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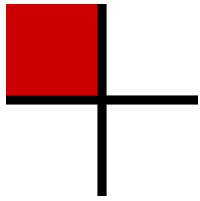
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Practical Criticism and the Social Sciences of Management

Peter Armstrong and Simon Lilley

Introduction

As we are reminded by the strong programme in the sociology of science, the processes by which ideas come to be counted as knowledge may have little to do with their intrinsic merits. Particularly in the social sciences and more particularly still in those social sciences relating to management, the absence of any analogue to Kuhnian anomaly enables certain favoured texts to circulate within the influence networks of academia, accumulating authority and creating alliances until certain of them achieve quasi-foundational status. Since these are the social mechanics of fashion, there is no more point in asking why the received ideas of an academic network are as they are than there is in asking why that which is in fashion is fashionable. Within this socially constructed reality there is a fusion of power and knowledge which places normative limits on the field of enquiry such that the questioning of certain foundational ideas and texts, even on the basis of empirical findings, is taken to symptomize not an active mind or an interesting piece of research but a failure of understanding (e.g. Sosteric, 1996: 305-6, O'Doherty and Willmott, 2001: 468).¹

Legislative put-downs of this kind are the prerogative of reputational capital accumulated through authorship-claims which translate all-too-readily into the instrumentalities of 'professorial power' (Bourdieu, 1988). Once this is accomplished examinerships, editorships, appointments committees, and the boards of grant-giving bodies become means whereby established authority-figures are able to favour loyalists and infiltrate them into positions of influence. Thus embedded in networks of mutual interest, professorial power is in a strong position to suppress any interrogation of its academic basis. Nor, in the ordinary way of things, is there much prospect of such impertinence. The socialization of the young academic sees to that. Woven into numerous textbooks and re-enacted in countless tutorials is the intellectually passive

1 To Sosteric's empirical finding that resistance to managerial control could be both collective and effective, in contradiction to their own theories on the matter, O'Doherty and Willmott responded with the following specimen of academic hauteur: "It would appear that a limited and cursory reading of Foucault leads Sosteric to overlook the complexity of surveillance and discipline associated with management control". As we know, however, appearances can be deceiving!

sylogism which informs the life of the undergraduate: what is on the syllabus is there because it is knowledge and it is knowledge because it is on the syllabus. Those in whom the worm of doubt stirs for a while learn to practise a suspension of disbelief which, slowly and sadly hardens into belief itself.

Coexisting with these repressive tendencies, the study of management has also undergone a kind of Balkanisation (Whitley, 1984). The same uncertainties which have created the enabling conditions for the currents of fashion have also made it possible for the energetic and determined scholar to manufacture 'new' fields of knowledge as an alternative to an apprenticeship of conformity and deference. Of this process, the creation of the Critical Management Studies brand (Thompson, 2005) out of some of the less inconvenient fragments of critical theory (Alvesson and Willmott, 1992; Gephart, 1993) is only the most prominent example. Once institutionalised, academic authority in these new fields becomes consolidated through the mechanisms of censorship and self-censorship already described. Thus insulated from criticism, the standing of the authority-figures within particular academic regimes becomes both self-confirming and self-perpetuating. Their standing as academics is attested to by a mass of publications certified by a refereeing process which simultaneously refracts their own authority and insulates it from criticism. These tendencies to stifle dissent are reinforced by a general norm within academic culture which insists that if it is to be expressed at all, it must be in moderate tones and depersonalised forms. To use 'inappropriate language' in this culture is to forfeit the right to a hearing at the outset. Appropriate language, on the other hand, requires a series of verbal genuflections to the effect that one is more than likely to be mistaken in one's objections and so need not be taken seriously. And nor, by the determined and unscrupulous, is one.

No doubt a carry-over from the preponderantly middle class origins of most academics, this Bowdlerization of the language of criticism is also a consequence of the manner in which academic work is organized. Since rewards and prestige at the individual and departmental levels are co-dependent, academics have a strong interest in the scholarly standing of their colleagues, in co-operating in a collective endeavour to enhance that standing and, conversely, in suppressing any reservations one might harbour as to the value of their work. The consequence, even in those schools which think of themselves as 'critical', is that little of the available critical intelligence is brought to bear on their own productions. For all of the current talk of reflexivity (Alvesson, Hardy and Harley, 2008), there is little of it in evidence.

The result of this dialectic of differentiation and conformity is a deformation and attenuation of the critical process which is particularly evident in the social sciences of management. Between the quasi-independent fiefdoms into which the field has fragmented, there is criticism a-plenty but little of it within them. Either way the matter of quality tends to be passed over. Within a field, there is every incentive to avoid the questioning of supposedly canonical works either on the point of their arguments or on their use of evidence (if any). Between academic regimes, meanwhile, criticism takes on the peculiarly abstract and ritualized form of anti-position statements. Rival approaches are denounced as incapable *in principle* of yielding knowledge which is meaningful, useful or valid. Arguments of this kind miss the issue of quality because they are pitched at a level which renders it irrelevant.

The contrast with the field of literary production is marked, and all the more telling because some variants of managerial social science are in the process of abandoning truth-claims and falling back on the defence that they *are* literary production. But they are not so in all (contextual) respects. A cursory glance at the literary reviews will confirm that the language therein is far more robust than that which is typical of critical exchanges in the social sciences and nor is criticism displaced from the particular work onto the genre of which it is representative. For all the supposed obsolescence of the ‘intentional fallacy’ (Wimsatt and Beardsley, 1954), writers are held personally to account for the quality of their work.

There is every reason why this should also be the situation within the social sciences. The quality of argument and evidential support is fundamental to academic production because what stands in the literature can legitimately be cited in the process of adding to it. That quality cannot be taken for granted unless it is permanently open to criticism. As the readings in this special issue amply demonstrate, the one-time processes of editorship and refereeing (including those presently in process) cannot be relied upon to guarantee even a basic competence in quite influential publications (McDonald and Kam, 2007). As well as the distorting pressures discussed earlier, referees are often called upon to pronounce on work which lies at the periphery of their competence and to do so in whatever time they can find between other deadlines. Against this justification for an expectation of competence it may be objected that it posits a cumulative social science which is not a realistic prospect; that such a science is a positivist fantasy and certainly not one which can be realized by legislating for a standardized methodology (Pfeffer, 1993; Van Maanen, 1995).

That, however, is to mistake the point. What is at issue here is not cumulation in a strict arithmetical sense but something closer to the idea of a body of knowledge or an integrity of a tradition: an expectation that what has the social standing of a contribution to the literature will not self-destruct when read with the attention which is the due of that standing. If there is an argument against a cumulative social science in this broader sense, it contradicts itself every time it cites an authority. Pfeffer’s mistake – and the ensuing controversy demonstrates that what *was* a mistake – was to try to impose standards by limiting argument. In a methodologically pluralistic field and in the absence of prior agreement on how to proceed, the only realistic alternative is to encourage as wide an argument as possible and to allow critical standards to emerge as community norms from that discussion.

Since established authority is at the core of the problem, the critical process should focus more particularly upon those published works which stand as icons of authority. And since the issue is quality (conceptions of which can also be expected to emerge as community norms), rather than the merits of rival paradigms, the dialogue needs to concern particular pieces of work, the obvious choice being those accepted as influential in their field. If discussion is to be genuine, the participants will be entitled to ask for justification of what is said of these works and that implies detailed reference to the text. The social conditions for such a dialogue will need to approximate to those of Habermas’ ideal speech situation. If the ‘better argument’ is to prevail, judgments will need to be sincere, unsparing and expressed with due force. On the other hand professorial power and the bluster with which it typically announces itself will need to

be recognized for what it is and discounted. It is not easy to get this right and there may be transgressions both in the critical pieces included in this special issue and in the replies to them.

For all this there is a model to be found in the history of literary criticism, albeit one which is flawed and which needs to be adapted with discrimination. It is that of I.A. Richards' Practical Criticism as fashioned by F.R. Leavis into the critical movement which came to be known as 'Cambridge English'. As a preliminary to drawing the implications for the social sciences of management the trajectory of this movement will be sketched out.

Leavisite Literary Criticism: A Brief Sketch

From the outset, Leavis was decisively influenced by a T.S. Eliot essay of 1919, *Tradition and the Individual Talent*. According to Eliot, the writer-artist stood in a dialectic relationship with the tradition of English letters. On the one hand the artist was the medium through which the tradition spoke to the contemporary world and on the other hand, the work was the means by which the tradition itself was extended so as to renew its relevance. For Leavis, this 'impersonal' process of creative renewal took place not in a separate realm of 'culture', understood in the popular and narrow sense, but at the heart of the very medium – that of language – through which the thought and feeling of an epoch might reach its fullest and most human expression. In the words of Paul Ricoeur, he believed that "self understanding passes through the detour of understanding the cultural signs in which the self documents and forms itself" (Ricoeur, 1981: 158).

Though anticipating the 'linguistic turn' in the social sciences in some respects, Leavis' conception of language was at once more specific, richer and more evaluative. For him the 'cultural signs' in question were those freighted by the canon of English literature – language as realised rather than language as a horizon of possibility: in Saussurian terms, parole rather than langue, and a rather specific parole at that. That English literature consisted of, or should consist of, texts which "propose a mode of being in the world" (Ricoeur, 1981: 192) implied that its study should not be thought of as a specialism on a level with other disciplines. Rather it should be a 'meeting-place' within which all disciplines might converse (Leavis, 1969). In the light of subsequent retreat of literary theory into a technical specialism, these claims for the centrality of the University English Department might sound extravagant, but they have a more recent counterpart in Gadamer's insistence that the province of hermeneutics is co-extensive with language itself (Gadamer, 1975: 397 ff.)

In this view of tradition the teacher-critic was not the slightly contemptible writer manqué depicted by Steiner (1967: 21)² but nothing less than the custodian of all that was vital in the culture itself. As Leavis put it in *Towards Standards in Criticism* (1976), "[l]iterary criticism provides the test for life and concreteness; where it

2 "When he looks back the critic sees a eunuch's shadow. Who would be a critic if he could be a writer?"

degenerates, the instruments of thought degenerate too, and thinking, released from the testing and energizing contact with the full living consciousness, is debilitated, and betrayed to the academic, the abstract and the verbal” (Quoted in Greenwood, 1978: 11). The critic’s task was vital and twofold. On the one hand, new works were to be ‘placed’ in relation to the tradition, assuming, that is, that they qualified as sufficiently significant expressions of it. On the other hand, there was the task of re-appraising the canon itself, of re-interpreting and re-positioning the works within it as they spoke to the contemporary world. Here again, the parallel with Gadamer’s hermeneutics is striking. All this was how things ought to be. How they actually were was another matter.

Both Eliot and Leavis held that there had occurred a corrupting discontinuity in the tradition of English letters. Whereas “[a] thought to Donne was an experience; it modified his sensibility” (Eliot, 1921: 287), Eliot believed that there had subsequently occurred a ‘dissociation of sensibility’, a kind of mental division of labour in which thought had become identified with instrumental rationality, whilst literature in general and poetry in particular had been relegated to an intellectually attenuated realm of the ‘purely poetic’. That, indeed, is a view which persists today. It is still widely held, especially by school teachers, that poetry is first and foremost a matter of ‘expressing one’s feelings’ and that its appreciation, accordingly, requires a suspension of the inquisitions of intellect. For Leavis, that was decadence, and it was a decadence which roughly correlated with, and related to, a break-up of the ‘organic’ society of Tudor England with its dense repertoire of common symbolic meanings and its robust physicality of language rooted in the experience of manual tasks.

This cohesive and coherent society, the literature of which spoke of ‘life’, had been replaced by a ‘mechanical’ civilization, one characterised not just by mechanized production, but also by a mechanistic (‘Benthamite’) view of human beings themselves.³ Latterly, according to Queenie Leavis’ doctoral dissertation, *Fiction and the Reading Public* (1932), there had also occurred the advent of ‘mass society’ – a large reading public cut off from the tradition of English literature by the seductions of commercialized publishing which prospered on the basis of a ‘levelled down’ pseudo-culture wherein clichéd writing catered for what Richards (1926) called the ‘stock response’. Any resuscitation of the real tradition of English literature, it followed, would need to engage on two fronts. On the one hand, there was the domination of high culture by the anti-life doctrine of the ‘pure aesthetic response’, epitomised and institutionally anchored for Leavis in the Bloomsbury Set (Virginia and Leonard Woolf, Lytton Strachey, Clive Bell, John Maynard Keynes, Bertrand Russell etc. etc.). On the other hand, there was the intellectual and emotional wasteland created by ‘Technologico-Benthamite Civilization’. Fulminations against both, but more particularly the second featured heavily in Leavis and Thompson’s *Culture and Environment* (1933), a workbook for the teaching of English in schools.

With hindsight, the dead-end contradictions at the heart of Leavis’ project of cultural regression were already discernible in this massively influential text. If the validity of a

3 Confusingly for the sociologist, Durkheim’s distinction between ‘organic’ and ‘mechanical’ solidarity is more-or-less reversed in Leavis.

literature depended on its interpenetration with 'life', what sense did it make to promulgate a literary canon which dismissed everything around it as anti-life? Relatedly, what could be the possible prognosis for a cultural enclave dedicated to the perpetuation of such a literature (c.f. Eagleton, 1983: 34)?⁴ Noticeably, the contemporary poetry advocated by Leavis himself – Eliot's *Waste Land* and Pound's *Hugh Selwyn Mauberley* – was that of terminal despair, a poetry whose organizing theme was its own impossibility. If these misgivings struck any of the Leavisite critics at the time, they certainly kept quiet about them. On the surface there was only a missionary zeal to awaken the nation's schoolchildren to the riches of the English literary tradition, to which end generations of them were taught that everything mechanical, popular or calculative was somehow spiritually impoverished. The result, for many, ironically enough, was to reproduce the facile gestures of disengagement which Leavis so disparaged in Edwardian romanticism.

Unlike most prophets of doom, however, Leavis was an activist and a fighter. The problem in asserting critical standards against the corruptions of the common culture lay in the fact that, for Leavis, critical standards only existed as an *expression* of a culture. He recognised there was nothing in a critical judgment, however justified by argument and reference to the text, which could command assent from a reader who simply refused the cultural presuppositions on which they were based. His was an essentially Wittgensteinian notion of literary criticism – one which saw critical practice as internal to a 'form of life'. In fact Leavis seems to have arrived independently at this view of the matter; though he and Wittgenstein were contemporaries at Cambridge, there was never much of an exchange of ideas between them.

Leavis' solution to the problem of the embedded nature of critical standards was, in effect, to *create* a culture, a social movement around the critical values through which continuities with the Pre-Lapsarian English tradition could be re-established. His method was to be teaching and his raw material, undergraduates, specifically the undergraduates of Cambridge University. Later in his career, Leavis became notorious for a narrow-minded authoritarianism which asserted that the English Novel died with D.H. Lawrence and Poetry with Eliot's *Four Quartets* (Shapiro, 1990).

He didn't start out like that; or at least he didn't represent himself that way. As an undergraduate he had attended I. A. Richards' classes in Practical Criticism. These seminars seem to have been Richards' response to the belief of Sir Arthur Quiller-Couch (he of the *Oxford Book of English Verse* and then head of English at Cambridge) that a course in English literature should equip the students to turn out a passable book review. Richards' approach was direct in the extreme. The students were to learn the craft of interpreting and appraising a text by doing it. They were asked to prepare critical appraisals of anonymised passages of poetry ('protocols') and these were then subjected to a searching examination by group discussion (Richards, 1929). The ground rules of these discussions laid down that all judgments were to be justified by reference

4 As Eagleton puts it, "[t]he whole Scrutiny project was at once hair-raisingly radical and really rather absurd". The absurdity, of course, was specific to the element of moral crusade in Leavis' project and there is no suggestion that the introduction of the techniques of practical criticism to the social sciences should carry any such baggage.

to the text itself, a procedure which disbarred interpretations based on extra-textual intuitions of the author's intent. For Richards himself this prohibition was an expression of his belief that the poem itself *was* a sufficient revelation of the writer's intent. For later critics, it was a critical practice which implied more-or-less the opposite: that the writer's intentions were *irrelevant* to the meaning of a poem and to believe otherwise was to commit the 'intentional fallacy' (Wimsatt and Beardsley, 1954). This view of the meaning of a text has its parallels in the hermeneutics of Gadamer and Ricoeur (Ricoeur, 1981: 149; Gadamer, 1975) and the semiotics of Barthes (1977).

Ground-breaking at the time, Richards' close-reading, argumentative approach also tended to undercut any pretensions to critical authority based on claims of a superior sensibility, the 'pure aesthetic response' which was so much the stock-in-trade of the Bloomsbury Group and their like. Against this, critical argument in such a setting virtually demanded the kind of fusion of thought and feeling which Leavis sought to reinstate at the centre of the English canon. In miniature, these sessions also offered a model of how it might be done. In effect, group discussion of the 'protocols' worked towards a consensus on critical standards, somewhat on the model which Habermas was later to theorise as the 'ideal speech situation'. In fairness it must be pointed out that Leavis never explicitly claimed (or admitted) that these practical criticism classes played a part in the formation of his conception of critical practice. Nevertheless the form of words through which he repeatedly explicated the argumentative character of critical judgments ('This is so, isn't it?', 'Yes, but', e.g. Leavis, 1974) had significant echoes of the exchanges in Richard's classes.

Creating a group of undergraduates which could serve as an ongoing medium of critical exchanges, meanwhile, presented problems for a man so adept at making enemies. Refused a teaching post at the University, Leavis was nevertheless adamant that it was Cambridge undergraduates he wanted to teach (recruit?). Living by now in the family home and having married one of his most talented students, Queenie Roth (herself a critic and considered by some at least the equal of Leavis himself), he managed to hang on as a part-time tutor at Downing College. On the basis of this slender connection, the Leavis' home became the regular meeting-place of a growing group of talented scholars convinced by Leavis' belief that there existed a widespread cultural malaise which could only be arrested by a resuscitation of the true tradition of English literature and that literary criticism was fundamental to this task.

In understanding the success of the social movement which Leavis set in motion, it is necessary to appreciate the role played by this hugely inflated view of literary criticism. Given Leavis' antagonistic stance towards the literary establishment of the day, the young scholars who formed the nucleus of the Scrutiny group were committing their futures to a cultural vanguard which had no existing base in the world of letters. This was psychologically possible because, like Mathew Arnold's 'guardians' (another profound influence on both Richards and Leavis), they saw themselves as nothing less than the embattled custodians of a civilization under siege. As such they positioned themselves in declared opposition to the corrupting influence of the mass media on the one hand and the 'flank-rubbing' critical standards of metropolitan London on the other. Such was the sense of mission which fired-up the contributors to Leavis' aptly-named critical magazine, *Scrutiny*. For some of Leavis' students, this sense of mission was

amplified by a sense that they were engaged in a struggle of cultural emancipation as well as preservation. In a memoir of Cambridge English in the 1960s, Roberts (1995: 265-266) speaks of the

impression [Leavis] gave to a student without a received background of literary culture, that English culture belonged to you. That was very fortifying when you were an obscure scholar from a lower middle class London suburb about to enter Cambridge. It meant that my ideal Cambridge centred on a man whose culture heroes were the tinker's son who wrote *The Pilgrim's Progress*, the clerk's son who wrote *Great Expectations*, the steward's daughter who wrote *Middlemarch* and above all the miner's son who wrote *The Rainbow*.

Propelled by these powerful motives, *Scrutiny* became the vehicle of Leavisite ideas on cultural decline as well as on what was of value in the English tradition. Taken as a whole, its critical essays aimed to re-shape the readers' notion of what that tradition was, reviving neglected writers of the past (notably John Donne, Gerard Manley Hopkins) and advocating new writing which exhibited the required combination of muscular language and intellectual engagement (above all, Eliot in poetry and Lawrence in the novel). On the offensive side, and possibly catering to the guilty pleasures of literary mayhem, *Scrutiny* became notorious for its scathing reviews of whatever was simultaneously fashionable and insufficiently attentive to 'life'. Of this genre Queenie Leavis' essay on Virginia Woolf, *Caterpillars of the Commonwealth Unite!* is an acerbic classic. Never selling more than 1,400 copies even at its peak, *Scrutiny* nevertheless became famous throughout the world as the house organ of 'Cambridge English'. As such it became something of an embarrassment to a University which never gave Leavis a full-time post until he was in his fifties and never gave him a chair at all.

At a time when most critical writing was the province of the gentleman of letters (Gross, 1991), *Scrutiny* could reasonably be regarded as the crucible of a new professionalism. To resurrect the somewhat outmoded trait theory for a moment, the Scrutineers certainly regarded literary criticism as a calling, and one of which it was not inappropriate to speak of 'qualification'. Certainly Leavis himself did so, more particularly when accusing his antagonists of lacking it. The young critics of *Scrutiny* were talented; their stock-in-trade was relentless argument coupled with concrete demonstration and, above all, they believed absolutely in what they were doing. This being the case, it was not surprising that, as the years went by, more and more of them attained positions of influence in English departments throughout the world, a Diaspora only hastened by the certainty that they were not going to be offered posts at Cambridge itself. If we fast-forward to 1962 an *Observer* article could speak of the "hidden network of the Leavisites" (Samson, 1992: 2), as if some sort of conspiracy was involved. And despite the disclaimers, there probably was.

The early 1960s, in fact, corresponded roughly to the apogee of the Leavisite dominance. After that time it became increasingly difficult to maintain its founding ideal of English literature as the product and expression of a single unitary culture. Besides the increasing globalization of English writing, or rather an increasing consciousness of a globalization which had already occurred, there were the cultural consequences of large-scale immigration into the UK itself. Bauman's transition from

legislators to interpreters (1987) came late to the study of English literature, but it came nevertheless.

The end of the story is one of decline, death and transfiguration. The qualities of combative obstinacy which had enabled Leavis to persist in his course throughout the vicissitudes of his middle years, soured into a narrow-minded authoritarianism which eventually found fault with both his critical allies and the writers he had sponsored earlier in his career. He died isolated and embittered, refusing even a late reconciliation with Richards, with whom he had broken many years earlier. At the same time the element of cultural critique in Leavisite criticism began to take on a life of its own in the hands of writers like Richard Hoggart and Raymond Williams – the so-called ‘Left Leavisites’. Unconvinced and repelled by the atomised and passive characterisation of the working class implicit in Leavis’ own concept of mass civilization, they began to interrogate the notion that

beneath all history, silently spanning past, present and future, runs a unifying essence called ‘tradition’. (Eagleton, 1983: 72, writing of Gadamer)

Possibly they were encouraged in this heresy by the performative contradiction involved in Leavis’ own earlier challenge to ‘the’ culture as he encountered it. From the beginnings established by the Left Leavisites, the story then moves to the Centre of Cultural Studies at Birmingham University (Easthope, 1991) and out of the remit of this essay.

The Prospects for Practical Criticism in the Social Sciences

As will by now be apparent, Leavisite literary criticism was an uneasy combination of a democratising practice in the service of an authoritatively-defined programme of cultural regression. These elements need to be disentangled if it is to serve as a model for disrupting authority in the social sciences.

On the democratising side, the ground rule of practical criticism – that critical judgments must be justified by reference to the text – entailed a denial that great literature constituted an ineffable mystery, only to be approached in a spirit of reverential awe and preferably under the tutelage of an aristocratic priesthood. Against this, practical criticism insisted that the greatness of literature, if any, consisted precisely in its workings and the detailed and even forensic analysis of these were integral to its full appreciation. In a similar spirit, Loic Wacquant has insisted that a detailed and comprehensive reading of Pierre Bourdieu is essential if the synergistic force of his conceptual apparatus is to be realised (Bourdieu and Wacquant, 1992: 3 ff.). Those concepts, on the other hand, are not to be applied ritualistically; they are invitations to think about the issues to which they are applied, not formulae which substitute for thought (Wacquant, 2008).

If practical criticism was the means of apprehending greatness in literature, it was also a means of unmasking its counterfeit twin; that meretricious afflatus which announces nothing but its own genius, but which, because of that very fixation, can do so only in derivation and cliché. That this has its parallels in recent writings in the social sciences

of management will not, we think, be disputed, though there may be disagreement on where they are to be found.

Closely related to the analytic stance of practical criticism was the anti-authoritarian practice built into Richards' classes: that all were entitled to make their interpretations and assessments and to lodge them in the common debate. It was the translation of this pedagogic practice into the arena of public criticism which provided Leavis with his answers to two key problems: that of the source of standards in criticism and the creation of a cultural enclave within which the tradition of English literature could be perpetuated. Critical standards were to emerge through the communal discussion of particular critical judgments and the community would cohere around a literary tradition ordered according to these standards. In its usurpatory aspect, this was a direct challenge to the idea that the formation of literary taste should be the province of the aesthetic wing of a leisured *rentiér* class. And it was from this appeal to those who felt themselves to be culturally excluded that the *Scrutiny* movement drew much of its energy.

Some of these ideas have their parallels in social science, though not, as yet, in those relating to management. Gouldner (1975: 78-80) has proposed a model of theory-making as reflexive debate and even of sociology as a Leavis-like 'meeting-place of the disciplines' rather than one specialism amongst others (Leavis, 1969). These proposals, however, have received little subsequent attention, probably because of the threat which they represent to sociological professionalism. What has clearly not happened is the emergence of common critical standards through the exchange of judgments on particular pieces of work, and it is worthwhile speculating for a moment on why this is so. There are certainly critical symposia on the work of particular sociologists but it could be that the debates within them simply stop short of the point at which differences in critical standards might surface. Since disagreement of this character implies different standards of competence: it could be that professorial power, in combination with the academic gentility principle (Alvarez, 1962),⁵ operates to exclude any suggestion that this might be an issue.

