Forgetting Alvin Gouldner: CMS does Reflexivity

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**Abstract**

The point of departure of this paper is a critical reading of Alvesson, Hardy and Harley (2008) on reflexivity in organization and management theory. A notable feature of that paper is the authors’ failure to relate their deliberations to the pioneering work of Gouldner (1970) - which is not cited at all - and to the major contributions of Bourdieu and Wacquant (1992) - which is mentioned only in passing. The present critique proposes to remedy this by suggesting how both of these original versions of reflexivity might be applied to the writings of the CMS school.

1 *Introductory Exposition*

Alvesson, Hardy and Harley (2008) (hereinafter ‘Alvesson et al’) position their paper as a response to an ‘increasing interest’ in reflexivity in organization and management theory, though what might have stimulated that interest is left for the reader to reconstruct, perhaps from the virtues which they claim for reflexive approaches. They begin by announcing that their attention will be restricted to ‘critical, interpretive work’, specifically that which employs a social constructionist ontology. Within that field-within-a-field they distinguish four putatively reflexive ‘textual practices’. These are (i) the employment of more than one theoretical perspective (multi-perspective practices), (ii) the incorporation of the interpretations of different parties to the research process (multi-voicing practices), (iii) some analysis of the theoretical standpoint from which interpretation(s) have been made (positioning practices) and (iv) challenges to the theoretical perspectives employed by other writers (‘destabilizing practices’). Alvesson et al then note that some of these approaches may achieve less in the way of reflexivity that might have been hoped for. In particular, they argue that positioning practices may lead to anti-reflexive claims that the researcher has somehow been able to transcend social influences in a manner only possible for the adepts of self-analysis; whilst destabilizing practices may simply seek to substitute one authoritative interpretation for another. Arguing that these and other limitations might be mitigated by employing the different approaches in combination, the authors go on to suggest that these constituent approaches can be categorized according to whether they seek to undermine existing ‘results’ (‘D-reflexivity’) or whether they seek to encourage ‘consideration of alternative views’ (R-reflexivity). The paper ends with a suggestion that D-reflexivity and R-reflexivity might best work in combination so as to
‘produce “better” research, ethically, politically, empirically and theoretically’. The claims are large and we will return to them later.

2 Internal Critique

2.1 Modes of Reflexivity?

. . . there are more than a few claims to reflexive sociology floating about and, left without further specification, the label is vague to the point of near vacuity (Bourdieu and Wacquant, 1992: 36)

Alvesson et al approach the question of reflexivity without a prior notion of what it might mean. Instead, and possibly in an attempt to be inclusive, they rely on a reputational approach:

we selected texts in OMT [organization and management theory] that have explicitly addressed issues related to reflexivity, as well as texts that are frequently referred to in contemporary writings as being reflexive, based on our general familiarity with the literature as well as recommendations from colleagues, reviewers and the editor

Almost inevitably, this reliance on claims and attributions leads to the discovery of reflexivity in a variety of ‘textual practices’, leading the authors to write of ‘reflexivities rather than reflexivity’. Besides opening up their discussion to Bourdieu and Wacquant’s charge of vacuity, the problem is that these attributions mean very little in the context of the ‘increasing interest’ noted by Alvesson et al. Once reflexivity became established as a theoretical good within the CMS speech community, there was every incentive for those with ambitions to publish in the relevant journals to claim it for their writings and to make similar claims on behalf of sympathetic colleagues. The result has been a long-run process of semantic leakage culminating in a kind of institutionalized false syllogism: that because reflexivity, in any of its meanings, implies an attentiveness to theoretical points of origin, any such attention amounts to reflexivity. The extent to which the concept has been emptied of meaning can be gauged by comparing the rhetorical formulæ discussed by Alvesson et al with Gouldner’s original usage, in which reflexivity referred specifically to a sociology which was able to account for its own production (1970).

This being the case, it is not surprising to find that most of Alvesson et al’s ‘textual practices’ do not, or need not, have anything to do with reflexivity in Gouldner’s sense. They make the point themselves that ‘destabilizing practices’ which simply substitute one authoritative interpretation for another cannot be considered to be genuinely reflexive – albeit with a reservation to be considered in a moment. Concerning ‘multi-perspective’ and ‘multi-voiced’ writings, Shapin (1995: 311)¹, has remarked that “new literary forms” of this kind ‘have the claimed capacity to break up authority only in the case of quite dim readers’. Even for these they can amount to no more than a demonstration that different points of view are possible, and, as such, need have nothing to do with reflexivity (see, for example, Hassard, 1993, Pinch and Pinch, 1988). It is simply incorrect to claim as do Alvesson et al that ‘It is the accumulation of these perspectives that amounts to reflexivity’ (original italics). Reflexivity for any one of the voices or perspectives depends on the existence of critical debate between them, not just their co-existence.

¹ This paper is cited by Alvesson et al, though they do not appear to have registered the views summarised in the main text.
That leaves ‘positioning practices’ as the only authentic representatives of reflexivity in Alvesson et al’s catalogue.

2.2 On the Origins of Reflexivity

Alvesson et al’s account of the concerns which lay behind the development of ‘positioning practices’ is decidedly hit-and-miss. There is no mention of the work of the ethnomethodologists, even though this figures in most conventional accounts (e.g. Ashmore, 1989; Bourdieu and Wacquant, 1992; Alvesson and Skoldberg, 2000). The glaring omission, however, is that there is no reference to the work of Gouldner (1970), with whom the notion of reflexivity first entered the sociological canon. Instead they look to the sociology of scientific knowledge, specifically citing two studies of scientific communities by Latour (1987) and Latour and Woolgar (1979) and a review article by Shapin (1995). On their account these were amongst the influences which directed attention to the politics of knowledge formation and thence to reflexivity.

Unfortunately for this version of the story, both Latour and Shapin are on the record as reflexivity sceptics, at least as far as ‘positioning practices’ are concerned (Latour, 1988, Shapin, 1995). Since Shapin’s discussion relies partly on Latour, it is reasonable that Latour should speak for both.

The objection to ‘meta reflexivity’, as Latour called it, was that it constructs the reader as a naive literalist whose tendency is to believe ‘too much’ in the external referents of a text. Lurking beneath reflexivity of this kind, he argued, is the idea that ‘truer’ texts can be produced if enough precautions are taken and on that basis meta reflexivity claims additional credibility for the accounts which it offers. The premise is that a text which reflects on its own production is more reflexive than one which does not. In fact, Latour argued, the two are on a level since they both refer to something outside the text itself. Instead of piling layer upon layer of self-consciousness to no avail, he asked, why not have just one layer, the story, and obtain the necessary amount of reflexivity from somewhere else? ‘Infra reflexivity’ as Latour proposed it, does just that, accepting that readers are sophisticated human beings who can be relied upon to allow for the origins of a text in their construal of its meaning. Reflexivity of this kind entails an ethics which asks no special rhetorical privilege for social science, a recognition that its texts are, like others, attempts to persuade. By this route, Latour arrived at a form of reflexivity which resembles Gouldner in its reliance on open argumentation, as will presently appear.

The consequences for Alvesson et al’s account of the origins of ‘positioning practices’ are serious since it turns out that the deliberations on the politics of knowledge which led Woolgar to that particular form of reflexivity, led Latour and Shapin to a rejection of it, and did so more convincingly, if anything. It follows that there must be other reasons for the vogue for reflexivity claims within CMS, and what those might be is suggested later in this paper. Meanwhile we can observe that it is tendentious to cite the sociology of scientific knowledge as a source for concerns with reflexivity, because the value of reflexivity is contested within that field and continues to be so (Woolgar, 1998).
2.3 Reflexivity as an Empirical Advance

One of the advantages which Alvesson et al claim for reflexive research is that it is empirically “better” than other research. Specifically cited as an example of that superiority is a study by a study by Collins (1998). This is the passage in which the citation occurs:

Reflexive practices thus explore the broader social landscape within which research and researchers are positioned: ‘the networks of beliefs, practices, and interests that favor one interpretation over another; and, ideally, the way that one interpretation rather than another comes to predominate’ (Collins, 1998, p. 297).

