparency of Evil*. London. Verso
Baudrillard, J.	1994	*Simulacra and Simulation*. (Translated by S. Glaser)

University of Michigan Press. Ann Arbor.

Baudrillard, J. 1995 *The Gulf War Did Not Take Place*. (Translated by P. Patton) Bloomington. Indiana University Press.

Becker, H. 1970 Whose side are we on?' in *Sociological Work*. Aldine. Chicago

Beer, M. 2009 Power through the algorithm'. *New Media & Society* vol 11 (6) pp 985 - 1002

Bittner, E. 1973 Objectivity and Realism in Sociology' in G. Psathas (ed) *Phenomenological Sociology*. Wiley. New York. Pp 109 - 125

Borgman, A. 2008 Reality and Technology. *Cambridge Journal of Economics* pp 1-9

Brannigan, A 2004 *The Rise and Fall of Social Psychology*. Walter de Gruyter. New York

Brey, P 2000 Disclosive computer ethics. *Computers and Society*, December, pp 10 - 16

Brey, P 2010 Values in technology and disclosive computer ethics, in L. Floridi (ed) *The Handbook of Information and Computer Ethics*. Cambridge. Cambridge University Press

Brown, R. 2009 *Are Science and Mathematics Socially Constructed?* New Jersey. World Scientific Press

Button, G. And Sharrock, W 1994 Occasioned practices in the work of software engineers in M. Jirotka and J. Goguen, *Requirements Engineering*. London, Academic Press 1994

Callon, M 2008 Economic markets and the rise of interactive agencements: from prosthetic agencies to habilitated agencies in T. Pinch & R. Swedberg (eds) *Living in a Material World*. Cambridge Mass. MIT Press pp 29 - 56

Callon, M. 2007 What does it mean to say that Economics is performative?' in D. Mackenzie, F. Muniesa, & L. Siu (eds) *Do Economists Make Markets?* Princeton. Princeton University Press, pp 311 - 357

Cartwright, N. 1999 *The Dappled World*. Cambridge University Press,

London.

Cartwright, N. 2007 *Hunting Causes and Using Them*. Cambridge University Press, Cambridge

Castells, M 2001 *The Internet Galaxy*. Oxford. OUP

Castells, M 2010 *The Information Age*. Malden, Mass. Wiley & Sons.

Cohen, G.A. 1978 *Karl Mar's Theory of History: A defence*. Oxford. Clarendon Press

Coles, T. C. Michael Hall and David Duval 2006 Tourism and post-disciplinary enquiry'. *Current Issues in Tourism* Vol. 9, No. 4&5, pp 293 - 319

Cummings, M 2006 Integrating Ethics in Design through the Value-Sensitive Approach. *Science and Engineering Ethics*, vol12, pp 701-715

Davis, J. 1994 What's wrong with Sociology? *Sociological Forum*, vol9 no 2, pp 179-197

Derrida, J. 1978 *Writing and Difference*. London. Routledge & Kegan Paul

Derrida, J. 1989 *Of Spirit. Heidegger and The Question*. Chicago. University of Chicago Press.

DiSalvo, C. 2009 Design and the Construction of Publics. *Design Issues* Vol. 25, No. 1, Pages 48-63

DiSalvo, C., Boehner, K., Knouf, N., & Sengers, P. 2009 Nourishing the ground for sustainable HCI: considerations from ecologically engaged art'. *Proceedings of CHI 2009*, April 4–9, 2009, Boston, Massachusetts, USA, pp 385 - 394

Durkheim, E. 1964 *The Rules of Sociological Method*, Free Press, New York

Ellul, J 1964 *The Technological Society*, New York, Knopf.

Faulhaber,G. & Baumol, W. 1988 Economists as Innovators'. *Journal of Economic Literature* vol 26 no2 pp 577-600