The notion that all critical appraisals of particular works are entitled to a hearing might be expected to have as much democratising appeal in the social sciences as they had in English literature. One has only to observe the silent majority at our massed conferences, as they are first softened up by the plenary speakers and then finished off as functioning intelligences by those of the great and good who feel it incumbent on themselves to make their contributions, to wonder how far the act of saying something truly reflects the condition of having something to say. One is struck too by the analogy between the oligopolization of knowledge-making in the social sciences and that of the formation of literary taste. The sceptical will object that this concentration of the act of creation merely reflects the distribution of ability, but the even more sceptical will look at the products of that ability and table a note of reservation.

5 'Beyond the Gentility Principle' was the title of Alvarez's introduction to his edited collection *The New Poetry*, in which he savaged the emotional reticence of the poets of the 'The Movement' (most prominent Philip Larkin), and sought to promote a poetry of extremity and crisis, notably the work of Sylvia Plath.

All that said, the trajectory of Leavisite criticism also reads as a cautionary tale. From the beginning its democratizing moments existed in tension with, and in the service of, a dogmatic view of literary tradition. What was not subject to critical examination within Cambridge English, and never could be, was its articles of faith, the belief that the true tradition of English literature was defined by a commitment to 'life', in opposition to the barbarism of 'Technologico-Benthamite Civilization'. Eventually, as related, the element of revealed truth prevailed and textual criticism was reduced to a matter of finding 'life' where one was supposed to find it and excoriating its absence where one was not (Wain, 1962: 176).

The condition of avoiding this slide into dogmatism with practical criticism in the social sciences is that it cannot be tied to one particular view of what constitutes good research or theory, and this requires that ritualized denunciations of entire genres of social research – such as 'positivist' on the one hand or 'impressionistic' on the other – are either disbarred or disregarded as meaningless expletives. If it is to generate standards in terms of which a conversation can be carried on within a presently-fragmented discipline, the sole organizing principle of practical criticism in the social sciences will have to be the interrogation of academic authority. Standards in themselves do not imply authority any more than do norms, in which connection it needs to be remembered that the alternative to some intersubjectively agreed means by which the Habermasian 'better argument' can be recognised is not individual freedom of expression, but a regime in which there are other means by which particular approaches, orthodoxies and points of view will come to prevail. Some of these have been discussed in the first part of this introduction.

Practical criticism has implications for the manner in which papers in the social sciences are read as well as produced. Recall that Richards' classes were originally intended to develop the craft skills of the literary critic: the production of interpretations and evaluations which could be asserted and defended with reference to the text itself. How many of us read the texts of social science with a similar degree of attention? And without that, how can we claim that ours is a field of knowledge in which assertion is tempered by criticism? What came to light in the course of Richards' classes was how necessary they were. It turned out that the highly educated students of Cambridge University were often incapable even of correctly reading the words on the page, especially when those closely resembled a clichéd alternative (another manifestation of Richards' 'stock response').⁶ The same is almost certainly true of the typical manner in which the texts of social science are read.

Relevant here is a study of citations of a well-known paper in the field of organizational studies (Granovetter, 1973) which the present writers carried out in collaboration with Steve Conway. Of 63 citations we examined in detail, 45 were substantial in the sense of adding something to the argument of the citing paper. Of this 45, 11 were mis-readings by any standard, 16 were flawed in the sense of inappropriate application to the case in hand, leaving only 18 which were simultaneously substantial, applicable and

6 An example was the students' reading of D H Lawrence's poem *Piano*. In place of Lawrence's evocatively tactile 'boom of the tingling strings', virtually all of the students read the nonsensical 'boom of the tinkling strings'.

defensible as readings of Granovetter's text (Armstrong, Conway and Lilley, 2004). Assuming that this does not simply reflect a widespread failure to read the cited texts at all, it points towards an inability to produce a reasonable interpretation of them on a massive scale. And if that is true of citations, to what extent is it also true of interview and ethnographic data, to say nothing of our interpretations of the less pellucid of current theorists? Critical reading is fundamental to any science, however 'science' is conceived, and practical criticism, at bottom, is nothing more than that.

The First Conference of Practical Criticism in the Social Sciences of Management

As a pilot-test of the ideas outlined above, the University of Leicester School of Management hosted a small conference in January 2008. Concurring with Alvesson, Hardy and Harley's (2008) contention that 'destabilizing practices' are a desirable feature of debate within management studies, it was felt that the immediate priority was to begin the work of disrupting the established pattern of authority within the field. Accordingly, contributors were invited to provide a close-reading critique of a prominent paper which they considered to exert an undesirable influence. One contribution, however (that of Monin and Bathurst, included in this volume) chose to practice close reading in its positive aspect: that of providing a detailed case for the reinstatement of a writer who they believe to be unjustly neglected.⁷

In literary criticism there is no general presumption of a right to reply, though editors may find space for disgruntled authors on a case-by-case basis. The counterpart of this norm in academic life is the lack of a right to appeal referee's decisions, or rather, the general futility of doing so. Otherwise academics tend to treat criticism of their work as an outrage which ought never to have been permitted. In deference to this latter feeling, though not the former, the authors whose work is criticised in this volume were invited to provide a reply of up to 2,000 words. The authors of two of the papers subject to practical criticism declined, on the grounds either that they had moved on from a position that made (more) sense at the time it was first ventured or that the critique did not seem to be of a nature to invite further engagement. The three substantive replies that were emerged are reproduced here as they were received and in full, despite one author feeling that 6,000 words were required needed to express himself adequately.

Though they differ considerably in their own tones, all three of the replies register an objection to the tone of the comments on their work and two of them additionally suggested that their publication in *ephemera* would damage the journal's reputation. In fact the language used in the critical readings we publish here is anodyne indeed compared to that which is commonplace in literary reviewing, and the protests may tell

⁷ Sixteen papers were presented at the conference, six of which are included in this special issue. Further details of this conference and five of the papers are available at http://www.le.ac.uk/ulmc/research/conf_jan08/index.html. The papers are also in the Leicester Research Archive at <https://lra.le.ac.uk/handle/2381/3591>. Roughly coinciding with the publication of this special issue, a second conference is being held on January 8th-9th 2009, also hosted by the University of Leicester School of Management. Details of this event can be found at http://www.le.ac.uk/ulmc/research/conf_jan09/index.html.

us something about the qualitative tendency in the European Sociology of Management. As mentioned earlier, this has seen a long-run and general retreat from overt truth-claims towards a position which holds that ethnography and its associated theorising are to be understood only as forms of 'writing'.

If the objections registered here are typical, however, they suggest that social scientists still expect more in the way of *ab initio* respect for their writings than do those who work in other genres. What, then, is the basis of this expectation? Is it that the fictions of theory and ethnography are a bit *more* true than out-and-proud imaginative writing and so should be taken a bit *more* seriously, whilst, at the same time, they are a bit *less* true than would require them to be accountable for the statements they make? Or, to put the matter in terms of occupational posture, is it that social scientists want to be writers of fiction when asked to justify what they say, but want to claim the respect due to serious attempts to understand the world when it comes to debate? Put either way, it sounds a bit like having one's cake and eating it.

Lightfoot's discussion of Ritzer's *The Globalization of Nothing* (2004) is animated by a sense that the book itself is an example of the worldwide marketing of standardised nullity. To Lightfoot, Ritzer's two-by-two matrix (which is reproduced in his critique) is itself 'nothing': a derivative and hopelessly flabby expression of Ritzer's personal (and somewhat snobbish) tastes tricked out as an academic thesis by dubious appeals to a selection of heavyweight thinkers. In his reply Ritzer alleges that Lightfoot has failed to understand his central concept. If 'nothing' is defined as "a social form that is centrally conceived, centrally controlled and lacking in distinctive content", it follows for Ritzer that his own work cannot be nothing because it is "indigenously (in this case, personally) conceived, controlled and rich in the distinctive content that informs all of my work". On this readers will have to make up their own minds. Whilst they are doing so, they might also like to ponder Ritzer's simultaneous denial that his concepts have any evaluative content alongside his claim that his literature review "builds on the shoulders of giants".

Both Maltby's critique of Power's *The Audit Society* (1994) and Power's response raise some interesting issues of rhetorical method. Maltby's fundamental objection is to the dependence of the audit society thesis on a terminology which represents a range of monitoring and reporting practices as variants of a unitary 'audit' and on a foreshortened and simplified history which depicts this generic 'audit' as diffused outwards from a private sector model. Almost as an afterthought – but it is an important one – she detects a partiality in Power's overwhelmingly negative portrayal of the consequences of audit for the auditee – an over-sensitivity to the point of view of middle-class professionals and a corresponding neglect of the public interest which they nominally serve.

Power's response on the methodological issue appeals to a model of the interdependence of practice and language which applies not only to the various forms of stocktaking, appraisal etc. which are now, as he rightly says, routinely described as 'audit', but also to the construction of his own thesis. In this latter sense, he might be said to agree with Maltby, though without the negative connotations. Would it then follow that the defining features of the 'audit society' could, with equal legitimacy, be

disaggregated in Maltby's preferred language of difference and complexity? If it is to be more than the expression of a tendency to similarity where others might see difference, then, the 'audit society' thesis depends crucially on the supposition that practices substantively change – and presumably converge – once they are commonly described as audit. On this point there seems to be little evidence either way, but if it is so, it could be that the wide currency of Power's thesis is on the way to constructing the reality which it purports to describe. On the point of public interest, Maltby is surely correct in pointing out that Power has more to say about the woes of those subject to audit than the perils of trusting those who are not, but that may be a consequence of the manner in which he has defined his subject matter.

Jackson and Carter's examination of McKelvey's 'From field to science: can Organization Studies make the transition?' (2003) is wide-ranging but their primary concerns are with his claim that the notion of paradigm incommensurability is self-refuting and with his suggestion that organization studies can and should be reconstructed as a unified field on the basis of a 'contra-science' ontology and 'normal science' epistemology. On the alleged self-negation of paradigm incommensurability, McKelvey argues that if we know enough about another paradigm to state that it is incommensurate with our own, that knowledge is sufficient to make it commensurate. Jackson and Carter point out that what is established by this argument is not the self-refuting nature of paradigm incommensurability but that of McKelvey's own argument. It is self-refuting, in other words, to suppose that the establishment of the incommensurability of another paradigm requires the extent and kind of knowledge which would establish its commensurability. The sentence in which it is expressed, they say, is syntactically correct but semantically false. McKelvey's response is to repeat his original argument and assert with some force that a sentence which contains meaningful words cannot be semantically false – from which it presumably follows that colourless green ideas do indeed sleep uneasily.

Quite what is meant by the opposition between 'normal science' and 'contra-science' in McKelvey's two-by-two matrix is not very clear, nor is the manner in which he understands 'science'. His biographical note, however, suggests that his models are physics and biology, but that still leaves much for conjecture. Is 'normal science' to be understood in the Kuhnian sense in which it is differentiated from 'revolutionary science', and if so, why is it only normal science that is opposed to 'contra-science'? What, come to that, *is* 'contra-science'? Given these uncertainties, Jackson and Carter do their best to make sense of McKelvey's proposed reconstruction of organization studies, interpreting 'contra science' ontology as an articulation in which the world is "socially constructed, observer-dependent and of dubious transcendence". On that basis, they ask how it is possible to produce a science-like knowledge of a world which has no objective existence. McKelvey's reply does not much clarify the issue, since his chosen illustration – 'Campbellian Realism' – appears to combine a realist ontology and relativist epistemology rather than the contra science ontology and normal science epistemology which he advocates. In fact 'Campbellian Realism' as McKelvey describes it, lies in a quadrant of his matrix which he says is inhabited by no-one but the odd 'nut'.

Hardt and Negri's *Empire* (2000) has been the subject of a great deal of debate, but Toms concentrates his attention on the concept of 'immaterial labour'. To Hardt and Negri, the essential characteristic of immaterial labour is its immeasurability. They contend that this arises because labour is now diffused throughout the entirety of life itself and also throughout the interlocking networks which constitute the 'social whole'. If labour really is becoming immeasurable in this sense, Toms points out, it follows there can be no analysis of the dynamics of capital accumulation because the key concept of socially necessary labour time loses its meaning – and Negri has indeed argued that the Marxian theory of value needs to be abandoned. This leaves Hardt and Negri with no theory of the origin of surplus value and hence no concept of exploitation beyond the blanket assertion that capital now subsumes the 'social whole'.

Hardt and Negri thus assimilate the brute inequalities of a globalized system of production and exchange into a single category of labour, which appears to be modelled on the creative and affective occupations in the advanced capitalist economies. So it is that third world debt-bondage farming, the sweatshops of South East Asia and the administration of corporate capitalism are all hoovered up into an indiscriminate celebration of 'multitude' which ignores all differentiations of function and all conflicts of interest within. Instead of recognising explicitly that some of this 'immaterial labour' consists of the very processes through which capital perpetuates its domination, for Hardt and Negri the latter is delivered by a transhuman agency called 'biopower' which is supposed to pervade human existence to the point where it coincides with subjectivity itself. It is on this basis that Hardt and Negri believe that the materials of autonomy are already to hand. Meanwhile, as Toms point out, the labour markets of global capitalism, internal and external, routinely accomplish what Hardt and Negri declare to be impossible: the measurement of immaterial labour.

In the midst of so much ill humour, it a relief to recall that there was a constructive aspect to Leavis' project: that of rehabilitating reputations which had been crowded out by more strident voices. Perhaps as a corrective to the testosterone-impregnated tendency of much writing on managerial leadership, Monin and Bathurst argue for a resuscitation of the work of May Parker Follett. An attentive reading of *The Essentials of Leadership* (1987), they argue, will challenge the dominant tendency to view the 'god-word' leadership in terms of will, aggression and assertion. Leadership to Follett begins with a quasi-hermeneutic openness to the group situation, an exercise in which Monin and Bathurst finds a parallel to Keats' 'negative capability'. To Follett, the challenge of leadership is not one of problem-solving but that of following the situation, of making sense of it in ways which will reveal the group's common purpose. Situational leadership of this kind is inherently anti-formulaic; it is inherently dynamic, transformative, and as much a matter of tone as substance. In this last connection, Monin and Bathurst suggest that Follett offers as much inspiration through the manner of her writing as its substance. Relying as she does on anecdote and metaphor, she does not force her interpretations on the reader, but invites a response in a quiet, conversational manner, which contrasts markedly with the sledgehammer use of metaphor in most managerial writings.

Whatever the merits of Monin and Bathurst's proposed return to Follett – and for us the notion that there is always a single group purpose to be discovered looks like a benign

variant of unitarism – their contribution raises a more general point about the role of tradition in social scientific understandings. As was argued for English literature by T.S. Eliot (1919) and philosophy by Hans Georg Gadamer (1975), the process of theory-making in the social sciences is informed by a “collective scientific unconscious” (Bourdieu and Wacquant, 1992: 40) – including an inherited language – which is the product of a history. Unlike the case of the natural sciences and that of postmodern social science in its messianic youth, this history is not relegated to the archive with each new advance, or change in fashion. Rather it is a living subculture, the relevance of which changes according to the circumstances in which it is drawn upon. This being the case, it needs to be revisited and reinterpreted from time-to-time. Leavis’ attempt to do this for English poetry was called *Revaluation* (1936) and that is what Monin and Bathurst propose in the case of May Parker Follett.

The target of Armstrong’s essay is Knights and Morgan’s Foucaultian take on strategic management, in which the latter is deemed to originate in the post-war business schools of the USA and in response to a communications gap between corporate management and other stakeholders. Knights and Morgan also contend that the main *raison d’être* and consequence of strategic management is its ‘power effects’, most of which have to do with pumping up the gendered egos (‘subjectivities’) of senior management. Armstrong finds nothing in the way of evidence to substantiate any of these contentions, and much to contradict them. An earlier version of the paper was rejected by a member of the editorial board of *Organization Studies*, Cynthia Hardy, as a ‘vicious personal attack’, and so, by the flank-rubbing standards of the dominant cabal of Critical Management Studies, it is. What seems to have jerked Armstrong’s chain is not so much his perception of Knights and Morgan’s intellectual pretension and ignorance of their subject matter but the fact that their paper is now taken at their own valuation in its chosen constituency: as seminal even. In this outsider view of the grooming behaviour of an academic huddle, there may well be a whiff of sour grapes.

Armstrong goes on to examine some more recent contributions to the literature of strategic management which have positioned themselves as validations and extensions of Knights and Morgan’s work. He finds that this has been achieved by authorial interpretations which translate field data into confirmation of Knights and Morgan’s speculations more-or-less irrespective of their actual substance. This is an allegation which raises some interesting questions of method on which Armstrong fails to elaborate. Does he perhaps think that there are ‘natural’ interpretations of field data or that there is some test by which the validity of interpretation can be determined? If so, he does not tell us what they are or why, correlatively, researchers should not be free to shape their field notes into any pattern they wish. All-in-all, this is a sour and irascible performance to which Knights and Morgan have declined to respond on the grounds that they have moved on to other things. And so they have. And so shall we.

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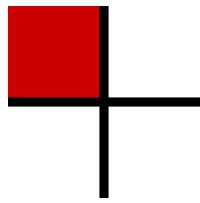
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Nothing Beats a 2x2 Matrix: a Short Commentary on George Ritzer's *Globalization of Nothing*

Geoff Lightfoot

abstract

This paper examines George Ritzer's *Globalization of Nothing* thesis and, in particular, his deployment of a 2x2 matrix – a device of cunning simplicity which is at its most effective when one corner is clearly 'good' and the opposite 'bad'. Armed with such a tool the world can be neatly packaged and our strategic direction decided. But beyond the mere banalities of management, a more sophisticated matrix might be necessary – one that captures the fluidity and ambiguity of the social world. And, to use such a device as part of the construction of a 'grand narrative', we shall need to carefully consider the axes by which we can cleave the world. We may require new terms that capture what has hitherto been missed, as well as audacious re-interpretation and re-examination of existing concepts (while ensuring that we continue to pay attention to other theoretical work within that canon). Not only that, to avoid (postmodern and feminist) accusations of judgemental elitism, it may be necessary to derive new objective criteria through which we can safely allocate and partition social artefacts. And through examination of these discursive moves, we may be able to better understand how knowledge is disseminated in "the critical study of contemporary social phenomena".¹

Introduction

Near the start of his keynote address at 2007's Critical Management Studies conference, Professor Ritzer boldly declared that he was critical of nothing. And, as his talk went on, so it seemed. Worse, perhaps, malcontents in the audience suggested that the organisers of the conference and the delegates may have received nothing new for their money,² for this was seemingly the same presentation (talk, slides and even the jokes) delivered in Australia two years earlier, itself derived from a paper – *Rethinking Globalization: Glocalization/Grobalization and Something/Nothing* – published two years before that.

In that paper, and in the presentation, Ritzer introduced a concept 'grobalization' that he positioned on the opposite end of a continuum to 'glocalization' and juxtaposed this with a second continuum, of 'something/nothing' to generate four quadrants. For Ritzer,

¹ Comment on cover of *The Globalization of Nothing*.

² £5,000 plus expenses, allegedly.

“[o]f greatest importance are the grobalization of nothing and the glocalization of something, as well as the conflict between them. The grobalization of nothing threatens to overwhelm the latter and everything else” (p. 193). But, within the globalization of nothing, Ritzer goes on to claim that, “... nothing can be marketed globally far more aggressively than something... [and that nothing]... generally can be easily and efficiently packaged and moved, often over vast areas” (p. 200).

Had I missed the joke? Was his show at CMS5 actually decorous: an elaborate performance critiquing academia and self? Whether by memory-stick, CD or just t’Net, PowerPoint slides slip easily from place to place. And a quick glance at Ritzer’s C.V. shows that the reach of the McDonaldization thesis approaches that of the eponymous company. Was Ritzer somehow arguing that his work and academic endeavour in general, was nothing? Or that no work was better than nothing? Perhaps a closer look at his ideas might reveal more and the obvious place to start is with the extended work *The Globalization of Nothing*.³

Nothing Comes Before the Introduction

Not long ago I was contacted by a producer for a syndicated TV show starring former *Today* host, Jane Pauley. In effect, the producer conducted an audition with me on the phone to see whether I could communicate my ideas to a lay audience. Things went well in a discussion of McDonaldization, but then she asked about a more recent work, *The Globalization of Nothing* (Ritzer, 2004). I demurred for a moment, knowing from experience that this is a more difficult set of ideas to communicate simply, but I plunged ahead. As I did, I could almost feel the producer lose interest. When I finished, she in effect said, “Don’t call us, we’ll call you”, and I said, “Well. I guess my ideas on ‘nothing’ and its globalization were not ‘McDonaldized enough’”. She laughed and said, “That’s right!” (Ritzer, 2006: 212)

We can perhaps see some of the steps that Ritzer makes in his own journey along these lines. For in his admirable extension of the McDonaldization thesis, he makes it clear that there is more than just *one* thing wrong with McDonalds. It’s not just that the food is a bit crap, tasteless and standardised, nor is it just that it is a monolithic capitalist enterprise that operates across the world. No, elements of both are the problem. And, if we look at some of Ritzer’s *bêtes noires*, we find a substantial list amenable to such dissection – Coca-Cola; Lunchables; Domino’s Pizza; Johnny Rockets; KFC; Pizza Hut; Taco Bell; and Starbucks – are all derided. But, as Ritzer argues, ‘It’s not just about the food’⁴ and the breadth of his analysis goes beyond mere food and drink to encompass GAP clothing, Gucci, IKEA, Microsoft, and Disney World as well as more generic targets such as cruise ships and Internet shopping.

But it is essential to find the right axes through which to cleave the world. Some obviously work better than others – the key often seems to be to find words that immediately capture the necessary distinction: Ritzer has chosen something-nothing and Glocal-Global for his. But this is not an uncontroversial choice and there are dark forces that seek to prevent Ritzer from speaking out in this way.

3 Unless otherwise annotated all references are to *Globalization of Nothing*.

4 Comment at CMS5.

Nothing Is Worse than Post-modernism

... our objective is to rescue our ability to use ideas like something and nothing, just as other scholars have sought to find ways of salvaging concepts like humanism, liberalism, justice and rationality in a postmodern age. (p. 207.)

To be sure, concepts like humanism, liberalism, justice and rationality are important, and it would be nice to think that 'ideas like something and nothing' are as vital, and it could be worrying if they are equally endangered by this postmodern age. But Ritzer's task is not easy, for his argument "involves what the postmodernists call a 'grand narrative', a story of a huge swath of human history" (p.4). For Ritzer, "[t]he idea of a grand narrative has been greatly criticized by postmodernists" (p. 217). Fortunately, however, "modernists continue to develop such theories of general historical developments" (p. 217) and Ritzer is particularly wedded to the idea that his work is a grand narrative⁵ – it crops up in the sales blurb even – rather than, say, Lyotard's synonym of meta-narrative. This may merely be that a grand narrative sounds, well, *grand* or it may be a more considered dig. And Ritzer certainly seems at least aware that there may be more to it than just a big story:

Also in the realm of the postmodern (and feminist) critique is the seeming "god's eye" perspective that pervades this discussion. It seems as if I, as the author, am able to make distinctions (as well as judgments) that most, if not all, others are unable to make. What gives me the right, or the ability, to make a set of distinctions that no one else seems capable of making? There is clearly an elitism associated with this self-aggrandizement, but there is an even more profound form of elitism associated with all of this. That is, since most people seem to prefer the nothing end of each of the continua discussed here (such as the Paris casino-hotel [in Las Vegas] to Paris, France), there is an implied criticism of those choices and a clear preference for the something end of the continuum. It seems as if I know more than most people and that I am capable of making judgments – especially that they are increasingly choosing nothing over something and that that is a problematic choice – that most people are unable to make. Thus, this analysis would seem to be subject to many of the same criticisms as those leveled at the Frankfurt School of social theory. (p. 204)

However, Ritzer is alert to part of this problem:

[There are] at least two major differences between the approach taken here and that of the Frankfurt School. First, there is a set of objective criteria developed in Chapter 2 (as questionable as such a modern undertaking might be). Second, [...] the bulk of [this analysis] is devoted to outlining what is meant by nothing and then describing the trend toward, and ultimately the globalization of, nothing. (p. 204)

Objective criteria are always useful in the social sciences and even the Frankfurt School were alive to the possibility (see, for example, as one reviewer kindly pointed out, Fromm's *The Sane Society*). However, Ritzer's criteria are particularly illuminating. They are: Unique – Generic or One-of-a-kind – Interchangeable; Local Geographic Ties

5 Specifically, "a story of a large swath of human history, asserting that we are witnessing a general historical trend in the direction of more and more nothing. That is, there is historical movement towards that which is centrally controlled and conceived and increasingly devoid of substantive substance (as well as, as we will see, of uniqueness and individuality, among other things)... While it would be tempting to over generalize, this grand narrative (as well as the rest of this book's main arguments) is best restricted to the realm of consumption" (pp. 4-5).

– Lack of Local Ties; Specific to the Times – Relatively Time-less; Humanized – Dehumanized; Enchanted – Disenchanted. The exemplars for each are intriguing: gourmet meal v microwave meal⁶; handmade pottery from a small town in Mexico⁷ v pottery mass-manufactured for a world market; VW Beetle and 1969 Pontiac Firebird⁸,⁹ v Kia and Dodge Neon; small teaching college v Internet university¹⁰; and gourmet meals (again) v Domino’s Pizza and Lunchables¹¹.