(Alvesson et al: 485)

The exciting story of the present writer’s experiences in the course of tracing this quotation is told in appendix 1. For those who find the longeurs of the academic lot a resistible read, the conclusion can be briefly stated. Amongst researchers in the sociology of scientific knowledge, Harry Collins is well known as a forceful opponent of reflexivity at the level of the text. On this kind of reflexivity in general, his view is that the question to which it is addressed, namely that of the social influences on sociological interpretation, is (i) relatively uninteresting, at least to himself, and ii), better approached through debates within the discipline than some methodological prestidigitation at the individual level. In the particular case of reflexive approaches to the natural sciences, he also maintains that social scientists have no basis for their claim that the same social processes account for the formation of knowledge in the natural and social sciences. Social scientists, he believes, are simply incapable of understanding how natural scientists interpret their data (See Ashmore, 1989: 41-46).

This revelation is deeply damaging to Alvesson et al’s contention that reflexive methodology is particularly suited to the study of social constructions of knowledge. It means that Collins was able to produce an account of the negotiated interpretation of scientific experiments which they cite as exemplary without recourse to ‘reflexive practices’, either as a means of framing the problem or as way of reporting his research. Collins’ work, in fact, demonstrates that reflexive approaches possess no special virtue in this respect.

2.4 A Note on The Ethics of Reflexivity

*When it is at its best, critical theory eschews all temptations to claims of moral elitism and superiority, as well as all posturings of innocence* (Gouldner, 1976: 293)

One of the advantages claimed by Alvesson et al (2008) for reflexive research is an ethical superiority over non-reflexive methods. They claim this both for their ‘D-reflexivity’ – that it offers a means of destabilizing ethically ‘dangerous’ knowledge – and for ‘R-reflexivity’ – that it opens up pathways to research which is ethically superior. In this world of multiple perspectives and uncertain values, these do not look like very clever things to say, but say them they do, and with neither explication nor justification. To all claims of ethical superiority there is, of course, a very obvious objection which it would be tedious to labour at length. For the moment, therefore, I will assume that Alvesson et al, I, and all our likely readers are in agreement on what constitutes ethical research.
Even with that question postponed, what still needs explanation is why Alvesson et al believe that their D-reflexivity could not serve just as well to undermine ethically benign forms of knowledge and, conversely, why they think that their R-reflexivity is inherently incapable of producing repressive representations. This is not just a debating point, since some of the researchers cited as leading practitioners of reflexivity by Alvesson et al can quite reasonably been accused of both of these things.

Concerning the first possibility, Hugh Willmott has repeatedly deployed the apparatus of a supposedly reflexive scholarship against Marxist-influenced studies of the workplace, which, whatever their other demerits, were at least produced with emancipatory intent. In Willmott (1997) for example, Nichols and Beynon’s (1977) interpretations of their data are pilloried as ‘blinded by the framework of orthodox Marxism’. The rationale for attacks of this kind has been that these ‘seemingly emancipatory discourses can be, or become, a form of normalizing, disciplinary domination’ (Willmott, 1996a – cited from p. 115 in Fournier and Grey, 2000). The sentence is worth examining in detail as it exemplifies the insinuating manner in which CMS typically goes about its ‘destabilizing practices’. Notice that it is not alleged that the discourses in question are repressive, only that they can be, and if the reader is still unconvinced that even the possibility exists, they are asked to consider a further scenario in which apparently emancipatory discourses might become repressive. So it is that pigs might fly, if we allow for the possibility that they might sprout wings and lose a little weight. Notice too the complete absence of reflexivity in the accusation, and even of elementary self-awareness, for if a seemingly emancipatory discourse can be, or become repressive, that must also be true of the seemingly emancipatory discourse within which the accusation is made.

Concerning the second possibility – the positive production of repressive knowledges by reflexive research - Wray-Bliss (2002) has drawn attention to the symbolic violence committed in a whole series of workplace studies by David Knights and various co-researchers. In these researches workers have been depicted as complicit in their own subordination through their constructions of identity - and so depicted, one might add, on the basis of little or no supporting evidence (see, for example Knights and Collinson, 1987, Knights and McCabe 1998). Formally indistinguishable from the most gratuitous attributions of false consciousness, such portrayals are also ethically indistinguishable. If the second constitutes ‘ethically dangerous’ knowledge, so does the first.

In defence of reflexivity as a generic approach, it could with some justice be pointed out that neither of these ethically questionably instances are actually the product of an authentic reflexivity. Willmott’s is the simple substitution of his own interpretations for those of the original fieldworkers and as for the representations of workers’ subjectivities by Knights and co-workers, Wray Bliss (2002) has also drawn attention to the prevalence of authoritative interpretations in these studies, notwithstanding the rhetoric of openness and indeterminacy within which they are framed. Arguing against the relevance of such instances on the grounds that the studies in question are not truly reflexive, however, raises a number of further questions as to how reflexivity might be more closely defined and how such an exercise might serve to guarantee ethical outcomes.
Ethics in the Pedagogy of Reflexivity

A minor theme in Alvesson et al, the purported connection between reflexivity and ethics is foregrounded in the work of Ann Cunliffe, an author extensively cited by Alvesson et al. In a paper of 2005, Cunliffe and Jun describe a form of management education designed to encourage ethical conduct in public administrators. To that end they encourage two forms of reflexivity in their students. ‘Self-reflexivity’ to Cunliffe and Jun is an act of introspection – sometimes a collective one - in which ‘we question our ways of being and acting in the world, explore our ways of making sense of our lived experience, and examine whether we act responsibly and ethically’. ‘Critical reflexivity’ in contrast, is ‘a basis for examining taken for granted assumptions, who may be excluded or marginalized by policy and practice, and the responsibility for ethical action at the organizational and societal levels’. The distinction is not very clear but it appears, roughly speaking, to be that between the psychological conditions of ethical conduct and its situational possibilities - the internal and external, so to speak.

Cunliffe and Jun contend that both forms of reflexivity have a contribution to make ‘when formulating and applying policies and construct[ing] dialectical possibilities for meeting social needs by placing their responsibility into the larger contexts of society, citizens, and ethical action.’ Assuming that the meaning of this sentence has been adequately grasped, the ultimate aim is to produce ‘Self-reflexive public administrators [who] recognize their place in creating ethical discourse, in respecting the rights of those around them to speak, and understand how their assumptions and use of words affects policies and, therefore, the social realities and identities of others. In this regard, reflexivity toward others is a civic virtue (reference omitted): a basis for social practice.’

From this all-too-brief summary, it should be apparent that Cunliffe and Jun’s approach is driven by an unstated – and therefore unexamined – and therefore anti reflexive - assumption that the results of a sufficiently thoroughgoing examination of oneself and one’s situation will always be ethically benign. That this is indeed their view can be seen in two examples of ‘unreflexive’ action which they offer: a case of judicial corruption and the Enron debacle. They offer not the slightest evidence that the individuals concerned had failed to think and act reflexively in the manner which they recommend. They simply assume that this must have been the case.

There is no basis for any such assumption. Prolonged introspection into one’s motives and deliberation on the situational pressures to which one is subject is just as likely to produce subtle rationalizations of self-serving conduct as resolutions to try to live a better life, assuming of course, that these are different. More specifically, this openness of the outcomes of introspection is a possibility outside the situational pressures of the academic seminar room, and who is to say that managers do not indulge in something like Cunliffe and Jun’s self and critical reflexivity of their own volition amongst themselves?

Now Cunliffe and Jun have an answer to this, in form at least. It lies in a in a definitional distinction which they draw between reflexivity and reflection. ‘Reflecting-in-action’, they say, ‘incorporates an experimental logic of exploration, move-testing and hypothesis testing (reference omitted), as a means of creating a better match between the professional’s strategies and situational and client conditions.’
Reflection so defined, they argue, ‘does not require an administrator to question the ends, means, and relevance of administrative practice.’

