Styles of Illusion

by

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Abstract

This paper argues that one of the most prevalent styles in contemporary sociology - style referring to a complex of theory, method and treatment of the literature – systematically allows space for the misrepresentation of reality. The theoretical core of the style in question is a view of identity as formed through the active consumption of discourse, its preferred methodology is that of qualitative fieldwork whilst a largely impressionistic literature is treated as a source of authoritative commentary on the influence of specific discourses. The theoretical and methodological elements of this style interact so that its treatemnt of ethnographic data functions as a self-fulfilling prophecy for whatever presuppositions can be constructed from its treatment of the literature.

This thesis is illustrated through an analysis of the claims made by Paul du Gay (1996), and Musson and Cohen (1997) that enterprise discourse had achieved hegemonic status in the UK during the last decade of the Twentieth Century.
Introduction

The thesis of this paper is that certain confluences of theory, method and treatments of the literature have occurred in contemporary sociology which systematically allow for the misrepresentation of reality. Though such a thesis is best argued through particular instances, the issue, as is indicated by the term ‘systematically,’ is not one of individual self-deception, error or incompetence. To a first approximation, failings of this kind can be assumed to be randomly distributed throughout the sociological spectrum. Rather, the problem lies in a dysfunctional interaction between the elements of what, for want of an established term, I will call sociological styles.

The core theoretical tenet of the style to be examined here is that individuality (or the self, or identity) is constituted though discourse, with the added stipulation that discourse is to be seen as actively consumed, rather than passively absorbed. Through whatever mechanism of elective affinity, this theoretical position has come to be associated with a marked preference for qualitative fieldwork over survey methodologies, and a tendency to treat the literature as a source of authoritative reinforcement (rather than, say, of testable hypotheses or relevant empirical evidence).

Working within this style, Paul du Gay (1996), and Musson and Cohen (1997) amongst others, have claimed, with supporting evidence, that enterprise discourse had achieved hegemonic status in the UK during the last decade of the Twentieth Century. Consistent with the methodological predilections of the style, the empirical side of the case depends on quite limited ethnographic data, rather than the kind of survey evidence which might actually verify statements about the prevalence of a discourse (Jaworski and Coupland, 1999). The field data, meanwhile, are (as is inevitable) interpreted through the lens of the theory. Thus the doctrine of the discursively constituted self encourages the authors to infer the penetration of a discourse from the mere fact of its presence. At the same time, any actions and expressions of opinion inconsistent with that discourse are attributed to the active consumption of it, rather than to the possibility that it exerts no influence. Meanwhile the credibility gap involved in arguing a thesis of hegemony from case study material is partially filled by arguing a case for the global hegemony of enterprise discourse from the literature. By presenting an argument of this kind in advance of the ethnographic account an ambiguity is created, in which it unclear whether the field data are to be regarded as a test of an hypothesis or merely as an illustration of an established phenomenon. The ethnographic data are thereby relieved of some of the burden of evidence. In order to accomplish this, ‘the literature’ is represented as an authoritative consensus on the

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1 The elective affinities between theory and method might runs as follows. The foregrounding of discourse rather than social structure suggests an approach in which the emphasis is on human action as an expression of meaning. The prioritisation of meaning suggests, in turn, that utterances need to be apprehended in their context, a task to which ethnographic fieldwork is far more suited than quantitative survey methods. Suggestions of this kind, however, need to be checked against the writings of individual authors.

2 The analyses are also reproduced in DuGay and Salaman (1992) and Cohen and Musson (2000) respectively.
dominance of enterprise discourse, through the citation of a network of like-minded commentary. It is a network within which impressionistic observations have acquired a patina of facticity through the process of repeated cross-citation.

In this manner, the concepts, methodological predilections and manner of writing the literature characteristic of this particular sociological style assemble into an apparatus through which individual authors, or more effectively a group, can claim truth for a preferred vision of the social. What adds to the persuasive power of the result, and possibly to the attractions of the process, is that this assemblage is not the property of any one individual. It is institutionalised in contemporary sociological practice, and arguments made through it draw additional credibility from that fact.

It may well be that there are other sociological styles, equally well established, within which the elements of theory, method and writing interact with equally disabling effect. That is a matter for future research. The present task is to examine the workings of the style identified above in more detail.

**Back to Reality**

Before doing so, it is necessary to clarify the sense in which representational fidelity might still be expected of sociological work, given that the past 20 years or so have seen a considerable demoralisation of the confident extraversion with which the social sciences once faced the world. A number of theoretical movements have contributed to this mood of self-consciousness and hesitancy, amongst them reflexivity, hermeneutics, the ‘linguistic turn’ and, above all, postmodernism. The new awareness of the interpenetration of sociological process and sociological representation is symptomised in a changed vocabulary, one in which it is now commonplace to speak of 'narrating' or 'writing' social situations instead of producing narratives *of* them or writing *about* them. Where the earlier usage stressed the ontological distinction between what was 'out there' and the account of it, the new signifies an erosion of boundaries in which the selectivity of perception and the interested framing of reports are seen as integral to the production of 'essentially contestable' ethnographic accounts (Geertz, 1973).