Ritzer is aware that this might be critiqued on the grounds of intellectual snobbery and he is keen to point out that he is proud to be an intellectual. On the snobbery, he is less forthcoming. Of those distinctions, more later. But to return to the grand narratives – since Ritzer has written on post-modernism and the only source he refers to on grand narratives is Lyotard, we might assume that he is alive to some of the issues that Lyotard raises. It would be unfair to merely suggest that Ritzer’s “grand narrative has lost its credibility” (Lyotard, 1984: 37) or that Ritzer’s work is part of the

6 This is not any microwave meal, this is Marks and Spencer’s... .

7 That still seems to be exported – so it’s not just about the market. But it’s not about being handmade, presumably, because that would replicate the previous distinction. So is it something to do with the small town in Mexico? That obviously differs from a pottery town such as, say, Stoke-on-Trent in all sorts of ways – but are the local ties of Stoke somehow different? A difficult question.

8 This is a truly intriguing set of examples to take that demands greater engagement from the reader. The Beetle debuted in 1938 and ended production in 2003 [http://en.wikipedia.org/wiki/VW_Beetle - consulted 11 January 2008] and was “the longest-running and most-produced automobile of a single design” (*ibid.*). Although the Beetle has its origins pre-war it is only in the 1960s on the back of a sustained advertising campaign (and a Disney movie) that it became iconic in America. Can advertising make something-of-the-times? Is it the very ‘nothingness’ that it would be seen as by other of these ‘objective’ criteria that gives the scope for a variety of other meanings to be leant to it – not least the engineered nostalgia underpinning the marketing of the New Beetle?

The 1969 Pontiac Firebird, rather than the 1967 (when the model was introduced) is also an inspiring choice. The changes for the 1969 model were: “a major facelift with a new front end design made of an Endura bumper housing the headlights and grilles. Inside, there was a revised instrument panel and steering wheel. Also, the ignition switch was moved from the dashboard to the steering column with the introduction of GM’s new locking ignition switch/steering wheel” [http://en.wikipedia.org/wiki/Pontiac_Firebird - consulted 11 January 2008].

Does this icon-of-the-times also include the unloved 6 cylinder base models as well as the V8 440? Why has Ritzer not chosen the Chevrolet, whose Camaro was virtually identical – but for whom the platform was created. And especially why not the Ford Mustang, from whom the whole concept was blatantly copied and which is more commonly seen as the iconic Pony Car? (Perhaps Ritzer is alluding to something else when he describes them as ‘muscle cars’.) We shall return to that later. Why is an entire company (Kia) that has half a dozen different models in its line-up chosen as comparator rather than a single model – would we find anything distinctive if we took Pontiac (even 1969 Pontiac) as a whole, as the exemplar of ‘specific-to-the-times’? Professor Ritzer asks some difficult questions of us.

9 Oh, and Herbie, the VW beetle from the Disney movie crops up reassuringly often (even in Disney parks). Maybe it’s not that hard to stick a number 53 and a couple of racing stripes on a Beetle (even a new one).

10 Perhaps a consideration of Foucault’s emphasis on the importance of the examination might be being lost here. The standardised processes of evaluation that unite both private teaching colleges and Internet universities may perhaps be an example of ‘dehumanization’ but the substantive difference between the two examples can largely be attributed to wealth and privilege – a slightly awkward position given Ritzer’s general praise for concepts such as ‘justice’ and ‘equality’ (pp. 205-7).

11 We return to the issue of ‘simulated’ and ‘genuine’ enchantment later.

‘unacceptable’ (*ibid.*: p.14) attempt to partition off ‘critical’ knowledge. And surely Ritzer wouldn’t be demanding that his work be seen as terrorist, silencing dissent (*ibid.*: 63-64)? So, it seems that Ritzer must be being a bit playful here and really pointing to something else in his Lyotard reference – the alternatives are otherwise just too unpalatable.

Nothing Ventured, Nothing Gained

It is not hard to find possibilities as to what Ritzer was perhaps alluding to:

[T]o speak is to fight, in the sense of playing, and speech acts fall within the domain of a general agonistics. This does not necessarily mean that one plays in order to win. A move can be made for the sheer pleasure of its invention: what else is involved in that labor of language harassment undertaken by popular speech and by literature? Great joy is had in the endless invention of turns of phrase, of words and meanings, the process behind the evolution of language on the level of *parole*. But undoubtedly even this pleasure depends on a feeling of success won at the expense of an adversary—at least one adversary, and a formidable one: the accepted language, or connotation. (Lyotard, 1984: 10)

‘Nothing’ might be found here. For when we look at Ritzer’s definition, we see that he “refers to a *social form that is generally centrally conceived, controlled and comparatively devoid of distinctive substantive content*” (p. 3, original emphasis). Intriguing – for Ritzer nothing does have *form* and *content*,¹² is conceivable and controllable. Perhaps manageable even. A marked move from most other definitions of nothing. Where can we go with this – perhaps the relatively rare discipline of nothingology might enjoy a welcome revival? Nothingarians might finally have something to believe in. No longer might we suffer absence as there is always something there. Perhaps we could even see the end of nihilism¹³ and following that, the end of all negativity and destruction? Maybe I should stop here then?

That may be a little too much to hope for. But such language games do have some benefits: Ritzer is able to cite several fine names who have written about nothing. Parmenides, Zeno, St Augustine, Shakespeare, Galileo, Pascal, Newton, Einstein, Kant, Hegel, Heidegger, Sartre, Marx and Simmel (as well as Huxley, Hemingway and Seinfeld) are included in the roll call. But Ritzer is soon able to mark the distinctive substantive content of his nothing.

From Kant’s point of view, it would be inaccurate to say that a form (or its content) is empty, that it is nothing.... However, this is all of little relevance to the argument being made here since Kant’s forms exist in the mind while those discussed here exist in the social world. While it may be that Kant’s forms logically cannot be empty of content (they cannot be nothing), that has little or nothing to do with the argument that centrally conceived and controlled *social* forms can be, and increasingly are, (largely) lacking in (distinctive) content. (p. 191)

12 To be fair, Ritzer does seemingly acknowledge this – that “nothing is nothing” (p. 14). Which adds to the complexity of his comparisons with eminent authors who also use the term ‘nothing.’

13 A little sneaky, to be sure, since (according to the OED) the definition of nihilism as nothingness is now obsolete. But well within the scope of the games that are being played here.

Is it starting to unravel? Surely Ritzer is not acknowledging that nothing is different? Maybe not. Hegel is congratulated for identifying the “dialectical relationship between nothing and being” (p. 191), their inseparability, and that they “cannot be disentangled because neither exists outside the relationship of *becoming*” (p. 191, original emphasis). And this dialectical approach is “of course, picked up by Marx” (p. 191). But:

Similarly, while we accept the view that there is a dialectical relationship between form and content and, more important, between something and nothing – that each is continually becoming the other – we, too, have in fact had to separate them out [like Marx] for analytical purposes. Of course, a concept like nothing is anathema to Marx. It is too abstract, too philosophical for his tastes and interests. After all, Marx has a materialistic interest in a concrete analysis of a specific economic form – capitalism. He is interested not only in understanding it, but in helping to hasten its demise. Given such an orientation, ruminating on form and content, their dialectical relationship, and whether form can be devoid of content, would not only not be issues of interest to Marx, but they would be abhorrent to him. (p. 191)

Kant too philosophical, Marx too anti-philosophical, Hegel refusing to allow the separation of nothing and being, thereby refusing pragmatic analysis. Sartre’s thinking on nothing has “little relevance” and “it is difficult to see much relationship between Heidegger’s sense of nothing and the way that concept is used here” (p. 195). Simmel, when talking about form and content is too close to Kant although his comments on the ‘tragedy of culture’ are cited approvingly as perhaps a fore-runner of Ritzer’s insight. And here we could start to see some problems with the originality of Ritzer’s work. For, as Habermas suggests in his introduction to the book of Simmel’s essays that includes ‘Begriff und Tragödie der Kultur’ (‘The Concept and Tragedy of Culture’), Simmel’s analysis has seeped into analyses across the social sciences:

Taking into account the historical influences of the Simmelian diagnosis of the times, one can ascribe to it what Gehlen once maintained about the enlightenment: its premises are dead, only its consequences live on. All parties seem to agree on the consequences, although some criticize as negative totality what others celebrate as crystallization, and some denounce as objectification what others technocratically safeguard as materialities. (Habermas, 1996: 405)

Nothing relevant is said about nothing, but plenty seems to have been said about the themes that run through the book. As Ritzer himself points out, Heidegger on technology and Sartre on loss also do seem to be talking about many of the same issues, and the unwary reader may also think that Marx, too, may have some relevance. Indeed, the more cynical may well believe that there has been plenty written on such issues such as *social forms that are generally centrally conceived and controlled and comparatively devoid of distinctive substantive content*, be it from art, through organization to consumption. But on nothing itself, nothing relevant. And such mastery of the ‘Which-I-call’ move may indeed be one of the signature flourishes of the masters of some post-modernist language games.

Nothing To Talk About

Now Ritzer has so carefully established his right to talk about nothing, we can see if there is something in it. Since we have already established the ‘objective’ criteria through which nothing is determined, we can now turn to the ‘nullities’ themselves. Ritzer identifies non-places, non-things, non-people and non-persons. For Ritzer, “The

distinction between places and non-places is closely related to Manuel Castells's view that we are moving from a world characterized by 'spaces of places' to one dominated by 'spaces of flows'" (p. 40), with the exemplars:

... of spaces of places a well-established local residential community in which each house has been built to the owner's specifications and is therefore different from every other house.... not centrally planned or controlled by developers and builders. The contrast... are the myriad tract house and planned communities that followed in the wake of the construction of the paradigmatic Levittown in the post-WII era... The houses in such communities are built according to limited number of designs so that many houses are identical to many others. The entire community is conceived by a central source (usually a developer) and once in existence is subjected to centralized control (especially today's gated communities).

While sounding attractive, Ritzer might be inputting just a little too much local content.¹⁴ But those are not the only examples: food is always on the menu. Ritzer (2004: 42) compares Oldenburg's (1989/1997) "great good places" the "core settings of informal public life" [Oldenburg (1989/1997: 16), c.f. Ritzer (2004: 42)] to McDonald's and McDonaldized restaurants – an argument run (at least initially) over some 13 pages – by the end of which we learn that some of the themes explored in Ritzer's McDonaldization thesis (the drives for efficiency, predictability, calculability, control by non-human technology and dehumanization) militate against McDonalds becoming a great good place. (Lucky Ritzer didn't McDonaldize his Globalization text too much.)

Non-things are "centrally created and controlled and [...] lacking in distinctive substance" (p. 55). A cynic, looking at the general examples, might consider this merely extends to all branded goods,¹⁵ but that would be too easy, for the exemplars are a Big Mac and Culatello ham. Now there may be some similarities between the two products (both are available globally and carefully marketed) but, "some (at least those who know about the distinctiveness and quality of such ham) will pay more for a Culatello ham than for virtually any other type of ham" (p. 58). Reassuringly expensive for consumers with reassuringly expensive tastes.¹⁶ Yet, somewhat oddly for a sociologist of consumption, nothing is said of the many other theorists who have ploughed this earthy furrow.

14 Such categories are difficult to sustain outside of the US – in the UK the examples of the former would only be seen in the wealthiest suburbs (such as, say, Bishop's Avenue in Hampstead) – rarely seen as models of tight communities and frequently populated by the wealthy mobile while perhaps the epitome of identical houses built from limited plans, and still tightly controlled (down to the type and colour of paint allowable for doors and windows), would be the Georgian terraces of Bath or Edinburgh. An even starker contrast would be with the glossy Modernist constructions of the wealthy glorified on Channel 4's *Grand Designs* and the inner city urban estates of streets of identical dwellings built to house the working class during Britain's industrialization.

15 Sealy mattresses, Martha Stewart sheets, Chanel perfumes, Victoria's Secret, Ralph Lauren and Mickey Mouse sleepwear, Taco Bell burritos, Benetton sweaters, Gap jeans and Gucci bags make up the initial list.

16 Culatello ham is similar to Parma ham, which is sliced and packaged across the world to the extent that Parma ham has become an effective and widely acknowledged brand. And it has become so established that there is no need to differentiate within the product by brand, as you might do for something more mundane, such as pork sausages. Culatello ham has not yet travelled as far or as fast, of course, but it is on the same trajectory.

Cleverly, the separation between people and non-people revolves around the difference between local bartenders and cast-members at Disney although in a presumable precursor to the discussion on Americanization, Woody from Cheers rather than Al Murray – pub landlord, is chosen as the exemplar with the unsurprising conclusion that, “[i]t is difficult to imagine the faceless non-person who plays a Disney character becoming a cultural institution” (p. 63). But the artifice involved in Disney also has unexpected outcomes.

However, [In Disney] the efforts to produce simulated enchantment are highly rationalized and are therefore better described as being disenchanting. The irony is that Disney World terms itself the “Magic Kingdom”. There is, of course, magic there, but it is of the simulated and disenchanting variety. Nothing militates more against “genuine” magic than simulation and disenchantment.

There is a little puzzle here. Ritzer cites Baudrillard approvingly on simulation, provocatively suggesting “Closely related to the idea of nothing... is the idea of simulations, most closely associated with the work of the French postmodernist, Jean Baudrillard. To Baudrillard, a *simulation* is a copy of a copy for which there is no original. It could be argued that *all* forms of nothing are also simulations. That is, it is originals that have distinctive content (and are something), but copies—simulations—are by definition lacking in such content; they are nothing!”¹⁷ (Baudrillard, 1983: 220) Further, it is precisely simulations that tend to be centrally created and controlled. Baudrillard’s assertion that the world is being increasingly characterized and dominated by simulations is consistent with this book’s grand narrative about the global proliferation of nothing.

What is awkward here is the residual attraction to ‘originals’ for, as the quote above makes clear, simulation destroys the distinctions that would matter here. In addition, and more tellingly Baudrillard says,

[In the order of simulation] When the real is no longer what it used to be, nostalgia assumes its full meaning. There is a proliferation of myths of origin and signs of reality; of second-hand truth, objectivity and authenticity. There is an escalation of the true, of the lived experience; a resurrection of the figurative where the object and substance have disappeared. (1994: 6-7)

Similarly, what might be authentic is open to question: for “in the US... culture is space, speed, cinema, technology. This culture is authentic, if anything can be said to be authentic” (1988: 100).

It therefore appears Ritzer may be being playful here with his distinction between what is simulated and what is genuine. And the examples suggest that this is so, as they slip unavoidably into parody. Since Ritzer’s first suggestion that ‘genuine enchantment’ is possible between bartenders and consumers is disarmingly deconstructed by Al Murray’s boorish landlord: “white wine/fruit-based drink for the lady?” (Murray, 2007: 170) we can turn to his extended example of enchantment when talking about non-service. Once again at a gourmet restaurant, we now find:

17 Oddly, some of Ritzer’s earlier examples are scarcely ‘originals’ – the 1969 Pontiac Firebird is a copy (of earlier iterations of that model) of a copy (the Camaro) of a copy (the Mustang). Something out of nothing, indeed!

Finally, the service at a gourmet restaurant is likely to have a magical quality about it, whereas the service provided by waiters on cruise ships, like the cruise ships themselves, is likely to be more simulated enchantment and therefore more disenchanting. In terms of the former, a good example would be one of Paris's great gourmet restaurants, especially during truffle season. Soon after diners are seated, and periodically during the evening, staff members circulate around the restaurant with a basket laden with truffles. Diners are allowed to gaze at this magical food and to inhale its aroma. The expressions on diners' faces indicate that this is clearly a magical moment. One is unlikely to experience such a moment, or such service, on a cruise ship, where the emphasis is on serving huge numbers of people lots of food quickly and efficiently. (p. 65)

It is as well that Ritzer is pulling our legs, for otherwise jaded readers might question how his objective criteria are being applied here and indeed how the distinction between real and simulated comes to be so apparent. Skeptics might also question why small rituals at specific restaurants are compared to more encompassing processes elsewhere.

Nothing Has Been Written on Globalization Yet

Ritzer is at his most modest when he discusses his contribution to the globalization debate. Take his introduction of the term 'grobalization':

While glocalization is an integrative concept, and Robertson is certainly interested in both sides of the glocal-global, homogenization-heterogenization continua, his work tends to emphasize the importance of the glocal and the existence of heterogeneity. This book seeks to offer a *more balanced view* on these issues by developing a second concept – *grobalization* – to supplement the undoubtedly important idea of glocalization. The concept of glocalization gets to the heart of not only Robertson's views, but also what many contemporary theorists interested in globalization think about the nature of transnational processes. *Glocalization* can be defined as the interpenetration of the global and the local resulting in unique outcomes in different geographic areas. The concept of *grobalization*, coined here for the first time as a much-needed companion to the notion of glocalization, focuses on the imperialistic ambitions of nations, corporations, organizations, and the like and their desire, indeed need, to impose themselves on various geographic areas. Their main interest is in seeing their power, influence, and in some cases profits *grow* (hence the term *grobalization*) throughout the world. Grobalization involves a variety of subprocesses, three of which – capitalism, Americanization, and McDonaldization – are, as pointed out above, central driving forces in grobalization, but also are of particular interest to the author and of great significance in the worldwide spread of nothingness. (p. 73, original emphases (unbelievably))

I should be clear here. Ritzer is being modest in his masterful play of another language game – a different variation on the 'Which-I-call' gambit. For while it is true that Robertson brought the term glocalization into the globalization debate, and that he has had a particular interest in exploring that aspect, globalization discussion is scarcely free from analyses that explore the drive of capitalism and Americanisation. (The more world-weary might even point out that glocalization was created, and works as a term, as a balance against overly consuming analyses that start from the effects of the development of capitalism and Americanisation.) The consummate skill that Ritzer shows is to include McDonaldization as a separate strand of thought from capitalism and/or Americanisation (for McDonalds' worldwide spread is often seen as mere exemplar of contemporary capitalism or Americanisation under globalisation (or often even cited as an example of Glocalization as menus and practices are adjusted to local cultures) through the coining of a new vocabulary. Grobalization includes McDonaldization, therefore if globalisation is to be discussed in these terms,

McDonaldization must be seen as a valid category of analysis. Or, more cynically, the contribution here is the globalisation of McDonaldization.

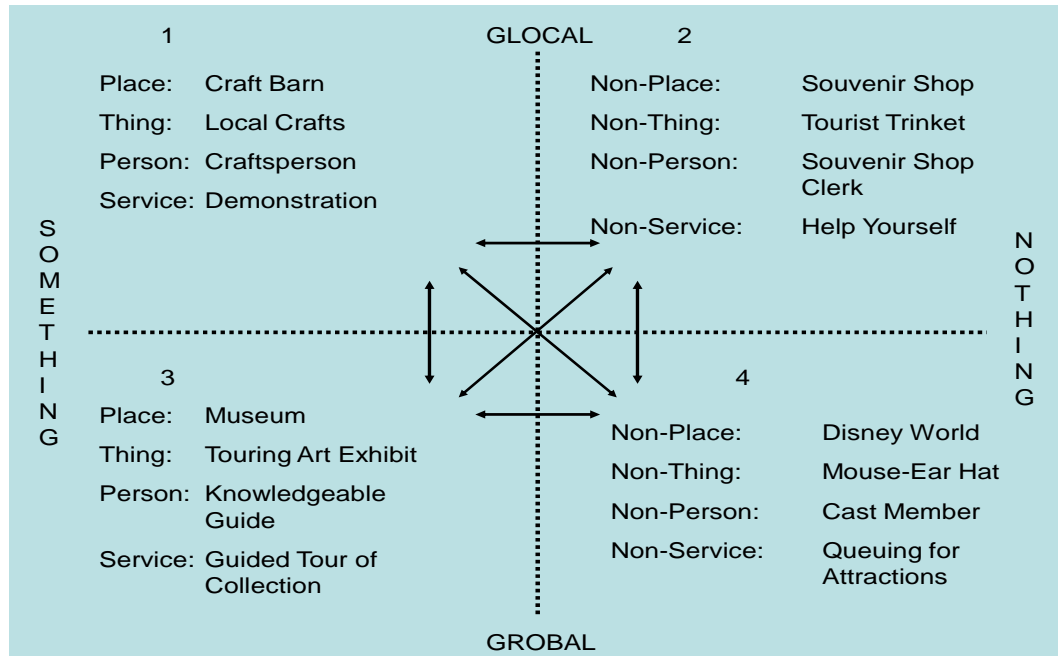
The McDonaldization of Globalisation

Helpfully, when George Ritzer gave his plenary presentation at CMS5 in Manchester, he used the 2x2 matrix from *The Globalisation of Nothing* to illustrate his thinking. Not only did this help to communicate and make intelligible the challenging ideas presented in his book, but it was a touching gesture for the audience of management academics as, arguably, such images are the defining legacy of management practice and theory. And why not? They are simple to understand, break the world down in to manageable categories that direct us to clear (strategic) directions. As the blurb for Lowy & Hood's (2004) book states, "2 x 2 Thinking is characterized by a fundamental appreciation for the dynamic and complex nature of business" (Wiley, 2009). Indeed, at their very best, they are able to inform us what is good, what is bad and what is somewhere in-between. Sadly their use outside management as guides to good and bad and right and wrong has been neglected. The potential is clearly there, as this matrix below indicates.

| | |
|---------------------------|------------------------------|
| I Willing & Able | II Willing & Not Able |
| III Not Willing & Able | IV Not Willing & Not Able |

Drawn from <http://www.lorober.com/favorites/favorites.htm> – "Another 2x2 matrix asking if people are willing and able for the task. Our response to each of the four types of people must be different". Last consulted 30/05/08.

This example clearly demonstrates how definitions of what is good and what is bad can be extended along two dimensions in order to give a better understanding of the many challenges in life. Thus this simple matrix enables the useless to be sorted from the feckless and from the fecking useless. However, Ritzer is not a shallow management academic or wincing psychologist but a public intellectual although, like Foucault and Baudrillard, one dismayingly unacknowledged in his own country. "Recently, on the occasion of the fiftieth anniversary of McDonald's, I was contacted by about a half dozen *different* BBC stations and programmes. In contrast, I believe at that time I gave a total of two interviews to small US newspapers" (Ritzer, 2006: 209, original emphasis). Consequently his matrix (below) is much more nuanced. There are no solid lines demarcating categories and, indeed, to illustrate the delicate ambiguity in his argument, he uses double-headed arrows to emphasise the potential for boundary-crossing. The potential revealed by this is immense: if the Boston Consulting Group had been as sophisticated, perhaps some of those dogs could have become cash cows or stars (or maybe daring new hybrids) and so much misery caused by firm closures and down-sizing could have been averted.



Derived from figure 5.1: The relationship between Glocal-Grobal and Something-Nothing with exemplary (non-) places, (non-) things, (non-) persons and (non-) services. (p. 98)

Now we have the key axes for Ritzer’s matrix set out, we can briefly consider the different sections. Rightly, Ritzer points out that “the other two quadrants (2 and 3) are clearly residual in nature and of secondary importance” (p. 97). Rightly, that is, for any decent 2x2 should be broken into good, bad and something in-between categories. And as such, we shall not heed them here, quickly turning to the good and the bad. Perhaps surprisingly, neither gourmet meals nor McDonalds appear in either box. Instead, we find a good place to be a craft barn, local crafts as good things, a good craftsperson and good service realised though demonstration. Disney World is a bad place, Mouse-Ear hats are bad things, cast members are bad people¹⁸ and queuing for attractions is a bad thing.

On the last we can perhaps agree.¹⁹ But the rest appear merely as value judgements (that differ markedly from mine²⁰) stitched into a framework that simply works to reinforce those judgements. Which is fine as far as the 2x2 matrix is concerned – that’s its job. But the value judgements remain disappointing. Can we try some different ones within the same matrix? Let’s start with the bad. Ritzer helpfully suggests that:

18 A little naughty, that one.

19 Fortunately at Disney parks, for less than the price of a gourmet meal, you can purchase fast-track tickets that enable you to queue-jump.

20 Nothing is more likely to ruin my day than going to a craft barn and watching some craftsperson demonstrate how they make their useless tat. Tastes differ, I guess.

Nothing has an advantage in terms of transportation around the world. These are things that generally can be easily and efficiently packaged and moved, often over vast areas. (Ritzer, 2003: 200)

His PowerPoint slides, seemingly the same as those illustrating a talk at a conference 2 years earlier, certainly appear to fit that bill. We might also suggest that recooked McDonaldization tends towards being devoid of distinctive content and that many of the ideas about ‘nothing’ are more impressively examined elsewhere. Only using the term ‘nothing’ to describe something and then shamelessly comparing it to other luminaries who write about nothing, as nothing, might be described as distinctive. Perhaps, overall, by discounting those language games we can pitch this as a non-thing. CMS⁵ could have been held anywhere, and simulated BAM²¹ quite adequately – non-place perhaps? And cynics have suggested that this was the same script as delivered before – non-service? And non-person – no. That would just be too rude.

But is it Grobal? Well, the McDonaldization thesis seems nearly as widely distributed as the eponymous restaurant and is scarcely a *local* response, the thesis has been proven more than capable of being McDonaldized itself, and its promulgation does seem fuelled by the pursuit of the dollar. Enough there to put it mostly in the Grobalization of Nothing box (albeit with maybe one or two of those dinky arrows across the permeable divides). Perhaps it sits as neatly as any of the other examples.²²

And as to what would then be in the Good category? Well, it wouldn’t be me, since I am under no pretence that my own work couldn’t be subjected to the same critique... although, sadly, I don’t get paid £5,000 to turn up at conferences. (But there again, as Baudrillard (1999: 199) pithily recognized “A negative judgment gives you more satisfaction than praise, provided it smacks of jealousy.”).

Maybe, in the end, within academia, nothing sits there and this simple matrix ultimately shows that any grand narrative, or indeed the successful promulgation of all academic work, always veers to the globalization of nothing.