How, then, is it possible to distinguish between reflection and reflexivity as these are practiced by students? Towards the end of their paper Cunliffe and Jun provide a passage from a student essay by way of illustration. In it, the writer comes to see that their reluctance to change had been ‘fuelled by what I can now see was an inherent fear of Change’, that they had been ‘trying to convince myself that a mere exchange of schemas (a new set of values for the ones I was contemplating to modify) would not be successful’ and that finally, ‘I became aware that these were defense mechanisms’. Reflexivity, it turns out, is to be known by its outcomes, preferably accompanied by a renunciation of one’s former ways. In play here is the psychology of the confessional, a regime of truth in which the protestation of one’s innocence merely confirm one’s blindness to sin. Like many such, its capacity to incapacitate and thereby re-assert the authority of the pedagogue, depends on a logical circularity, in this case one in which the presumptive association between reflexivity and ethical conduct is protected from empirical refutation by the get-out clause that any such refutation must have been the outcome of reflection rather than reflexivity.

As reflexivity, both Cunliffe and Jun’s teaching methods and their account of them fall short on a number of counts. The first is that requiring reflexivity of one’s students is not in itself reflexive. The second is that they do not subject their own assumptions to examination. Their teaching practice seems to be expressive of a profound believe that there is good in everyone and that this good will be catalyzed by a sufficiently penetrating self-scrutiny – ‘seems’ because there is no certainty to be had in these matters. There are also exclusions in Cunliffe and Jun’s conception of the world in which their students’ newly acquired commitment to ethical conduct will have to make its way. There is no recognition that the administrators of public services, like other managers, are often faced with hard moral choices, for example one between respecting the dignity and feelings of subordinates and ensuring that clients receive a standard of service to which they are entitled.

Mention of hard moral choices resurfaces the question of ethics which was placed on standby at he beginning of this discussion. What are the ethics of requiring reflexivity of one’s students if that can only be symptomised by the announcement of a change of heart, particularly when that is to be judged by the person responsible for assessing the students’ performance? Finally, returning to Alvesson et al’s rather more peremptory annexation of the moral high ground: what are the ethics of claiming ethical superiority for one’s own approach to research as compared to that of others? Is that not the height of arrogance?

2.5 Reflexivity of the Fourth Kind: the Seductions of Marta Calás and Linda Smircich

At a guess, the bulk of the writings which Alvesson et al would like to classify as reflexivity fall into their fourth category: that of ‘destabilizing practices’. The term is a euphemism. The characteristic posture of CMS is that of the ambush predator, feeding primarily on a practitioner-oriented ‘mainstream’ but secondarily on forms of critique which draw their inspiration from Marxism. As has already been pointed
out, Alvesson et al themselves draw attention to the ‘paradox’ of proposing authoritative re-
interpretations of an author’s findings in the name of reflexivity, but they hedge this admission with the
claim that those ‘destabilizations’ which limit themselves to the negative task of challenging the
‘assumptions, reasoning and knowledge inherent in someone else’s research project’ thereby ‘avoid
making the truth claims that they would dismiss in the case of others’.

Unfortunately for this proviso, they instance Knights’ (1992) claim that ‘he has been able to identify the
philosophical, political, social and economic rules of formation that underlie the development of specific
management practices and discourses, which would “ordinarily elude the conscious awareness” of the
researcher’. Leaving aside the question of whether Knights’s claims to have understood and transcended
the intellectual horizons of other researchers are actually substantiated, what can be said with certainty
is that they are truth-claims. More generally, it is hard to see how any challenge to an author’s
interpretation of their own work could be made without making truth claims of some kind. The apparent
exception of deconstruction is only apparent because deconstruction, subverts without actually
challenging. Based on a theory of language which asserts that all discourse is self-
subverting, deconstructive readings immediately deconstruct themselves, leaving ‘everything in place’. Put
otherwise, it is no criticism of a text to demonstrate that it deconstructs itself if that is true of all texts.

The nearest thing to a destabilization which avoids making truth claims of its own is probably Calás and
Smircich’s prototypical and hugely influential attempt to subvert the received wisdom on management
leadership by representing it as a discourse of seduction which works by suppressing its true nature
to show how the success of leadership can be explained by its seductive nature, which is hidden behind
knowledge claims’. Quite how Alvesson et al manage to read Calás and Smircich as discussing the
success of leadership is a mystery for another time and place. Perhaps it is intended to demonstrate the
ultimate instability of meaning.

Like many CMS scholars since, Calás and Smircich portray themselves as ‘insurgent’, as ‘willing to
unsettle the academic community, and to make trouble, especially for research that is readily accepted
in the wider academic community’ (description by Alvesson et al). Their rationale for this presentation of
self is that they see a relatively unified and dominant discourse of managerial leadership as imposing an
intellectual stagnation which rules out the possibility of saying something new on the subject, whilst
-ironically or cynically- bemoaning the lack of fresh thinking. Whilst Calás and Smircich’s is a posture
which has resonated with subsequent scholars, it could be accused of over-dramatization; it is never
made clear how this regime of intellectual censorship was supposed to work and Calás and Smircich’s
paper was, after all, published in Organization Studies. A sceptical reader might also observe that fresh,
or at least dissident, thinking on the writings of the managerial mainstream had been around for some
time (e.g. Bendix, 1956; Baritz, 1960) but that would be to miss their point. The calls for new thinking to
which Calás and Smircich refer were heard at the meetings of the Academy of Management and it was
the right to be heard in this setting which was at issue, not the freedom of thought as such.

Their actual procedure might seem to escape the charge of authoritative re-interpretation since they
make their case through the creation of ‘reading effects’ rather than ‘typical argumentative logic’, as
they put it. Ingeniously, they argue for this approach on the grounds that ‘typical argumentative logic’ is the discursive form that attempts to cover “seduction” with “leadership” (Italics in original). Having thus excused themselves from the normal courtesies of rational discussion, they set about the creation of ‘reading effects’ which will find seduction and sexuality in the canonical texts of management leadership.

Authorizing their procedures as ‘feminist deconstruction’ and, less convincingly, as Foucaultian genealogy, they produce their lampoons by means of two principle tactics. The first plays upon alternative dictionary meanings\(^2\) of the words used in their subject texts, chasing through chains of near-synonyms until one is found with a sexual connotation. Thus leadership → motivation → desire → seduction/sexuality. The second tactic interweaves each subject texts with a second chosen for its overt sexual content, thereby inviting the reader to see a subtext of sexuality and seduction in the former.

The first tactic depends on the popular notion that words relate to discourse as bricks relate to an outhouse: that an utterance is an assemblage of fixed meanings. Seduced by this misconception, many of Calás and Smircich’s substitutions for the words of their target texts are actually synonyms for different words which happen to share the same spelling. In fact the smallest unit of meaning in a discourse is the sentence, not the word (Benveniste, 1971), always remembering that there are single-word sentences in which the other terms are tacit. This means that the signifieds of a particular signifier depend on its context as well as the way it is spelt. That, indeed, is how dictionaries are compiled. If I prick my finger with a needle, my description of that eventuality does carry a connotation of the heartless buffoon who it amusing. Nor can I warm my fingers at a heat of the races on school sports day.

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\(^2\) Calás and Smircich are not even particularly honest in their employment of dictionary definitions. At one point they use them to make a case that the act of seduction is implicitly female. Their case rests on the claim that the potentially gender-neutral ‘seducer’ does not have a sexual meaning and is now obsolete in any case, and that ‘seductor’ (referring specifically to men) is also obsolete whereas seductress (referring specifically to women) is current. This is the definition they quote:

**SEDUCER**

One who tempts or persuades (another) to desert his allegiance or service. Now rare or Obs.

What they have suppressed is the fact that this is only one of three usages in the dictionary which they are using. The two alternates are:

2. One who or that which entices (a person) into error or wrong-doing.
3. a. One who tempts (a female child) to leave her parents for marriage or otherwise. Obs. b. One who seduces a woman.