But if accounts are contestable, how are they to be contested? On what terms are critical readings possible in the context of a ‘crisis of representation’ (Taylor, 1999), within which the criteria, and even the possibility, of ethnographic validity have themselves become contestable? It is not nostalgia for a prelapsarian positivist innocence which prompts such questions, but worry that a failure to pursue them will allow ethnography to drift into solipsistic self indulgence at one extreme, or, at the other, to be captured by a regime of socially constructed truth, within which theories, methods and topics are legislated by academic gatekeepers. Concerning the first possibility, Linstead (1993) has expressed the concern that the influence of postmodernism is leading to ‘a tendency for the processes of interpretation to be abandoned to a free play of unlimited signification in which any and all meanings are possible, which jettisons rigor and with it critical acerbity.’ Concerning the second, Van Maanen (1995) has argued powerfully against Pfeffer's 1992 address to the Organisation Theory section of the Academy of Management (Pfeffer, 1993), in which he suggested that organisation science could only advance on the basis of a legislated paradigmatic uniformity.

The worry is intensified by a discernible tendency to avoid the question of validity altogether. For some, it seems to have become an act of reckless naivety even to raise
the matter, an embarrassment to be avoided in respectable company\(^3\) (Gergen and Gergen, 2000). For others, the crisis of representation has displaced ethnographic work onto the planes of literature, personal morality or social construction. Each of these responses has implications for the attention-claims made on behalf of ethnography, and consequently, for the terms on which critical debate can take place. More germane to the intentions of this paper, all of them diminish representation as an issue.

In the case of Stanley (1990), the retreat from epistemology leads to a radical empiricism of the text. If all we can know of text is text itself, questions of validity dissolve into those of rhetorical pragmatics, so that truth-value reduces to an ability to persuade. ‘How ethnographic texts convince’ is, of course, an important issue (Golden-Biddle and Locke, 1993), but it is a considerable further step to approach them as nothing but persuasion. Stanley begins with the (probably realistic) observation that most readers of ethnographic texts are not going to replicate the fieldwork on which they are based. In consequence, the criteria by which they are to be judged cannot include representational validity. The only people competent to make a judgement on this are members of the culture in question, and most of these would doubtless reject academic descriptions as tangential to their own structures of relevance. There follows the ‘inescapable’ conclusion that ethnographies are academic products which can only properly be judged by academic standards and criteria. For Stanley, this means that they are to be approached as texts, with attention focussed on their ‘structures, “voices”, rhetoric, metaphors and other devices conventionally seen as located within “literature” and not “science.”’\(^4\) In this manner, the issue of representation, in any realist sense of the term, is erased. All that is visible to the academic audience, and hence all that counts, is the representation of representation, the textual device of ‘description’ through which that audience can be led to believe that the researcher has indeed got to grips with the culture in question. What is not clear is why this should be so persuasive when both researcher and reader also believe that nothing but the text is the case. If the word for world is text\(^5\), there is nothing outside the text which might authenticate it.

Nor is it clear what significance might be claimed for ethnographic work on such terms. Most schools of literary criticism would claim a wider relevance even for

\(^3\) This form of words is not idly chosen. In their contribution to the 2000 edition of ‘Handbook of Qualitative Methods’, Gergen and Gergen report that some of their referees advised them to avoid the term ‘validity,’ ‘given its embattled history and predicted demise.’ (p. 1031).

\(^4\) In the limit, of course, readings of this kind are a logical impossibility, since the figures of rhetoric must ultimately draw their force - and words their meaning - from experience outside the text.

\(^5\) The implicit reference is to Ursula le Guin’s ‘The Word for World is Forest’(1977), an ethnography of the Athsheans who inhabit a completely enforested planet some 27 light years from earth. If Clifford (1986a) is correct in his contention that ethnographies are essentially allegoric representations of the ethnographer’s own culture, Le Guin may have been telling us something about the text-bounded world of academic anthropology. It may also be relevant that she depicted the Athsheans as making no distinction between dreams and waking consciousness.
works of self-declared fiction (see, for example Eagleton, 1983, Ch. 1). It is moreover, a sad conception of the potentialities of academic culture which writes off the possibility of connecting with the concerns of those outside it. Retreating into the workings of the text may achieve defensibility for the ambitions of ethnography, but only at the cost of reducing them to vanishing-point.

Smith and Deemer’s response to the crisis of representation (2000) is to argue that sociological enquiry should be seen through a metaphor of construction rather than one of discovery. Social construction implies personal responsibility, and this in turn suggests that the value of ethnographic work ought to be seen as a matter of practical morality rather than epistemology. It follows that, ‘As finite beings, all we can do is construct social and educational worlds, social and educational constructed realities for which we are morally responsible.’ There are affinities here with Clifford’s (1986b) contention that the attempt to make sense of an alien culture, inevitably constructs it as an allegorised (and morally charged) version of the ethnographer’s own, although Clifford also stresses the importance of ‘facts and accurate accounting’ (see footnote 6)

Whilst few would deny the moral dimension to ethnographic accounts, this does not, in itself, diminish the relevance of representational accuracy. Why should morality and epistemology be alternatives when quite traditional Weberian conceptions of value-freedom allow space for both? Smith and Deemer’s position appears to reduce the value of an ethnographic account to that of the moral vision of which it is an expression. Yet their reference to ‘worlds and ‘realities’ suggests that they expect more of their ethnographies than this. How is their work be accepted as a ‘reality’ unless ‘it appears plausible against our own set of explicit and implicit assumptions about social process . . . [and] against the interpretation framework we have systematised throughout our lives.’ (Rosen, 1991) - and is this not a claim of realism, at least of a general kind?