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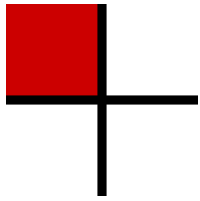
21 British Academy of Management. Don’t even go there. But if you do, don’t pay.

22 But wait. Clearly the performance at Manchester would then have to have been a demonstration of the grobalization of nothing and thus something. And therefore, as Ritzer points out, “[w]e accept the view that there is a dialectical relationship... between something and nothing – that each is continually becoming the other...” (p. 191). Maybe I’ve been too cynical.

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Comment on Lightfoot

George Ritzer

Geoff Lightfoot's commentary is so personally offensive, so poorly done, and so wrong in so many ways and places, that my first inclination was to ignore it and let it die the rapid death it deserves. However, the more I thought about it, the more I decided that there are many things in the essay that cannot be left to stand without correction and clarification. The author needs to be called on many things. I call him on those things in this response even at the risk that such a response will only serve to bring more attention to his reprehensible piece of work.

I also think the journal needs to be admonished for allowing this commentary to be published in its present form. It is one thing to publish an academic critique of my work – in this case *The Globalization of Nothing* – but it is quite another thing to allow the author to use such a critique to criticize me so personally and to extend his critique to a wide range of things, many of which have nothing to do with the work in question. Below is an iteration of at least some of the things about which the author is wrong and/or wrong-headed.

1 – Most damningly, the author clearly does not understand my most fundamental theoretical concept – ‘nothing’ (and therefore ‘something’). If the author does not understand the most fundamental idea in the *Globalization of Nothing*, how seriously are we to take the rest of his critique? Early on, the author wonders whether one of my statements implies that I (“self”) and my “work and academic endeavour” are nothing. While this allows him to take a cheap shot at me, the author is so anxious to demean me that he demonstrates his ignorance of the meaning of nothing. Recall that nothing is defined as a social form that is centrally conceived, centrally controlled and lacking in distinctive content. Unless one assumes there is some malignant evil force out there (later the author does mention “dark forces”, but never states what they are) controlling me (my publisher? my university? the U.S.? capitalism? Darth Vader?), it is impossible for me and my work to be nothing. It may have its weaknesses, but it is mine, warts and all. That it is mine is clear in a series of consistent themes that run through all of my books of the same genre as *The Globalization of Nothing*. My work is, by definition, the opposite of nothing, it is something because it is indigenously (in this case, personally) conceived, controlled and rich in the distinctive content that informs all of my work.

Similarly, near the end of his diatribe, the author attacks the McDonaldization thesis (inappropriate in a critique of the *Globalization of Nothing*, but not in an all-out assault

on me and my work) for being “scarcely a local response” (something) and therefore being nothing. Why is the McDonaldization thesis nothing? Because it is “nearly as widely distributed as the eponymous restaurant”. While this allows the author another cheap shot (the analogy between a scholarly work and a restaurant chain), the fact that the book has sold well and widely has nothing to do with whether the thesis expressed in that book is nothing. Of course, on the contrary, it is the ultimate in that which is something, it was created by and is the unique product of a single individual. Once again, the author shows his lack of understanding of the central ideas of something and nothing.

It is also asserted that the “promulgations” of the McDonaldization thesis have made it nothing and have McDonaldized the McDonaldization thesis. Do original scholarly monographs and scholarly articles (to say nothing of edited volumes of analysis and critique [Alfino, Caputo and Wynyard, 1998; Smart, 1999]) on the McDonaldization of the church (Drane, 2001: 2007), higher education (Parker and Jary, 1995; Hayes and Wynyard, 2002), social work (Dustin, 2007), etc., make the thesis nothing (centrally conceived, etc.) or McDonaldize the thesis (make it more efficient, predictable, etc.)? I think not. Rather, the fact that other scholars have found the idea useful and applied it in a wide array of contexts demonstrates its strength and its broad applicability.

2 – The author appears not to understand the nature and function of a review of the literature. Much is made of my review of previous work on nothing. I am accused of “shamelessly comparing it [my ideas on nothing] to other luminaries who write about nothing”. It is sarcastically noted that I “carefully established [my] right to talk about nothing”. That is, of course, exactly what I was trying to do and that is the function of a review of the literature. Further, these ideas influenced my thinking – as previous work always does – in various positive and negative ways. Thus, the author suggests that there are “some problems with the originality of [my] work” because, for example, the ideas of my predecessors (e.g. Simmel) have “seeped” into my analysis. Well, of course they have and they should have. Theoretical ideas from my predecessors – Marx, Weber, Simmel, Baudrillard – have strongly influenced all of my work.

The author is apparently unaware of Isaac Newton’s (and Robert Merton’s) ideas on building on the ‘shoulders of giants’. I have explicitly built on the work of others in all my work – the McDonaldization thesis is built mainly on Weber’s theory of rationalization, thinking on the cathedrals of consumption on Weber and Baudrillard, work on credit cards on Simmel and his philosophy of money, etc. Even the idea of ‘globalization’ is developed on the basis of an analysis of the idea of ‘glocalization’, most often associated with Roland Robertson. Does the fact that they are built on the work of important predecessors make my work unoriginal? Perhaps, but the alternative would be to practice the ‘cerebral hygiene’ that characterized the work of Auguste Comte.

Guilty as charged! I did try to carefully establish my right to talk about nothing. I did build on the work of predecessors. Silly me! I thought that was sound scholarship.

3 – In a related attack later in the essay, the globalization of nothing is described as “recooked McDonaldization” This, like much else, is asserted and not explained. In

what ways does the later book recook McDonaldization? In fact, as I see it, the *Globalization of Nothing* is an effort to raise to a more general and more theoretical level my previous work on fast food restaurants, cathedrals of consumption, credit cards, etc. The ideas developed in that book overarch and extend my previous work.

4 – Interestingly, after seeming to accuse me of devoting too much attention to and being too dependent on, the work of my predecessors, the author then goes on to say “somewhat oddly for a sociologist of consumption, nothing is said of the many theorists who have ploughed this earthy furrow”. I’m damned if I don’t and damned if I do. The main goal of the author is damn me whatever I may do.

5 – My work is accused of being “part of the ‘unacceptable’ attempt to partition off ‘critical’ knowledge”. However, if my work is anything, it is critical... critical of McDonaldization, of the consumerism spawned by the cathedrals of consumption, of the hyper-indebtedness fed by credit cards, and most generally of the destruction of something by the globalization of nothing.

6 – The author uses the terms ‘good’ and ‘bad’ to describe various places and non-places. This is done without recognition of, and in spite of, my assertion (see p. 13 of the first edition of *The Globalization of Nothing*, and elsewhere) that: “nothing, as well as something, are ideal types that offer no evaluative judgment about the social world”. I further argue: “no overall value judgment needs to be made here – forms laden with content (something) are *not inherently better* than those devoid of content (nothing), or vice versa” (p. 140).

7 – The Abstract implies that the paper will deal with my use of a 2x2 matrix. However, that is not the focus of the paper and it is not dealt with until the closing section. Everything that precedes that final section is irrelevant to a critique of 2x2 tables (which seems to have much to do with some grievance against management thinkers). That critique, when it comes, seems to be little more than an excuse to excoriate me and my work in the preceding pages. The author is not content to critique *The Globalization of Nothing*, but uses the occasion to attack – quite inappropriately – many other aspects of my work and, most inappropriately, to attack me personally. Among the examples:

- a. It is implied that I am guilty of snobbery,¹ of McDonaldizing my own work, and of being greedy (“pursuit of the dollar”). I have never met this author (that I can recall) and he knows nothing about me as a person and certainly nothing about my motives. Why has this journal allowed this person to impugn my motives, without any evidence, in a journal ostensibly devoted to scholarly pursuits?
- b. Related to the latter is the author’s seeming obsession with the 5,000 pounds (it came up twice in his critique) I was supposedly paid to give my keynote address. First, he is wrong on the amount and he should not have reported, or been allowed to publish by this journal, a rumor (“allegedly”) about my fee. Secondly, whatever the amount, it has nothing to do with the scholarly merits of

1 I anticipated this critique and many others in the Appendix to the first edition of *The Globalization of Nothing*.

The Globalization of Nothing. Indeed, it has nothing at all to do with scholarship.

- c. The author fails to understand the nature of a keynote address in critiquing me for giving what appears to be the same address in Australia and in Manchester. (That he does not know this, but is surmising it, is clear when he says that my power points were “seemingly the same”.) First, unlike a paper at a conference, one is NOT expected to give an original paper in a keynote address. In fact, I am almost always asked to speak about my best-known work such as that on the globalization of nothing or McDonaldization. While it is not expected of me, I often make a strong effort to relate my talk to the themes of the conference.

Second, it is highly unlikely that those in the audience in Australia would have turned up in Manchester; the material was (largely) new to that audience.

Third, I cannot assume that any of my audiences, often in fields different from my own, had read my previous books and articles.

Fourth, and once again, all of this has nothing to do with the intellectual merits of *The Globalization of Nothing*, the ostensible concern of the essay under consideration here.

- d. The author indicts me for something that “crops up in the sales blurb”, but as any book author knows, such blurbs are written by the publisher’s marketing people and authors generally do not see such things until a book is published.

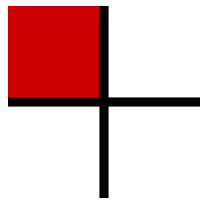
Enough! However, I can’t close without noting that the commentary in question is not only studded with innuendoes, but that much of what the author says is both elusive and allusive. As pointed out above, while some of the negativity is quite overt, much more of it is to be found just below the surface. I trust that there is no ambiguity about my distaste for virtually everything about this critique of me and my work.

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There Is No Such Thing as Audit Society¹: A Reading of Power, M. (1994a) 'The Audit Society'*

Josephine Maltby

abstract

Building on The terms 'audit explosion' and 'audit society', first coined by Michael Power, have become shorthand for a set of beliefs about the ubiquity of an audit regime which began to permeate the UK public sector in the Thatcher era and has had a detrimental effect on its conduct by permeating public culture. The present paper attempts a critique of this doctrine. In a series of papers, Power has presented audit as intrinsic to modern society – 'a constitutive principle of social organizations' and an 'institutional norm'. He derives audit from the fundamental importance of agency relationships, based on the absence of trust, within the public as well as the private sector, which require workers to become auditees and make auditability the most important component of work. The paper challenges Power's ahistorical approach to the origins of audit, suggesting that he unjustly presents it as an immobile form rather than an activity whose incidence and function continue to change, and questioning his aversion to the use of audit as a means of making 'experts' in various fields accountable to the public.

Any discussion of Power's Audit Society paper of 1994 has to start by acknowledging that it has enjoyed an extraordinary degree of success for an academic paper, let alone for an academic paper about audit. His terms *audit society* and *audit explosion* have gained very wide currency within the social sciences and on the wider stages of quality journalism and serious-minded websites. Some of this is, admittedly, due to Power's own copious output on the topic (Power 1994a, 1994b, 1997, 1999, 2000a, 2000b, 2000c, 2000d, 2002, 2003a, 2003b, 2005a, 2005b), some to exegeses of it (see for instance Bowerman et al, 2000; Humphrey and Owen, 2000; Courville, Parker, and Watchirs, 2003; English and Skærbæk, 2007 and Lindeberg, 2007) but a great deal to admirers from all sorts of disciplines.

For some of them, the admiration is rather shakily based, e.g. Quam (1998), who writes off a large swathe of accounting research, and a number of journals by concluding that

1 Adapted from Keay, 1987.

* Thanks to people at the Queen's University Management School, at the Accounting, Business and Financial History Conference, University of Cardiff, 2007, and at the Conference of Practical Criticism in the Managerial Social Sciences, University of Leicester, 2008, for their helpful comments.

Power's "book is valuable precisely because of the obscurity of its subject. Very little has been written about the purpose, context, or process of auditing". There are more encomia written by those who have spent a lot of time thinking about audit, such as Dame Onora O'Neill who in a Reith lecture to an audience of hospital staff invokes the 'Herculean micro-management' under which they labour (O'Neill, 2002), or the audited academics trapped in 'cages on hamster-wheels' like Aidan Foster-Carter, Honorary Senior Research Fellow in Sociology and Modern Korea, Leeds University (1998). On the basis of such endorsement, Power's diagnosis of an audit explosion has been received as a verdict on a radically changed governance environment more generally – see for instance Caulkin's warning (2002) that "an exploding audit culture drains the public sector".

The present paper takes issue with the case that he has made, offering three arguments against it. These address the effects of Power's reluctance to define audit, the omission from his account of empirical, and particularly of historical evidence, and most importantly his aversion to the role of audit in requiring accountability from certain kinds of skilled worker. These arguments are developed in the course of a page-by-page reading of Power (1994a) and all unattributed page references are to this chapter. This is supplemented by a consideration of his 2003b paper and an evaluation of his other writings on the subject.

Power's work on the prevalence of audit now enjoys classic status for a number of reasons. One of these, as suggested above, is its wide dissemination, in various formats, in the literature of accounting, politics and public sector management. But his winning strength is his depiction of the irresistible and irreversible global rise of audit, and of its disastrous effects on society. He begins his 1994a chapter innocuously by observing that "there is little doubt that the word 'audit' is being used, in the UK at least with growing frequency" (p. 299), and proceeding to list its manifold uses:

In addition to financial audits we now hear of environmental audits, value for money audits, management audits, quality audits, forensic audits, data audits, intellectual property audits, medical audits and many others besides. (p. 299)

He has made a strong beginning – nine uses of the word 'audit(s)' in eleven lines of the original text quoted above, as well as the looming menace of the unnamed 'many other' audits besides. There is the risk, though, of the objection that activities may have significant differences despite sharing the same name. Power hurries to forestall this (and thereby help to give his paper a safe passage):

The significance of audit is not located in a "definition" of its role. Indeed it matters less what different audit practices "really" are in some operational sense than how the idea of audit is appropriated and mobilized. Audit is a particular manner of *(re)presenting* administrative problems and their solutions, one that is becoming universal. (p. 299)

This is a major tactical stroke. Power has released himself, at a very early stage in his argument, from needing to define, or indeed 'define' the audit which is creating the audit society. Quality, forensic and medical audits all boil down to the same idea. So presumably do the environmental studies project in which 10-12 year old pupils in Scotland are to "demonstrate skills by preparing and organising plans to improve or start a 'fruit' tuck shop – organising a health audit of their snacks and lunches", and so

do McCain's investigation into frying practice in chip shops. The company, "wants to combine producing healthier chips with educating caterers on how to fry chips in a more healthy way". Behind this is a new scheme called "Best Fry Practice", in which McCain works with customers to 'audit' the way chips are being fried and show them how to do it (Caterersearch, 2005).

Under the guise of difference – the masks put on as audit is purveyed by ACAs, tubby schoolchildren, or concerned food processors – Power warns us that we need to discern fundamental identity. If it is not important what audit practices 'really' are, all that is necessary is for the audit word to be invoked for all sorts of social problems (dishonesty, malnutrition, lardy chips) to be constituted in the same way. And this identity means that Power's argument gains weight and momentum as it rolls on, collecting in its course more and more evidence of the pervasiveness of the audit idea.

The paper's account of the history of audit is brief to the point of being perfunctory. Power nods towards medieval origins by pointing out that the audit was initially a hearing, but proceeds rapidly to agency theory, the "widely accepted characterization of the source of demand for audit" (p. 301). The agency theory explanation, since its enunciation by Watts and Zimmerman (1983) has enjoyed wide acceptance because it offers a satisfyingly straightforward mechanism for the introduction and popularity of financial audit. The separation of ownership and control means that investors distrust managers to the extent that they cannot monitor them personally – so it is advantageous for managers to invoke a third party to attest their reliability. For Watts and Zimmerman (1983: 614) audit has been "part of the efficient technology for organising firms" from the 14th century onwards – it has always been there, in merchant guilds, joint stock companies and limited liability companies (*ibid.*: 616-628) doing the job of "restoring credibility" (Power, 1994a: 301).

Audit is, for Power, essentially grounded in the financial audit, a way of compensating for the inherent mistrust that necessarily exists between investors and managers. The audit society "trusts auditors before operatives" (p. 301) and so audit has developed as a way of making financial markets work. Because audit has arisen from the unhappy relationship between principals and agents, the people who commission audits will always be reconstituted as principals, and those who are audited will become untrusted agents. And agents will become auditees, whose work has to be executed in an auditable form, which entails not only *ex post* inspection, but specification by the auditor of what is actually being done.

The next thing that happened in history, quite abruptly (in this case in the middle of page 302, second paragraph) is "the diffusion and generalization of the financial accountability model, particularly in the public sector". Financial audit has been exported to the public sector, via new public management. The Thatcherite revolution, (presumably hers – Power never quite says who effected the revolution) devolved public sector organizations but still required central control, so audit was invoked to reconcile the two contradictory movements, by being "the shadow of hierarchy which saves the appearance of central control" (p. 302). The metaphor has a vaguely Shakespearean resonance – or possibly it recalls Darth Vader deputising for the

Emperor Palpatine. Audit in its ominous ambivalence is a “political technology” (p. 302) that has arrived to loom over the public sector on behalf of the controlling centre.

The important effect of Power’s audit is not to make public sector organizations accountable, but to make them auditable. The latter is not a way of arriving at the former. The new technology is one that displaces previous methods of accountability such as inspection and quality control (p. 303). Power sums up the main strengths of audit that make it win out. It “has a special versatility in which submission to audit establishes legitimacy regardless of the operational substance of audit” (p. 304). Audit, in other words, is well thought of, so that organizations that are audited are well thought of as well: and it does not matter what auditors do, they will always confer a blessing.

This is the next major element of Power’s explanation. Not only does it not matter what audit practices really are – the idea is what has an impact. It does not matter what some people expect or hope for from an audit. One of the staples of textbooks about auditing is the chapter on the audit expectations gap in limited company accounts, the disparity between what auditors do and what various people who use the audited accounts would like them to do. In particular, it refers to the expectation that the audit will be a reliable method of detecting fraud and/or impending corporate collapse. This is an expectation that is often disappointed – there is a long history of audit failure, because auditors did not or chose not to notice that financial accounts were misleading (see for instance Teo and Cobbin, 2005; Sikka, 2003).

And there is a corresponding history of attempts to reduce the gap, in case or statute law or regulation or the actions of pressure groups. Sometimes there are failures – e.g. the long series of counter-findings in the Caparo case in the UK in the 1980s which ended in the absolution of auditors from most kinds of responsibility – sometimes there are at least temporary frights for the profession, recently the death and dissolution of Arthur Andersen after its conspicuous failure to detect the Enron fraud. One view of the auditor’s response to the gap (Sikka, 2003) has been that the accounting profession is constantly revising its excuses: the complaining public is being unrealistic or unreasonable and needs more education; the profession will do better, but self-regulation will be more effective than state intervention – and so on. Power’s distinctive view of the expectation gap is that, rather than a threat, it is a ‘political resource’ for audit. If the public expects that an audit provides a certain kind of assurance, whereas in fact it provides much less, then such a resource can be managed for both parties as follows: auditors can help themselves to the high fees that correspond to high expectations and regulatory publics can help themselves to the appearance of high levels of assurance necessary to legitimate regulatory programmes (p. 305).

Part of this is unsurprising: auditors get paid for doing less than they claim, because they give unqualified reports to accounts that are not true and fair. But the other clause is initially difficult: the public *benefits* from getting less assurance than it expected. This is because Power has shifted from the public in general to a new entity, the ‘regulatory public’. This is not defined, but it seems to include the bodies of various kinds presiding over banking, investment, stock markets and so on. They are happy because the imposition even of ineffectual audit makes them look as if they are doing their

supervisory job. They too believe that it is more important to appropriate and mobilize the idea of audit than to do anything with it.

The public that has been forgotten between the beginning and end of the sentence, however, is the public at large – not only disappointed investors but also the employees or creditors who might have cut their losses if the auditors had warned them earlier (Swash, 2007). The possibility that an effective kind of audit would in some circumstances be welcomed – and that there is a public who would benefit if the expectations gap shrank or disappeared, and which is aware of that possibility – is not present in Power's picture. His audit continues to become even more of a super-thing: it is omnipresent, it is an ambivalent shadow, but one that confers the benediction of legitimacy, it is so versatile that it can mean anything, and it makes a success out of failure. The class of person that might appreciate less audit failure and more audit success is written out of the story.

So, in the following paragraph, is the class of person who is naïve enough to agitate for audits to be carried out. Environmental audits of various kinds and their close relatives, ethical and social audits, have been regularly demanded by charities, pressure groups and trades unions to monitor the costs imposed by corporations on resources, neighbours and employees, and equally regularly resisted by their objects (Hickman, 2007). Even environmental audit reports commissioned and paid for by companies can produce unwelcome results as when a 1997 Ernst & Young audit disclosed lethal dangers for Nike employees in Vietnam – (CNN Interactive, 1997). For Power, though, even if environmental audit results are publicised, they are meaningless: “the public perception *that* an activity is performed, while remaining in substance invisible, is an important strategy of legitimacy in the audit society” (p. 305). The public according to Power does not understand the difference between an independent audit and one that serves its own and its auditee's interests, nor is it intellectually capable of criticising audit failure: “The audit society idealizes ‘independent validation’ without public transparency” (p. 306).

This general public does dimly, fretfully, know that something is the matter – and as a result “the audit society is the *anxious* society” (p. 307, italics in original). But its only recourse is to commission more audit: “The ‘fact of audit’ reduces anxiety, or more positively, produces comfort” (p. 307). This public is trapped because its only “hope for control in the face of increasing evidence of its absence” (p. 307) is to pretend that audit will make things better. It can delude itself more easily partly because “audit success or failure is never a public fact” – nobody is quite sure whether audit has failed or succeeded (p. 307) and partly because it is not allowed to talk about audit anyway: “criteria of success are withdrawn from public discourse” (p. 308). So audit failures are obliterated by its “durability as a ‘political rationality’”; the public knows that it has to put up with audit, because it is a new and improved technology.

But that is enough about the anxious, deluded public. “It is to (the) role of audit as a technology of representation that we now turn” (p. 308). Audit's next trick is to design the rules which auditees have to observe. It does not monitor or check compliance with standards set elsewhere: audit, in whatever form, creates the requirements which it will impose. The provisions of Companies Acts, the BSI and the Research Assessment

Exercise all have more in common than might appear – “the ‘performance’ of the auditee is reduced to the possibility of being audited” (p. 310). There are other seeming uses for at least some of the rules – for instance that BSI standards help to discriminate between electrical products that do and not electrocute, or that the Companies Act 1985 creates sixty-nine indictable offences of which directors may be convicted. But that is to miss the point. They are, Power contends, theatres in which audits can take place.

Audit has itself altered so that it can more effectively monitor compliance with the rules it is creating – it has evolved away from inspection, towards systems audit, “whereby it is the auditee’s own system for self-monitoring that is subject to inspection, rather than the real time practices of the auditee” (p. 310). This is initially puzzling if dwelt on, as Power has earlier claimed that audit has done away with self-monitoring systems such as inspection (p. 303), but audit thrives on challenge and contradiction (*passim*) so we can move smoothly on to take the next hurdle – Power’s treatment of financial audit in relation to systems. We have already seen that financial audit is the origin of and template for all other types of audit via “the diffusion and generalization of the financial accountability model” (p. 302). This makes it odd that systems audit, whose paradigm is the compliance checklist, is alien to financial auditors “for whom checklists impose too much structure on the audit process and thereby inhibit professional judgement” (p. 311). Again, this is difficult. Power has recently explained (p. 310) that “financial auditors promote the ‘systems audit’” – and here they are on page 311 finding it inhibiting. If all the manifold new varieties of audit are inspired by financial audit, how can they have developed a strategy – the checklist – that undermines financial auditors’ professional feelings?

It is not massively important, though, because audit is only pretending to do systems, just as it is only pretending – and here once again we change topics in mid-paragraph – to do statistical sampling (p. 311). And, moreover, (p. 312) it is pretending to do risk analysis. Risk in all its forms is a new phenomenon (p. 311), created by a new group of experts. Once they have fabricated it, they can audit for it, and here environmental audit reappears. Earlier (p. 305) it was a trick played by companies on the gullible public to whom they presented their reports: now it is a trick played by environmental consultants on corporate clients “in an emerging market for managerial advice” (p. 312). But the treatment of artificial risks is not confined to the new environmental profession.

And here we end with the last role of audit. It is, Power concludes, a process, “in which newly perceived difficulties and dangers can be ritually purified and reconciled to existing managerial and economic practice” (p. 313). Purification and reconciliation are constituents of religious ceremonies: the auditor is acting as a priest, carrying out a ritual that makes managers feel comfortable with newly-discovered dangers. This is the last and most impressive of the tricks that audit can perform. Thanks to its aura of authority, its versatility, its effortless surmounting of failure, audit has been able to capture hearts and minds. We have been conquered:

In the audit society a reduction in audit intensity, and the possibility of leaving groups and individuals to themselves, is literally unthinkable. (p. 314)

We are all auditees now. It is a splendid final paragraph, as rhetorically effective as the opening. Instead of the customary throat-clearing academic Conclusion, calling for

additional case studies and possibly some international comparisons, Power simply announces that we do indeed love Big Brother and leaves us to carry the weight of our own audit anxiety and audit dependence.

The difficulty with disagreeing with his argument is plainly that the dissenter is open to the charges of gullibility and addiction: gullibility in believing that audit really is necessary for organizations to be accountable and addiction in being so dependent on audit as to wish to absolve it from the various accusations of trickery and failure that permeate Power's characterisation of it. There is, though, a further, implicit criticism in Power's chapter, which recurs but is not spelled out. Power accuses auditability of making itself a substitute for accountability. But he does not really like accountability either. The supporter of audit may invoke in its defence its potential usefulness in producing credible statements from organizations about their performance, financial or otherwise. But, according to Power, this is the point where the people who want such statements (shareholders, taxpayers and so on) admit to their own inner mistrust. And from there on they are doomed to the downward spiral of anxiety that grows the more they try to allay it.