That ‘seducer’ is indeed a term currently applied to males is additionally confirmed by a considerable list of titles in print. The following is one of the more recent:

*Jack: The Great Seducer*, Edward Douglas, Harper Collins, 2004, *Jack Nicholson is one of the longest-lasting and most recognized sex symbols of our time. This sizzling biography goes deep in-depth, relating exclusive interviews with past flames and flings*] ‘Sizzling’ – hot sausages, in depth – need we go on?
The case of their second tactic is no better. If one interweaves a text with a second, with the implication that the second makes explicit what was a subtext in the first, the choice of that second needs to be justified on grounds other than the point one is trying to make. That Calás and Smircich do not do.

Typical argumentative logic, therefore, leads to the conclusion that Calás and Smircich’s procedures assume their conclusion in advance. Calás and Smircich, however, have seen this one coming and have cleverly declared themselves beyond the jurisdiction of T.A.L., and have done so even more in advance - so it counts. Perhaps that is another reason for their appeal to the scholars of CMS.

For all their careful talk of the ‘possibilities of other meanings’ (italics added), though, Calás and Smircich repeatedly slip into forms of words which carry traces of the old authoritarianism. ‘What is Barnard saying?’ they ask after their first passage of innuendo-sniffing (italics added). And they provide their sexualized parody as the answer.

Perhaps, though, usages of this kind are not to be construed as authoritarian re-interpretations, since they refer to meanings which are only ‘really’ there in ‘Lampoon-World’. Lampoon-World is a theme park of limitless possibility in which all readings are misreadings and all are therefore equal in their emancipatory implications. In Lampoon-World, the rules of Typical Argumentative Logic do not apply. Things run into one another as if by contagious magic. The liberation of a discourse of feminine seduction from its incarceration in mainstream management punditry somehow sets free other pleasures of the text so that it becomes possible at last to say new things about managerial leadership. In the normal way of things, the fact that a text can be parodied would not count for much. In Lampoon-World, things are different. The spell is uttered, the Big Beasts of the managerial mainstream are slain and the children can come out to play. ‘What is your pleasure?’ ask Calás and Smircich at the end of their paper.

If their text can be absolved from the charge of authoritarianism in the foregoing terms, does it follow that it is reflexive? Not at all. Calás and Smircich make no attempt to question the presuppositions with which they approach their chosen texts let alone relate them to their social position as (then) aspiring female academics. The pity is that there was – and is – a great deal to be said from the standpoint of a reflexive feminism concerning the mix of machismo posturing and paternalistic condescension on display in the writings and plenary addresses of the academy of management (uncapitalised) but it will not be effectively said if the ground of Typical Argumentative Logic is to be ceded to masculinist ideology.

3. Gouldner on Reflexivity

This section of the paper sketches out Gouldner’s ideas on reflexive sociology as a preparation to applying them to the writings of the CMS school. These were first and most famously set out in the chapter Living as a Sociologist: Towards a Reflexive Sociology which concludes The Coming Crisis of Western Sociology (1970). If the problem to which it is addressed is to be properly grasped, though, It is important to read this in conjunction with an earlier chapter, Sociology and Sub-Sociology.
Gouldner begins with an observation which is probably as true today as when he made it in 1970: that we lack any serious understanding of why one piece of sociological research is regarded as good and another poor – or of how sociologists discard one theory in favour of another. He begins his search for an answer with a rejection of what Bourdieu was later to call ‘the intellectualist illusion’: that sociologists evaluate a theory on purely cognitive and rational grounds. He justifies this rejection on two grounds. The first is its presumption that sociologists differ from other people in the manner in which they arrive at their images of society. The second is that sociological theories typically contain terms which are inaccessible at the level of the empirical, even in principle, and also postulates for which there exists little or no evidential support. Instead, social theory seems appeal or repel largely at the level of intuition, a feeling that it conforms, or fails to conform some deeply held prior conviction as to how the world must be. ‘Background assumptions’ of this kind come in several sizes, as Gouldner puts it. Some are very general ‘world hypotheses’ of a metaphysical character: a sense that the world is one or many, for example, or that it is basically coherent or disparate. Others are more restricted ‘domain assumptions’: concerning the nature of human beings, that they are rational or irrational, for example, and concerning society, that its default state is one of harmony or conflict.

These background assumptions are developed early in the course of our socialization into a particular culture, Gouldner continues. They are imbibed along with its language and its categories. Being sedimented deeply into our character structure, they function as affectively-laden cognitive tools which shape our encounters with experience. As a result, they are not easily modified and are highly resistant to evidence. As far as social theory is concerned, these basic presuppositions have the effect of dividing images of the social order into ‘permitted’ and ‘unpermitted’ worlds. A permitted world is one which conforms to the theorist’s prior conceptions of how the world is constituted; an unpermitted world is one which violates these preconceptions in some way. Much social theorizing, Gouldner suggests, is set in motion by some dissonance between a social event or process which is taken to be real and yet is at odds with the analyst’s conviction as to how the world must be. Theorizing, he further suggests, typically takes the form of explaining the unpermitted in terms of what is permitted. In a more current terminology it is an act of sense-making applied to what initially seems to make no sense. Much of the reception, of social theory, likewise depends on ‘the proposal of a mode of being-in-the-world, which the text discloses’ (Ricoeur, 1981: 192), on the extent to which it is found congenial by its potential audience.

These somewhat deflationary observations lay the ground for the final chapter of Coming Crisis, which are more concerned with elaborating the social and biographical influences on the sociologist’s permitted and unpermitted worlds. Though this chapter makes some scathing remarks on the manner in which the academic way of life infuses academic social theory, Gouldner subsequently moved away from the idea of relying on the insights of the individual scholar to produce a reflexive sociology:

Unfortunately, no more than other men do sociologists tell us what they are really doing in the world, as distinct from what they think they should be doing . . I greatly doubt that all they want from the world is just to be adequately supported but otherwise left alone so that they can continue to study it. (Gouldner, 1970: 25)
This points towards the idea that reflexivity can only be achieved through a theoretical dialogue which assumes the right of the listener to question and critique the speakers’ assumption and in which there is a rotation of the positions of speaker and questioner. Gouldner’s further thoughts on the community conditions of reflexive sociology are set out in two chapters of *For Sociology* (1975).

If there is to be rational discourse about the social world, two condition have to be met. The first is some degree of insulation from the press of conventional definitions of social reality and the institutions that maintain them. Though this seems to point towards the university as a site in which rational discourse about social worlds might occur, Gouldner is sceptical. In his view, rational discourse has been superseded as a dominant value in the university in favour of ‘knowledge products’ which are tradable for funding, prestige and power. This being the case, discursive communities need to reach beyond the university if they are to maintain rationality.

Gouldner’s second condition of reflexive theory-making is that there must be commitment to the norms of open discussion on which he specifically cites Habermas’ ideal speech situation. It is also important that the participants in the process of theory-making should resist the temptations of mutual ingratiation: such truth as is possible for mortal beings, in Gouldner’s world, is that which emerges from the contestful friction of minds (Gouldner, 1975: 99).

He also offers some thoughts on the composition of his ‘theoretical collectivities’. Because of the necessary interdict on authority-claims, there is no place for the idea that theory-making can be a specialized activity within sociology, or that it needs to be the product of sociologists at all, ‘Practical reason in the service of human emancipation cannot rest on sociology, anthropology, economics or political science alone . . An understanding of the concrete social totality, which is what we require, cannot derive from such disconnected specializations’ (ibid: 111).

Reflexive theory for Gouldner is intrinsically radical since the free and open enquiry on which it depends can only proceed in opposition to the ideological understandings promulgated by the existing centres of power. This means that reflexive theorists should engage themselves politically in ways which ‘bring themselves into tension, conflict, opposition and resistance to established authority, institutions and culture’ (ibid: 121). As well as committing themselves to their own collectivities, therefore, the theorist, should seek out involvements with those social strata and social movements which ‘are evolving in direction compatible with human emancipation’, whilst insisting at all times on their own independence.

