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Contemporary Sociology and the Challenge of Descriptive Assemblage

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Abstract

This article argues that the descriptive turn evident in contemporary capitalism challenges orthodox sociological emphases on the central importance of causality and the denigration of descriptive methods. The article reviews the different evocations of descriptive sociology pronounced by three very different contemporary sociologists: Andrew Abbott, John Goldthorpe, and Bruno Latour, and lays out their different approaches to the role of the 'sociological descriptive'. It is argued that their apparent differences need to be placed in a broader re-orientation of sociology away from its historical interface with the humanities and towards the natural sciences. How this reorientation involves a new role for visual methods which have traditionally been decried in orthodox sociology is examined, and the article concludes with suggestions for how sociology might best orient itself to the descriptive turn.

Key words

■ causality ■ description ■ methodology

In an age of informational, or 'knowing capitalism' (Thrift, 2005), the practice of description is fundamental to the generation of 'useful' knowledge. Market researchers profile different types of consumer, thus allowing firms to position their products and brands in a competitive marketplace. The security services profile criminal types by a process of association, linking a series of 'suspect' characteristics together so that the search for the perpetrators can be narrowed down. Government departments construct, and are themselves judged by their ability to meet, welters of 'indicators'. This potential for 'descriptive assemblage' is dramatically enhanced by the infrastructure of information technology and more particularly the digitalization of social relations. The complete accounts of purchases made through loyalty cards allow the automatic marketing systems of supermarkets to post out inducements for specific purchases. After making a web purchase, the consumer is confronted by screens informing him or her of other goods that have been made by others who bought this product. The medical sciences proliferate scanning devices which allow doctors to describe what is inside

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people's bodies, so allowing diagnosis to be anchored to inspection. In none of these cases is it deemed essential to understand why someone is doing something or why something is happening. Such causal concerns are not necessarily discarded but can be bracketed out. What is more important is to associate actions with other actions, leading to a concertina-like process of ever more elaborate description.

This is a politics of the whole (rather than a sampled) population, a concern to do better than rely on generalization through aggregation, through deploying and elaborating unprecedented information on all individuals. It involves a strategic concern with the delineation of particularity and specificity (see, generally, Ruppert, 2007). The governmental 'desire' for identity cards in the UK – despite the cost, technical difficulty, and implications for human rights – is a telling indication of the power of this administrative current. Yet Thrift (2005) is also right to insist on the capitalist dynamics which lie at the heart of this descriptive turn: it is likely that one of the products of the current sub-prime finance crisis will be to elaborate ever more refined credit scoring to differentiate 'better' from 'worse' risk clients. And this is therefore simultaneously a politics of small numbers, one concerned to delineate and make visible highly a-typical groups who are not revealed through the mobilization of large sociological aggregates: the concern to uncover 'terrorists' in the post-9/11 environment being emblematic of this broader shift. In knowledge- and information-saturated relationships, new possibilities for descriptive assemblage are routinely generated

It is clear that this descriptive turn is dramatically affecting the nature of contemporary expertise, in ways which challenge academic authority. In the biological sciences, Jane Calvert (2007) has shown how biologists have reacted to the proliferation of computer-generated genetic sequencing by insisting that only those descriptions which can be linked to accounts of gene 'function' should be treated as scientific. Yet she also notes how (analytical) bio-chemistry is losing ground to (descriptive) bio-informatics. Nikolas Rose reflects on how the biological sciences are redefining the meaning of life itself away from a causal, depth, model:

> Contemporary genomics is beginning to operate in a flattened world, a world of surfaces rather than depths. In the developing explanatory schemas of postgenomics, the genetic code is no longer thought of as a deep structure that causes or determines, but rather as only one set of relays in a complex, ramifying and non-hierarchical networks filiations, and connections. (Rose, 2007: 130)

Donald MacKenzie (2001) has shown how mathematicians have fought a long, rearguard action since the 1970s to resist the legitimacy of mechanical, computer-generated, mathematical proofs, insisting instead for the need for proofs to be demonstrated by the elaborate calculations of the sentient human scientist.

Until recently, however, sociologists have not felt similarly concerned about the descriptive turn. This is puzzling. The social sciences came to prominence during the twentieth century in an age of aggregation, when the specification of groups (classes, ethnicities, nations, etc.) was central to modes of ordering and

classification (see, generally, Bauman, 1989). Social scientists of all hues became wedded to a 'depth model', whereby they were able to order the social by separating the 'wheat' – in the form of deep, causal or structural processes, usually linked to the 'master' variables of class, gender, nation, etc. – from the chaff of superficial, contingent observations. It is precisely this kind of analytical procedure which is challenged by the new politics of descriptive assemblage.

Hitherto, the main response from sociologists to this new politics of description has been to rest, rather complacently, on methodological and theoretical oars which were first tried out a hundred or more years ago. It is rightly claimed that description, in the form of inductive empiricism, is impossible: we only organize knowledge on the basis of predefined categories. There are impressive theoretical arguments about the values of causal analysis, construed in non- or post-positivist terms, from proponents of critical realism and complexity theory. Yet these kinds of meta-defences, however reasonable they are in their own terms, float above and are not linked to actual practices of social research.[1] In fact, there continues to be a strong condescension to the descriptive. The claim that a piece of writing – whether a student essay or journal article – is 'rather descriptive' is an enduring rhetorical criticism. The belief that social science is fundamentally concerned with analysis, explanation and causality, and thereby parts company with 'description' is a core tenet, almost an article of faith, to many who would otherwise have fundamental disagreements about their epistemological and ontological framings.