The problem with all of these attempts to avoid epistemological responsibility is that they co-exist with a desire to retain some distinctive truth-status for ethnographic production. Rosen (1991), for example, expressed concern that the writing-up of fieldwork, ‘is, in the view of some particularly fertile minds, fundamentally a function of fiction in the sense of 'something made or fashioned' of something literally made up, 'of inventing things not actually real', where good ethnographies are 'true fictions' (Clifford, 1986b, p. 6)’. The quotation of Clifford’s self-conscious oxymoron, however, fails to resolve the performative contradiction within which anti-realist ethnography is caught. The real confusion within the fictive quotations is neatly illustrated in Denzin’s (2000) summation of the current state of qualitative methods. On the one hand he states that, ‘I have no desire to reproduce arguments that maintain some distinction between fictional (literary) and non-fictional (journalism, ethnography) texts . . . These are socially and politically constructed categories.’ Yet, in a discussion of narrative experimentation on the same page, he writes, ‘Writers such

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6 The interesting suggestion that categorical distinctions may be disregarded solely on the grounds that they are ‘socially and politically constructed’ raises the question of whether Denzin believes that all categorisations are of this nature, or whether there are some which are more fundamental. Whilst the former position leads to a collapse of language the latter must be inaccessible to a language which makes no distinction between fact and fiction.
as Harrington (1992) use the methods of descriptive realism to produce in-depth, narrative accounts of everyday life, lived up close. They use real-life dialogue, intimate first- and third-person voices . . . " (Italics added)

Accepting the honest concern (and concern with honesty) which lies behind the interrogation of realist epistemology, the persistence of unreconstructed truth-claims within avowedly anti-realist texts of the kind quoted above suggests that they are hard-wired into ethnography as a practice. As a matter of practical morality, moreover, one has to question the value of sociological work whose truth claims extend no further than the moral purpose behind it or the internal structure of its narration. If ethnography is nothing more than a fiction or morality tale, by what right can it claim the attention of those who have their own moralities and tales to tell? Must the game really be given to those impermeable souls for whom the whole of social science is nothing more than ‘people’s opinions’? Questions such as these, suggest that the response to the crisis of representation should be a re-examination, rather than an abandonment, of the possibilities of realism.

Smith and Deemer’s (2000) critique of what they call ‘neo-realism’ is fairly typical of that made from social constructivist positions. In essence it rests on the seemingly plausible contention that if observation is theoretically-laden it cannot act as a test of the theory of which it is an expression and nor can it adjudicate between rival theoretical interpretations, since what is ‘observed’ will not be independent of these interpretations.

In fact both parts of the argument are overstated; the first because it assumes that ‘theory-laden’ is the same thing as ‘theoretically determined’ and the second because it reads into post-positive realism an ambition to provide a single valid account of any given social situation. Concerning the first, it is perfectly possible for theoretically-grounded expectations of a situation to be contradicted, even though these are formulated in the terms of the theory, and the data interpreted through its associated methodological grid. Concerning the second, it is quite consistent with realist positions of any sophistication that a given social situation may be approached via a number of theoretical perspectives, each of which will yield a different theoretical description. If the observational data, as apprehended through the categories and methodologies attached to each theory, are consistent with the expectations derived from it, these descriptions may be equally valid (Hammersley, 1990). If, on the other hand, they contradict, there are grounds for rejecting, or at least revising, any theory.

7 The confusion is typical. Here are two of Clifford’s (1986) contributions to the volume ‘Writing Ethnography.’ On the one hand:

‘There is no way definitively, surgically, to separate the factual from the allegorical in cultural accounts. The data of ethnography make sense only within patterned arrangements and narratives, and these are conventional, political and meaningful in a more than referential sense.

Clifford, 1986a p. 119

on the other hand:

p. 26 ‘. To recognize the poetic dimensions of ethnography does not require one to give up facts and accurate accounting for the supposed free play of poetry.’

Clifford 1986b p. 26
for which this is the case. Doing so, of course, will offend one of the key articles of
the postmodernist faith: that there can be no grounds for privileging—or de-privileging
- any particular discourse. This, however, does not seem an adequate reason for
clinging onto theories which self-destruct on contact with the empirical, particularly
when that contact takes place on terms stacked in their favour.