Feenberg, A 2002 *Transforming Technology*. Oxford. OUP

Fine, A.	1990	Causes of Variability: Disentangling Nature and Nurture. *Midwest Studies in Philosophy* vol XV pp 94-113
Finlay, L.	2002 (a)	"Outing" the researcher: the provenance, process, and practice of reflexivity. *Qualitative Health Research* vol12 no 12 pp 531-545
Finlay, L.	2002 (b)	Negotiating the Swamp: the opportunity and challenge of reflexivity in research practice. *Qualitative Research* vol2 no 2 pp 209 - 230.
Freeman, D.	1983	*Margaret Mead and Samoa*. Harmondsworth. Penguin Books
Geertz, C	1988	*Works and Lives*. Stanford University Press, Stanford.
Geertz, C	1993	*The Interpretation of Cultures*
Geertz, C	2000	*Available Light*. Princeton University Press. Princeton
Gibson, J.	1979	*The Ecological Approach to Visual Perception*. Boston. Houghton Mufflin.
Goffman,E.	1969	*The Presentation of Self in Everyday Life*. Harmondsworth. Penguin.
Graham, S.	2005	'Software-sorted Geographies', *Progress in Human Geography* 29 no 5 pp 1–19
Gramsci, A.	1996	*Prison Notebooks*. Columbia University Press, New York
Hacking, I.	1999	*The Social Construction of What?* Cambridge, Mass. Harvard University Press.
Hare, R.M	1986	What's wrong with slavery? P. Singer (ed) *Applied Ethics*, Oxford, Oxford University Press, pp165-84
Heidegger, M.	1962	*Being and Time*. Oxford. Basil Blackwell
Heidegger, M.	1977	*The Question concerning Technology and other Essays*. New York, Harper & Row
Heinich, N.	2010	What does 'Sociology of Culture' mean? *Cultural Sociology* vol 4 no 2 pp 257 - 65

Hofkirchner, W	2007	A critical Social Systems View of the Internet. *Philosophy of the Social Sciences, vol. 37, 4: pp. 471-50*
Homans, G. J.	1930	*English Villagers of the Thirteenth Century*. Russell & Russell. New York
Hutchins	1995	*Cognition in the Wild*. Cambridge Mass. MIT Press.
Introna, L & Nissenbaum, H.	2000	Shaping the web: why the politics of search engines matters. *The Information Society*, vol 16, pp 169-185
Introna, L & Whittaker, L.	2005	Power, cash and convenience: the political space of the ATM. *LUMS Working Papers*, Lancaster University Management School
Introna, L & Wood, D.	2004	Picturing Algorithmic Surveillance. *Surveillance and Society* vol2 nos 2-3 pp 177-98
Introna, L.	2007	Maintaining the reversibility of foldings. *Ethics and Information Technology* vol 9 pp 11-25
Jay, M	1996	*The Dialectical Imagination*. University of California Press, Berkeley.
Jessop, B & Ngai-Ling Sum	2001	Pre-disciplinary and post-disciplinary perspectives. *New Political Economy*, vol 6 no 1, 89-101
Joerges, B	1999	Do Politics have Artefacts? *Social Studies in Science* vol 29 no 3 pp 411 -31
Kaplan, A.	1998	*The Conduct of Inquiry*. Transaction Publishers, New Brunswick
Kaufman, F.	1944	*The Methodology of the Social Sciences*. Oxford University Press, Oxford.
Kuorikoski, J., Lehtinen, A., and Marchionni, C.	2010	Economic modelling as robustness analysis. *British Journal for the Philosophy of Science*, vol 61 pp 541-67
Labinger, J. And Collins, H.	2001	*The One Culture: a conversation about science*. Chicago. Chicago University press
Lash, S.	2001	Technological forms of life'. *Theory, Culture and Society*, vol 18 no 1 pp 105 - 120.

Lash, S.	2006	'Dialectic of Information? A Response to Taylor', *Information, Communication & Society*, vol 9 no 5 pp 572–81.
Lash, S.	2007	Power after hegemony'. *Theory, Culture and Society* vol 24 no 3 pp 55 - 78
Latour, B	1992	Where are the missing masses? W. Bijker & J. law (eds) *Shaping Technology/Building Society: Studies in Sociotechnical Change*. Cambridge, Mass. MIT Press
Latour, B	2002	Morality and Technology. *Theory, Culture & Society* Vol. 19 nos 5-6 pp 247–260
Latour, B	2005	*Reassembling the Social*, OUP, Oxford
Latour, B	2010	Coming out as a philosopher'. S*ocial Studies of Science* vol 40 no 4 pp 599-608
Law, J.	2009	*'Collateral Realities'*, version of 29th December 2009, available at http://www.heterogeneities.net/publications/Law2009CollateralRealities.pdf (downloaded 14/02/2012)
Law, J.	2004	*After Method*. London. Routledge.
Levi Strauss, C.	1969	*The Raw and The Cooked*. New York. Harper Row.
Levi Strauss, C.	1988	*The Jealous Potter*. Chicago. University of Chicago Press
Lindblom, C. & Cohen, D.	1979	*Usable Knowledge*. Yale. Yale University Press
Livingston, E.	1986	*The Ethnomethodological Foundations of Mathematics*. Routledge, London
Lucy, J.	1997	Linguistic Relativity. *Annual Review of Anthropology*, vol 26, pp291-312
Lukes, S.	1974	*Power*. London Macmillan
Lynch, M.	1997	*Scientific Practice and Ordinary Action*. Cambridge University Press, Cambridge.
Lynch, M.	2000	Against reflexivity as an academic virtue and source of privileged knowledge'. *Theory, Culture & Society*, vol

17 no 3 pp. 26 - 54

Lyotard, J-F. 1984 *The Postmodern Condition*. Manchester. Manchester University Press.