This posturing is deeply problematic in the current conjuncture with its multiple mobilizations of the descriptive, and may even amount to self-marginalization. We need to recognize that academic sociologists are only one – and by no means the most privileged – group vested in social research and analysis. If flows of digital data entail the marginalization of the interview and the sample survey – the two main mechanisms on which sociological expertise came to prominence – we need to recognize that what sociologists ideally think constitutes the best research strategy may not persuade other agents. Savage and Burrows (2007) argue that the empirical expertise of social scientists, notably in the post-war years, rested on the fact that in order to diagnose the social, it was necessary to conduct special kinds of fieldwork on it. The analysis of social relations depended on particular research interventions, which social scientists were well placed to police. But, in an era when information is routinely gathered with no special efforts required in the form of fieldwork, interviews, or surveys, how do the social sciences, and more particularly, sociologists, engage with this descriptive turn?

This article is a speculative attempt to reflect on what a sociological descriptive may entail. It picks up on an inkling of change which can be detected, where some sociologists argue for a re-assessment of the descriptive, seeing them as increasingly central to effective social scientific engagement (see, for instance, McLellan, 2006). The aim of my article is to take stock of this emerging body of work and reflect on the problems facing the deployment of a descriptive sociology.

My aims are threefold. First, I argue that the appeal of the descriptive is interesting in that it bring together sociological camps which are often thought of as

being opposed, so suggesting that it serves, in Deleuzian terms, an emergent plane of immanence which challenges assumed categories and indicates the possibility of a new, more productive surface for inquiry.[2] Second, I argue that the relationship of sociology to the descriptive needs to be placed in the context of the tortuous relationship of the discipline to the sciences and the humanities, both of whom claim the descriptive as their own (though in different ways). Third, I argue that the relationship between visual analysis, narrative and number is central to the debate about the descriptive, leading to my conclusion that there needs to be more sociological interest in visualization as process, social artefact, and research tool.

In developing these arguments, I repeat that I do not focus on abstract theoretical and methodological issues about causality, induction, deduction and the like, the kind of topics which litter methods textbooks. These usually fail to engage with how methods are actually deployed and themselves multiply construct the social. It is more pressing to consider the mundane practices and interventions which are possible in the name of description. I seek a modest lever, focusing on the methodological and practical resources which can be claimed in the name of the sociological descriptive. I take the work of three influential sociologists, from very different research traditions, as indicators of the stakes involved here. My guides are the very different writings of Andrew Abbott, John Goldthorpe, and Bruno Latour. These point towards emerging possibilities for sociological research under the banner of description. Their arguments are instructive in what they do, as well as what they don't say, and for being the accounts of practising sociologists.

In the first section I introduce their different arguments, showing how their interest in description is related to other aspects of their work. This leads me to explore why such apparently different sociologists can nonetheless find a certain common cause around the need for description. In the second section, I show that their apparent diversity of their theoretical framings can in fact be understood once they are placed in the context of a wider re-orientation of the relationship between the humanities, social sciences and the natural sciences. I argue that all three of them point away from a concern with the interface between the social sciences and the humanities – the kind of encounter central to classical sociology – and how it is the interface with the natural sciences which is the intensive arena of conceptual engagement. In the third section, I explore how the descriptive is associated with growing use of visualization, and examine the implications of this move for the primacy of text and number in conventional social science. In the conclusion I offer some thoughts about the potential for developing the sociological descriptive.

Three Sociologists Embrace Description

On one level, Andrew Abbott, John Goldthorpe and Bruno Latour represent the co-ordinates of bitterly opposed positions and locations within contemporary

sociology. To be sure, they have one thing in common: they are all senior white men, occupying the most pre-eminent positions within the academic hierarchies of their own nations: Abbott is professor of one of the elite American departments, at the University of Chicago, and is recent editor of the world-renowned *American Journal of Sociology*. Goldthorpe is Official Fellow at Nuffield College Oxford, Fellow of the British Academy, and internationally fêted leader of research on social stratification. Latour recently moved from the Ecoles des Mines to a Deanship at Paris's leading social science institution at Sciences Po, and is now a central figure not only in the specialist area of the sociology of science, but in the wider discipline. Yet intellectually – surely – they have nothing in common? . . . apart, I would suggest, from an interest in description.

Abbott is the contemporary torch-bearer of Chicago School ecological sociology, with its links to symbolic interactionism and its concern with micro-social relations, leading to interests in processes of competition, succession and emergence. His most famous book remains his path-breaking *The System of Professions* (1988), an account of how changing forms of professional jurisdiction are related to conflicts over expertise between different expert groups. Goldthorpe is a quantitative sociologist of stratification, notably class mobility, author of still the most authoritative account of inter-generational social mobility in Britain (Goldthorpe, 1980) and also the most widely respected account of comparative social mobility in leading industrial nations (Erikson and Goldthorpe, 1992). He has come to endorse rational action theory (see, notably, Goldthorpe, 2007) and is usually seen intellectually as neo-Weberian. Latour came to prominence with one of the early 'laboratory ethnographies' (Latour and Woolgar, 1979), and has subsequently written influential studies of the 'Pasteurisation of France' (Latour, 1988a) and the Paris subway system (Latour, 1996). He has championed 'actor-network theory', a central current in the recent concern to make science and technology more central to sociological concerns.

These three work independently of each other, with their own networks of followers and admirers, and they are in no way part of a common 'school'. They rarely, if at all, refer to (or possibly even know of) each other's work.[3] It is precisely these divergences which make it more interesting that they have all come to find a prominent role for the descriptive. It suggests that the evocation of the descriptive stands outside any particular self-identified theoretical framing.