Hammersley (1990), has argued against this rather conventional view of ethnographic
description as a test of theory. On his view, ‘theory’ as employed in most
ethnographies, is best understood as a set of ideal-typifications through which the data
are ordered and described. The distinction, however, is not as clear cut as it may
appear. Most ideal typifications are not inert categories but working models which act
and interact. Ordering (relevant) data against social choreographies of this kind is
most certainly a test of them, albeit not a conclusive one, since all that is required for
a pass-mark is that they do not contradict themselves when reflected back off the
empirical data. It may also be that the success-rates of the theoretical perspectives
employed in ethnographic reports give the impression that they are not genuinely
under test. Here it may be the traditional authorial posture of interpretative
omnipotence (Van Maanen, 1988) which is at fault, since this conventionally
suppresses any theoretical false-starts. Finally, the empirical contact which a theory
must survive is more demanding than it may first appear. Theories are not tested
against a single case, but through an ongoing series of applications carried out by a
community of researchers, so that the validity ascribed to them is a cumulative
assessment of these applications, akin to that of an inductive generalisation. Most
ethnographic texts, in fact, are prefaced with an account of the provenance of the
proposed theory, and this ordinarily includes a summary of this process of communal
testing.

In Kuhn’s (1962) model of ‘normal science’ the application process expands the
domain of a theory until it begins to encounter anomalies. Interestingly, the social
sciences which have so enthusiastically adopted the Khunian notion of ‘paradigm’
have virtually ignored the equally important concept of ‘anomaly’—empirical results
which contradict theoretical expectations. To be sure anomalous findings alone, even
a whole series of them, are insufficient to discredit a well-entrenched theory.
Routinely, they are constructed as the result of incompetence, inappropriate
operationalisations or exceptions, and even when all of these attempts to deny their
significance fail, they will turn out, in the opinion of the committed, to call for
nothing more than a minor adjustment to their favoured theory. In the opinion of
others, meanwhile, the build-up of anomalies may reach a point where they would be
better accounted for by quite a different theory—usually the one on which they
happen to working at the moment. The consequences of empirical application, in
other words, do not determine the fate of a theory. Rather they provide raw material
for the social processes which do. Social construction, on the Kuhnian model, is
interwoven with impacts against the real, not an alternative to them.

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8 Kuhn himself (1970) greatly deprecated the use of the concept of paradigm in the
social sciences for precisely this reason. Because paradigm shifts in the natural
sciences are a response to the concrete puzzles presented by anomalous results, rather
than a social construction of intellectual elites, Kuhn doubted that the concept could
be applied at all to the arts or the social sciences.
As it is in sociological controversy, so it is in Smith and Deemer’s (2000) world of practical morality: the truth claims of ethnographic description are integral to the rhetorical force behind its morality. To take a concrete instance, McDonald’s ethnographic study of the consequences of the enterprise allowance scheme (1996) is a powerful comment on the ‘enterprise culture’ thesis, not because of the sincerity of its moral commitment and not because of the literary quality of its representations of reality, impressive though these are, but because, having learnt in a general way to relate ethnographic texts (including those of everyday life) to the events they purport to represent, we are led to believe that it is the product of a genuine encounter between theory and reality (See the quotation from Rosen (1991) on page 6). McDonald formed a coherent theory from the ideology with which he took issue, and tested it in terms and against data which the adherents of that ideology would be expected to admit as relevant. The realist epistemology was integral to its rhetorical force, and that is not undermined by the fact that it was the product of a particular theoretical take on the struggles of self-employed North Easterners, nor that there were other things to be seen from other perspectives. Nor finally, was it devalued because it was not the last work on the subject. Like Kuhn’s ‘normal scientists’ in retreat, the ideologists of enterprise doubtless found ways of distancing their favoured beliefs from the failures of the enterprise allowance scheme.

Truth-claims, then, remain central both to the integrity to the ethnographic project and to its argumentative effectiveness. Whilst the interrogation of positivism and other variants of naïve realism has undoubtedly served to clarify the terms of engagement between social theory and the empirical, it has not demonstrated the impossibility of such an engagement. The death of realism has been considerably exaggerated, and it cannot constitute a rationale for claiming nothing more than aesthetic, moral or socially-constructed truth for ethnographic accounts. On the contrary, such moral authority as these accounts possess largely depends on their credibility as explanation. This, in turn, requires that the explanatory theories within them must contain contingent statements about the social world, statements, that is, which are capable of consistency or inconsistency with reported events or situations.

Testing Theory

Much of the theorisation behind ethnographic production, it has to be said, does not claim to be of this nature. All of the discussions of what anti-foundationalist validity might mean apply with full force to writings which offer only ‘insights’ or ‘fresh perspectives.’ It seems entirely fitting that these should be judged as literary production, statements of personal morality or allegorical representations, because that, epistemologically speaking, is what they are. It also seems reasonable to understand their appeals to the empirical as little more than a gesture, since these are not logically coupled to their declared intentions.