**4. Bourdieu and Wacquant on Reflexivity**

Despite their undisputed standing in the world of social science, Bourdieu and Wacquant (1992) are distinctly ungenerous in their references to Gouldner, despite the fact that their own approach to reflexivity is marked more by its similarities to his original than its differences. In particular, their accusation that Gouldner ‘makes the individual analyst the pivot of reflexivity’ (ibid: 40) cannot be sustained in the light of Gouldner’s advocacy of theoretical collectivities. Nor is it the case that Gouldner ‘fails to objectify the academic spaces and the particular position within it of sociology’ (ibid: 72).
First, the similarities. The problem to which Bourdieu and Wacquant’s reflexivity is addressed - the ‘unthought categories of thought which delimit the thinkable and predetermine thought’ - bears a striking resemblance to Gouldner’s background and domain assumptions. Like Gouldner, they locate the sources of these unconscious predispositions in the individual’s social origins, their position in the academic field and the intellectualist bias inherent in the pose of academic detachment. As does Gouldner, they caution that these sources of bias are hidden by the rationalizing self-image of professional sociology – the ‘charismatic self conception of intellectuals who like to think of themselves as undetermined, “free floating” and endowed with a form of symbolic grace’, as they put it. Also like Gouldner, albeit less explicitly, they argue that a reflexive sociology is inherently radical, ‘By uncovering the social mechanisms which ensure the maintenance of the established order and whose properly symbolic efficacy rests on the misrecognition of their logic and effects, social science necessarily takes sides in political struggles’ (ibid: 50-1, italics in original).

Where they differ from Gouldner is in their retention of the idea that a reflexive sociology can be produced by sociological specialists, that it can be produced by individuals or collaborative partnerships rather than multidisciplinary groups involving a wider public, and that it can be achieved through the fusion of theory-making and observation which occurs during ethnographic practice rather than the public engagement with political activism advocated by Gouldner. They also differ from Gouldner in providing an example of a reflexively achieved theory, and it is an important one.

During fieldwork in North Africa and in his native Béarn region of Southern France, Bourdieu found that the categories of structuralist anthropology with which he was then working, were discarding variations in the enactment of custom as so much redundant ‘noise’. His search for pattern and meaning in these variations therefore involved a simultaneous auto-critique of his current conceptual tools, specifically of their projection of the theoretical reason of academic argument onto the practical reason involved in the strategic use of custom. Thus his theory of strategic practice and his critique of intellectualist tendency of academic theory-making were linked outcomes of the same empirically-grounded exercise in reflexivity. It is important to recognize this theoretical achievement for what it is, since other anthropologists have described similar instances of theoretical blow-back from fieldwork without calling it reflexivity (e.g. Geertz, 1973). It means that the point of leverage in Bourdieu and Wacquant’s reflexivity is fieldwork whereas in Gouldner’s it is public dialogue. In that of Alvesson et al, it will now be recognized, reflexivity hinges on the self awareness of the theorist, a position which is explicitly rejected by Bourdieu and Wacquant. As they put it:

Reflexivity is not to be achieve by reflections on post festum (?) fieldwork nor elaborations of the situation of the observer in relation to the observed. It is not the individual unconscious of the researcher but the epistemological unconscious of his discipline which must be unearthed’. (ibid: 41)

On this point, the reflexivities of Gouldner and Bourdieu and Wacquant concur. Since it is also evident that Alvesson et al’s own stab at reflexivity, an act of introspection which yields nothing more than ‘a preference for forms of reflexivity which make a productive difference’ comes nowhere near surfacing the disciplinary unconscious of CMS, it is to that task that the present paper now turns.
5. Towards a Reflexive CMS

Alvesson et al are at least aware that an argument for reflexivity needs, in consistency to make its own pass at what they call ‘self-reflexivity’. Their unctuous ‘confession’ of a preference for reflexivity which make ‘a productive difference’, however, is testament only to the ‘illusion of self confrontation that serves to disguise a new form of self-celebration’ as Gouldner puts it (1970: 489). As such, it is scarcely the basis for a convincing sociology of CMS. What is attempted here, therefore, is a sketch of what such a sociology might look like, a preliminary gathering of materials along the lines of Gouldner’s approach.

In the light of Gouldner’s later thinking on reflexivity, a preliminary disclaimer is in order. Reflexivity on the model of his theoretical collectivities would require the scholars of CMS to engage seriously with the perceptions of themselves held by people outside academia, with the emphasis on those engaged in emancipatory struggles. Such an encounter might prove salutary. In what might have formed the beginnings of an authentic exercise in ‘self reflexivity’, Alvesson and Willmott (1992) have observed that to ‘practitioners’, critical theory ‘may seem remote, aloof, and idealistic’, that its ‘highly abstract, inaccessible form of communication ... is easily perceived to express an elitist, pontificating attitude toward understanding people and change’ and ‘a cavalier and dismissive attitude toward the mundane detail of key institutions and features of modern society’. As an exercise in taking the role of the other, unfortunately, these remarks were negated by the fact that they were directed at the critical theory of the Frankfurt School, rather than their own.

What can be offered in the present paper meanwhile is clearly not reflexive in itself, even in the sense of critical introspection, since its source is external to the CMS research community. It can only become reflexive in Gouldner’s sense of dialogue if it is chewed over in the course of debate, debate involving not only interested parties but also those with a stake in how management is theorized though currently indifferent to the intellectual currents within CMS.

The starting point is Gouldner’s working presumption that sociologists create and adopt their views of society in much the same manner as everyone else, that social theory is an expression of the whole person, not just that of an a professional persona and that the crucial determinant is its capacity to articulate certain deeply held background assumptions in a convincing public idiom. In his scattered remarks on the application of this perspective to academic sociology, it is clear that the kind of background assumptions he had in mind were those rooted in the experience of academic life itself. For example, he saw ‘The present dominance of the belief that social forces shape men’ (this was written in the USA in the late 1960s remember) as ‘rooted in and confirmed by the special experience of academicians, particularly their political impotence in the university and their docility towards its authorities’ (Gouldner, 1970: 440).

Bearing in mind Gouldner’s insistence that social theory is not the exclusive province of academic sociology, he was not the first to approach its making in this manner. When social theories are held by the subjects rather than the perpetrators of social research, they are called - or used to be called -
‘images of society’ and as such they have been studied in exactly the manner proposed by Gouldner. The basic theorem – if it can be called that – was enunciated by Bott (1937): ‘when an individual talks about class he [sic] is trying to say something, in symbolic form, about his experiences of power and prestige in his actual membership groups and social relationships both past and present’. This blatantly patronizing pronouncement was taken up by Lockwood (1966) and transformed into an illuminating theory of the experiential bases of working and middle class images of society, the latter of which will be applied in a moment to the case of management academics. Meanwhile Bott’s dictum can be adapted a little to read thus: ‘When a critical management scholar creates or adopts a social theory, they [sic] are trying to tell us in symbolic form of their experiences of their actual membership groups and social relationships both past and present’.

Like most discussions of CMS, the present one owes a considerable debt to the taxonomy of Fournier and Grey (2000). Theirs, however, is an insider account and as such it takes for granted what, from an external point of view, is its most outstanding characteristic: that it is almost exclusively the creation of career academics. ‘Career academic’ in the present context will refer to a person who has spent twenty or more years in full-time education, from infancy to qualification at the post-graduate level, before opting for employment in a research-oriented university. In the case of some variants of critical research, notably that carried out in departments of industrial relations and that which holds to an older tradition of left-oriented empirical research in industrial sociology, the socializing effects of this prolonged exposure to the academic ethos may to some extent be mitigated by dialogue with activists in the course of teaching, fieldwork, consultancy or involvement in academic trade unionism itself. This has not typically been the case with the scholars of CMS. They are academics through-and-through and, following Gouldner, the consequences of that fact are the obvious place to begin the construction of a reflexive sociology of their theory-making.