Goldthorpe's account of description, most recently elaborated in *On Sociology* (2007) is in some respects the most conventional, insofar as he still sees it as ultimately secondary to the task of causal explanation, yet it remains highly revealing as a critique of what is termed 'variable-centred' sociological explanations which continue to dominate much quantitative social science. Goldthorpe stands opposed to the standard approach which focuses on explaining discrete 'dependent' variables or outcomes. In such methods, the extent to which the values of a dependent variables can be correlated with those of various independent variables is seen as central to the elaboration of causal relationships (see, more generally, Lieberson, 1985, to whom Goldthorpe admiringly refers). Instead, he sees it as a matter of sociological concern to first unravel the patterning of social

regularities, only on the basis of which can causal accounts then be deployed. He is emphatic that these social regularities are not of the kind which can be defined by common sense, empirical (i.e. sensual) observation or policy concerns but instead need to be detected by skilled social scientists, manipulating the right kind of data, normally that of the sample survey. He is thus committed to a particular kind of 'descriptive assemblage', one which involves the elaboration of national level patterning of social indicators. It is in this vein that he developed his famous argument that relative social mobility rates tend to be stable. Although it may appear that there are shifts in the amount of social mobility according to structural changes in the economies of different nation, so that controlling for such shifts shows remarkable obduracy in social mobility rates. The detection of such regularities depends on complex quantitative analysis of survey sources, comparing the mobility of different cohorts, and holding constant changes in marginal frequencies, using log linear analysis and related methods.

We might make the further point here, that much of Goldthorpe's own research, widely celebrated (not least by himself) as 'world leading' within social mobility studies, focuses most importantly on these descriptive aspects. His central endeavour is to demonstrate the patterning of mobility rates over time, and across different nations. By contrast, it is not clear he has explained processes of mobility in any especially convincing way (see, generally, Devine, 1998, 2004). He proposes various causal possibilities for why middle-class children have advantages compared to working-class children, but his actual research here is limited to assessing whether his descriptive data has a pattern which might be consistent with these hypothesized causal forces (see Erikson and Goldthorpe, 1992). It can be claimed that it is the descriptive which takes pre-eminence over the causal.[4] Seen in this light, therefore, Goldthorpe's contribution, both methodologically and substantively, has been as a descriptive sociologist, unravelling the patterning of social mobility in unparalleled detail. It is on the basis of this, that we can compare the arguments of Abbott and Latour.

Andrew Abbott's (2001a) contribution is more rhetorical and more ambitious than Goldthorpe, in that he specifically argues against prejudices in favour of causality, insisting that taking the descriptive seriously entails rethinking explanation itself. Like Goldthorpe, his point of departure is a critique of 'variable-centred' causal explanation. Like Latour (and unlike Goldthorpe), he takes up the mantle of description in explicitly provocative fashion:

> One central reason for sociology's disappearance from the public mind has been our contempt for description. The public wants description, but we have despised it. Focusing on causality alone we refuse to publish articles of pure description, even if the description can be quantitatively sophisticated and substantively important. (Abbott, 2001a: 121)

Abbott's arguments begin from a critique of what he terms the 'general linear model', the idea that it is possible to separate out dependent variables from independent variables, and then use regression methods to compute the relative causal importance of discrete independent variables. All such methods, he claims,

can do no more than establish correlations between variables: the attribution that there are causes leading from one to the other is simply a post-hoc attribution. Like Goldthorpe, Abbott wants to retain a conception of causality, but only by extending and revising its understanding, so that it is based on an account of process. Causality, Abbott argues, must be construed in temporal terms, through processes of emergence, on which basis linear quantitative models fail.[5] Like Goldthorpe, Abbott sees the focus of this kind of explanation as resting on fundamentally descriptive procedures: 'the idea is to find categories and patterns in social processes so that one knows, in the first instance, what regularities one is trying to explain' (Abbott, 2001a: 293). Here Abbott draws upon his ecological Chicago School background, with his emphasis on the mutation of social processes through evolutionary and competitive processes, and champions 'case-centred' methods which are attuned to field-specific relations, rather than the attribution of cause to reified social variables (see Abbott, 1992).

He methodologically argues for the value of the kind of clustering methods which are widely used in descriptive social research such as in marketing. He is particularly well known for championing sequencing methods, first used by biological scientists to unravel the structure of DNA (see Abbott and Hyrcak, 1990). Biologists sequence DNA bases (nucleotides) on the chromosome, and amino acids in proteins and cluster their patterns into distinctive types. Abbott uses this insight but focuses instead on how repeated observations (sequences) of an individual or an organization over time can be clustered into groups. The central endeavour here is not to 'explain' these patterns but cluster them into the groups which are clearly distinct from one another so that their complex relations, differentiations, and associations can be unravelled. Abbott's emphasis on the processes by which clustering and description proceed is an instructive focus, as we can see by turning to our third case, Bruno Latour.