Particular economic rationales of audit represent a social world in which action can no longer be coordinated by trust alone. (p. 301)

'No longer' is an intriguing phrase. Is Power suggesting that there was a lost Eden in which social structures operated on the basis of trust and there was no need for accounts to be rendered? If so, his reference (p. 300) to the medieval audit process tacitly acknowledges that it vanished a long time ago. Or does he mean that there was a world in which the demand for accountability did not imply an absence of trust, and that the 'economic rationales', presumably shorthand for variants of agency theory, have subverted the possibility of such a type of accountability?

However the phrase is interpreted, it is a reminder that Power's very brief history of audit allies him with the academic writing that in Napier's words, treats accounting phenomena "with little or no suggestion that these might have been different at some earlier time (and therefore by implication might be different again in the future)" (Napier, 2006: 449). This is easy to do if audit, as with Power, exists in a foreshortened past in which everything has happened at once. Financial audit has emerged from the separation of ownership and control (p. 301), rewritten company law and British Standards (p. 309) and colonised the public sector (passim). There is no more detailed sequence than this, and no causality, other than the fell hand of audit, which changes everything around it but is not itself altered.

There are two ways in which a longer history of audit presents a challenge to Power. One is the recognition of the possibility that much of the history of public sector audit preceded, or at least ran concurrently with, that of limited companies. This involves challenges to Power's notion that financial audit is necessarily the model for all audit activity. Jones and Pendlebury (2000: 233) claim that government audit is "the oldest aspect of the auditing profession" and Hoskin and Macve (1986: 114) that accounting and auditing change took place in "the administrative arena before... the merchant world". Medieval Exchequer audit was a powerful practice, recorded in the Exchequer Dialogues (*Dialogus de Scaccario*) of the late 1170s. Jones emphasises not only its role

as a model for that of other institutions, but also the existence in the Exchequer of other forms of internal control that were transported into manorial systems (and subsequently into trade) – seals, personal supervision, sampling and division of duties (2006: 11, 14, 15).

The 19th century history of public sector audit is equally problematic for Power's thesis of colonisation by the profession. Coombs and Edwards (1990 and 2004) find that three groups – the elected ratepayer auditors, government-appointed district auditors, and professional accountants – competed for work. There was a “strenuously contested power struggle between vested interests” that began with the upsurge in new municipal corporations after the Industrial Revolution, and in which the three groups all had their own cases to make about the importance to the audit process of democracy, efficiency and expertise (Coombs and Edwards, 2004: 80). By 1974 when the municipal corporations disappeared, one-third had district or elected auditors versus two-thirds professionals (*ibid.*: 82). It was a victory for the professional firms, but it was neither rapid nor overwhelming.

There is another challenge, within financial audit itself, to Power's view of audit as a thing in and for itself, immutable and inflexible. The significance of audit has varied according to the different players in corporate governance – large and small investors and directors. For the ‘generality’ of 17th century investors in the East India Company, their appointment of auditors was not necessarily a breakdown of trust: Bryer (2000: 328) argues that they had thereby “abolished (their) feudal directorate and replaced them with modern managers accountable to a feudal capital”. Similarly, Toms (2002:81) suggests that the socialised capital (one shareholder, one vote, very widely generalized shareholdings) of the Oldham Limiteds of the mid-19th century used amateur shareholder auditors to give accurate accounting information. They placed heavy reliance on audited accounts which were published in the local newspapers. By the end of the century, the small local shareholders had been bought out by blockholders (who changed the company articles to one share, one vote) and who preferred financial controls, and insider knowledge to audit as a means of control.

Napier (1998: 117) finds two successive models of the limited company in the 19th century: “a collectivity model of the interests of shareholders, with directors and auditors being elected from the mass of shareholders” and a “business company” where “the auditor ‘intermediates’ between shareholders and directors *whose interests are not necessarily aligned with those of the shareholders*” (emphasis added). Like Toms and Bryer, Napier finds that auditors were initially seen as allies of the shareholders (the 19th century auditor often was a shareholder): the late 19th century saw the movement to the auditor as ‘guide, philosopher and friend’ to the manager. The late-19th century profession was not promoting itself to shareholders to reduce their anxiety: it was promoting itself to company directors with the mission of protecting them against what it portrayed as demanding and unrealistic investors (Maltby, 1999).

Part of the fundamental weakness of Power's treatment of audit lies in its aversion to empirics. The history of auditing – in the public or in the private sector- might provide some evidence of the behaviour of auditors and auditees, as would an examination of audit in practice, in an individual organization or comparatively over a variety of

locations. Power (2003b: 190) puts it rather differently: “The empirical challenge of the audit explosion idea is to focus research and data-gathering effort on the evidence of negative consequences”. Some negative consequences have, fortunately, risen to the challenge and Power lists them (*ibid.*) but they are slim pickings: although the evidence “exists and is increasing”, he has to admit it “is by no means inclusive” (Power, 2003: 191). Hood (2005), commenting on a study of public sector management in eight countries, comes to the disappointing conclusion that the role of audit there has not increased dramatically – has indeed declined. He suggests that “the audit explosion thesis seems to be “a textbook case of selection on the dependent variable”.

Studying audit with the expectation that it may mean different things in different contexts offers a variety of interesting projects – but Power’s 2003b study moves hastily on from the danger that audit, if looked at, may not always be found pursuing a relentless project of colonisation. In particular, Power avoids the distinction between audits that are aimed at financial control and those which require accountability towards a variety of users. This is an unattractive distinction for him because it admits the possibility that audit may serve social needs, and that the people who use services may want to make managers say what they have done. These users of audit are a drab lot. Power is more interested in its victims, the auditees.

So his 2003b paper, *Evaluating the Audit Explosion*, devotes only slightly more space to a survey of the empirics than it does to what is presumably included in the ‘most interesting’ part of the hypothesis, in his argument that “the hypothesis is most interesting at precisely the point where systematic evidence may be most limited and problematic” (Power, 2003b: 186). The area that deserves attention, he explains in his Conclusion, is “the social construction of a new subject, the ‘auditee’” (*ibid.*: 199). This Conclusion, like that of the 1994 paper, is a splendid piece of work, but in a radically different genre. Where the earlier piece was brief, bleak and unsparing, this is a rich slice of humanity, and it deserves to be quoted in full:

The auditee is undoubtedly a complex being: simultaneously devious and depressed; she is skilled at games of compliance but exhausted and cynical about them too; she is nervous about the empty certificates of comfort that get produced but she also colludes in amplifying audit mandates in local settings; she fears the mediocrity of the auditors at the same time as she regrets their powerlessness to discipline the really “bad guys”; she loathes the time wasted in rituals of inspection but accepts that this is probably what “we deserve”; she sees the competent and the excellent suffer as they attempt to deal with the demands of quality assurance at the same time as the incompetent and idle manage to escape its worst excesses; she hears the rhetoric of excellence in policy documents but lives a reality of decline; she takes notes after meetings with colleagues “just in case” and has more filing cabinets now than she did a few years ago; she knows the past was far from being a golden age but despairs of the iron cage of auditing; she knows public accountability and stakeholder dialogue are good things but wonders why, after all her years of training, she is not trusted as an expert anymore. (Power, 2003b: 199-200)

A brief analysis cannot do justice to this. It is, obviously, the outline of a tragedy of disappointed idealism that needs an Ian McEwan or any of the other modern English writers who appear in long extracts in the Guardian Review. But what is really important about it is that it encapsulates a key theme of the audit society thesis. The nameless auditee is a fairly senior person, important enough to amplify audit mandates and requisition her own supply of filing cabinets and take notes at meetings out of

precaution, not because she is going to have to type them up and circulate them. She is, in fact, an expert, with 'years of training'. And the grim reality of decline is the same for her as for Dame Onora O'Neil and the Honorary Senior Research Fellow in Sociology and Modern Korea. *She is not trusted as an expert any more*. Public accountability and stakeholder dialogue are good things, it is implied – but these people, the patients and students and so on, the public, ought to trust the educated middle classes to get on with being expert. *The Audit Society* and its progeny, Power's own papers and the wails of unhappy academics and doctors and civil servants, are ultimately not a protest about the creation of an iron cage round society. They are a stifled chorus of fury at being made accountable.

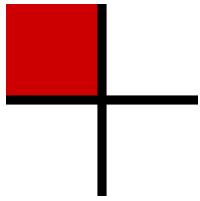
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In Defence of *The Audit Society*: A Reply to Maltby

Michael Power

In an age where citations seem to count for more than intellectual engagement and where genuine curiosity, at least in the field of management research (Hopwood, 2008), is increasingly rare, it is normally encouraging to come across examples of sustained textual engagement and critique. For that reason, Maltby's paper 'There is no such thing as the audit society' ought to be welcomed as an example of a genre which is fast disappearing. Equally, an author under the microscope approaches such a critical examination with some trepidation, knowing full well that all research efforts are flawed by definition. Yet, the key question is whether the flaws of a body of scholarship are outweighed by its benefits, and it is on this point that I must take issue with both the content and manner of Maltby's critique. Maltby has three arguments which, though distinguished at the beginning of the essay, become progressively intermingled. These arguments are directed mainly, but not exclusively, at *The Audit Society* and I address them in sequence.

First, Maltby argues that my reluctance to define audit is a "tactical stroke" which helps to suggest that "quality, forensic and medical audits all boil down to the same idea". She suggests that it is this identity of all practices called 'audits', and some which are not, which allows my argument "to gain weight and momentum as it rolls on". This is not a new criticism but, as I have argued in several other places, the apparent refusal to define is not some kind of tactical evasion. It has a perfectly coherent methodological basis which I try to transmit to research students, and which has been most recently applied in the case of risk (Power, 2007: 3-4). The methodological orientation is hardly original, taking its lead from Hacking's (1986) work on the dynamic role of ideas within practices – 'dynamic nominalism' as he calls it. From this point of view, definitions and labels are to be approached as contingent features of practices which also have potential to forge institutional similarity from difference. I could agree in part with Maltby that quality assurance, forensic analysis, and medical review may involve different tools, experts and policy expectations. But when these and other practices come to be called 'audits', they are changed and registered in a new policy space and new kinds of expectation are formed around them. They can become, in Anthony Hopwood's terms, what they are not.

From this point of view, the refusal to begin *The Audit Society* with a definition of audit is not, as Maltby seems to suggest, an elaborate trick, but reflects a respect for a complex phenomenon of expansion whereby concepts and practices co-evolve. Rather than offering a definition which somehow sits outside changing social expectations, a more oblique approach to the object of interest is necessary. *The Audit Society* is partly an attempt to understand how rationalized and abstract conceptual elements of audit have enabled its wide diffusion and appeal to different groups, not least policy makers. Audit can be understood as a 'boundary object' for different groups which express their interests in terms of its presumed potential (Bowker and Star, 2000). Nowhere in my work is it stated that all these different things which rapidly came to be called audits are literally 'identical' as Maltby suggests. The conceptualization is more that of 'family resemblance' in Wittgenstein's sense.

Beginning with sharp definitions of the object to be analysed damages any project of understanding how different things can come to be more similar over time. For example, much has been written about how medical 'audit' has shifted from the domain of local professional reflection to become part of the apparatus of something called 'medical governance.' The idea of 'audit' played a non-trivial role in this transformation. Or again, the similarity of financial and environmental audit, at a certain level of abstract representation, comes about as part of a complex dynamic within what Abbott (1988) has called the 'system of professions'. And taking Maltby's apparent counterexample of a "healthy food audit" for schools, I would hope for this to be effective in some fundamental dietary sense as she does. Yet, as such practices became enmeshed in wider policy discourses, as expert 'health auditors' are validated, and as formal systems, processes and health rankings are created, I would also expect these benefits to be at risk because of the very tendencies described in *The Audit Society* and elsewhere.

Defining practices in advance of analysis forecloses methodological sensitivity to the way boundaries between practical fields are often blurred, and how elements of practice travel across these boundaries and are re-assembled in new settings. Audit was, and is, a concept that is invoked and mobilised in the name of many different policy goals (efficiency, transparency, sustainability, teaching quality, risk management etc) but the defence against Maltby offered here is much broader than *The Audit Society*. It is a defence of a certain kind of methodological respect for the trajectory of language and ideas. This ought not to be dismissed as mere evasion.

Maltby's second strand of criticism concerns the empirical base of my work on audit and, in particular, the paucity of historical evidence which she claims to be highly damaging to the overall argument (although curiously Maltby's earlier demand to "define" audit is itself a historical!). To the charge of "perfunctory" history I plead guilty, and were *The Audit Society* intended as contribution to historical studies it would be a serious crime against scholarship. But rather than trying to do history and failing, it is perhaps better understood as a diagnosis of the present. I am entirely persuaded by Maltby's claim, based on her own work, that there is a strong independent tradition of public sector auditing which pre-dated and shaped so-called 'private' sector practices. I also concede that *The Audit Society* is vague on the precise timing and mechanisms of the diffusion of private auditing methods to the public sector. Yet, while we might agree

that the abstract elements of the financial audit ‘model’ can be derived from an earlier public propriety and stewardship conception, this only reinforces the interest in exploring how the elements of that model have evolved over time, and how the so-called ‘new public management’ reforms shaped and rationalized them yet further. The point is that ideas which get exported by domains may then be later re-imported in a new form via new carriers and agents.

On the more general “aversion to empirics” of which I stand accused, I would re-pose the question of gains and losses. *The Audit Society* does have an empirical base of sorts, although not one which is formalized methodologically. I did talk to many people in practice and continue to do so, but the enterprise is perhaps closer to a form of theory-building, a synthesis and meta-analysis of other studies which builds incremental insights. I began with a puzzle about how UK and Danish accountants were moving into the field of environmental auditing, and then the question became a much bigger one about the apparent expansion of auditing per se. A more traditional empirical project would never have been able, or even permitted, to ask this question. Was it so wrong to attempt a project at a level of generality where its execution was bound to be flawed and where depth was traded for scope, but where a new field of studies might be created? Or is this, as Maltby ungenerously claims, a simple case of intellectual aversion to doing proper work?

Maltby seems to enjoy Hood’s comments that *The Audit Society* is a “text book case of selection on the dependent variable”. But my question to Maltby (which I have also posed to Hood on several occasions) is this: how do researchers know which phenomena as ‘variables’ to focus on and to explore their dependencies? How does a collection of practices become variables worthy of research enquiry? Whatever its weaknesses, *The Audit Society* is not an imperfect and confused study of empirical variation; it was intended to create a new kind of attention to auditing and its effects. Audit practice may not even be the appropriate ‘dependent’ variable if the most interesting effects are to be found in an ‘audit mentality’. The latter is, of course, much harder to code for comparative empirical work.

The Audit Society may well be a case of ‘UK exceptionalism’ (Moran, 2003) with limited comparative appeal, but the focus on audit as a technology for representing problems and solutions is one that many scholars have found useful and is consistent with work in science and technology studies (STS). The language of ‘dependent’ and independent variables is provisionally useful for a certain kind of political scientist, but is also epistemologically inadequate; auditing practices broadly understood are transformative, and relations of dependence and independence are emergent rather than fixed and given.

Finally, Maltby complains that in *The Audit Society* and elsewhere, real users of audits, the public at large, are voiceless and forgotten at the expense of an ultimately uncritical focus on auditing as a “technology of representation”. Indeed, she suggests that the focus on the technology of audit provides implicit support for those who would wish to remain unaccountable, and renders the “public at large” a rather “drab lot”. Even leaving aside concerns about Maltby’s somewhat uncritical appeal to something called the ‘public’, the objection is curious. Surely an understanding of the dynamic by which

practices expand and reshape themselves provides a basis for users and designers of audit practices to avoid illusions of control and comfort? *The Audit Society* also suggests how ‘real users’ are reduced to highly abstract reference points in a self-referential dynamic and makes some comments about democracy and dialogue (pp.127-8) which, while brief, Maltby has chosen to ignore.

Efforts in the social and environmental auditing space to involve the publics and to create actionable discomfort are admirable attempts to get beyond a closed world of audit, but hardly provide a knock-out blow to the arguments in *The Audit Society*. Maltby also chooses to read the long quotation from the end of a 2003 paper as a rage against a lost Eden of unaccountability, when it might equally support the kind of accountability which would be effective and targeted, rather than cosmetic and costly. So, I fail to see how *The Audit Society* is only “on the side of the auditee” or, more importantly, that there are two clear sides – the public and auditees.

In the end, it is Maltby’s evident disdain for *The Audit Society* which is the most prominent feature of her essay. The book is flawed, there is no doubt of that, and there may be an excess of polemic in parts but, again, do the gains in terms of an imaginative stimulus for looking at auditing in a new way outweigh these weaknesses? Maltby’s style, as much as her arguments, suggest they do not. It is a style which actually prevents her from directly addressing the proposition in the title of her essay, and results in an extended sneer all the way to its concluding two sentences: “*The Audit Society* and its progeny, Power’s own papers and wails of unhappy academics and doctors and civil servants, are ultimately not a protest about the creation of an iron cage round society. They are a stifled chorus of fury at being made accountable”.

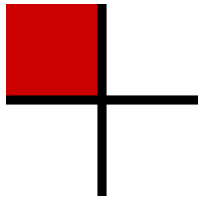
I re-read *The Audit Society* in order to identify the anger which Maltby lays at my door. If anything, the ‘audit society’ thesis is articulated rather moderately as the risk that the tail of audit may wag the dog of accountability. Sustaining an awareness of how that relationship may become unbalanced is hardly very controversial, and speaks much more directly than Maltby herself to the concerns of a general public. In the end, the only “chorus of fury” I can find, not at all stifled, is in the tone and language of Maltby’s essay itself.

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Baffling Bill McKelvey, the Commensurability Kid

Norman Jackson and Pippa Carter

abstract

This paper offers a critical reading of the influential position paper, McKelvey (2003), inspired by that paper's claim to refute paradigm incommensurability. However, it does not seek to address, examine or evaluate the arguments presented by McKelvey, but rather to focus on issues of scholarship raised by his paper. That focus reveals an alarming array of claims, assertions, biases, misperceptions and misreadings, which render any substantive conceptual content extremely difficult to evaluate. This paper illustrates the importance of undertaking such critical readings in the general process of evaluation of contributions to the debates and controversies so prevalent in the field of Organisation Studies.

Introduction

The debate in Organisation Studies (OS) about knowledge paradigms and paradigm incommensurability continues unabated, yet also continues to generate more heat than light. A major problem is the imprecision with which these issues are addressed. As regards the term 'paradigm', there are three prominent uses of the word: an ordinary language one, the 'Kuhnian' usage, and the use in reference to Burrell and Morgan. The ordinary language sense of the term, when used by academics, may include, or subsume, either or both the Kuhnian and the Burrell and Morgan senses (but may not). The term 'incommensurability' should present less of a problem since its meaning is quite specific: lack of a common measure. However, the stock refutation of paradigm incommensurability is based on the argument that there exists a commonality of language among members of a particular field – in this case, OS. This refutation ranges from the assertion that students of organisation share a common vocabulary (signifiers), to the more significant assertion that they share common meanings (signifieds), though perhaps the most popular stance is somewhere in the middle. In this case, any perceived or apparent incommensurability will be resolved over time by further discussion and/or demonstration.

One recent addition to the above debate is Westwood and Clegg (eds.) *Debating Organization: Point-Counterpoint in Organization Studies* (2003), which seeks to update the articulation of 'paradigm differences'. The scare quotes are in the original (see the jacket notes), as the editors state that they do not themselves believe in the existence of paradigms (p.24). It is from this collection of relevant papers that we draw

the inspiration for our own paper here, and in what follows all references which contain only a page number refer to Westwood and Clegg (2003). One notable contribution is that of McKelvey, *'From Fields to Science: Can Organization Studies make the transition?'* (pp. 47-73), which, we would suggest, is an exemplar (a paradigm case even) of the predominance of heat over light with regard to paradigm incommensurability.

Although to advocate paradigm incommensurability is, in itself, unpopular amongst students of organisation, even its staunchest critics tend to acknowledge its potential for resisting the inevitable imperialistic tendency of the dominant perspective – that is, a perspective rooted in the assumption that production of knowledge about organisation(s) should serve the interests of Capital – while, at the same time, by implication at least, denying that such resistance, and protection of other approaches, is necessary. And it is just this authoritarian tendency that is so well represented, if not explicit, in McKelvey's position paper. Indeed, as the editors note in their commentary, he

would write the epitaph of most current OS researchers by arguing that they are, for the most part, classical positivists, flawed logical empiricists, or relativists, and that each of these epistemologies has no legitimate philosophical basis, *and thus should be terminated with prejudice.* (p.25, our emphasis)

Be afraid, be very afraid of Bill McKelvey!

What's All This About Incommensurability?

Before examining the 'rationale' for McKelvey's proposed 'reign of terror', however, it is necessary to return to the matter of the imprecision in the usage of the terms 'paradigm', 'incommensurability' and, of course, 'paradigm incommensurability'.

The word 'paradigm', used in an ordinary language sense, can evoke synonyms such as 'map' (see, for example, Burrell and Morgan, 1979: xi), representation, exemplar, (or, indeed, possibly any of Kuhn's supposed 22, or thereabouts, different ways of using it). In other words, a paradigm is a model. There are two characteristics that all models share: they must contain less information than that which they model ('the real world') and they are 'good' or 'bad' as models (of 'the real world') only insofar as they serve the purposes of the person creating/using the model (see, for example, Ashby, 1970). What is *not* contained within the concept of 'paradigm' (model) is an *inevitable* incommensurability with other models. *Incommensurability, if it exists, is a 'design feature' of the model.* Thus, to speak of the incommensurability of an unspecified sense of 'paradigm' is meaningless, not to mention irrelevant. Even if we are specific that our paradigms are knowledge paradigms, and indeed knowledge paradigms of organisation theories, incommensurability is not a given quality. For incommensurability to be a factor, further specification is necessary.

The second type of usage of the term 'paradigm' is that of Kuhn, however many ways he does it. This need not detain us in this context, because Kuhnian paradigms relate to natural science – specifically, to physics – and not, as he himself notes, to social science

(Kuhn 1970, Westwood and Clegg, 2003: 24). However, it is worth stressing one point here: Kuhn's model includes a time dimension which facilitates the means of resolution of any apparent incommensurability in natural science paradigms, by the elimination of the paradigm that has the lesser explanatory power and the less powerful support.

The third kind of usage of the term is that employed in Burrell and Morgan's paradigm model – no, this is not a tautology, and yes, it is a model of a model! When writers in OS invoke the concept of 'paradigm', it is predominantly Burrell and Morgan's model that they refer to, even if not explicitly. This is hardly surprising as it is *the* model that is explicitly about knowledge of organisation(s). This model differs in a number of ways from that of Kuhn, but one especially important difference is that it does not have a time dimension over which incommensurability can be resolved (see also Jackson and Carter 1991). In other words, incommensurability is built into their model. Like it or not, it is there, and it is incumbent on users of the model to recognise this. Sadly, many users do not (want to) recognise it, and seek to refute the model's incommensurability – and a particularly common vehicle for this, though wholly inappropriate, is to invoke Kuhn's model as a refutation of Burrell and Morgan's.

What we are suggesting here is that, when speaking of (knowledge) paradigms, and especially of paradigm incommensurability, in OS, it is necessary to be specific about which sense of paradigm is being used, and not to conflate the various senses.

As regards the term 'incommensurability', this is an issue of common measure. In the ordinary language sense of paradigm, incommensurability may or may not be present. With respect to Kuhn's usage, incommensurability between any two paradigms is a 'pathology' which will be 'cured' over time, when, for example, one dies out and the other becomes dominant. In Burrell and Morgan's usage, incommensurability is built into their model as an 'inherent' feature, and no mechanism for resolution, over time or, indeed, any other way, is available. Knowledge paradigms are, therefore, commensurable in so far as they share common understandings of their subject matter – in Burrell and Morgan's model, along the two axes that they specify. Thus, to be commensurable, it is not sufficient that different paradigms share a common vocabulary (signifiers), they must also share common meanings (signifieds). Unfortunately, although the latter is generally claimed to be the case by those of an authoritarian disposition, such sharing of meanings cannot just be asserted by one of the proponents in the so-called paradigm war.

McKelvey On Paradigm Incommensurability

This paper offers a close reading of its subject text, contextualising that process in Richards: *Practical Criticism* (1929). One feature of his readers that Richards notes is 'immaturity'. Indeed, he laments that

... an educational and social system which encourages a large proportion of its most endowed and favoured products to remain children *permanently* is exposing itself to danger. (Richards [1929] 1964:311, emphasis in original)

having already noted that age and experience will not necessarily improve this immaturity in the reader. How much more at risk of continued immaturity might students of OS and Management be, who study a ‘discipline’ so replete with ideological biases, simplistic and unilateral explanations and justifications of organisations? It is precisely this ‘perpetuation of immaturity’ (Sievers, 1993)¹ that is being encouraged by McKelvey.