The sociological style outlined in the introduction, however, does make substantial truth-claims. Talk and action are reported as support for the writer’s interpretation of them, an argumentative tactic which only makes sense if they are regarded as at least partially independent of that interpretation. The contact with the empirical within this style, however, is not a genuine one. Whilst presenting a surface of empirical engagement, the style actually incorporates a kind of general-purpose interpretative filter through which all possible realities can be assimilated to its theoretical core. This machinery of interpretation goes well beyond the theoretical framing inherent in all reports of talk and action. Framing at this level cannot fully determine the
interpretation of ethnographic data, otherwise such data could never serve as a test of theory, and nor could the theory explain anything in the data. Rather, it is a question of the further interpretation of the data as reported. As is demonstrated by the texts produced by its adherents, the doctrine that human identity is formed through the active consumption of discourse is reconcilable with any conceivable ethnographic reports. In order to see how this theoretical apparatus, together with its associated methodologies and treatments of the literature, can function as a self-fulfilling prophecy, two recent examples will be examined in detail. Both Paul du Gay (1996) and Musson and Cohen (1997) present empirically-backed arguments that the discourse of enterprise had become dominant in UK culture during the 1990s.

**Identifying Discourse**

The discourse-constituted view of individuality runs into a number of problems when it is used to interpret empirical data. A first, outlined on page 14, is that of independently apprehending identity and discourse. A second is that of identifying the discourse which is supposed to be constitutive. As is characteristic of the sociological style within which he works, du Gay exhibits a pronounced distaste for the survey methodologies through which this might be accomplished. His rationale is that ‘Attitude surveys and other large-scale quantitative methodologies simply cannot deal with action . . . They lead researchers to divorce identity from action, thereby effectively eradicating meaning from the research agenda through a process of decontextualisation.’ (du Gay, 1996, p. 27)\(^9\). Whilst there is something in this, one has to ask how else the pervasiveness of a system of ideas might be determined?

The methodological lacuna resulting from this combination of theoretical and methodological preference results in an apparatus of interpretation which lies wide open to impressionistic generalisation. It is part of the sociological style, however, that this element of arbitrariness is concealed by the conventions of academic writing. Implicit in the valorisation of scholarship, and perhaps in the very idea of ‘a body of knowledge,’ is the practice of citing previous work in support of an argument. In this respect, du Gay is merely following good academic practice in arguing his hypothesis (if that is what it is) through the literature. His ‘literature’, however, turns out to be both highly selective and suspect as support. Almost exclusively it consists of impressionistic social commentary by a coterie of post-structuralist authors, principally Miller and Rose (1990), Gordon (1991, 1987) and Rose (1990, 1992). None of these authors present, or even refer to, survey findings. Indeed they present little empirical evidence of any kind, and what there is could be interpreted quite differently\(^10\). For the most part, their case for the alleged dominance of enterprise discourse (and for some of them this is not even a central theme) is built up in the

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\(^9\) Actually, this is the conclusion of Marshall (1988), but du Gay’s manner of quotation makes it clear that he endorses it.

\(^10\) The example of Gordon (1991) will have to suffice. Gordon’s ‘evidence’ for the penetration of enterprise discourse consists of 1) The new political acceptability of mass unemployment. This, he says, is linked to the notion of the individual as an enterprise. 2) The concept of insurance which, he believes, signifies the individual's acceptance of a responsibility to minimise the risks which he or she imposes on society. A quite different argument could, of course, be constructed around the role of insurance in minimising the risk to the insured.
same manner as du Gay’s – by the citation of past writings in the same idiom. Tapping into the generalised legitimacy attached to the citation of authority in academic practice, the result is a substantial-seeming case. Successive citations tend to erode away any qualifications made by a source author whilst adding to the patina of facticity which conceals the flimsy empirical base. The important point to note is that writing practices of this kind could justify the selection of just about any discourse as a candidate for hegemony. It is a process which constructs reality in Berger and Luckman’s (1967) original strong sense.

**Actively Consuming Fieldwork**

Understood in any conventional sense, the idea that late 20th Century British culture was dominated by enterprise discourse is easily disposed of. Despite the expenditure of at least £1bn on various schemes of enterprise support by the Thatcher and Major governments, together with a roughly equal amount on ‘enterprise education’ (Morris 1991, Corner and Harvey, 1991), the British Social Attitude Surveys relating to the period show that attitudes towards individual opportunity social inequality and the welfare state remained stubbornly opposed to those of ‘enterprise culture’ (Jowell, Witherspoon and Brook, 1989; Jowell, Brook and Dowds, 1993; Jowell, Curtice, Park, Brook, and Ahrendt, 1995; Jowell, Curtice, Park, Brook, and Thomson, 1996). For du Gay and Musson and Cohen, however, the dominance of a discourse is something both more and less penetrating than the mere acceptance of attitudes. Following a number of prominent theorists of the de-centred subject, they assert that human subjectivity itself must be seen as constituted by discourse (or regimes of truth, or rationales of governmentality). Given this view of the matter, discourse of some kind must be dominant by definition. The only issue left open to empirical investigation is that of which discourse. At the same time, discursive dominance is theorised through the framework of ‘active consumption.’ Rather than implying the passive acceptance of a set of ideas, the dominance of a discourse is taken to mean that its language, idiom and saliences form a medium through which people relate to one another, formulate their views of the world, and ultimately, their own identities. In this sense, enterprise discourse can dominate even when people define their beliefs in opposition to it. As du Gay repeatedly puts it (Du Gay and Salaman, 1992, Du Gay, 1996) enterprise discourse does not have to be ‘loved’ in order to dominate, a form of words cited with approval by Musson and Cohen (1997).