Latour's impact within the social sciences has been profound. Unlike both Abbott and (especially) Goldthorpe, he writes from an ethnographic tradition, which, as the example of Clifford Geertz's advocacy of 'thick description' indicates, has always been the most predisposed towards the descriptive call. His initial interest in description was announced in *Laboratory Life* where it is annexed to his ethnographic approach: 'we attach particular importance to the collection and description of observations of scientific activity in a *particular setting*' (Latour and Woolgar, 1979: 28, emphasis in the original). This concern has been restated on several occasions, and forms a central feature of his recent manifesto, *Reassembling the Social* where he calls for an associational sociology, one which refuses to treat social relations as deep ontological realities but ones which are made up through processes of assemblage. Such is his insistence on the contingencies of these associational relationships that he is insistent that his is a descriptive enterprise which demands the study of surfaces. Those seeking a causal sociology necessarily invoke social objects and thereby a depth model, whereby underlying forces or processes hold sway. Like Abbott, he takes up the descriptive call in rhetorical fashion, refusing its subordination and marginalization. 'No scholar should find humiliating the task of description. This is, on the

contrary, the highest and rarest achievement' (Latour, 2005: 137). In an imaginary debate with an enthusiastic student who is not easily convinced that causal analysis should be abandoned, he repudiates the idea that

> [D]escription is easy . . . You must be confusing it, I guess, with strings of clichés. For every hundred books of commentaries and arguments, there is only one of description. To describe, to be attentive to the concrete state of affairs, to find the uniquely adequate account of a given situation, I myself have always found that incredibly demanding. (Latour, 2005: 144)

Theoretically, Latour embraces an 'unbound Machiavellianism' exploring how various actants are involved in the mobilization of heterogeneous elements.[6] Unlike both Goldthorpe and Abbott, he questions the distinction between human and natural agency, though his emphasis on 'irreduction', the idea that any element can combine with any other element. Latour's substantive work involves detailed, descriptive case studies which emphasize the need to 'follow the actor' through a complex unravelling of the networks of association between material, animal and human agents.

In some respects, as I have suggested through my discussion of Goldthorpe's and Abbott's work, Latour overstates the novelty of his arguments, since he is not alone in seeking to champion descriptive sociology and criticize the deployment of depth models.[7] Latour's work is interesting in part, through his exploration of how social agents, such as natural scientists, gain their knowledge through a process of 'descriptive assemblage'. They derive their findings through the deployment of inscription devices which permit the circulation and enactment of both realities and findings for instance, and as I discuss further in the third section through the use of figures and graphs. In short, and radicalizing and extending Abbott's arguments, Latour disputes that the natural sciences rely on apparently venerated causal models, but instead he shows they owe their significance to their deployment of descriptive devices, hence showing how the descriptive needs to be mobilized, rather than treated as the representation of some underlying reality.[8]

Readers may object that this yoking together of three very different sociologists under the label of the sociological descriptive is misguided, such are the differences in their perspectives. Can Goldthorpe's concern with regularities in class mobility really be seen as having anything in common with Latour's evocation of flow, mobility and contingency? Actually, this is precisely my point. Rather than understanding sociological disputes in terms of how writers are affiliated in self-conscious intellectual camps (Goldthorpe's rational action theory versus Abbott's ecological sociology versus Latour's actor-network theory), the descriptive operates as a more challenging and emergent terrain of potential convergence. It is not difficult to show that if we take away the intellectual verbiage associated with these different 'schools', there actually are some surprising affinities: Goldthorpe's sociology, with its strong statistical framing, assumes the fundamental centrality of randomized, stochastic processes, which only in certain cases become regularized (and hence subject to patterning). This is similar

to Latour's insistence on contingency and flux which is only in specific situations stabilized through 'immutable mobiles' in fixing specific kinds of connections. It is also similar to Abbott's recent evocations of complexity theory. My point is precisely that the figure of the sociological descriptive cannot be contained within established methodological and theoretical divisions and demands a more radical reworking of established divides in sociology.

Sociology Sandwiched between History and Science

These leading sociologists all, in their very varying ways, champion a sociology of description. All of them use description to offer powerful ways of connecting substantive, theoretical, and methodological currents. But they construe the descriptive in different ways, and offer us different co-ordinates for understanding how the causal became so entrenched in sociological identity, and what contemporary strategies are possible in the name of the descriptive.

We should remind ourselves that the social sciences emerged in the late nineteenth and early twentieth centuries out of an encounter with history and cultural disciplines, on the one hand, and the natural sciences, on the other. This process involved settling terms with both these powerful and institutionalized bodies of experts, and finding some means of showing how the social sciences had their own distinctive area of jurisdiction. The terms of settlement generated out of the late nineteenth-century *Methodenstreit* pitted the social sciences, able to generate wide-ranging social laws, against the humanities, whose descriptive concerns were linked to its deployment of narrative and its emphasis on specific, particularistic, accounts of unique states. These stakes were linked to the championing of narrative by history and the cultural disciplines, where such narrative methods were deemed to require the mobilization of unique, specific, events as central to their ability to tell their story. It was against this that the social sciences could establish their expertise in terms of their ability to generalize across unique stories. Against the natural sciences, by contrast, social sciences were aware of their limited powers of 'accurate' observation. In contrast to the research tools of the natural sciences, increasingly organized in laboratories which allowed them to create ever more 'exact' observations, social scientists had to be content with studying the mess of complex social encounters. Therefore they could not easily defend a form of expertise based on the descriptive as being linked to precise observation either.

It is this conundrum which explains the appeal of 'causality' to sociology, since it was a means of explaining that although social science observation could not match that of the natural sciences, it could nonetheless be sufficient for generating causal analyses. The social sciences could thus pitch against two kinds of description: that of the humanities with the focus on narrative specificity, and that of the sciences with their ability to minutely observe patterning and regularity. Originating as part of a wider positivist current, the cause of 'cause' proved pragmatically an effective bolt-hole which allowed sociologists to find an alternative arena of expertise.