McKelvey’s paper is, ostensibly, a plea that OS should be turned from a disparate collection of competing opinions into a unified body of knowledge: a science. To do this we must adopt a singular understanding of organisation(s) derived from Complexity Science. So far, so good. To any student of the debates in OS such a position (as one of many) is instantly recognisable. In this case, however, it is an example of the authoritarian view of OS. McKelvey has the true understanding, and everyone else is just wrong. Thankfully, he is ready to explain it to us, to bring us the ‘good news’ – see Donaldson (1985) for the same sort of approach 20-odd years ago. It may be harmless enough for a knowledgeable reader, but, for Richards’s ‘gullible’ readers – who, some might suggest, may even be the predominant type among students of organisation and management – such didactic certainty may well be dangerous. And why might McKelvey be so concerned to be able to convert OS into a science, *as he understands the term*? It’s because, following Pfeffer

multiparadigmatic disciplines are held in low status by members of other sciences in universities when it comes to funding and salaries. (p. 48)

In other words, the whole point is not to improve its ability to furnish greater insights into nature’s secrets, but to impress the neighbours, and to get more money! Clearly, for McKelvey to be correct, there can be no such thing as incommensurable paradigms, with their representation of a plurality of understandings about organisation(s). However, this is no problem for McKelvey, who deals with the question thus:

The incommensurability thesis is *self-refuting*, as follows. If we know enough about the terms of one paradigm to say that they are incommensurable with the terms of another paradigm, then we know enough about the terms to render their incommensurability false. (p. 49, our emphasis)

This is, as previously noted, not an uncommon point made against the incommensurability thesis, so not particularly original (so some supporting references might have been both useful to McKelvey, and in order) and he seems to think that this statement is all that is needed to show how wrong any dissent from the position would be. Before unpacking the statement, let us note the background to it that McKelvey supplies. To help us to grasp what he is saying, he gives us an example of a self-refuting statement: “All generalisations are false” (*ibid.*). This statement is paradoxical. It is syntactically correct, but semantically false. It is self-refuting because it is, in itself, a generalisation and, therefore, according to the statement itself, must be false.

However, when we return to McKelvey’s application of this to the incommensurability thesis, it becomes clear that this passage does not say what he thinks it says. What it does say is that his proposed refutation of the incommensurability thesis is a self-

1 In Sievers this phrase refers, not inappropriately, to a chapter on ‘leadership’.

refuting statement – in his own terms, and like his introductory example, syntactically correct, but semantically false! It is a sentence, but it is logically incorrect. What he actually says – as opposed to what he thinks he says – is that the refutation of the incommensurability thesis on the basis of common language, (both signifiers and signifieds), is false. Ironically, if he had simply said that the incommensurability thesis was wrong, this would still be an assertive statement but it would not be as problematic to his cause. However, this style is typical of McKelvey's looseness of argument, his desire to blind us with his technical philosophical competence, his condescension towards all those who disagree with him. Still, we feel sure that this is just a careless mistake on McKelvey's part, so will pass over it. Though it does show how problematic language is – a phenomenon that McKelvey ignores at *our* peril.

More problematic than this is McKelvey's use of the word 'paradigm'. He is not at all specific about what he is referring to in his usage of this concept. We know, or can assume, that he is speaking of knowledge paradigms in OS and, therefore, it would not be unreasonable to infer that his usage includes the concept of paradigm as used by Burrell and Morgan, (with its inherent incommensurability), since that is precisely their sphere of interest. Indeed, his other references to their work do indicate that they come under his umbrella concept of paradigms. Examining what we think McKelvey intended to say, he asserts: if we know enough to be able to recognise that one paradigm is incommensurable with another, different, paradigm, then we know enough to make them commensurable. The problem here is picked up by the editors, though, we would suggest, inappropriately. They paraphrase McKelvey thus:

If paradigms were truly incommensurable one would not be able to talk about them simultaneously. (p. 24)

In other words, one would not be able to generate commensurable signifiers. But clearly we can, since proponents of different paradigms do have debates, conversations and so on, about their subject. We take this argument to be similar to, for example, Hassard's (1988) position on incommensurability. However, we think that McKelvey is saying something else: *'if we understand, or comprehend, how another argument is different to ours, or to some third, or fourth, argument, then we can resolve any apparent contradictions'*. This suggests that McKelvey confuses and conflates comprehension and belief. Indeed, his editors also point out that incommensurability is an issue of belief, not comprehension (p. 24). There is, of course, no reason at all why we should not be able to understand something at the same time as not believing it. But, for McKelvey, to understand is to believe, and this is the foundation of his 'refutation' of the incommensurability thesis. If understanding does not entail belief, then the argument collapses.

So what are the (knowledge) paradigms the incommensurability, or otherwise, of which so concerns McKelvey? He certainly uses the concept 'paradigm' in what we have described as an ordinary language sense, but, as we have suggested, incommensurability is not automatically relevant in such usages. He also makes much reference to Kuhn's paradigms, which could also be seen as irrelevant in this context, as also noted by the editors (p. 24). Although Kuhn's model was inspired in part by his experience of working among social scientists (Kuhn, 1970: vii-viii), he is, as we have already pointed out, quite specific that his model deals exclusively with physical sciences, although he

hoped, in the future, to be able to extend it to other areas. Still, Kuhn’s model, as it stands, applies only to physics. Of course, there is nothing to prevent McKelvey showing by argument and analogy that Kuhn’s model is relevant, or not, to OS, but he does not do this. As regards Burrell and Morgan’s model, given that McKelvey is writing about OS and that their model is, understandably, the ‘premier’ paradigm model in that field – love it or hate it, agree with them or disagree – we might well anticipate that McKelvey would have something to say about it. But no, they are mentioned only in passing, and so we can say nothing about how, for example, McKelvey might refute the incommensurability of, say, Determinism and Voluntarism.

Let us parenthesise McKelvey’s lack of specificity in these respects, however. Notwithstanding such looseness of argument, the more significant problem with his proposal is that incommensurability is an issue of belief, not one of truth. Where does that leave him? For there to be an absence of incommensurable beliefs in OS, one of two things would have to happen. Either disbelief must somehow become unimportant, as occurs in Kuhn’s model, for example, when the ‘Old Believers’ die out. Or belief *per se* must be purged from OS, as, for example, conventional positivistic science might advocate. It would appear that the former case would be the one to elicit McKelvey’s support – via his extreme prejudice, perhaps! This inference seems appropriate because belief clearly plays a part in his model of OS. For example, there is McKelvey’s view that (good) OS must be able to furnish “concrete problem solutions” (CPSs) for such constituencies as “owners, CEOs and managers, aimed at economic rents (above industry-average profits)” (p. 64). So the purpose of OS is not about unlocking nature’s secrets, it is, *inter alia*, about the generation of private wealth – a belief rather than a truth, is it not? We will return to this point shortly.

The McKelvey Matrix

This is not the only substantive problem with the paper. For example, McKelvey offers a model of his understanding of the relationship between natural science and contra-science positions, with regard to ontology and epistemology (p. 57).

| | | Epistemology | |
|----------|----------------|----------------|----------------|
| | | Normal Science | Contra-science |
| Ontology | Normal science | 1 | 2 |
| | Contra-science | 3 | 4 |

His *only* comment on it is this:

The paradigm war {references here are to Pfeffer, Perrow and Van Maanen} pits 1 against 4. This debate is stalled. No one advocates 2. *Only 3 is left.* (p.57, emphasis in original)

However, it is worth examining this four cell matrix further.

What McKelvey asserts here is that the two cells which exhibit consistency are unable to resolve their differences (of belief) – anyone else might describe this as incommensurability. Hence, he implies, these two have to be set aside, since, presumably, there is no means of synthesising them into the meta-paradigm that he advocates as necessary, not just for OS to make the transition into a science, but also for the very health of the field, and its members. What is not in question is whether one of these two cells might be ‘correct’, represent the ‘truth’. The assertion that no-one supports a position based on a realist ontology and a relativist epistemology – that is, that the world exists independently of our awareness of it but we can only gain a partial knowledge of it, through, for example, narratives, conversations, ethnographies, and so on – seems to be a highly contentious one. Indeed, this appears to be a not uncommon position and McKelvey simply ignores, or dismisses, the New Physics, say, not to mention Critical Realism, Critical Theory, Freudian Psychoanalysis, amongst others that could be argued to fit into this cell.

McKelvey makes much use of the work of Donald T. Campbell to reinforce his position, although, as an ontological realist and, at least *pro tem*, an epistemological relativist, Campbell would appear to fit neatly into McKelvey’s cell 2. A further point worth noting in the context of Campbell’s work, with reference to McKelvey’s purging by fiat of variety in approaches to understanding organisation(s), is that this appears to violate the necessary conditions, delineated in Campbell’s seminal 1959 paper, for a *science* of social groups (see especially pp 174-5).

The one cell that is worth our attention, according to McKelvey, is that based on a relativist/contra-science ontology and a realist/normal science epistemology. So what does this mean? He claims to be inspired to this view by ‘complexity science’ and ‘postmodernism/poststructuralism’. In itself, such a rapprochement between poststructuralism/postmodernism and complexity science/chaos theory is not exactly novel, as we have previously noted, although McKelvey does not address previous examples. A particularly noteworthy case is the dialogue between Deleuze and Guattari and Prigogine and Stengers, resulting in wide-ranging and multi-faceted mutual influences (see, for example, extensive notes in Massumi (1992), and further comment in Carter and Jackson (2004)). However, McKelvey wants us to adopt an ontology that sees the world as socially constructed, observer dependent, of dubious transcendence, about which we can produce science-like knowledge. What he claims he is doing is “‘marrying’ normal science epistemology with postmodern ontology” (p.56, quotation marks in original). But a health warning is also appropriate here: we shall turn in a moment to what McKelvey means by postmodernism/poststructuralism.

Prima facie, what is asserted here is that entities that may have no existence, or have an existence that has yet to be determined, and about which there is (and can be, from a relativist position) no agreement in the subject area, can be studied in such a way that

absolute and factual knowledge of them can be gained (notwithstanding that a relativist/contra-science ontology might have problems with any such knowledge claims). You can use scientific, or quasi-scientific, methods to gain knowledge about a world that is, purportedly, not amenable to – not even knowable through – science, according to the contra-science ontology. If we only consider the matter in the context of language, (since that is a significant context, in various ways, to McKelvey), this approach advocates an epistemology based on a correspondence view of language in relation to an ontology firmly grounded in a semiotic approach to language.

McKelvey On Postmodernism

In order to try to examine what McKelvey means by all this, it is relevant to consider his use of the terms ‘postmodernism’ and ‘poststructuralism’, terms which he uses interchangeably, (though some might want to disagree with that interchangeability), and ‘philosophies’ that he wishes to embrace – up to a point. In practice, however, what McKelvey means by the terms is definitely an idiosyncratic interpretation. He acknowledges basing his usage on the work of Cilliers (1998), but seems to take a rather unreflexive and loose approach to Cilliers’s arguments. Cilliers seeks to show how postmodernism can be seen as relevant to understanding, and consistent with, Complexity. Acknowledging the problem of the distinctions between postmodernism and poststructuralism, he constructs his argument on the basis of what he calls the early works of Derrida (1973, 1976, 1978), and Lyotard: *The Postmodern Condition* (1984). The flavour of his approach is perhaps captured in his assertion that anyone who thinks that Derrida is a relativist is ignorant (Cilliers 1998:21-22).

From this, in McKelvey, we get a dichotomisation of postmodernist/poststructuralist (which is identified depends on which McKelvey paper you are reading; see, for example, Maguire, McKelvey, Mirabeau and Oztas, 2006) writers into a ‘responsible core’ (p.66) – mainly those identified by Cilliers, namely Derrida, Baudrillard and Lyotard – and a generally unspecified, apparently irresponsible, rest. From a wide range of writers, and texts, which might be seen generally as contributing to the ‘basic’ ideas of postmodernism and poststructuralism, McKelvey (via Cilliers, but without Cilliers’s discussion of his choice) identifies a tiny proportion which he declares to constitute what is worthy in those areas. This is also to ignore that these very writers have produced other works that, clearly, would not fit into this appellation of ‘responsible core’: *Specters of Marx* (Derrida, 1994), for example, or what many argue is Lyotard’s greatest work, *The Differend* (1988), which contains one of the most powerful pro-incommensurability arguments to be found anywhere, in his discussion of the characteristics of victims (*ibid.*: 3ff).

And what is it that makes these lucky writers ‘responsible’, ‘worthy’? McKelvey rejects all ‘skeptical’ (radically relativist, according to Cilliers) postmodernists-/poststructuralists, in favour of ‘affirmative’ ones, dismisses the ‘over-zealous’ – not that we are treated to explanations of these terms, let alone examples – and plumps for those, be they postmodernists or poststructuralists, that Cilliers identifies as significant (to him, Cilliers). What attracts Cilliers to these writers is that he feels able to classify

them as realists. However, McKelvey, although claiming to follow Cilliers, is attracted by the exact opposite, their relativism:

[But] relativist-based postmodernism is misguided in its epistemology, since it offers little by way of justification logic. Still, *its ontology is correct*. (p.56, emphasis in original)

To muddy the waters even further, in his conclusion McKelvey notes:

[Furthermore] the more serious elements of postmodernism {is this the same as “responsible”, that which McKelvey wants to embrace, or is “serious” different?} rest on Kuhnian (1962) relativism, a perspective also buried three decades ago by most respectable {?} philosophers. (p.66)

Are you keeping up?

McKelvey’s Rhetorical Tools

It is also pertinent here to illustrate McKelvey’s general approach to dealing with these contentious issues through his very varied use of assertion and other rhetorical tools, which, we would suggest, is analogous to Richards’s (1964 [1929]) general arguments about the pernicious effects of using authority as a substitute for analysis and argument. And, of course, assertions based on authority evoke the syntactic and semantic relationship between ‘authority’ and ‘authoritarianism’. Firstly, there are cases of straightforward assertion. A good example of this is the description of the ‘end-users’ of OS knowledge:

(T)he success of organization studies depends on bringing more findings to constituents that have the “reliability of use” value people are accustomed to receive from effective sciences. (p.64, quotation marks in original)

This could be seen as an unusual view of what the production of scientific knowledge is about. It appears to be based on the assumption that such knowledge is produced solely with the end-users in mind, contra the more usual view that, although such knowledge may have end-users, that is not necessarily a primary motivation in the actual knowledge production. By McKelvey’s reasoning, if science can produce improved ways of conducting genocide that satisfy the ‘need’ for CPSs of the managers of death camps, as happened under the Nazi regime in the Second World War, then that science gains legitimacy – not an irrelevant example in light of a point to be raised in a moment. McKelvey continues:

Thus, if organization studies were to produce findings of *epistemological quality* for the following external constituencies, its legitimacy would be greatly enhanced... (p. 64, emphasis in original)

What is meant by epistemological quality? Who defines the yardsticks against which such quality might be measured, on what basis? What is meant by legitimacy? Is it rightness/wrongness? Is it usability, independent of rightness/wrongness? Enhanced in what way?

Then we get the list of constituents. In “descending order” (editors’ note p. 46), we get: owners, CEOs and managers, for whom OS knowledge (in the form of concrete problem solutions) should be “aimed at economic rents (above industry-average

profits)”; employees, for whom it should aim at “employment, careers, livelihood”; constituencies worrying about externalities, such as “broader societal policy and environmental issues”; customers, vis à vis the quality and price of outputs; consultants, to be provided with CPSs that they can take to clients (p. 64). Even at first glance it becomes obvious that CPSs for some of these groups are very likely to be in direct conflict with CPSs for other groups. McKelvey himself acknowledges this, but not to worry – he deals with it thus:

There is a zero-sum game among the aims of the first four groups – as each benefits the others are apt to suffer. As each user community is more clearly served, however, more globally optimal CPSs become more salient and possible. (*ibid.*)

This statement, concluding the relevant section, must fall into a special category of vague assertion! Not to mention that ‘zero-sum game’ would seem to imply incommensurable objectives among these ‘constituents’.

This matter of the end-users is really just straightforward unargued and unjustified assertion, but the range of assertive techniques in this paper is much more varied than this. There is, for example, what can be called affirmative assertion. A good example is the demand to adopt a/the ‘responsible core’ of postmodernists (or poststructuralists in Maguire, McKelvey, Mirabeau and Oztas 2006). Note 2 (p. 66) refers to a list of authors on page 48. The remarks in the note are not entirely consistent with the text referred to, but it seems that Saussure, Derrida, Baudrillard, Culler and Lyotard are ‘responsible core’ postmodernists and/or poststructuralists, though possibly only in the particular works cited. How this list is arrived at, and what exactly constitutes ‘responsibility’ in this context, we are not told. What is obvious, however, is that the denotation of ‘responsible’ is meant positively – and, by the same token, all the rest (and not just the others in the list, who are significant enough, it seems, to be named and shamed), are apparently irresponsible. Of course, this also begs the question of ‘responsible to whom?’

Then there are the cases of negative assertion. We pick just two examples, both relevant to relativism. On page 56 we get “*Relativism* now receives virtually no support by modern philosophers” (emphasis in original). So those who do support it are, presumably, minimally, either not modern, or not philosophers. The other example is the recurring connection of the words ‘relativism’ and ‘rhetoric’ (e.g., pp. 56, 57). The word ‘rhetoric’ here clearly implies something pejorative – the dishonest use of language – and the phrase is apparently used to imply that (irresponsible? or all?) relativists cannot produce a reasonable or justifiable argument to support their view, but merely use words to trap the gullible.

More Of McKelvey’s Rhetorical Tools

There is also another kind of negative rhetorical tool in McKelvey’s paper. This amounts to casting serious doubt on someone’s work through a dismissive misinterpretation (wilful or otherwise). One example is the following comment in note 9 (p. 67), (which refers to text on p. 57, although it is, again, not clear what the connection between the text that is noted and the note itself is)

Chia (1996) centres much of his discussion of epistemology on the reflexivity issue. I do not disagree that reflexivity is present in organizations; I am just not sure it counts for very much. If our “science” is so reflexive – *meaning that scientific findings feed back to managers to affect their behaviour and organizational functioning in ways that alter the phenomena we study* – why do we need all those consultants to put academic ideas into practice? Managers would read our journals, put the ideas into practice, and save billions. OD would be history! (p. 67, quotation marks in original, emphasis added)

The very kindest interpretation of this would be that McKelvey does not understand the nature and role of reflexivity in Chia’s work, though it could be a deliberate misinterpretation. What Chia says, for example, is:

[Thus] entry into reflexive awareness brings with it a realization of the essentially groundless and undecidable character of *representational statements*. (Chia, 1996: 9, emphasis added)

and:

What constituted the initial “reflexive turn” in *academic theorizing* resulted from a heightened *self-awareness* associated with the increasing realization that the researcher/theorist plays an active role in constructing the very reality he/she is attempting to investigate.... The hitherto privileged objectivist status of the observer/researcher/theorist has since been rendered problematic and social scientists including organization theorists are now increasingly called upon to *reflexively justify the knowledge claims they make*. (Chia, 1996: 79-80, emphases added)

In other words, according to Chia (and this is not an unusual interpretation of the term), reflexivity is an issue for researchers, not for managers.

There are other examples of this derogatory technique to be found. One example, in note 2 (p. 66), which refers to text, or rather a list of references, on page 48, and is a continuation of the assertion that there is a ‘responsible core’ of postmodernists, is the following:

The antiscience group is prone to make accusations such as... Latour’s (1988) attack against Pasteur’s modernism that ignores the countless millions of lives Pasteur saved as a result of his modernist scientific and political organizing efforts.

However, what Latour says is:

[N]o one – except extreme cynics – can doubt the value of Pasteur’s discoveries to medicine. All of the other technological conquests have their embittered critics and malcontents... but to prevent children from dying from terrible diseases has never been seen as anything other than an advantage... [I]t seems impossible to deny that Pasteur’s rapid successes were due to application at last of scientific method in an area that had been left too long to people groping in the dark. (Latour, 1988: 8)

Latour’s point, using Pasteur as an example, is that science does not progress in the orderly, logical and controlled way that is so often portrayed, but is a product of a large network of often conflicting influences. O’Doherty (2007: 860) comments on McKelvey’s attack on Latour that “the idea that Latour’s *The Pasteurisation of France* intends or provides an ‘attack against Pasteur’s modernism’ neither inspires confidence that Latour has been particularly well read or understood”.

Another, perhaps even more disturbing, example in the same note is:

The antiscience group is prone to make accusations such as Burrell's (1996: 656) assertion that modernist science (epitomized by Einstein the Zionist who was invited to be the President of Israel) caused the holocaust of 6 million Jews....

This is not what Burrell says, and it cannot reasonably be inferred from what he does say. For example, he says:

[In the 1960s] few saw the defining organizational form of the whole twentieth century to be the death camps of Auschwitz. *Modernism* is about the death camps in a fairly uncontentious way even though its apologists seek to distance the likes of Auschwitz from the achievements of the *modernist* society, based as it is supposedly upon critical enquiry and the pursuit of truth. (Burrell, 1996: 656, emphases added)

Burrell's point is a not unfamiliar one about the impact of modernism on the twentieth century (see also Burrell, 1994, among many other commentators), and to interpret this as saying that modernist science caused the Holocaust is, to say the least, a perverse reading. It is noteworthy that, in this entire section of his paper, Burrell makes no mention of modernist science, or of Einstein, or of Zionism or the Presidency of Israel, let alone characterising Einstein as the epitome of modern science. Perhaps more importantly, McKelvey's imputation that these comments attribute causality is misleading and mischievous. But there seems to be more to this calumny on Burrell's paper than we have so far highlighted. We would suggest that a naïve reader of this quotation would imagine that there is some relevance for modernism that Einstein was Jewish and could have been President of Israel. This comes very close to implying that to be anti-science is also to be anti-Semitic. However, one of the best illustrations of the contradictory, and even vacuous, character of McKelvey's position is the concluding point of this long footnote:

There is also considerable evidence that postmodernism was a convenient, self-indulgent philosophy promulgated by godfathers who were closet Nazis. (p. 66)

Could he be inferring that those who use postmodernist knowledge are somehow expressing some agreement with Nazism? But just a moment! McKelvey himself uses postmodernism. Indeed, he says "*its ontology is correct*" (p. 56, his emphasis). So is he really declaring his own political position? Or was it some other McKelvey who wrote the quotation above? How does such confusion help to explain anything? What are we, the inquiring readers, meant to learn from all this?

McKelvey's Claims

Yet another rhetorical tool in McKelvey's paper is based on absence. It is conventional in works of scholarship to acknowledge one's debt to the authors whose work informs your own, and also to acknowledge instances where your usage of their work differs from their own usage of it. While McKelvey is assiduous in the first convention, the second is notably absent in this paper. The classic example of this is the use he makes of the work of Cilliers – who, as it happens, we would suggest, in effect belongs in McKelvey's normal science ontology/contra-science epistemology cell 2, that he asserts is supported by no-one. Cilliers adopts, in practice, certain works by Derrida and Lyotard (Baudrillard is mentioned only passing), as his 'responsible core' of

postmodernists on the basis that he construes their work, at least, as not relativistic, at most, as realist. What McKelvey wants from that is that they should be responsible relativists. He continues to assert that his usage is derived from Cilliers, but that usage tends in precisely the opposite direction to the arguments presented by Cilliers. This also leads McKelvey into various infelicitous attributions, such as that which designates his 'responsible core' of postmodernist thinkers as belonging to OS (p. 48).

In general, McKelvey's approach to referencing his understanding of postmodernism is difficult to follow, not least because his approach often amounts to not much more than name-dropping. Take, for example, the case of Lyotard: we know that Lyotard is one of the 'responsible' postmodernists (in the case of *The Postmodern Condition*, at least), and that McKelvey is following Cilliers's use of Lyotard, which is extensive. It is strange, however, that McKelvey does not feel the need to address Lyotard's statement, in the Introduction to *The Postmodern Condition*, and cited in Cilliers (p. 114):

Postmodern knowledge is not simply a tool of the authorities; it refines our sensitivity to differences and *reinforces our ability to tolerate the incommensurable*. (Lyotard, 1984: xxv, our emphasis)

As noted, one can point to the same problem with McKelvey's use of Cilliers. Though, as it happens, Cilliers's use of both Derrida and Lyotard is also quite problematic. We referred earlier to the comment by Cilliers that only the ignorant could see Derrida's early work as relativistic. What Cilliers says, by implication, is that only an ignorant person could do a deconstructive reading of the texts by Derrida that he has selected and conclude that they are relativistic. This seems somewhat to undermine his use of Derrida, if he is denying the possibility of what Derrida himself insists upon! Cilliers's 'refutation' of relativism, which he understands as 'anything goes', is based on Lyotard's *The Postmodern Condition*. The idea that relativism can be captured in the phrase 'anything goes' appears to be based on a characterisation of relativism where 'each individual has only herself as a point of reference with no way of grounding any knowledge objectively' (Cilliers, 1998: 115). This characterisation, as Cilliers notes, is rejected by Lyotard – however, this is, by any standard, a highly simplistic version of relativism. For Lyotard, 'individuals' form part of local 'communities' in which narrative understandings of knowledge of a plurality of 'smaller stories' function well within the particular context where they apply (Cilliers, 1998: 114) – what some, indeed, might call knowledge paradigms. And, if there are knowledge paradigms, however they are called, then there is relativism. All this makes McKelvey's wholesale adoption of Cilliers's work increasingly bizarre.

There is another kind of absence in McKelvey's work, the making of large claims (such as that to 'refute' paradigm incommensurability) without clear evidence, argument or explanation. Although our primary concern in this paper has been to examine this claim to refute paradigm incommensurability, in this process it became appropriate, on occasion, to spread the net somewhat wider in respect of McKelvey's work. One statement that particularly caught the eye was the claim to have modernised Ashby's Law of Requisite Variety (Maguire, McKelvey, Mirabeau and Otzas, 2006: 202). Such a feat would be of immediate interest to anyone of the 'contra-science' persuasion who adopts a systems-theoretic approach, not least because it was, and is, precisely the over-attenuation of organisational variety, characteristic of scientific approaches to studying

organisations, that has encouraged the proliferation of alternative modes of organisational analysis (so deplored by McKelvey). It seemed that the claim to have modernised such an iconic Law was worthy of further investigation.