DuGay’s empirical support for this thesis consists of study of managerial attempts to inculcate cultures of ‘excellence’ in two retail companies. A first problem, therefore, is that his fieldwork is about the management of quality rather than the culture of enterprise. Despite his attempts to argue commonalities between the two, Fournier and Gray (1999) are surely correct in pointing out that quality management involves standardisations and bureaucratic process controls which are quite incompatible with entrepreneurial autonomy. Although this means that the dominance of enterprise ideology cannot be argued from a penetration of quality discourse, it need not mean that du Gay’s fieldwork is irrelevant. If enterprise ideology is as pervasive as he thinks, it should still be detectable, even if it cannot be identified with quality management initiatives.

The problem is that it was not detectable. In his Chapter 7 (Consuming Organisation), du Gay (1996) reproduces 27 extracts from his interviews with sales assistants and their on-site managers. Not one of these shows the slightest trace of the language or ideas of enterprise discourse, not even as something to be opposed. Despite this he
feels able to conclude that the ‘tactics of consumption’ (i.e. of enterprise discourse) ‘did not indicate the failure – in some absolute sense – of the discourse of enterprise.’ (p. 174). That such a conclusion can be drawn from such data demonstrates one of the persistent features of this sociological style: a tendency to demonstrate the truth of its core theoretical presupposition by assuming it in advance. Because (what du Gay takes to be) the discourse of enterprise pervades (as he sees it) his case-study organisations, the identities of those employed within them must be fashioned from it, because the core theory of identity allows for nothing else. Any empirical discrepancies are easily mopped up by the concept of active consumption, which is easily able to explain why the absence of enterprise discourse is actually symptomatic of its omnipresence. In this manner, the sociological style within which du Gay works is able to draw credibility from its apparent ability to interpret empirical data, whilst simultaneously avoiding any real contact with it.

If the talk runs unkindly for du Gay, so does the action. Consider the case of the deputy manager DM who believed that ‘Basically we’re not allowed to think any more. It’s being controlled from head office.’ Du Gay glosses this, not as an experience of bureaucracy, but as experiencing ‘enterprise’ as a debilitating constraint (p. 151). Unexpectedly, this same DM was presented with an opportunity to act in the absence of these centralised controls. Finding spare space in his department due to the early completion of building work, he ‘took visible delight in informing the store manager that he would take responsibility for filling it with his own Father’s day promotion.’ He was, du Gay tells us, ‘obviously very proud of his display and spent a considerable time putting the finishing touches to it.’ Then ‘one of the younger sales managers, commonly regarded as the most ambitious and ‘entrepreneurial’, walked up to DM near his display and gave him a book of raffle tickets. Going slightly red around the collar, DM asked what they were for. His colleague nodded her head in the direction of the display and said “I thought you might need them for your Oxfam raffle.”

Du Gay’s reading of this brutal incident is as follows. DM’s initiative in taking responsibility for the display was not entrepreneurship but an expression of nostalgia for past styles of retailing. Conversely, the younger sales manager’s mockery of it in the name of conformity to a ‘professional’ house style, is supposed to indicate that she was more at home with the discourse of enterprise than with that of the ‘art of retailing’ (p. 151-153).

In a second incident, a group of workers was asked to disregard the normal hierarchical relationships between manager and worker during a quality circle meeting. The manager attempted to signal this by switching the drinks machine to free vend. Du Gay’s account continues, ‘In a move every bit as calculated as that of their manager, staff signalled their disbelief in the apparent reversal of work roles by collectively ignoring this invitation . . .’ Instead of actively participating in the meeting, the staff employed a tactic of minimum response, in which they answered questions put directly to them but otherwise sat in silence. By these means they forced their manager to extracting his quota of three suggestions from the meeting by making them himself. (p. 169)

It takes Du Gay a full page of commentary to arrive at his interpretation. ‘. . . they [the workers] could not escape ‘enterprise’ as a regulatory ideal even while their actions challenged it, precisely because their actions were only called forth in relation to that ideal.’ (p. 171-2).
In the after-shock of the crisis of representation, neither the construction nor the reading of empirical data can be taken to be theory-independent. Du Gay’s way with his fieldwork, however, tests this doctrine to the limit. My own reading of the Father’s Day incident is that the culture of the organisation concerned was decidedly antipathetic to the initiative (enterprise?) which this displayed, that this culture was built around bureaucratic controls and that it placed a high valuation on conformity to a house-style. To me, the quality circle incident indicates that the employees saw management - worker relationships in terms of conflicting interests at least some of the time, and that group solidarity and passive resistance were still part of their tactical repertoire. The argument, however, is not that these interpretations are correct and du Gay’s incorrect, nor even that essentially contestable ethnographic interpretations are, indeed, contestable. Rather the point is that if the events as du Gay reports them are taken to be symptomatic of the active consumption of enterprise discourse, then so could any others. Like the elaborations of Khunian normal science in retreat as the anomalies pile up, du Gay’s theoretical apparatus is able to assimilate anything and everything. By the same token, however, it can explain nothing.