The power of this historical framing remains evident even a century later. Many eminent sociologists continue to see the descriptive as about the claiming of specificity. Consider Runciman's recent account of description as inherently bound up with normative and subjective awareness. In McLellan's (2006: 45–6) gloss, the aim of descriptive theorizing 'is to vividly reconstruct and encapsulate particular forms of life . . . Evoking people's cultural experiences/resources, re-figuring their predicaments and utilizing a range of expressive, conceptual, narrative and metaphorical tactics in the process.' We can see hence how the demarcation of explanation as generalizing and as concerned with structural forces from description as embodying humanistic, experiential and specific events emerges out of the terms of settlement with the humanities.

Yet an interesting feature of our triptych (Goldthorpe, Abbott, Latour) is that they are not agitated or exercised by this encounter with the humanities, but are instead energized by the relationship with the natural sciences. It is this which now forms the active, intense, boundary around which the descriptive is mobilized. Abbott, Goldthorpe, and Latour are not worried about differentiating themselves from historians, literary critics, or linguists, in part, because they all repudiate traditional humanistic conceptions of humanities disciplines. Abbott embraces an interest in narrative, but yokes this to a call for 'narrative positivism' thus publicly wresting it from humanistic formulations. Latour explicitly espouses a non-humanist ontology, scornful of the idea that human agency has any priority over that of other kinds of matter. Goldthorpe's position is one in which rational action theory stands as a default assumption for the sociological analyst: we can never know what people really think or feel, so it is best to assume rationality as the most parsimonious assumption, reflecting that other states of mind will tend to cancel each other out. For none of these sociologists is there a sense that the 'cultural' matters: Latour and Goldthorpe explicitly dislike references to 'culture' and do not give it much role in their sociological analysis. Abbott is happy to claim an affiliation to cultural sociology as part of his identity as a Chicago School sociologist, but this is a rendering of culture which is more related to its biological and evolutionary aspects than to its links to the humanities.[9]

This point can be expanded. All three of these sociologists are interested in history, but not History. There is no sense that they are wrestling with history conceived of as tradition, as a live process. History, from which the social sciences splintered, and in the process of separating from which it defined its own terms and concerns, has become an empty territory, to be colonized by descriptive sociologists, or alternatively to be left as a kind of *terra incognita*. This latter point has been most emphatically announced in Goldthorpe's (1991) notorious critique of 'grand historical sociology' where he argues that because sociologists are able to conduct their own fieldwork, pursuing concerns of their own choosing, they are fundamentally different to historians who have no choice but to work with the 'relics' of the past. Abbott's work is highly historical, with his most famous work on professions examining changing forms of professional jurisdiction over the past two centuries, yet he does this through criticizing teleological histories and championing historical social science. Latour, similarly, while deeply

interested in the work of historians of sciences such as Stephen Shapin, also refuses a conventional historical teleology in favour of a novel conception of a history in which 'we have never been modern' (Latour, 1994), and in which tools, techniques, objects and nature are seen to be social agents.

Now, this is not to deny that some (though only a few) practising historians have been influenced by these concerns, especially Latour's, but it is to point out that the interface between history and sociology is not a 'live' one for these three writers, in the sense that any of our three sociologists find it is necessary to actively defend their own practices from historians in order to justify their own expertise. It is this sense which differentiates them from earlier generations where the arguments of Marx, Weber, Durkheim and Tönnies were fundamentally concerned to differentiate sociological from historical expertise. For our three sociologists, the classic humanities concerns with 'Bildung', culture and civilization are fundamentally inert.

By contrast, it is the interface with the natural sciences which is the live one for all these writers, though in very different ways. Abbott champions the deployment of methods developed in the biological sciences in the social sciences, and it is precisely this which explains the positive way that he values description. In answering those who think that 'causal analysis is the only true science' he responds:

> Yet what produced biology's modern understanding of evolutionary trees? Accurate description and numerical taxonomy, known as cluster analysis. What has quintupled our ability to find drugs with specific powers? Sequence analysis, a descriptive technique. Most causal discoveries about protein mechanisms are premised on the descriptive geography of proteins produced by the sequence analytic community. (Abbott, 2001: 122)

The interesting link with research in biological sciences can be extended through reference to Nikolas Rose's (2007) reflections on developments in the brain sciences. Rose shows that whereas 20 years ago it was thought that certain genes were 'causes' of illness or behaviour, such arguments have been displaced by a recognition that at most, they are associated with 'susceptibilities' for particular outcomes: 'in the thought styles of contemporary psychiatric and behavioural genomics, we are now in a post genomic era of polygenetic susceptibilities' (Rose, 2007: 207). Here classificatory concerns take precedence. Now in fact, we might note that this deployment of descriptive methods from the natural sciences is not without its problems. Calvert (2006) notes that many geneticists are in fact worried about the mindless empiricism associated with sequencing methods, as are developmental biologists and clinicians.[10] Here, Goldthorpe's orientation to statistics, with its tried and tested procedures for distinguishing between significant and insignificant relationships might be seen to be preferable. His main concern here has been to champion modes of statistical analysis which focus on the analysis of categorical rather than numerical variables, using methods such as log linear modelling and latent class analysis. His target, in this sense, is rather similar to that of Callon (1998) who criticize economists for examining

relations only insofar as they have been made into numbers. Goldthorpe's focus, by contrast is on exploring the properties of categories, most famously social class, which are not numerical in nature (see, more generally, Savage, 1997). The main point here is that whereas early social scientists thought they could not compete with natural scientists in deploying detailed descriptive inscription devices to 'map the social', Abbott and Goldthorpe are now more confident of the range or repertoires and methods that they might be able to use for this purpose.