It is stated in the 2006 paper that the claim follows Allen's (2001) "extension of Ashby's Law to develop the Law of Excess Variety" (*ibid.*). However, Allen makes no such claim in his paper, and there is no reference to the Law of Excess Variety. What Allen says is:

In dealing with a changing environment... we find a "law of excess diversity" in which system survival in the long term requires more underlying diversity than would be considered requisite at any time. (2001:40, quotation marks and lower case in original)

It is not relevant here whether or not Allen has indeed developed a Law of Excess Variety in his paper, though he himself makes no such claim. What is relevant is that a passing reference to a "law of excess diversity" – (it is not clear why Allen uses double quotation marks – is he acknowledging a borrowed expression, or, more likely, is he using the expression metaphorically, for example, to emphasise 'law' rather than 'Law'?) – has been metamorphosed by McKelvey into the 'Law of Excess Variety'. While it may well be reasonable to suggest that diversity is the same as variety, what is more questionable is McKelvey's transmutation of Allen's comment into a (capitalised) Law.

Turning attention to McKelvey's claims for his own work in this respect, comment in the 2006 paper, beyond the actual claim, is passing, but McKelvey cites as a source for the claim another of his papers, Boisot and McKelvey (2006). Yet neither does this paper furnish an explanation of how the authors have 'modernised' Ashby's Law. What there is, however, is a reference back to McKelvey and Boisot (2003) – a conference paper which is apparently so far unpublished, although McKelvey (2007) claims that he is publishing, in 2008, a paper with the same title as the conference paper of 2003, a paper which may or may not be the same, and which may indeed demonstrate that he has 'modernised' Ashby's Law.²

It appears that the product of this 'modernisation' is that "only complexity can destroy complexity" (Boisot and McKelvey, 2006: 27), complexity being that which is manifested phenomenologically as variety – or, as Beer puts it, rather more succinctly, "the measure of complexity is VARIETY" (Beer, 1994: 32, capitalisation in original). It is not at all clear whether McKelvey has indeed 'modernised' Ashby's Law, other than semantically, because, at this point, the evidence is, apparently, unavailable. Argument, demonstration, illustration might well prove to be persuasive, but, in the absence from the public channel of the paper that makes the substantive claim, there is no way of assessing it – there is only McKelvey's word for it. Given the significance of Ashby's

2 It is relevant that the 2007 paper claims that the 2008 one establishes the 'Law of Requisite Complexity'. Reference to the publisher's website (Edward Elgar) now indicates publication in 2009. This means that McKelvey's 'modernisation' of Ashby's Law has, by now, passed into the public channel without, as yet, any substantiation other than his word for it, and without, so far, the opportunity for peer assessment. This hardly seems to constitute the 'quality science' that McKelvey advocates.

Law, however, this is not enough. Imagine that someone claimed that they had modernised the ‘Law of Gravity’, as, of course, has been done – would scientists not expect something more than an apparently unpublished conference paper before they accepted such a claim as a new ‘truth’?

Finally...

It must be acknowledged that, in terms of our own particular research interests, and in the normal course of events, we would not have paid much attention to McKelvey, however ‘influential’ his work, had he not trodden on our ‘blue suede shoes’ by seeking, in his own particular way, to dismiss paradigm incommensurability. Quite apart from being central to our own work, we see (paradigm) incommensurability as a fundamental facet of poststructuralism, and to seek to refute it by (ab)using poststructuralism was especially provocative.

We embarked on our quest assuming that we could, through a critical reading of McKelvey’s text, demonstrate fairly easily the flaws and/or omissions in his argument. But the closer that we looked at his text, the more the metaphor that came to mind was that of wrestling with an octopus. No sooner did we pin down one arm of his argument than another poked us in the eye, and, all too often, the next one, in some way, contradicted the previous one. Further wrestling with McKelvey’s work did not lead to the taming of the beast, for McKelvey is not a creature of the natural world, but of the surreal – he is a shape-shifter. No good looking for the next thrashing tentacle when it has turned into a non sequitur.

This recurring sense of surprise – incredulity, even – is demonstrated in two final classic McKelvey-isms. The first appears in Henrickson and McKelvey (2002):

The term postmodernism originated with the artists and art critics of New York in the 1960s. Then French theorists such as Saussure... took it up. (p. 7293)

But Saussure was not French, he was Swiss. He has been called ‘the grandfather of structuralism’ – he was certainly not a postmodernist (it may not even be reasonable to call him a structuralist!). He did not himself publish his own major theories – *The Course in General Linguistics* was published, after his death, on the basis of his students’ notes. Most relevant of all is that Saussure died in 1913.

The second example brings us back to the paper that has been the prime focus of this examination:

Because [postmodernism] is very diffuse in its subject matter and often pointedly obscure in its use of language {here McKelvey cites Foucault, *Discipline and Punish* and *Power/Knowledge* as sole examples}.... I focus my critique on the relativist foundation. (p. 66)

From this we can infer that McKelvey finds Foucault obscurantist, but, given the extensive reference to, and ‘responsible core’ location of, Derrida and Lyotard, we might also infer from this comment that McKelvey finds their work crystal clear. While not denying the challenge presented by Foucault’s work, to imply that he is more difficult to understand than, for example, Derrida, goes against the prevailing view of

Derrida, as exemplified by the comment from Macey: “Derrida is notoriously difficult” (2000: 92). Although Macey acknowledges that “Foucault’s major works can be seen as abstract and even arcane”, he does add that they have “surprisingly concrete and immediate implications” (*ibid.*: 135) – which seems to be exactly what McKelvey might be looking for. Ignoring work because it is difficult seems to be a particularly immature (in Richards’s sense) approach. And, even setting aside the obscurantist elements of McKelvey’s own work, it is precisely this kind of immaturity that Richards sees as so profoundly dangerous for us all.

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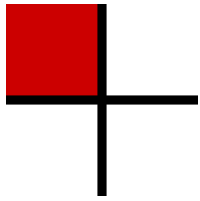
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Pippa Carter: Having spent a long time working for a living, Pippa decided to try living for a living instead, and finds that it 'works' very well. As part of this, she continues to research and publish on organization, inspired by Deleuze's 'active escape', following an unidentified Jackson, "I don't stop running, but while running, I look for weapons". She is further comforted by her appointment as a Visiting Fellow at the School of Management, University of Leicester, UK.
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Norman Jackson: Reassured by Deleuze and Guattari's confirmation that the 'capitalist machine does not run the risk of becoming mad, it is mad', Norman spends his time being cynical about organizations and organization studies. After too many years serving the 'mad machine', he is experiencing respite care, as a Visiting Fellow at the School of Management, University of Leicester, UK, amongst other misfits.
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Commensurability, Rhetoric and Ephemera: Searching for Clarity in a Cloud of Critique

Bill McKelvey

Introduction

A favourite phrase of Hollywood movie moguls is ‘Any news is good news’. For academics in the modern day, this translates as ‘Any citation is a good citation’. Of course, I am honoured that Norman and Pippa have spent so much time trying to fathom the meaning of my paper – ‘From Fields to Science...’. Being elevated to a fearsome authority figure is a status I never dreamed of achieving. Still, I don’t really believe it and will try not to let it go to my head.

More to the point, Norman Jackson and Pippa Carter (JC), perhaps unwantingly, join with good science in noting that “models contain less information than that which they model” and that models are designed to serve the purpose of the model designer. The evolutionary epistemologist and scientific realist, Jane Azevedo, captured this idea very well in the title of her book, *Mapping Reality* (1997). Physicists such as Newton made great progress by simplifying all the degrees of freedom of planets (different size, less than perfect round shape, varying distributions of lava, earth, water, trees, animals, critical theorists of all variants, etc.) down to a ‘point mass’. Darwin’s framing of evolution was surely simpler than we now experience it. But they got the job at hand done. If Newton, Darwin, Adam Smith, and Milton Keynes had been influenced by the modern-day over-problematizing seen in critical theory in OS, we probably would never have heard of them.

In burying every term/word in a sea of seeming random meaning-attachment, JC have lost sight of one of their few clear notions: what is a good model? Nobel Laureate Murray Gell-Mann has been talking about ‘effective complexity’ for two decades (1988, 1994, 2002). ‘Complexity’ is often defined in terms of ‘degrees of freedom’. Effective complexity, then, holds that for human schema development to offer functional advantage it can be neither too simple nor too complex but, as Goldilocks put it, ‘just right’. In what follows I will try to re-capture the ‘just right’ degrees of freedom and essence of some of the ideas obscuritized in JC’s ever-ephemeral meanings. I realize that ‘just right’ clarity in a journal called *ephemera*, could be oxymoronic. Sin by sin I try to clarify in what follows.

My Sins

1st Sin: Un-incommensurability

I can't imagine anyone beginning a paradigm-shift discussion without starting from Kuhn (1962). Yes, in 1962 he saw it focused on physics (what else), but since then his has become the most widely read philosophy of science book (Garfield 1987) and has affected thought in most disciplines. The idea that any reader in philosophy of science would begin a paradigm discussion with that of Burrell and Morgan (1979) is beyond imagination, even ephemeral. I focus on Ch. 2 – fields – because this is the only part of Kuhn's classic that hasn't been discredited by normal-science physicists and philosophers of science (see for example, Masterman, 1970; Kordig, 1973; Bishop, 1991; Weinberg, 1998; Ladyman, 2002). Unfortunately, its legacy of incommensurability stemming from paradigm shifts, morphed into relativism, remains undaunted in most social science disciplines except, perhaps, economics.

Technically, incommensurability is a term from mathematics meaning 'lack of common measure'. As the term spread out of math, its meaning broadened to 'meaning'. I have never seen any discussion in physics supporting the idea that physicists couldn't understand different measures, no matter how weird. Of course, logical positivists, using 'correspondence theory' tried to have exact meanings extending from observation terms (measures) to theory terms; but, needless to say, at this they failed (Suppe, 1977).

Again, consider the statement: 'All generalizations are false'. JC say "it is syntactically correct, but semantically false". How it can be semantically false is beyond me. It only includes four words and the meaning of each is clear. Despite saying it is semantically incorrect JC, in fact, appear to accept it; I have to be careful here because so many of their terms have so many meanings I frequently have no idea what they mean by any specific term (shades of Derrida and Foucault?).

So, now my statement: 'The incommensurability thesis is self-refuting'. In physics, to repeat my logic, if we know enough about the terms of one paradigm to say that they are incommensurable with the terms of another, then we must know enough about the terms to render their incommensurability false. JC say it would have been ok if I had used the term 'wrong' rather than 'false'. Talk about 'baffling'! Presumably saying my statement is 'wrong' implies that said differently it would be correct. Hum. So, which of the words should be changed to make the statement correct? Perhaps they would prefer this: If a physicist knows enough about two measures to say they are incommensurable, then he/she knows enough about the measures to equate them in some way, thus making them un-incommensurable; hence self-refuting. Admittedly, I am assuming that physicists aren't totally stupid.

If the above is not weird enough, JC also say (I am tempted to put 'says') "Incommensurability, if it exists, is a 'design feature' of the model". This is beyond comprehension or belief (their terms). 'Design' is, well, design ['to form or conceive in the mind; contrive; plan' (dictionary.com)]. Why would any scientist wanting to make a contribution, wanting to be cited, wanting to become famous (but not infamous), knowingly design (conceive, contrive, plan...) theories/models to be *not* understood?

Name one physicist that by conscious design created a model to be *not* understandable by other physicists? Perhaps by mistake a model doesn't make sense. Perhaps even after trying his/her best a model, as designed, is generally misunderstood. But incommensurability by design? Hard to accept. Were Newton's *Principia Mathematica* (1687) written to confuse by plan? Was Darwin's 1859 book contrived to confuse? Did Samuelson conceive of his 1947 book as consolidating mathematical approaches in economics written to fool wgi [Sic.] were obviously going to be math-trained economist readers? Did Dawkins design his 1976 'selfish-gene' book to be misunderstood by palaeontologists like Eldridge and Gould (Eldredge, 1995). If so, it didn't work; they and thousands of others understood it instantly.

JC note that the Burrell/Morgan 'model' "does not have a time dimension over which incommensurability can be resolved". I can't think of anything that would make their model more misrepresentative and useless. Yes, it is likely that the social construction of meaning pertaining to new terms in fast moving new fields in physics, biology, nanotechnology, dark matter, neurology, whatever, could for some period of time be beyond the understanding of some scientists and their students *outside* the new circle of insiders. But, in fact, physicists look at the day-to-day idea developments in physics and totally reject the idea of paradigm shifts and incommensurability; if you don't believe me, go talk to them! To pick a classic example, when Fleischman and Pons (1989) announced their claim to have discovered cold fusion, scientists around the world figured out their measures, tried to replicate their experiments, and quickly could say exactly which of the Fleishman and Pons measures failed. In this case, the would-be new paradigm died within days – but read on below.

But of course, physics and biology, and even psychology, are 'real' sciences grounded in realism, as opposed to whatever kinds of inquiry we see in UK-style 'organization studies'. If you disagree, go read physics journals and then go read articles in *Organization Studies* (like Jackson and Carter, 1991) – or *ephemera*. Scientists are, believe it or not, smart enough to change; they read, study, and learn, they build on new ideas instantly, especially in the current world of virtual publications. JC note that Kuhn treated paradigms as pathologies that will be cured over time. The primary difference between him and most physicists is that he saw the pathology lasting a long time; physicists see it passing quickly.

I said earlier that most scholars outside of UK organization studies pay little attention to the Burrell/Morgan model. Now you can see why. Is there anyone who has read their book who doesn't understand each of their four cells? *In fact, if the Determinism and Voluntarism cells are incommensurable, how could Burrell and Morgan write about both of them? That all four cells are in one book written entirely by the same two authors, virtually by definition defeats their incommensurability.* Well, ok. I guess critical theorists have trouble since they still think incommensurability applies. Is this a case of feeble-minded thinking rather than 'understanding' or 'belief'? But give a better answer, if you can!

2nd Sin: Understanding vs. belief

The mystery deepens. JC say,

we think McKelvey is saying something else: *if we understand, or comprehend, how another argument is different to ours...then we can resolve any apparent contradictions.* This suggests that McKelvey confuses and conflates comprehension and belief.... There is...no reason at all why we should not be able to understand something at the same time as not believing it. But for McKelvey, to understand is to believe.

In the part of the foregoing quote (in their italics) they somehow read into it the idea that I think resolving contradictions in understanding automatically leads to belief. Why would resolving misunderstandings and contradictions in, say, a religion (pick any) lead me to believe any of it. Absurd!! Apparently they think I am not only authoritarian but an idiot besides. For a person, like me, who claims to be a scientific realist and is well versed in this aspect of philosophy of science, understanding and belief are steps on the way toward developing truth claims having the highest probability of truth possible.

But, let's see. I *understand* all of the history, logic, and thinking behind the idea that the Earth is flat; but sad to say, I don't *believe* any of it. In this case, 'flat-earth logic' has been shown to be false; therefore I believe the Earth is round (more or less). I *understand* the logic and wishful thinking underlying creation science, Intelligent Design, and 'born-again Christians' (I grew up with all this stuff as a missionary-kid in India), but I don't *believe* any of it since I don't see any valid research supporting the 'faith'. JC might better have said: 'understanding is independent of believing'. But it appears that their *belief* in the logic of critical theory obscures their *understanding* of anything, certainly anything pertaining to scientific realism and science in general.

The Burrell/Morgan model is the premier paradigm model? Are you kidding? Is this a case of *believing* without *understanding*? Do JC believe in incommensurability without comprehending it? JC say that "incommensurability is an issue of belief, not one of truth". What happened to understanding? Well, yes, I agree that people can 'believe' in incommensurability without asking whether it is 'true' or not. No problem here. People, even academics and especially critical theorists can 'believe' anything; truth be damned. There is no rule against believing without understanding, believing what is false, even believing when all of the facts appear to line up against the belief. We still have people believing in God and we still have the Flat-Earth Society. Some people will believe anything; people in the UK apparently still believe monarchies are still relevant.... Is this a truth? I am not in a position to say since I am from the New World – where we long ago rejected such nonsense.

3rd Sin: Campbellian Realism

Since JC show the epistemology-ontology matrix in their paper, I don't show it here. In referring to Cell 2, I said that "no one supports a position based on a realist ontology and a relativist epistemology". JC suggest that "the New Physics...not to mention Critical Realism, Critical theory, Freudian Psychoanalysis, amongst others that could be argued to fit into this cell". Yes, I agree, 'no one' is an overstatement. There is always some nut who will take some strange position. Thus, someone could 'argue' (and some

still do) that the Earth is flat. But most real scientists probably wouldn't. Even 'critical realists' who appear to be the Brit version of scientific realists (I realise I may be being charitable here), would not buy into the idea of wanting, first, to adhere to the challenges of 'scientific realism' and then to let go and accept 'anything goes' (Feyerabend, 1975) as bases for accepting what philosophers would like to think of as science-based 'truth claims'.

I mention Scientific Realism just above. Logical positivism and logical empiricism have, for philosophers of science at least, been replaced by scientific realism (Bhaskar 1975; Suppe, 1977; 1989; De Regt, 1994), which focuses on searching out 'underlying causal mechanisms'. Nothing offered by the contra-science folks would be acceptable as epistemology, much as they might hope. Water does not flow uphill. More precisely, no 'resident' of Cells 1 and 4 would support Cell 2 as offering any positive hope toward improved 'truth'; by definition no Cell 2 resident could satisfy the standards of either Cell 1 or Cell 2. Frankly, I don't see how Cell 2 can be inhabited by knowledgeable researchers.

JC mention Donald Campbell in a footnote as fitting Cell 2. I have Campbell as one of the leaders for developments in Cell 3 – what I term 'Campbellian Realism' elsewhere (McKelvey 1999). Why? Indeed, Campbell: (1) accepted the reality of individual interpretations of the phenomenal world; and then (2) accepted the reality of social construction toward a common belief in a truth claim within a social group (a scientific community in this case) – which looks like relativism and contra-science; and (3) since 1965 has accepted the evolutionary epistemological view of scientists' progression toward Popper's 'verisimilitude', i.e., his "truthlikeness" (Radnitzky and Bartley, 1987; Hahlweg and Hooker, 1989), which we now see as progression toward realism's more probable "probabilistic truth claim" (Campbell, 1965; 1974). JC's placement of Campbell into Cell 2 is ephemeral logical for sure. As Campbell put it:

...[T]he *goal of objectivity* in science is a noble one, and dearly to be cherished. It is in true worship of this goal that we remind ourselves that our current views of reality are partial and imperfect. We recoil at a view of science which recommends we give up the search for ultimate truth and settle for practical computational recipes making no pretense at truly describing [and explaining] a real world. Thus our sentiment is to reject pragmatism, utilitarian nominalism, utilitarian subjectivism, utilitarian conventionalism, or instrumentalism, in favor of a critical hypothetical realism. (1974, p. 447; his italics)

4th Sin: Putting science to good use, heaven forbid

In the U.S., most of the people creating the organization theory/science literature are employed in business schools; of those JC mention in their paper, Pfeffer is at the Stanford b-school and Van Maanen is at the MIT b-school; I also work in a b-school. In England, perhaps critical theorists in OS don't actually work in b-schools; perhaps they work in London where people make money or in The North where they make lots of grass, cows, and sheep. But surely, some critical theory types must work in b-schools since they are doing organization studies research – well sort of research; like critical theory style research.

To beat a dead horse, there is increasing concern in America, and I think in England, about the seeming irrelevance of organizational and management research as

promulgated in the journals and by members of the Academy of Management; this applies to both quantitative and qualitative research. Builders of tall buildings, ships, bridges, airplanes, and the Internet, etc., are users of engineering-school research. Doctors with sick or dying patients are users of medical research done in medical schools and as drawn in from disciplines in biology. B-schools are usually characterized as professional schools training MBAs who then become practicing managers. A growing number of b-school academics do *not* think Pfeffer, Van Maanen, and the many others I can cite (see for example McKelvey 2006) are off the track in wanting b-school research to be relevant to practicing managers. And yes, then, this includes owners, CEOs...employees and other constituents who worry or suffer if firms don't make economic rents (i.e., above industry-average profits). In any field, that profession schools (should) worry about relevant research is hardly at odds with the objectives of basic research in disciplines outside b-schools. Only critical theorists, apparently, would think otherwise.

Yes, it is true that Einstein wrote President Roosevelt a note saying theories of nuclear physics contained the makings of an atomic bomb. But, he obviously was not thinking bombs when he wrote his 1905 paper outlining relativity theory – which as I note in a footnote was *not* incommensurable with the Lorentz transformation equations (McKelvey, 2003, even though it was too much of a seeming paradigm shift to the Nobel committee to award him the Nobel Prize for it in 1921. True, then, 'Basic Science' hasn't sold its soul to generate ideas for practical benefit; but researchers in professional schools in some sense, are under pressure to do so. And of course, herein lies the relevancy problem; b-school researchers do get promoted for statistical significance, and findings mostly irrelevant to practitioners.

Given 'Agency Theory' (Jensen and Meckling, 1976), we now have CEOs with stock options making \$millions; and by cleverly pre-dating their options contract (i.e., cheating) they can sell out and make \$millions even as their firm goes bankrupt (Countrywide Financial and Lehman Bros. being good recent examples). Does my concern – and the concerns of all others worried about research irrelevant to practitioners – about 'relevant' research mean we all are doing so to make CEOs wealthy? Should we do the opposite – so as to keep CEOs poor – by publicizing research findings we 'believe' (understand? think?) will make firms perform poorly, thereby dashing the hopes of lower-level workers, families, and communities dependent on firms for employment and taxes, etc., while we try to flat-line stock options so CEOs don't make big bucks? If you critical theorists want to live in the Land of Illogic yourself, fine; but please try not to impute it to others. Most economies, even the UK, depend on firms offering employment, producing products, and paying taxes. Of course, CEOs make more money than others. But to say that my focus on researching so as to learn how to help firms work better is only to make CEOs richer is stupid, to say the least. In fact, I don't even consult! I am a 'left-bank intellectual'; I despise the rich! It goes back to the 'mish-kid' beginning.

5th Sin: Reflexivity

JC quote me as saying, "I do not disagree that reflexivity is present in organizations; I am just not sure in counts for very much". Of course, in principle, reflexivity exists.

Researchers well versed in research methods in b-schools have long been aware of Chia's concern that "...the researcher/theorist plays an active role in constructing the very reality he/she is attempting to investigate" (quote by JC). This concern lies at the basis of research training for any PhD student in 'good' b-schools (yes, there are always places that don't know any better). Since at least the 1950s good researchers have all worried about the questionnaire design that, once given to respondents, sensitizes them into thinking and seeing in ways they had never imagined. Even more so, good academics worry about interview protocols, case-study designs, and other researcher influences on the members of an organization that might inadvertently create behaviours that weren't present before the researcher entered the organization (see Yin 1989 for example).

Does reflexivity still exist in research here and there? No doubt. Does reflexivity "...render groundless and undecidable..." all findings about organizations? Hardly. The assumption set of modern statistical practices are surely much more confounding, meaningless, and worrisome as to impact. Of course, this is more of a problem in top American journals where quant. studies take up 80%+ of journal space. The journal, *Organization Studies*, gives some 20% of its space to quant. research. Perhaps Chia is mostly aiming his pointed remark at UK organization studies researchers. But even here, UK researchers worry constantly about case study research approaches and they are not mindless about reflexivity. I repeat, reflexivity concerns are well down the list of much needed researcher concerns. But, of course, reflexivity will never go away and could easily move up the list absent constant wariness.

6th Sin: Latour, Burrell, Pasteur, Nazism

JC quotes (whoops) quote me saying:

The antiscience group is prone to make accusations such as Burrell's (1996: 656) assertion that modernist science (epitomized by Einstein the Zionist who was invited to be the President of Israel) caused the holocaust of 6 million Jews....

Then they quote Burrell (1996: 656)

[In the 1960s] few saw the defining organizational form of the whole twentieth century to be the death camps of Auschwitz. *Modernism* is about the death camps in a fairly uncontentious way even though its apologists seek to distance the likes of Auschwitz from the achievements of the modernist society.... (JC's italics).

Boiled down, I said: 'Burrell's...assertion that modernist science caused the holocaust...'. Boiled down, Burrell said: 'Modernism...its apologists seek to distance the likes of Auschwitz from the achievements of modernist society'. I used the term 'caused'; Burrell used the term 'achievements'. Let's see: if Auschwitz is a result of the achievements of modernism (which leads to Burrell's 'distancing' idea), then it must be party to whatever caused the achievements, which was modernism. Presuming that achievements don't happen without causes, then it appears that I captured what Burrell said rather more clearly than how he said it in the first place. Case closed.

7th Sin: Equating variety with diversity

Let's start with the origin of Ashby's Law of Requisite Variety. In his classic work, *An Introduction to Cybernetics*, in defining variety, Ross Ashby (1956: 124–25) pointed to the following series: 'c, b, c, a, c, c, a, b, c, b, b, a'. He observed that a, b, and c repeat, meaning that there are only three 'distinct elements' (his term) – three kinds of variety or three degrees of freedom. He also notes that order (organization) exists between two entities, A and B, only if the link is 'conditioned' by a third entity, C (Ashby, 1962: 255). If we take C as constituting an 'environment', external to A and B, then C can be taken as a source of order-generating constraints that helps to organize the relation between A and B (Ashby, 1956). The influence of such external constraints, gives rise to his famous *Law of Requisite Variety*, which states that "ONLY VARIETY CAN DESTROY VARIETY" (p. 207; his capitals). It holds that for a biological or social entity to be efficaciously adaptive, the variety of its internal order must match the variety imposed by environmental constraints.