In Europe and North America – and that is what concerns us in the case of CMS - the institutional ideology of education lays great stress on the development of the individual as an independent thinker, with the corollary that the merit of ideas is independent of who happens to hold them. Promoted as an ideal under the artificial conditions of classroom debate, this tyranny of the quick-witted and articulate is reproduced as an image of undistorted communication in the universal pragmatics of Jurgen Habermas. The ideal speech situation, in which all that prevails is the force of the better argument, is thus rooted in the biography of the career academic, becoming a benchmark against which we are urged to measure the systematical distortions of the life world by alternate powers. It is rooted, though, as an ideal rather than a reality. It is obvious, surely, that it is an ideal contradicted by almost everything that happens to the career academic in their long subordination to pedagogic authority. Twenty years of following an authoritatively administered syllabus, of ‘required reading’, of commands to ‘discuss’ or ‘evaluate’, of submitting the results to be marked, graded and handed back with suggestions for ‘improvement’ have taught the aspiring student that the only theory of truth which counts in the world to which they are becoming committed is that which equates knowledge with power and power with

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3 This view of social theory is widely accepted in organizational theory through the work of Gareth Morgan (1986), but not in social theory at large.
In this respect, Habermas relates to Foucault as does ideal to reality. When, therefore, the student encounters the equation of truth and power plainly stated in Foucault’s account of disciplinary regimes, the effect is that of a revelation. Even now, more than thirty years after the English publication of *Discipline and Punish* (1977), there is a perceptible excitement in the otherwise pedestrian prose of CMS scholarship when come to letting the reader in on the truth of ‘truth-effects’. Despite the uncertainties as to how it might actually work and the limits of its applicability, the idea of a regime of truth, took the world of the career academic by storm, it is being suggested, because there is a sense in which it was that world.

As well as a regime of truth, academia is a meritocracy, broadly speaking, albeit one in which the indices of merit are externally imposed from the individual’s point of view. That is to say, the same powers which decide what is to count as knowledge in a field also dispense positions within it according to the individual’s contribution to the production and dissemination of that knowledge. The outrage which attends the occasional tamperings with that state of affairs only testifies to its generality. The immediate experience of life within such a meritocratic regime corresponds to a ‘ladder’ image of society (Lockwood, 1966), one in which position in a social hierarchy is seen as the outcome of individualistic competition. For people whose concept of self has been importantly formed over many years by the gradings of their academic performance, the concept of social stratification detached from some index of individual achievement may lack personal resonance. The unthought impulse of the individualistic cast of mind is to place notions such as class under suspicion as concealing variations in motive, competence or even preference, as for instance Knights (1990) attributes the position of the unskilled working class to a macho preference for hard physical labour. Most CMS scholars, it has to be said, are less obvious. Class is either tacitly ignored or explicitly dismissed as belonging to an obsolete discourse (Clegg and Hardy, 2006: 687). The outcome is a form of scholarship which claims to be critical and yet proposes to study the capitalist corporation without a concept of class (see Salaman, 1981, Scott, 1985 and Zeitlin, 1989 for an idea of what this omission entails).

The contrast with gender is instructive in respect of the relationship between social theory and its experiential roots. With the exception of some recent scholarship which finds that gender alone is insufficient to explain the experience of ex-working-class women in higher education (Mahony 1997), gender obtrudes on the lives of career academics as class most often does not. Accordingly gender is (quite properly) foregrounded in the discourses of CMS as class is (quite improperly) not.

In the field of research output, competition for the career academic places a high premium on originality which, in the case of the CMS scholar takes a very particular form. Its particularity in the first instance is

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4 ‘In the case of literary works, there is also sometimes a practical situation which excludes some readings and licenses others, known as the teacher’ (Eagleton, 1983: 88)

5 It needs to be remembered that during the 1960s when Lockwood’s study was written, there was a good deal of social mobility driven on the demand side by a rapid expansion of the professional managerial workforce and on the supply side by an opening up of higher education to the sons and daughters of the aspirant working class, all of which lent a certain credibility to a meritocratic view of the social order. Nowadays the professional managerial occupations are no longer expanding and the parents of middle class children are finding extra-meritocratic means of ensuring that their social status is inherited.
theoretical rather than empirical: papers which apply an existing framework to new case material (as was the tradition in anthropology for example) are thought to be uninteresting, to fail to ‘say something new’. Conversely, Calás and Smircich (1991) is widely acknowledged as a CMS classic merely for subverting existing theories of leadership, because – so the paper claims – that created the possibility of saying something new. In this the paper typifies – and perhaps played some part in instituting - a kind of theoretical writing which is characteristic of CMS: the theoreticized knocking-copy which Alvesson et al call ‘destabilizing practices’. Despite its deficiencies of self-awareness – which have already been pointed out - this kind of writing makes it onto their list of reflexivity-approved modes, whilst empirical application – which, remember, was the point of leverage of Bourdieu’s reflexivity – does not. That it does so points to a peculiarity of theory as it is practiced within CMS.

To most social scientists, theory closes things down, reduces complexity. It offers to explain something or, at a lower level of aspiration, to render it comprehensible in terms of a model or metaphor. To the scholar of CMS that is conceptual violence, an authoritarian legislation against alternative understandings or even, for those with an over-developed capacity for lurid visualization, symptomatic of a phallocentric urge to penetrate and control. Fruitful theory to the CMS scholar is that which is not only original in itself, but makes possible further originality. Instead of closing things down, it opens them up. Its aim is to make it possible to say new things, as if that were a virtue in itself (‘What is your pleasure?’). This is Alvesson et al’s ‘R-reflexivity’: that which promotes ‘consideration of alternative views’. It ‘problematises’ and ‘complexifies’ existing understandings in a constant battle against closure. In doing so it continually re-creates the conditions of its own proliferation and it does this because the production of theory is what has enabled the career academic to achieve a modicum of prosperity in the schools of business and management.

According to Fournier and Grey (2000), the theoretical resources which have found their way into CMS include ‘neo-Marxism (labour process theory), Frankfurt School of Critical Theory, Gramscian “hegemony theory”), post-structuralism, deconstructionism, literary criticism, feminism, psychoanalysis, cultural studies, environmentalism’, to name but ten. In their account, the vector through which this infusion of sophistication occurred was an influx of social scientists from contracting sociology departments into the expanding schools of business and management (see also Rowlinson and Hassard). Whilst there is

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6 Whilst resembling the critique of ideology in the respect that they seek to undermine the overt meanings of their subject texts, ‘destabilizing practices’ in their pure form (i.e. those which conform to Alvesson et al’s strictures against the assertion of authoritative meanings of their own) abstain on principle from any attempt to reveal a truth behind those texts, seeking merely to re-affirm that this text, like all texts, exhibits an indeterminacy of meaning. As well as being self-negating (as Derrida himself readily conceded) deconstructive writings of this kind seem to be the product of a kind of binary logic which sits oddly with the fluidity of interpretations to which it leads: that because the meaning of texts is not determinate, it must be indeterminate. The idea that the meanings of texts might lie somewhere between these extremes, that they are produced in and for communities of discourse wherein they permit a fairly well defined range of readings is put out of mind as an empirical inconvenience by a mindset which is temperamentally inclined to confine its attention to the text itself. Were it otherwise, Calás and Smircich (1991) might have needed to allow for the fact that the practitioner-oriented writings they picked over to such satirical effect were simply not intended for people like themselves and that the resonances they found within them did not exist for their intended readership. The failure, once more, is one of reflexivity.
undoubtedly some truth in this, it is not the whole story. Critical Accounting began for all practical purposes with the take up of Foucault’s *Discipline and Punish* by the accounting academic Anthony Hopwood⁷ and his colleagues at London Business School, an event which pre-dated the appearance of Foucault on the citation lists of organization studies by some years. The point of making this point, so to speak, is that instead of simply attributing these imports to the hand-baggage carried by asylum-seekers from the social sciences, we also need to ask what was their appeal to the career academics employed in the schools of business and management.

There may be several answers. One is that the application of these ideas to the topics of management (or accounting) or even positioning oneself as the advocate of such an application could be made to count as an original contribution to critical research, particularly since such an act could be construed as opening up possibilities for further research in the manner outlined above. In critical accounting, careers have been made on just this basis. From that point of view, it is not the particular nature of the theories concerned which is important, but the fact of their variety.