If Abbott and Goldthorpe seek to borrow the methods of natural science for sociology, Latour traverses the boundary in a different way, through presenting the natural sciences as a form of social science. The inscription devices deployed by scientists allow them to construct a world which is simultaneously natural and social. Technology is the social made durable. He draws attention to the way that inscription devices are vital for the mobilization of 'immutable mobiles', points of consistency which are made central to heterogeneous networks and connections. He points to the role of figures, graphs, and other representations, and valuably shows how scientific practice relies on deploying such methods. It is precisely these kinds of devices that Abbott is trying to encourage in sociology under the name of the sociological descriptive. Yet Latour's own sociology uses such inscription devices sparingly, and only to illustrate features of a narrative, structure. Latour's preferred *modus operandi*, notably in his account of Pasteurisation, and of the Aramis subway station evokes a form of experimental, modernist, literary form, using aphorisms, dramatized exchanges, and asides. This literary strategy seems to sit rather oddly with his emphasis on the role of figural inscription devices. There is a certain Catch-22 situation here. If Latour is right that inscription devices are central to effective mobilization, one might expect his own work to use them much more than he does. And, to the contrary, to the extent that Latour is influential without recourse to such devices, it suggests either that the social sciences are very different to the natural sciences, or that the importance of inscription devices is overstated. There is a certain lack of reflexivity in Latour's account here, which is that, while recognizing how inscription devices litter the social and natural worlds, he is not prepared to participate in this process except insofar as they can be harnessed to literary, narrative-based, accounts. This leads us to the central issue of the *visual descriptive*.

Visual Inscription Devices

There is a growing interest in the role of visual devices in social research. Mackenzie observes how the battle between mechanized proof making through computing and proof making as demonstrated by human mathematicians plays out through the mobilization of the visual: 'as computer graphics developed in the 1980s and 1990s, visual display became an ever more important expository tool, and some mathematicians began to suspect that computerized graphics were replacing traditional proof' (Mackenzie, 2001: 145). A similar emphasis on the figurative is evident in Donna Haraway's (1997) interventions in feminist studies.

One of Latour's major insights has been to emphasize how the descriptive current within the natural sciences involves visualization (see also Fyfe and Law, 1986). He shows how scientific practice involves the routine deployment of visual description (here, we might note a striking contrast with the deployment of description within the humanities as a form of textually based narrative). Drawing on the work of a colleague in science studies, he notes:

> Lynch, like all laboratory observers, has been struck by the extraordinary obsession of scientists with papers, prints, diagrams, archives, abstracts, and curves on graph paper. No matter what they talk about, they start talking with some degree of confidence and being believed by colleagues, only once they point at some geometrized two dimensional shapes. (Latour, 1988b)

He goes on to show how these visuals work through being mobile (as representations they can be moved around, transmitted, etc.), immutable, flat, of modified scale (whether magnified, or miniaturized), reproduced at little cost, and be able to be recombined, superimposed, and made part of a written text.

What is striking is that such visual inscription devices have rarely figured within sociology, which has been preoccupied with textual and numerical methods, especially in the second half of the twentieth century. The focus on causality involved abstraction from the visual. There is an interesting reflection here on the work of early sociologists, who deployed maps, plans, and diagrams, sometimes in the form of the thought diagrams of Branford and Geddes, with considerable vigour. Some effective social sciences, notably psychology, have used such inscription devices: thus Rose (1990) has shown how the psy-sciences developed a prominent place in the mid-twentieth century through deploying effective visual inscription devices such as growth charts for young children. However, by the middle years of the twentieth century, these visualizations were increasingly written out in sociological work in favour of tabular and textual accounts (see, more generally, on the denigration of the visual, Jay, 1993). It is interesting to note that until the past decade much ink has been spilt on the merits of quantitative and qualitative research methods, where the latter are taken to involve textual elaboration, and the former tabular elaboration, with very little explicit consideration of the role of the visual which defies this easy categorization. In this discussion, the deployment of textual strategies is usually associated with the emphasis on specificity and uniqueness, and numerical strategies with concerns to establish general descriptive patterns.

Once again, the work of our three sociologists is illuminating because all three of them do use visual, figural methods, though in contrasting ways. It is striking how much of the 'descriptive' work of Goldthorpe increasingly uses visual representations of numerical data as a means of presenting the descriptive patterns that he insists are vital to sociology. There is an interesting shift here from his first major work, the *Affluent Worker* study (Goldthorpe et al., 1968a, 1968b, 1969) which reports no less than 151 tables (mostly cross-tabulations), but no figures. His later work sees a growing interest in the use of visuals, with his account of social mobility in Britain (1980) reporting 60 tables (of a greater

variety than in the *Affluent Worker*, including frequencies, cross-tabulations and more complex log linear models) but there are also 22 figures, all of which portray mobility flows in two or three dimensions. This is similar to his comparative study of social mobility, in *The Constant Flux* (1992), there were 23 figures and 65 tables, most of which are used for analytical models. What has happened over these 30 years is that 'descriptive' findings are being increasingly reported in figural form, and causal models are more likely to be presented in tabular form: a different situation from the *Affluent Worker* where descriptive findings were presented in cross-tabulations. Two-dimensional visualizations (arrayed at times to allow three-dimensional displays) have become a standard mode of reporting descriptive patterns. This dimensionality of the visual allows comparison to be made between and across cases.

This deployment of visualizations is especially interesting in the context of Abbott's championing of sequencing methods, which require specifically visual form. These methods array a series of observations for one case, for instance at different time points, by case, so allowing viewers to compare between and within clusters. Figure 1 (derived from Schroeder et al., 2008) is an example of the kinds of visualization which is possible with this method: it uses colour coding to mark the occupational class in which women are located at different moments in time. One can thus trace a specific woman's career over time (tracing left and right), and also compare it with other women (comparing up and down). Cluster analysis can then group these sequences into similar kinds of groups.