The pity is that active consumption is a valuable analytic tool, and has often been a potent social force. In the work of de Certeau (1984, and one of du Gay’s principal sources) the concept was developed in opposition to the consumer society thesis, according to which individuals are the passive receptors of cultural production. Against this, de Certeau argued that consumption should be seen ‘as an active, committed production of self and society,’ a process in which cultural artefacts are detached from their intended meanings, appropriated and turned against the very ideas they were intended to express. In applying this concept to the consumption of ideas, rather than artefacts, du Gay arrives at a position already familiar in the sociology of the media as ‘reception theory’ (Hall, 1980). It is, however, a move which involves a methodological problem: that of identifying the material from which ideas are fashioned. Since the active consumption of discursive materials needs to be grounded in ideas or experiences which in some degree contradict them, it may be difficult to distinguish active consumption from unmediated expressions of these contradictory ideas and experiences. The negative evaluations of entrepreneurship reported by Musson and Cohen (1997) on page 15 are a case in point. They may be the products of ‘active consumption’ of enterprise discourse, as is claimed by these authors. If so, they are also expressions of a distaste for profit-making, at least of a particular kind, a distaste which presumably has its own ideological or experiential roots. But how, if this is the case, do we know that they are anything other than straightforward expressions of these same ideologies and experiences? The only cases in which active consumption can be clearly identified would seem to be those in which the verbal formulae characteristic of a particular discursive formation are fashioned into alternative meanings. Occurrences of single terms, such as ‘entrepreneur’ will not

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11 Perhaps the best known instance of active consumption, and arguably the one which has made the most difference, has been the Christian appropriation of the Roman method of executing slaves.

12 An example of this occurred in an interview conducted by the writer over a quarter of a century ago. At the time, Galbraith’s title ‘The Affluent Society’ had entered the language, carrying with it connotations of embourgeoisement and the defusing of social divisions. Whether by accident or design, the interviewee used the phrase ‘an
do, since, as was also pointed out by Musson and Cohen’s interviewees, the original meanings of these terms were not the exclusive property of enterprise discourse.

In the absence of these kind of discursive traces, the concept of active consumption degenerates into a catch-all rationalisation through which virtually any action or utterance can claimed as symptomatic of a particular discourse. Thus, in du Gay’s department store study, complaints about loss of managerial control, limited promotion opportunities for the unqualified, the concentration of particular ethnic groups in certain areas and the ‘soft’ treatment of customers are all indiscriminately glossed as ‘operationalisations’ of a managerial discourse of enterprise (1996, p. 151-160).

It is unclear whether the theory of the discursively-constructed subject, in its general form, could ever be written in a form which might explain ethnographic data. Interpreted strictly, this would require situations or events to depend on whether or not identities are discursively constituted. This, in turn would require some independent means of apprehending both identity and discourse. Unless, as is encouraged by the core theoretical presuppositions of this sociological style, it is crudely assumed that the mere presence of a discourse is enough to ensure its penetration (Page 12), this calls for some means of separating the manifestations of identity from those of the assimilation of discourse within the talk of single individuals. It is hard to see how this might be done.

What is much clearer is the answer to the more localised question: that of the impact of managerial programmes of cultural engineering.

Ogbonna and Wilkinson (1997), Rosenthal, Hill and Peccei (1997) and Knights and McCabe (1999) have all carried out qualitative fieldwork on managerial promotions of ‘excellence’. In their different ways, all of these studies contradict du Gay’s. Rosenthal, Hill and Peccei (1997) report the most positive changes in attitude towards customer service, but they also observed that workers turned the rhetoric of reform against any managers they considered to be acting unreasonably. ‘Active consumption’ of this kind, of course, depends on some prior conception of the proper limits of managerial power as well as an appropriation of enterprise rhetoric. Both of the other studies found that there was outward conformity to the programme of re-education but little change in attitudes. Ogbonna and Wilkinson (1997) found that the new rhetoric of empowerment and team-working foundered on the brute fact of an annual cull of the lowest performing 20% of store managers. Knights and McCabe (1999, p. 445) conclude that, 'For TQM to transform employees, some form of constant coercion or incentive seems to be required.' These latter two studies may have wider implications. They suggest that the terms in which people make sense of their immediate experience, the interpretative repertoires through which active consumption takes place to use the terminology under discussion, are not bounded by authoritatively promoted systems of talk.

**Performing Hegemony**

Like du Gay, Musson and Cohen (1997) claim that ‘enterprise discourse can be seen to be hegemonic in that it currently has no serious rivals in the western world, and people appropriate aspects from the enterprise discourse (and therefore enterprise affluent society’ (italics added), in a sense which emphasised differences of economic privilege.
culture) which are relevant for them, and which complement their particular kind of complex common sense.’ Though Musson and Cohen do not use the term ‘active consumption’, this is a clear statement of its core ideas and they, like du Gay, make liberal use of the concept. An additional theoretical engine through which they process their evidence, however, is a performative notion of hegemony, an assumption that action in accordance with the prescriptions attached to a discourse symptomises, in some sense, its dominance at the level of ideas.