A second contributory factor is that the critical commentary becomes the normal mode of writing for the career academic during the later part of their education. The examination halls groan with it. Sedimented as habitus, the recursion of that normal mode prompts a search for sufficiently recondite subject texts when the institutional call goes out for ‘original’ research - and the theoretical traditions listed by Fournier and Grey (2000) are certainly rich in writings of this kind, more particularly those of the Gallic Maitres a Pensée.

More important than these factors, though, may be the structured nature of competition within and between the schools of business and management. The career academic, remember, has opted to remain in academia following twenty or more years of full-time education. Some of their colleagues, however, have prior managerial or professional experience. These, and perhaps others too, may maintain such outside relationships by working as consultants. Broadly speaking the two groups - the career academics and the ‘consultants’, as they will be called here - bring quite different forms of cultural capital to the tasks of teaching and research – and they are in competition. As teachers, the stock-in-trade of the consultants is an air of practitioner credibility, particularly with post-experience students, plus a clear notion – clear to themselves, that is - of what is relevant to managerial practice. Their research tends to follow that pattern, typically taking advantage of their industrial contacts to produce a-theoretical reports of some managerial technique in action. In accounting research, the early papers on activity-based costing, were of that kind.

As those CMS scholars who have experienced life in the former polytechnics will testify, this was the culturally dominant form of management research in those institutions, before they aspired to the trappings of university status, largely under the impact of research assessment. In such an environment, the would-be management intellectual had a thin time of it and one could make a case that critical management began as a usurpationary project designed to open up the channels of career progression.

⁷ In fairness, it should be pointed out that the influence of the sociologist David Rosenberg may have been at work here. Rosenberg’s premature death in 1984 was a great loss and critical accounting could well have developed differently had he survived.
for those of a more reflective and bookish disposition. Significant in this connection is the fact that the two major examples of ‘destabilizing practices’ quoted by Alvesson et al (Knights, 1992 and Calás and Smircich, 1991) are attempts to undermine the credibility of the kind of mainstream managerial prescription which would figure in the teaching and research of the consultants. Illuminating too is an ill-tempered exchange between the ‘critical’ Hugh Willmott and the ‘mainstream’ Richard Whittington, with Whittington (1992) accusing Willmott of ‘one-eyed reductionism’ for insisting on the capitalist context of managerial action and Willmott, for his part, quoting Whittington as an exponent of the kind of ‘bourgeois analysis’ from which he proposed to distance himself (1997). At stake in this formally academic controversy was the question of how management might be legitimately researched and it is noticeable that the right to publish in reputable management journals was overtly at issue in such papers as Alvesson and Willmott’s (1992) discussion of emancipation. Theory, therefore, was to be the career academic’s equalizer in their competition with the consultants, the more inaccessible to said consultants and even the more repugnant to their version of common sense, the better. In this aspect, theory serves to exclude and discomfit the consultants exactly as avant-garde art serves to exclude and discomfit the bourgeois (Bourdieu and Darbel, 1992). ‘Conspicuous outrage’ - to adapt Quentin Bell’s analysis of the superfluities of Restoration finery (1976: 44) is much of the point.

In pursuit of the cultural transformation needed to establish the legitimacy of recherché theory within the business school, the critical management scholar could make use of two sources of leverage. The first was a certain embarrassment at the perceived low intellectual level of practitioner-oriented management scholarship in the councils of the university. The second was the possibility of re-exporting theory imported from the social sciences in re-worked forms to actual social science journals, thereby helping to establishing intellectual parity for the business school. In both respects the critical management scholar, once an irrelevance to the no-nonsense business of vocational management education gradually became an asset, a living icon representative of the fact that the study of management was now no longer the property of the university. It did not happen overnight and nor were the intellectual merits of the case the sole determinant, always supposing that these could ever be independently established. It took a lot of work: the organization of conferences, the lobbying of gatekeepers, the establishment of new journals, and so on. Alvesson et al themselves capture something of this process in their inadvertent pen-portrait of the CMS scholar as a party animal: ‘The reflexive researcher is thus a networker and politician (references omitted), able to identify conventions, fashions, and conformist pressures embedded in publication outlets, journal formats, conferences, and funding arrangements’. For this labour of schmoozing, those who have benefitted should be grateful.

If CMS is characterized by a hypertrophy of the theoretical, it also presents with an atrophy of the empirical. According to Alvesson and Skoldberg (2000), this is a characteristic which it shares with the critical theory of the Frankfurt school, the rationale in the latter case being a concern with emancipatory possibilities rather than existing oppressions. The reasoning is not entirely convincing since possibilities

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8 ‘The fact that structuralism offends common sense has always been a point in its favour’ (Eagleton, 1983: 108)
9 ‘Inadvertent’ because the passage is intended to identify the pressures which the reflexive scholar needs to resist. The reader is invited to consider how plausible it is that someone determined to resist those pressures would need to know so quite much about them.
which are actually possible ought to be rooted in some presently existing condition, or so it ought to be believed by those CMS scholars who speak retrospectively of ‘conditions of possibility’. Whether the argument applies to CMS is debatable in any case, since, for all its talk of emancipation (e.g. Fournier and Grey, 2000) its writings are noticeably short of the kind of Utopian vision which might need to be defended against the disillusions of empirical investigation.

A more convincing explanation of the tendency of CMS scholars to avoid fieldwork had been offered from within CMS itself. In effect, the theoretical apparatus which it characteristically deploys to undermine whatever empirical basis there might be for mainstream management writings (‘destabilizing practices’ once more) has made it difficult for the CMS scholar to conduct empirical research without the embarrassments of performative self-contradiction. According to Marsden and Townley (2006), a recognition of impossibility of theory-neutral research has undermined confidence in empirical work and encouraged a retreat into the relative safety of pure theory. Or, as Clegg and Hardy, 2006) put it:

‘Maintaining consistency between ‘doing’ research and the tenets of postmodern theory is particularly difficult because of the significant relationship between author and text . . . Little wonder then that many researchers avoid this dilemma by eschewing field work altogether’

What an admission! In most speech communities, the word for theory which avoids contact with the empirical for fear of contradicting itself is ‘wrong’.

The most convincing explanation of the tendency of CMS scholars to avoid empirical work, however, might be the simplest: the biographical fact that the career academic has chosen to remain in academia after twenty or more years in full-time education. Thoroughly imbued by this time with its highly specific cultural nuances and likely to be unconscious of its febrile intolerances, such an individual might not find it easy to achieve the kind of rapport which fieldwork demands, particularly within organizational cultures which may be bullying (possible), racist (likely), anti-academic (also likely) or sexist (almost certainly)10. Fieldwork for the career academic is a terrain of relative disadvantage vis-à-vis the consultants and insofar as the objective of research is to redress the balance, it is best avoided.

As a minority concern within CMS, it is perhaps not surprising that some of the empirical research which has been reported has been perfunctory and badly documented, the latter because the former, possibly. One has to rummage through the chapters of Harding’s Social Construction of Management (2003) to discover that its empirical basis consists of six one-hour interviews11 whilst Casey’s (2002) evidence for a new corporate spirituality consists of nothing more than some ill-specified conversations with friends, so

10 There is a biographical basis for this observation. In 1978 the author took over the remaining 15 months of a two-year programme of observational fieldwork on the departure of the previous fieldworker. Also inherited from that individual was a lengthy disquisition on the research methods he proposed to use, featuring many of the names now fashionable in the study of qualitative methods. Some weeks into the fieldwork, in a factory which was all of the things mentioned in the main text, one of the managers happened to mention that ‘the other bloke’ had only lasted one day. Of course this could have been for reasons other than culture shock.