Lyotard, J-F. 1991 *The Inhuman*. Oxford. Polity Press

Macbeth, D. 2001 On "Reflexivity in Qualitative Research'. *Qualitative Inquiry*, vol7 no 1 pp 35 - 68

Macfarlane, A 1978 *The Origin of English Individualism*. Oxford. Basil Blackwell

MacIntyre, A 1990 *Three Rival Versions of Moral Enquiry*. Notre dame, Indiana. Notre Dame University Press.

Mackenzie, D & Wacjman, J. 1985 *The Social Shaping of Technology*. Milton Keynes. Open University Press.

MacPherson, C.A 1962 *The Political Theory of Possessive Individualism*. Oxford. OUP

Manders-Huit, Noemi 2011 What Values in Design? *Science and Engineering Ethics*, vol 17 no 2, pp 271-287

Marcuse, H 1964 *One Dimensional Man*. Boston. Beacon Press

Marshall. G. 1983 The Containers and the Contained. *Times Literary Supplement*. January.

Marx, K, & Engels, F 1938 *The German Ideology*. London. Lawrence & Wishart

McCarthy, T. 1990 The critique of impure reason'. *Political theory*, vol 18 no 3 pp 437 - 69

McChesney, R 1997 *Corporate Media and the Threat to Democracy*. New York, Seven Stories Press

McGowan, J. 1991 *Post Modernism and Its Critics*. Ithaca. Cornell University Press.

Mcluhan, M 1964 *Understanding Media*. New York. McGraw Hill

Merton, R 1949 A Paradigm for Functional Analysis in *Social Theory and Social Structure*, Glencoe Ill., Free Press, pp 49 - 61

Merton, R.	1948	The Self-Fulfilling Prophecy.' *The Antioch Review*, Vol. 8, no. 2, pp 193-210
Merton, R.	1976	Sociological Ambivalence' in *Sociological Ambivalence and other Essays*, New York. The Free Press, 3-31
Mirowski, P. & Nik-Khah, E.	2007	Markets Made Flesh: performativity and a problem in Science Studies, augmented with consideration of the FCC Auction in D. Mackenzie, F. Muniesa, & L. Siu (eds) *Do Economists Make Markets?* Princeton. Princeton University Press., pp 190 - 224
Mirowski, P. & Nik-Khah, E.	2008	Command Performance: exploring what STS thinks it takes to build a market' in T. Pinch & R. Swedberg (eds) *Living in a Material World*. Cambridge Mass. MIT Press 89 - 130
Mumford, E.	1995	*Effective Systems Design and Requirements Analysis: the ETHICS Approach*. London. Macmillan
Nagel, T.	1991	*Equality and Partiality*. Oxford. Oxford University Press.
Neuberg, L. G.	1989	*Conceptual anomalies in economics and statistics* Cambridge University Press Cambridge
Newell, A & card, S.	1985	The prospects for psychological science in human-computer interaction. *Human-Computer Interaction*, Vol1 no 3 pp209 - 242
Pattison, G.	2000	*The Later Heidegger*. London. Routledge.
Pickering A	1995	*The Mangle of Practice*. Chicago. Chicago University Press.
Pinker, S.	2004	Why nature & nurture won't go away. *Daedalus*. Fall, pp 5-17
Putnam H	2004	*Ethics Without Ontology*. Cambridge Mass. Harvard University Press
Rooksby, J, Rouncefield, M and Sommerville, I.	2009	Testing in the wild. *Computer Supported Co-operative Work*, vol18 nos 5-6 pp 559-580
Rorty, R.	1980	*Philosophy and the Mirror of Nature*. Oxford. Oxford University Press.