Latour would no doubt see the increasing deployment of these kinds of visual descriptions as precisely part of his argument that scientific expertise is bound up with the deployment of novel inscription devices. His work, in fact, marks an interesting alternative strategy. In two of his books, namely *Laboratory Life* and *Aramis* he makes considerable use of visuals. Latour and Woolgar (1979) quote extensive visual material, with much being made of 14 photographs of the laboratory, and 13 figures, including plans of the laboratory itself. This material is extensively referred to and forms a core part of the study. Similarly, *Aramis* draws on photographs of the Paris subway systems and figures of features of its engineering. Yet his use of these is to buttress a predominantly narrative strategy, in which the visuals illustrate particular and specific points, and rarely figure as a descriptive object in their own right, in the way that Abbott and Goldthorpe intend. Latour's recent work marks an even more clear shift in this 'auratic' deployment of the visual, through his role in the Iconoclash exhibition (Latour and Weibel, 2002), though there is evidence of an increasing interest in the potential of digital 'datascapes' in his most recent work (Latour, 2008).

Latour seems concerned to differentiate himself from the practice of scientists, but it leads him to ultimately rely on a narrative format (albeit often of an experimental form) concerned to report unique and contingent events – in fact, precisely the standard form within the humanities and cultural disciplines from whose preconceptions he elsewhere seeks to distance himself. The most frustrating feature of *Assembling the Social*, is having embraced the importance of description, it fails to provide an account of how one can distinguish good from

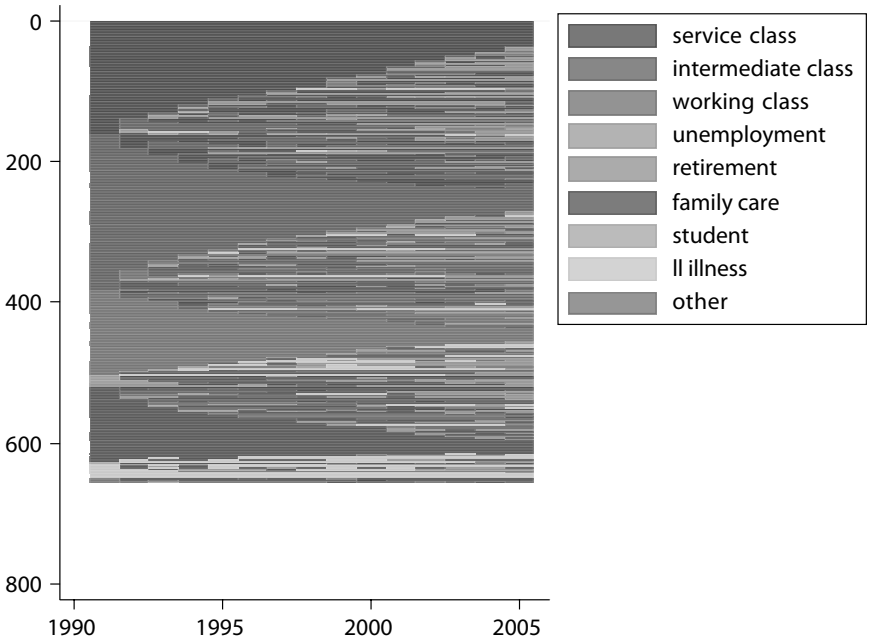

Figure 1 Sequence analysis of female careers. This figure depicts the trajectories of women born between 1940 and 1951 over the 14-year life of the British Household Panel Study. Every individual woman is represented as one line. Different colours are used to picture her employment or non-employment state.

Source: Schroeder et al. (2008).

bad description other than through the effectivity of its own narrative. Yet this cannot be held up as any kind of general model. It was difficult enough for historians to write historical narrative when they confined themselves to human agents: now that Latour wants to admit any and everything as potential agent, how does one go about constructing an adequate description?

The general point is clear. Those pursuing descriptive strategies are drawn to visual methods: examples range from Bourdieu's (1985) use of multiple correspondence analysis (see also Bennett et al., 2008) to the recent popularity of social network analysis (see various examples in Savage and Williams, 2008). Yet we need more reflection on how these are related to descriptive strategies. The choice, then, appears to be between a kind of particularistic narrative description on the model of the humanities disciplines which Latour's practice, if not his overt advocacy, ultimately resolves into, or the deployment of modes of visual inscription, modelled on natural scientific practice, which is what Abbott and Goldthorpe are more predisposed towards. And this allows us to see how the social sciences remain pincered between two forms of descriptive, one mobilized

by the sciences and the other by the humanities. It is this complex relationship which ultimately explains the difficulties of the sociological descriptive.

Conclusion: The Politics of Surface descriptors

We have traced how our three authors lay out valuable co-ordinates which betoken an emergent 'sociological descriptive'. They all show how the descriptive involves a process of assemblage, where processes of creativity, conceptual innovation, and observation can be used to mobilize novel insights. They challenge the orthodoxy that sees description as inert, a means of arraying given, fixed, categories, in contrast to the mobilization of explanation which is seen as intensive and creative. Abbott, Goldthorpe, and Latour all unpick this assumption, though in different ways. I have argued that the descriptive cannot be seen in terms of any of the favoured given categories of sociological thought: quantitative (Goldthorpe) versus qualitative (Latour) methods; hermeneutic (Abbott) versus structuralist (Goldthorpe) perspectives, etc. It evades these terms of reference, pointing to a more fundamental need to rethink the sociological enterprise. The act of description involves conceptual and methodological work, a form of creativity that by working on the surfaces, might produce durable and effective outcomes. I have also shown how the mobilization of visualities is central to this kind of descriptive enterprise

However, our discussion also indicates why the descriptive appears to be a fundamental impasse for sociology. Three main advocates of the sociological descriptive seem to have failed to provide a clear rationale for a specifically sociological framing of their concerns. The issue, put bluntly, is that social scientists might seek to elaborate a set of social scientific inscription devices, borrowing from their colleagues in natural sciences, in market research, information technology, etc., or they may prefer to champion description in the form of unique narratives, much as it has been deployed in the humanities and cultural disciplines. Both strategies are reasonable enough in their own terms, and might even be justified in view of sociology's status as a 'parasite discipline'. Yet, we still need to reflect, in the spirit of C. Wright Mills, how these descriptive modes can be energized by the sociological imagination.