11 In fairness it should be pointed out that some of the interviews are interesting inasmuch as they include accounts of the self-observed changes experienced by experienced nurses on becoming managers. As such they offer one of the few empirical substantiations of the widespread assumption that managerial work constitutes the subjectivities of those who perform it.
far as one can tell. Possibly taking the gold in this respect is Samra-Fredericks’ (2005) claim that two recorded fragments of conversation - about one hundred words in total - amount to an empirical verification of Knights and Morgan’s (1991) Foucaultian take on strategic management. It is not the case that all empirical research within CMS has been of this casual nature – the studies by Knights and McCabe (e.g. 1998), for example, are an obvious exception – but the fact that work of the kind instanced is treated seriously is indicative of a culture in which questions of evidential support are regarded as secondary.

Texts, on the other hand are another matter. Here, the career academic is on home ground. Besides their passivity, there are the advantages that writings can be scrolled at will and endlessly pored over, and that they can be the subject of the endless sophistications of hermeneutics, deconstruction and all of the other apparatus of the linguistic turn. Texts are popular with CMS scholars as the subjects of empirical work, and they can be argued to be as valid as fieldwork itself on the basis of Ricoeur’s contention that meaningful social action (and what other kind is there?) can be retrospectively considered as a text (1981, Ch. 8). That texts might offer partial representations of reality (in both senses) need be no drawback since it is a known and incontrovertible fact that the concept of a disinterested version of reality is one which lacks a referent. Looked at in this light it would be interesting to consider how much of the theory which habitual circulates amongst CMS scholars serves as a rationale for the production of research based solely on written texts. In advance of such an exercise meanwhile, we are free to enjoy the moments of unintentional comedy provided by those who would warn us of the pitfalls of inferring behaviour from texts whilst simultaneously doing just that (Townley, 2005).

As well as playing to the strengths of the CMS scholar, the linguistic turn also resonates with a working life spent in the lecture hall and the seminar room. Unlike the laboratory and fieldwork-based fields of study, that of which CMS speaks enters the university setting only as it is spoken. In a performative sense, therefore, the social construction of reality, more particularly its discursive bringing-to-presence as an object of debate, is a matter of everyday experience for the CMS scholar. What could be more natural than to generalize this experience as a general account of the human condition on the one hand and as methodological edict as to how it should be studied on the other?

And yet - just as the desk-bound scholar craves an outlet for the animal within, the CMS tendency to dematerialize the social, to dissolve both action and structure into the endless circulations of discourse (Geras, 1987), seems to leave an unsatisfied hunger for the concrete – or perhaps creates an atavistic guilt for having forsaken it – a sense of lack which surfaces in its incidental phraseology. Why else would Alvesson et al so persistently use the term ‘textual practices’ to refer to the act of writing, as if that were essentially equivalent to action in its everyday sense? Without launching into a full-blown exercise in deconstruction, this instance sensitizes the reader to a general preference within CMS for metaphors which concretize the discursive: conceptual violence, analytic tools, the materiality of managers as human subjects, technologies of managerial control, and so on. So it is that the materiality of the CMS scholar as a human subject returns to haunt their writings in the ethereal form of a performative contradiction with the scholarly interdict against reification.
7 Conclusion

Alvesson, Hardy and Harley (2008) may well be correct in reporting an increasing interest in reflexivity in critical management circles (CMS). A close-reading of their paper and of some of the examples they quote, however, suggests that the interest is in talking and writing about reflexivity rather than actually doing it. This paper has sought to alleviate that state of affairs by offering an outline sociology of CMS on their behalf, not a reflexive exercise in itself, but one which might help those concerned to achieve reflexivity.

The major theoretical resources employed are the treatments of reflexivity by Gouldner (1970, 1973) and Bourdieu and Wacquant (1992), the latter being largely overlooked by Alvesson et al (2008) and the former, completely so. Based on the supposition that the circumstances of knowledge production are reflected in the substance of what is produced, it has been possible to account for some of the major characteristics of CMS. The relevant circumstances are that CMS is almost entirely the product of career academics working in the schools of business and management, and that these are in competition with colleagues who have outside experience or contacts. The characteristics of CMS which relate to these circumstances are:

- its tendency to dissolve questions of truth into those of social process, specifically those of power and the discursive construction of reality.
- its tendency to avoid or challenge vocabularies of social inequality, other than those of gender.
- its marked preference for the production of theory over empirical work.
- within theory, its preference for those forms of it which resist closure.
- within empirical work, that which can be pursued on the basis of texts rather than fieldwork.

It is not a flattering picture.

References


Appendix 1. In Search of Professor Collins

It would be nice to pretend that my knowledge of the sociology of scientific knowledge was sufficient at the start of this investigation to have immediately spotted that the reference to Collins (1998) was to an opponent of reflexivity. Sadly this was not the case. The source was Malcolm Ashmore’s entertaining romp around the crazy-golf course of reflexivity (1989). From this I learnt that Harry Collins, a major voice in the sociology of scientific knowledge, regarded reflexivity as a ‘Bad Thing’ (Ashmore’s phrasing).
Chancing to recall that Alvesson et al had made reference to a work by Collins as exemplifying the virtues of reflexive methodology, my first thought was that this must be a different Collins. My second thought was that it might have been the same Collins, writing after an episode of recantation which occurred sometime in the sixteen years between Ashmore’s interview (1982) and the publication of the cited paper in 1998.

The next move was to consult Alvesson et al’s table of authorities for the information which would enable me to access the 1998 paper. There I found a listing for Collins, W.H., author of *The Meaning of Data: Open and Closed Evidential Cultures in the Search for Gravitational Waves*, a paper published in the *American Sociological Review*, Volume 104, Number 2. The initials W.H. could fit Ashmore’s ‘Harry Collins’ since it was possible that Collins favoured his middle name over his first. On that basis I opened up the Web of Science database and looked up ‘Collins WH, 1998’. Drawing a blank, I then tried ‘Collins W* 1998’, still without result. I then tried ‘Collins, 1998, American Journal of Sociology’ and there I found a paper not by Alvesson et al’s W.H. Collins but by H.M. Collins – none other than Ashmore’s ‘Harry Collins’.

Not to pick nits – I had clearly found the correct paper since the title and quoted extract were exactly as reproduced by Alvesson et al. Since it was possible that Harry Collins had taken time out from his consideration of the evidence for gravitational waves to express a 1998 view on reflexivity, it seemed worthwhile to search the downloaded pdf for any mention of ‘reflexiv*’. As luck would have it, this yielded the following:

> Consider again that those who insist that reflexivity is a vital part of the sociological analysis of scientific knowledge are individualists in the sense used here, because they believe that it is the duty of the individual to produce a complete analysis: the analysis must include not only a discussion of the social influences on the science under examination but also an analysis of the social influences on the analyst. The collectivist (such as myself, in this instance) believes it is satisfactory to complete the analysis of the social influences on the science, leaving other members of the community to analyze, if they are interested, the social influences on me (as in 12 Ashmore 1989).

(Collins, 1998, p. 304)

This is clear enough. Harry Collins turns out to have been as implacably opposed to methodological reflexivity in 1998 as he was in Ashmore’s interview of 1982, and for the reasons given in the main text.

12 At first sight, Collins’ rather dismissive delegation of reflexivity to the province of communal debate resembles the collective dimension stressed by both Gouldner and Bourdieu. The resemblance is only superficial however. In both Gouldner and Bourdieu, the intra disciplinary debates presuppose a normative commitment to an exposure of the doxae of social interpretation. Without such a commitment, it is more likely that internal debates within a scientific community will result in a sedimentation of scientific routine into the collective unconscious (Bourdieu) of the discipline – the precise opposite of the intentions behind reflexivity. Is it fanciful to propose that the ‘debate’ on reflexivity to which Alvesson et al see themselves as contributing (p. 481) exemplifies just such a process? If this is really a debate within CMS, as it was in the sociology of science, where is the other side of it? Where are the reflexivity sceptics?
This being the case, the nature of Alvesson et al’s encounter with Collins’ paper remains something of a mystery. The incorrect initials are trivial; it is a mistake anyone could make. But how is it that they have read enough of Collins’ paper to provide an accurate quotation from it and yet miss a passage only seven pages earlier which clearly negates the argument they are trying to make?