The first dataset treated in this fashion consisted of a longitudinal study of the attitudes and behaviour of 18 general practitioners conducted in 1990 and again in 1993/5. This, say Musson and Cohen, showed an increasing penetration of enterprise discourse. The evidence for this was:

1) The increasing use by GPs of ‘enterprise’ terminology such as ‘target groups’ or ‘seasonal variations’. These usages, say Musson and Cohen, ‘indicate the possibility if not the actuality of the business or enterprise discourse beginning to appropriate or colonise the medical discourse.’ Aside from the fact that these phrases are not specific to enterprise discourse, it is equally possible that they figured in tactical discussions on how to preserve medical priorities in the face of an imposed structure and vocabulary of control.

2) That the GPs increased the frequency of tetanus injections beyond what they judged to be clinically necessary, in response to financial inducements to do so. From this, Musson and Cohen conclude that ‘It seems clear from this data that the understanding of the clinical service had been reconstructed in the light of the business requirements.’ Given that the consequences for patients were neutral at worst, it is more likely that the GPs were simply responding to financial incentives.

3. That many of the GPs practices moved to fundholding status. Quite how this is supposed to signify the influence of enterprise discourse is unclear. The respondents themselves explained that ‘we thought if we don’t do it we’ll not survive financially.’ And Musson and Cohen themselves observe that ‘Resistance would have had severe financial consequences since the institution which was imposing the changes also held the purse strings.’

In summary, all of the evidence from the GP study is consistent with a purely instrumental conformity to a system of financial incentives. Musson and Cohen’s interpretation depends on a blurring of categories in which all forms of power are taken to involve hegemony. Conformity to the dictates of what Etzioni (1961) would have called ‘remunerative power’ has been taken, without further evidence, to constitute active consumption of the ideas behind it, and thus to symptomise the hegemony of these ideas.

Musson and Cohen’s second dataset consisted of interviews with 24 women who had recently become self employed. Essentially Musson and Cohen make their argument by assuming its truth in advance. Specifically the salience of enterprise discourse within the thinking of these women is inferred from the fact of their self-employment.

Only two of the women, Musson and Cohen tell us, unequivocally identified themselves as entrepreneurs – and this despite the celebration of self-employment as an expression of the entrepreneurial spirit throughout the 1980s (Dale, 1991). For many, indeed, the label was something of an insult. One was ‘horrified’ when her boss described her as entrepreneurial, and thought it ‘the worst thing anyone could say.’ Another thought of the entrepreneur as ‘a money grubber like Richard Branson.’
goes without saying that these expressions of distaste and rejection were taken to be further evidence of the dominance of enterprise discourse.

Despite their rejection of the entrepreneur label, some of the women approved of certain ‘entrepreneurial’ qualities when these were put to them. To Musson and Cohen this meant that ‘respondents appropriated aspects of an enterprise discourse while simultaneously rejecting the concept of “the entrepreneur”’. One of the interviewees tried to explain that an approval of qualities in question need not signify and acceptance of enterprise discourse because, ‘entrepreneurial skills like perseverance, innovation, autonomy, creativity and decision-making are not embodied in the entrepreneur, and are not tied to a particular era.’ Though they reproduce the quotation, Musson and Cohen were clearly not listening. Two pages later they conclude that ‘[the women] can be said to have actively chosen to embrace the discourse of enterprise in their decisions to leave organisational life and start their own businesses.’

Reading data of this kind as evidence of an hegemony of enterprise discourse signifies a marked theoretical regression, one in which the public forms of words and concrete practices in which people find themselves enmeshed are assumed to be constitutive of what is going on in their heads. It is as if Goffman had never lived.

**Conclusion**

Conventionally, sociological controversy tends to compartmentalise discussions of theory and methodology, whilst studies of the manner in which the literature is deployed are largely ghettoised within research on citation practice. The treatment here suggests that there might be merit in bringing together discussion of these three elements of what I have called sociological ‘style.’

The particular style discussed here pervades modern sociological practice. Its theoretical core is a view of identity as formed through the active consumption of discourse, its preferred methodology is that of qualitative fieldwork whilst a largely impressionistic literature is treated as a source of authoritative commentary on the influence of specific discourses. These stylistic elements interact so as to confirm whatever presuppositions concerning the discursive environment can be constructed from such a literature. Quantitative survey methodologies which might yield evidence relevant to this question are dismissed as irrelevant. In default of some means of apprehending identity which is independent of the discourse from which it is supposedly formed, the penetration of this discourse tends to be assumed from the mere fact of conformity to its verbal formulae and behavioural prescriptions. Where conformity at this level is not in evidence, the theoretical presuppositions of the style encourage researchers to assume that the influence of a discourse can be inferred from the mere fact of its presence. Talk or action inconsistent with this assumption, meanwhile, can be mopped up by a post-hoc deployment of the concept of active consumption. Employed in this fashion, the concept allows virtually any ethnographic data to be claimed as a symptom of virtually any discourse. In this manner the treatment of ethnographic data characteristic of the sociological style considered in this paper works as a self-fulfilling prophecy for whatever presuppositions can be drawn from its treatment of the literature. In summary, it is a style of sociological work which allows wishful thinking to present a surface of empirical credibility.

**References**