Ryle, G.	1954	The world of science and the everyday world.' in *Dilemmas*. Cambridge. Cambridge University Press pp 65 - 81
Ryle, G.	1971	Philosophical arguments' in *Collected Papers vol II*, London, Hutchison, pp194-211
Satchell, C.	2006	Cultural Theory - from armchair critic to star performer', *Proceedings of OZCHI 2006*, November 20-24, 2006, Sydney, Australia
Satchell, C.	2008	Cultural theory and real world design - dystopian and utopian outcomes'. *Proceedings of CHI 2008*, April 5–10, 2008, Florence, Italy pp 1593-1602
Savage, M. and Burrows, R.	2007	The coming crisis of empirical Sociology. *Sociology*, vol 41 no 5 pp 885 - 889
Sayer, A	2003	*Long Live Postdisciplinary Studies! Sociology and the curse of disciplinary parochialism/imperialism*', published by the Department of Sociology, Lancaster University, Lancaster LA1 4YN, UK, Sociology, Lancaster University, Lancaster LA1 4YN, UK, at http:// www.comp.lancs.ac.uk/sociology/papers/Sayer-Long-Live-Postdisciplinary-Studies.pdf. Accessed 12/1/2012
Schutz, A.	1962	On multiple realities' in *The Problem, of Social Reality*. The Hague. Martinus Nijhoff. pp 207 - 259.
Schutz, A. & Luckmann, T	1974	*The Structures of the Life-World*. Heinneman. London
Sengers, P., Boehner, K., David, S., & Kaye, J.	2005	Reflective design'. *CC '05 Proceedings of the 4th decennial conference on Critical computin*g: between sense and sensibility, AARHUS'05 8/21-8/25/05 Århus, Denmark pp 49 - 58
Sengers, P., McCarthy, J., & Dourish, P.	2006	Reflective HCI: articulating and agenda for critical practice. *Proceedings of CHI 2006*, April 22-27, 2006 • Montréal, Québec, Canada, pp 1683 - 1688
Sharrock, W & Anderson, B.	1996	Organisational innovation and the articulation of the design space. T. Moran & J. Carroll eds, *Design Rationale*. New jersey. Lawrence Erlbaum. Pp 429 - 452.

Silverstone, R.	2007	*Media and Morality*. Polity Press. Cambridge
Sokal, A.	1996	Transgressing boundaries. *Social Text*, vols 46 & 47, pp 217-252
Strauth, S., & Turner, B.	1988	Nostalgia, postmodernism and the critique of mass culture'. *Theory and Society* Vol 5 pp 509 - 526
Strong, P.	1979	Sociological imperialism and the profession of medicine A critical examination of the thesis of medical imperialism'. *Social Science & Medicine*. Part A: Medical Psychology & Medical Sociology Volume 13, 199–215
Suchman, L.	1987	*Plans and Situated Actions : The Pproblem of human-machine communication*. Cambridge University Press, New York.
Taylor, A. & Vincent, J	2005	An SMS History'. *Computer Supported Cooperative Work*, 2005, vol 1, 75-91.
Taylor, C	2004	*Modern Social Imaginaries*. Duke University Press.
Taylor, C.	1995	Liberal Politics and the Public Sphere, in *Philosophical Arguments*, Cambridge, Mass, Harvard University press, pp257-288
Thrift, N.	2005	*Knowing Capitalism*. London. Sage
Turkle, S.	1995	*Life on the Screen*. New York. Touchstone Press
Turkle, S.	2009	*Simulation and its Discontents*. Boston, MIT Press.
Tylor, S.	1986	Post-modern ethnography' in J. Clifford & G. Markus (eds) *Writing Culture*. University of California Press. Berkeley pp 122-140.
Urry, J	2007	*Mobilities*. Cambridge. Polity Press.
Urry, J.	2000	Mobile sociology'. *British Journal of Sociology* Vol.51 I no. 1 pp 185–203
Van Den Hoven, J & Weckert J.	2008	*Information Technology and Moral Philosophy*. Cambridge. CUP
Weber, M	1930	*The Protestant Ethic and the Spirit of Capitalism*.

London. Allen Unwin

Weber, M 1948 Science as a Vocation in *From Max Weber* (H. Gerth & C. W. Mills eds), London. Routledge & Kegan Paul, pp129-158

Weiser, M. 1991 The computer for the 21st century'. *Scientific American,* September, pp 94 - 104

Wilkinson, D 1968 Sociological imperialism: a brief comment on the field'. *The Sociological Quarterly*, vol 9 no 3 pp 397 - 400

Winch, P. 1958 *The Idea of a Social Science*. London. Routledge

Winner, L 1985 Do Artefacts have Politics?' in Mackenzie, D and Wacjman, J. (eds) *The Social Shaping of Technology*. Milton Keynes. Open University Press pp26-38.

Winner, L. 1989 The reactor and the Whale in *The Reactor and the Whale*. Chicago. Chicago University Press pp 164 - 178.

Woolgar, S. 1988 *Science, The Very Idea*. London. Routledge

Woolgar, S. & Cooper, G. 1999 Do aretfacts have ambivalence?' *Social Studies of Science* vol 29 no 3 pp 433-449

Worsley, P 1973 The distribution of power in industrial society in J. Urry & J. Wakeford (eds) *Power in Britain*. London, Heinnemann pp 247-65

Wynne, B. 1982 Natural Knowledge and social context' in B. Barnes & D. Edge (eds), *Science in Context*, Cambridge, Mass. MIT Press, pp 212-232

Zammito, J. 2004 *A Nice Derangement of Epistemes*. Chicago. Chicago University Press.