I have tried to show how the concern of the social sciences to emphasize their causal credentials needs to be related to their efforts to delineate their own jurisdiction. Notions of cause allowed a means of explaining how generalizing disciplines were fundamentally different from particularizing concerns. It follows that we need fundamentally to think in inter-disciplinary ways, alive to the processes by which disciplinary jurisdiction affects research strategies. If the social sciences themselves were central agents in the propagation of causal and explanatory knowledge, then claims for description cannot be detached from a rethinking, even a radical questioning, of the role of social scientific knowledge itself. We need to face up to the serious current challenges to social scientific expertise that we can find a means of grappling with the contemporary condition.

Perhaps there is a modest path which sociologists might follow. This begins from a reflexive recognition that as sociologists we are thrown into the world, that we are not Olympian beings whose job it is to stand outside the social in order to pass remote judgement on it, but rather we need to act as situated agents, with all the compromises and imperfections this entails. This is an argument extending Latour's position to further consider the unbound Machiavellian politics of the social through working with the myriad inscription devices that litter it (see Latour, 2008). Here, useful intersection with the arguments of Abbott and Goldthorpe and interest in the devices themselves is welcome, indeed necessary, for an engaged sociology.

Taking this point further, we might note some differences between the inscription devices of natural scientists, which remain wedded to the auratic (in Walter Benjamin's sense), and those of social scientists. It is pictures from the Hubble space telescope (not the digital camera pointing into space from the back garden) and the CERN particle accelerator which define the high ground, or cutting edge, of scientific practice. The thresholds of scientific description depend fundamentally on having the 'best kit'. This is an area of major differentiation from the social sciences, where mundane descriptions, evoking ordinary transactions, from websites, Tesco loyalty cards, CCTV cameras in your local shopping centre, etc., are the stuff of the new social. In these environments, the issue (to again evoke Walter Benjamin) is the mechanical reproduction of social figures, what might be seen as 'the diagrammization of society' which is the terrain on which sociology should now operate. The task of sociology might not be that of generating exceptionally whizzy visuals, using the most powerful computers or an unprecedented comprehensive database, so much as subjecting those which are routinely reproduced to critique and analysis. This involves making the deployment of these devices a subject of social science inquiry.

It follows that a core concern might be to scrutinize how pattern is derived and produced in social inscription devices, as a means of considering the robustness of such derivations, what may be left out or made invisible from them, and so forth. We need to develop an account which seeks to criticize notions of the descriptive insofar as this involves the simple generation of categories and groups, and instead focus on the fluid and intensive generation of potential. In Deleuze's terms, this involves an intensive sociology concerned with processes of de-territorialization as well as re-territorialization.

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Notes

1 The most telling example is that of critical realism, which claims that it allows a means for championing ontological concerns over epistemological, yet even after 30 years, finds it difficult to provide more than programmatic – indeed, epistemological – statements to this effect and has not generated significant research outcomes. For a recent example, see Cruikshank (2002), and the valuable critique by Kemp and Holmwood (2003).
2 The Deleuzian undercurrent is not something I develop extensively here, though the arguments here are indebted to my reading of *A Thousand Plateaus* in a CRESC reading group. Readers may find Nicholas Gane's account of the 'New Empiricism' in this Special Issue relevant to the arguments being made here.
3 The slight exception to this point is Goldthorpe's passing references to Abbott's work on description in *On Sociology* (2007), linked to personal acquaintanceship facilitated by the fact that Abbott has been visiting fellow at Nuffield College.
4 Such a perspective is related to Goldthorpe's Popperian concerns with falsificationism.
5 This concern with emergence explains his recent interest in complexity theory, on which, see Abbott (2001).
6 See his comments, 'there are only trials of strength, of weakness. Or more simply, there are only trials' and

> [W]hat is a force, who is it? What is it capable of? Is it a subject, text, object, energy or think? How many forces are there? Who is strong and who is weak? Is this a battle? Is this a market? All these questions are defined and deformed only in further trials. (Latour, 1988a: 158, 159)

which indicate how his thinking is derived from a foundational Machiavellianism but one which extends beyond human agents to include heterogeneous elements of any and every kind.
7 Latour does not address the sociology of Abbott and Goldthorpe, but he does mistakenly criticize Bourdieu, whose advocacy of field analysis also places him into the category of a 'descriptive sociologist' (where indeed, his criticisms of variable-centred analysis predate those of Abbott and Goldthorpe). See more generally, Bennett et al. (2008).
8 See also John Pickstone (2000), who also shows that analytical, causal analysis has historically been only one mode of work within the natural sciences,
9 It is an interesting biographical point that Abbott's undergraduate studies were in literature, Goldthorpe in history, and Latour in philosophy.
10 Here, as in other parts of this article, I am indebted to Nik Rose's comments regarding recent trends in biological knowledge.

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