Our 'updating' of Ashby's Law began with McKelvey and Boisot (2003) and then McKelvey and Boisot (2007; appeared in the Acad. Mgmt. *Best Paper Proceedings*) and most recently in McKelvey and Boisot (2009). We simply take Ashby's 'variety' ('c, b, c, a, c, c, a, b, c, b, b, a') and then refer to his 'distinct elements' as degrees of freedom – which are one of the typical ways of defining complexity: the more degrees of freedom, the more complex (Gell-Mann, 1994, among others). I/we don't take any particular pride in our 'modernization' or 'updating'; we are simply using Ashby's Law to offer one definition of Gell-Mann's 'effective complexity'; to wit: internal complexity is effective if it destroys external complexity. I/we are just stating the obvious: nothing more, nothing less. Thus:

Only variety can destroy variety

Only degrees of freedom can destroy degrees of freedom

Only internal complexity can destroy external complexity

Following Gell-Mann, I take degrees of freedom to be the most basic phenomenological manifestation of complexity. Since Ashby was writing about 'self-organization' as far back as 1947 (first time the term was used in print), and since self-organization is a key element of the Santa Fe Institute's vision of order creation (Holland, 1988; 1995; Kauffman, 1988; 1993), our updating is surely in line with Ashby's thinking. JC quote Stafford Beer as saying, "the measure of complexity is VARIETY". Great quote! Thank you! This quote of Beer helps cement the connection between variety, complexity and degrees of freedom.

As for Allen's (2001) 'Law of Excess Diversity', JC's issue with diversity vs. variety is a tempest in a very small teapot, indeed. We now have three key terms in text: variety, distinct, and diversity. Quick definitions from 'dictionary.com' are:

Distinct: "not identical; unmistakable".

Variety: "the state of being varied or diversified".

Diversity: “the state or fact of being diverse, difference, unlikeness, variety”.

Allen’s point is simply that, given all of the external degrees of freedom (which, even worse, are frequently changing their nature), and given that an organization doesn’t know in advance which external degrees of variety are going to cause trouble, it needs to create more internal degrees of freedom in advance than appears obvious at any given time. Whether Allen’s concern is written down as variety, diversity, or degrees of freedom is irrelevant. But of course, critical theorists make their contribution by seeing shadows behind every rock; whoops, I mean every tiny stone....

Finally: come on now, JC, enter the modern world. OS could very well be the last discipline not to want to get works out and visible on the Internet as soon as possible. Physics and Biology have had highly used electronic journals for years. Most academics present their newest ideas at conferences; ideas that then appear in conference proceedings or as chapters in books. Many, if not most, initial ideas leading to Nobel Prizes in economics, for example, do not appear in the so-called ‘A’ journals; just to pick one, Robert Lucas’s rational expectations idea first appeared in the 4th volume of a new journal. Since I mention Jenson and Meckling’s very influential article on agency theory, note that it appeared in the 3rd volume of a new journal. Einstein’s 1905 paper introducing relativity theory was published by his friend, Max Planck, without refereeing. Besides, what studious risk-averse referee would have accepted something as silly is wobbling time?

Oh yes, you complain about my “modernization of Ashby’s Law passing...into the public channel without...peer assessment”. Maybe this is my 10th sin. But, let’s see. Did we pay money to the Internet to have the ‘updating’ idea appear in electronic papers? *No*. Did we pay you to cite it and even discuss it in your paper? *No*. Did *ephemera* referees (well ok, reviewers) reject your paper because you cited an un-refereed virtual paper? *No*. Some people cite papers they like and some cite papers they don’t like. Hence my opening line: ‘Any citation is a good citation’. Think how many cites and how much fame the cold fusion guys, Fleischmann and Pons (1989), got for telling everyone about their mistake (Huizenga, 1993; Goodstein, 2002, U.S. Department of Energy, 2004; Plotkin 2005)!

Frankly, I don’t waste my time looking at ‘print’ journals any more. I click key words into Google and then read papers that appear – hard copy or as yet only virtual. Do you really think I think I need some unknown, risk-averse referee to tell me whether or not to pay attention to the content of some paper? Hardly! Yes, I am very thankful to the one referee in my 40-year career who made five comments on a paper, one of which doubled the quality of our empirical results. I have had one other paper in which the referee improved our theory. Most of my papers, in my view, were better before the refereeing. So, besides finding new papers quickly, the best thing about Google is that one can avoid the referee process. What a concept!!! Your complaint sucks you back to the age of clanking keys in typewriters.

8th Sin: Keeping Saussure alive

This is a sin? Would that mean someone would sin by keeping me alive when that time arrives? I hate to tell you, but Cilliers cites Saussure’s 1974 book, not his dead body.

Here is what I said about Cilliers' drawing on poststructuralism in my 2003 chapter titled, 'Postmodernism vs. Truth in Management Theory' (how could you miss this one?) Cilliers (p. 6) first sets out ten attributes of complex adaptive systems, and then makes his foundational argument as follows (p. 37)

Complexity is best characterised as arising through large-scale, nonlinear interaction. Since it is based on a system of relationships, the post-structural inquiry into the nature of language helps us to theorise about the dynamics of the interaction in complex systems. In other words, the dynamics that generate meaning in language can be used to describe the dynamics of complex systems in general. Connectionist networks share the characteristics of complex systems, including those aspects described by a post-structural theory of language. It should therefore be possible to use them (or other distributed modelling techniques with similar capabilities) as general models for complex systems. These models can be physically implemented or simulated computationally.

These three points link the *poststructuralist responsible core* of postmodernism and complexity science together by virtue of their common focus on connectionism. Why 'responsible core'? Because Cilliers focuses on connectionist networks: these are real, have underlying generative causes, and are amenable to realist inquiry. But yes, one may have to go through the Campbellian Realist sequence of idiosyncratic individual perception and social construction to get past the eye of the beholder to discover the actual working network connections. I do admit to one source of possible confusion: While Cilliers and I draw on Saussure as a poststructuralist, it is possible that I presumed that more postmodernism seeped his 1974 book, based on his works but written up long after he died, than is really the case.

9th Sin: Faith in Cilliers

Paul Cilliers says (personal communication) that his original title was 'Complexity and Poststructuralism' but that the publisher thought it would sell better if 'postmodernism' was in the title rather than 'poststructuralism'. Given this, the book as written, and its use of works by Saussure, Derrida, Baudrillard and Lyotard, reflects Cilliers working from the poststructuralist literature rather than the postmodernist one. But this distinction is not terribly important here since the argument is about who supplies the underlying support for reframing organizational research into Cell 3 rather than leaving it fighting between Cells 1 and 4. Since I focus on 'contra-science' ontology to make the case, both poststructuralism and postmodernism are included. Since I also draw on Campbellian Realism and complexity science in making the argument for focusing on Cell 3, Cilliers' book is a key step in the storyline; it is he who makes the 10-point connection between poststructuralism and complexity science.

Conclusion

In principle the role played by critical theorists is important. There are a number of early postmodernist studies about what actually happens in research laboratories and how much lab power plays, social collusions, and misunderstandings contribute to what are the basic 'facts' being written up in ways that obscure and seem to create ephemeral facts rather than tell a clear story about what really happened. These studies are especially valuable because they are able to connect down to true facts, as opposed to stories created by variously motivated researchers – especially doctoral students, who

are well known to produce the facts the professors want to see rather than facts really there.

On the other hand, it appears that some critical theorists could very well be doing more to confuse than clarify; more to create the incommensurability pathology than reduce it; more to obscure than clarify. Talk about reflexivity: critical theorists can easily or wilfully create wishful confusion by obscurantist attachment of weird meanings to otherwise well understood words. Whereas doctoral students may create nonexistent facts to get their professor a publication, critical theorists could easily be creating wishful incommensurability to get a publication. No wonder this journal is titled *Ephemera*.

Still, as I note at the outset, any citation is a good citation. I very much appreciate the opportunity to be forced to rethink and hopefully clarify 5-year old arguments that seemingly are open to misinterpretation. I enjoyed the style of JC's article. It was fun to read!

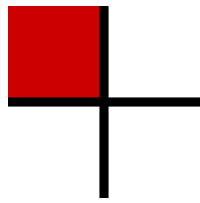
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the author

The fearsome Bill McKelvey: Having rejected God, physics and mathematical syntax along with meaningless memorizing early in my 20s, I have been searching for truth from science ever since. Semi-life in a business school, surrounded by economists, nearly-autistic males, intangible phenomena – and wanting to help people make organizations work better – has complicated the search. Philosophy of science, focusing mostly on physics and biology, opened windows of delusion and challenge. The search goes on. But I see promise in elevating fractals, power laws and Pareto-based science over the misguided over-emphasis of Gaussian statistics.



'Immeasurability': A critique of Hardt and Negri*

Steven Toms

abstract

The paper offers a practical criticism of Hardt and Negri's *Empire*, focusing on the concept of 'immeasurability'. It analyses key sections of the book and related passages from Negri's other works, illustrating their vagueness and inconsistency. Their relationship to Marx's theory of value is analyzed and the robustness of their arguments contrasted. A further section contrasts the constructs used by Hardt and Negri with developments in management theory and organizational economics and assesses their real world practicality. The paper concludes that the immeasurability concept fails to undermine value theory and is itself valueless.

Since its publication in 2000, Hardt and Negri's *Empire* has been the subject of controversy and intense debate (Balakrishnan, 2003) and attracted sustained criticism from a diverse range of commentators. The book's empirical claims, which include inter alia, that with globalization comes the end of the nation state, US hegemony and that what proletarian internationalism fought for has come about despite its defeat. (Hardt and Negri, 2000: 50), have been criticized for their evident implausibility (Brennan, 2003). Similarly the claim which has been advanced by others (Drucker, 1993, Reich 1992), and upon which Hardt and Negri (2000: 289-294, Hardt and Negri 2004: 113) depend for their sociology of immaterial labour, that industrial organization has passed from 'Fordism' to 'post-Fordism' is empirically contestable (Callinicos, 1990: 136-137). Many of the book's conceptualizations are equally disputable, but less easily refutable with reference to evidence, and, in claiming to reinterpret Marx, represent a potentially more serious challenge to radical thought.

Unsurprisingly, this has given rise to a large literature. Even so, subsequent debates have given little attention to socio-economic conceptual claims, particularly the nature and apparent importance of 'immaterial labour' (Camfield, 2007). Less attention has been given to value theory (for exceptions, see Caffentzis, 2005a, 2005b; Cleaver, 2005; Harvie, 2005; De Angelis, 2005; De Angelis and Harvie, 2006; Dowling, 2007), which, with its origins in classical economics and its radical implications confirmed in 'Second

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International Marxism', [Hardt and Negri counter-pose Second International Marxism and its mechanistic legacies, economism, positivism, and evolutionism with council communism, post Fordism and Deleuze and Guattari (Brennan, 2003: 107)] has accordingly been defined out of existence by the notion of 'immeasurability'. Such an argument is developed on the basis of Negri's assertion that Marx's *Capital* is a flawed work serving "to reduce critique to economic theory (and annihilating) subjectivity in objectivity" (1989: 19).

The concept of "value beyond measure" (Hardt and Negri, 2000: 355), or 'immeasurability', therefore issues a powerful challenge to radical thought, classical value theory as well as to the practicalities of accounting and management. On the one hand, according to this view, immeasurability has arisen because labour has become 'immaterial', that is, better qualified, more individually and collectively complex. On the other, capital becomes more financially orientated and more abstract in its mediation of diverse sectors of the economic cycle (Negri, 1999: 78). Bryan and Rafferty (2006: 66) draw the opposite conclusion, suggesting that the emergence of derivatives facilitates "the commensuration of values across time and space". The immeasurability thesis constitutes an explicit rejection of classical value theory and with it a rejection of the possibility of the creation of an accounting system based on non-psychological valuation criteria.

In response, this paper develops a critique of Hardt and Negri's notion of 'immeasurability', examining it as a logical concept. The approach adopted is the 'practical criticism' of IA Richards (Richards, 1924), which uses 'close reading' and the deliberate removal of context, so that the critique concentrates on the inter-relation of meaning in the text under consideration in itself. Extensive literature review, which readers can get from other sources, is therefore not the purpose and it has been minimized in order to explore the detailed arguments presented on the subject of value in *Empire*, with references to the prior literature only where Hardt and Negri themselves use as substitutes for necessary parts of their argument. For example the notion of power to act (Hardt and Negri, 2000: 356) is explained in more detail in Negri (1999). Nor is the purpose of the paper to question the wider claims of autonomist Marxism beyond the notion of value as measurement in labour process and market exchange, itself the subject of previous debate (c/f Negri, 1989 and Cleaver, 2005 {Cleaver (2005) was first published in 1989, thereby predating the debates on *Empire*}), for example value as belief or experience (Graeber, 2001) or as part of a measurement discourse (DeAngelis, 2005). Rather, because Hardt and Negri utilize and develop Marx's method, suggesting for instance that *Empire* allows for the first time the analysis of the state and the world market using Marx's approach (2000: 236-237). Marx's original works are used in the paper to test consistency and the logic of suggested differences.

The paper is structured as follows. The first section offers a practical critique of Hardt and Negri's concepts of immeasurability and immaterial labour concentrating on certain key passages of their work and illustrating their theoretical and empirical inconsistencies. A second section examines how Hardt and Negri's concepts perform in the real world context, assessing their performance relative to existing management theory. A final section draws conclusions, including an assessment of what if anything

can be salvaged from the concepts of immeasurability and immaterial labour that might be helpful in constructing a workable theory of value for the age of global capital.

Immeasurability: a practical critique

Hardt and Negri reject classical value theory using reasoning consistent with Negri (1997 [2003]), which argues for a redefinition of productive time. Negri (1997 [2003]) develops two concepts of time, co-operative, productive proletarian time and capitalist analytical command. In the first case there is time as substance, which is irreducible to elementary temporal units, so that there is now self-valorization, which is a form of re-appropriation based on the separation of co-operative labour from capital. In the second case there is time-as-measure, or quantities of labour time.

In *Empire* the co-operative essence of productive labour is generalized a stage further. Hardt and Negri's new political theory of value incorporates inter alia the Foucaultian notion of biopolitics, according to which power extends into the consciousness and bodies of the population and at the same time across the entirety of social relations (Hardt and Negri, 2000: 29). Hardt and Negri (2000: 28) use Deleuze and Guattari's post-structuralist development of biopower as the productivity of social reproduction (creative production, production of values, social relations, affects, becomings). As biopower achieves command over the entire life of the population only when it becomes a vital function that every individual embraces and reactivates of their own accord, it follows that for Hardt and Negri, value is determined only by humanity's own continuous innovation and creation and, as such, labour is productive activity outside measure (Hardt and Negri, 2000: 23-24, 356). Hardt and Negri (2000: 356) go on to assert that the excess of value is determined today in the affects, in the bodies crisscrossed by knowledge, in intelligence and in the sheer 'power to act'. The 'power to act' is Negri's (1999: 79) definition of affected labour.

The implicit non-sequitur is the first of several definitional and conceptual problems. If the first premise, of labour as an activity which is 'beyond measure', is accepted, it does not follow in the subsequent statement also that 'value can be determined', since determination implies measure and 'excess of value' implies measurement against some normal level. It is possible that by 'excess', Hardt and Negri mean profit, but their explicit rejection of the labour theory of value leaves them also without a theory of surplus value or profit. So Hardt and Negri cannot, for example, provide any explanation of why the average rate of profit is positive (Caffentzis, 2005a: 104). One might add that without a theory of profit creation, there can also be no theory of profit distribution. Without a theory of profit, there can be no theory of rent, nor a theory of price or market competition. Of course, immeasurability implies that all these theories are now surplus to the requirements of a value theory that only requires us to acknowledge there is a power to act and with it comes an associated excess or social surplus.

If this argument is accepted the result is merely tautology. The implication is that all actions, because subjective, immeasurable and biopolitical, are also creative and that all actions are by definition value creating. Destructive acts such as graffiti spraying, which

might be subjectively classified as art, are value generating activities according to this theory. The difficulty is that the theory does not recognize the ontological distinction between labour and action (Caffentzis, 2005a: 97), nor does it recognize the symbiosis between individual and collective power to act and over-arching power as suggested by Holloway (2002). A further problem is that there is no distinction between labour that is performed efficiently and inefficiently. Power to act implies power to work at one's own pace. If it takes creative workers in one firm twice as long to produce an output as it does the workers in another, it does not follow that the less efficient workers have produced more value, notwithstanding their greater power to act. Whilst Hardt and Negri retain the notion of socially created value, they reject the notion that there is some amount of labour that is socially necessary for that value to be created.

Much of Hardt and Negri's objection to socially necessary labour depends on an argument put by Negri (1997 [2003]: 28-29) which is worthy of detailed examination. Although the discussion here focuses on only two works (Negri, 1997 [2003] and Hardt and Negri, 2000) the arguments discussed are of a much longer vintage dating back to Negri (1978), and reflect the culmination of Negri's ideas. The conclusion reached by Negri (2003) is also a precise statement of the immeasurability hypothesis: "When the entire time of life has become the time of production, who measures whom" (1997 [2003]: 29)? Formulated in these terms 'immeasurability' means that as far as Negri is concerned, Marx's value theory needs to be "put out to pasture" (*ibid.*). At least Negri applies this conclusion to the parts of Marx's theory where there are possible aporias. Most fundamentally Negri's aporia arises because the quantity of simple necessary labour is established, citing Marx (1990: 135), "by a social process that goes on behind the backs of the producers" (Negri, [1997] 2003: 24). For Negri, 'behind the backs' means by a process external to production, which in turn means externally and immediately determined exchange values founded on the temporal quantities of use value.

An unresolved enigma, or aporia, as far as Negri is concerned is the problem of reducing heterogeneous complex labour to its simple equivalent. If aporia is taken as an implicit criticism of Marx, it is unwarranted since Marx makes it clear in Volumes 1 and 2 of *Capital* Marx that exchange values of commodities equal the 'socially necessary labour' time currently necessary to produce them, leaving Volume 3 to explain capitalist production as a whole. The reduction of complex labour to simple labour depends therefore on simplifying assumptions which Marx used for the purpose of developing his analysis, as the final sentence in the paragraph and a footnote adjacent to the above citation make clear (Marx, 1990: 135, n.15). Marx's footnote states: "the reader should note that we are not speaking here of the wages or the value the worker receives for...a day's labour, but of the value of the commodity in which his day of labour is objectified" (Marx, 1990: 135).

Even so Marx's analysis still faces the challenge either as posed by Negri or of the problems that occur once his own limiting assumptions are removed. These alternatives amount to the same thing and are overcome if there is a method of transforming values to price consistent with a general rate of profit. In effect, Negri appears to have rediscovered the old chestnut of orthodox Marxism, 'the transformation problem' by the back door of Spinoza's *Ethics*. Hardt and Negri (2000: 65-66) use Spinoza to elaborate

their 'power to act' doctrine (manifested here as the subjectivity of the labour process) by the multitude's ability to utilize society's pre-existing resources for its own ends. His formulation of the tautology in terms of externally determined proportions of value consistent with the temporal split of s/v , is paralleled in the transformation problem by the need to form prices at levels consistent with the underlying value and an equalizable rate of profit. The consequences of removing the price = value assumption in particular is well known in the unresolved debate on the transformation problem (Steedman, 1981, Desai, 2002). Subsequent attempts to resolve the transformation problem in a fashion consistent with Marx's formulation have achieved some success (Foley, 1982; Dumenil, 1983; Bryer, 2007), in the sense that the fundamental tautology suggested by Marx's critics and reformulated in the above aporia by Negri, is replaced by less fundamentally damaging assumptions to the traditional labour theory of value.

Even if these developments are ignored, as by Negri, the transformation problem is not the tautology that Negri's aporia suggests. In Volume 3 of *Capital*, Marx considers the problem of the equalization of profit and price formation in terms of a clear sequence of events, "The rate of profit arises from the temporal and quantitative s/v relation, in which capitals of equal magnitude put into motion different quantities of surplus labour" (Marx, 1984: 158) and its transformation into the general rate of profit through the process of competition, "Prices are then set for individual capitals at cost plus the general rate of profit" (Marx 1984: 156-158). Of course, these prices are of no use for the purposes of resolving the quantitative temporal s/v relation, whether individual profits are transformed to the general equilibrium or not. The point is that Marx's certainty and causality are substituted for aporia and tautology by Negri. For Marx, the temporal s/v question is resolved by the measurement of the socially necessary labour time, for Negri, all is displaced into the time of the subsumed being.

Even if the limitations on Marx's analysis imposed by the transformation problem are factored in, even as posed by Marx's strongest critics, they are not sufficient conditions for the conclusion that all is immeasurable and that value theory be abandoned. It is perfectly straightforward for example to transform heterogeneous labour with differential real wages into the rate of profit without reduction to simple labour (Steedman, 1981: 88-94). Like Hardt and Negri, Steedman and the Neo-Sraffans abandon value theory as the corollary of such transformations. Even so it is possible for example to measure the profit for an individual capital, retaining the labour theory of value, but without imposing the condition that the rate at which it is earned should be equal to the rate earned on all other capitals.

To deal effectively with the problem of valuing creative labour requires not just a definition of value consistent with socially necessary labour, but also a theory of rent. As socially necessary labour is necessary to valorize labour of differing efficiency consistently, a theory of rent is necessary to explain differing returns on investments of equal capital or why labour of similar skill and intensity might command vastly different remuneration. At this stage, it is useful to return to Negri's question who measures whom? An employee has the ability to measure the intensity of their own application to the production process and thereby the creation of physical or mental use values. Such self measurement, whilst dependent upon individual perception, approaches an objective understanding of value. At the same time, the greater the

degree of skill, associated with either tacit or specialist knowledge, the greater the potential measurement problem from the perspective of the employer. Imperfections in these monitoring arrangements create the possibility of labour rent earning capacity in complex, imperfectly monitored and therefore from capital's viewpoint, stochastic labour processes that embed tacit knowledge (Toms, 2005).

The normal response to these difficulties is the imposition of output controls based on financial or quantifiable targets as the quid pro quo of the abandonment of supervision and direct monitoring. Although these forms of measure seem arbitrary, they are made effective by the requirement of invested capital to earn rates of profit commensurate with socially necessary labour that maintains and accumulates capital utilizing the relations of law, governance and ultimately force, available to the employing organization. As the motive force of capital, the rate of accumulation assumes a collective objectivity as the required rate of return for stock market investors. The remoteness of investors from the productive process renders such rates of profit entirely arbitrary when applied to individual circumstances of value creation. Between the objectivity of the value creation process at the individual level and the imposed collective objectivity of required rates of return and accumulation, are the subjective interactions of accounting and accountability, between the measurer and the measured, in which knowledge is hidden as often as reported and effort disguised as much as recognized.

For Negri, the existence of co-operative labour compounds the problem of measurement and reinforces the conclusion of immeasurability. Again he draws on Marx as his immediate authority referring to Marx's description of the role of managers, engineers, technologists etc as socially combined labour (Marx, 1990: 1039-1040, cited in Negri, 2003: 28-29). Without contextualizing the quotation or commenting on it, Negri then suggests that "the concept of the socially combined worker represents the subsumption of the social whole by capital..." (Negri, 2003: 29, Hardt and Negri, 2000: 365). Negri then assumes that "social labour covers all the time of life" and then the conclusion referred to above "when the time of life has become the time of production, who measures whom" (*ibid.*).

Here we have three non-sequiturs joining the four citations together. The first of these is again particularly unfair to Marx. In describing managers and others as socially combined labour, Marx shows that in circumstances where their co-operative activity leads to the reproduction of variable capital, they can be considered to be productive labour. Marx's definition (in the section not cited by Negri, 1990: 1041) explicitly classifies certain types of wage labour and certain services, included those provided through taxation as unproductive and as not being factors of capital. Because barristers and doctors become wage labourers, means that as far as Marx is concerned they are too easily and incorrectly confused with productive workers. Also capital may well have a tendency to make all labour productive of value, for example reflecting competition by nation states to attract and retain private capital (Harvie, 2005: 150). After all, unproductive labour increases circulation time and reduces profit. None of this, however, amounts to the subsumption of society by capital. Where Marx means the real subsumption of productive labour (including technical and co-operative elements), so Negri means the total subsumption of society by capital. At this level of abstraction, it is

hardly surprising that Negri and Hardt and Negri need to jettison distinctions between productive and unproductive labour, socially necessary and unnecessary labour, value and surplus value as corollaries of their argument. Another corollary of defining "biopolitical labour" as the creator of "social life itself" is the collapse of the distinction between material and immaterial labour (Camfield, 2007: 24).

The second non-sequitur is that the subsumption of the social whole by capital implies that social labour covers all the time of life. Once again the assertion follows from a confusion of Marx's meaning. In the same sub-section of Volume 1 dealing with real subsumption (1990: 1035), Marx argues that with real subsumption comes *sui generis* capitalist production which through increases in the minimum capital requirement causes capital to assume social dimensions, thereby shedding its individual character. By this Marx means that pooled, collectively owned share capital is the concomitant of increasing scale of production under the real subsumption of labour. To argue that this constitutes the subsumption by capital of society itself (as opposed to societies of capitalists) is a misprision, as is the related claim that real subsumption is a sufficient condition for this to happen.

If their argument does not follow from Marx's logic, Hardt and Negri need some empirical justification for their claims. The analysis of s/v is clearly problematic if the bounds of the working day ($s + v$) are subsumed into life in general and it is easy to think of examples of grey areas where the bounds of the working day are uncertain, including cases of workers applying creative skills in a networked society. However this cannot be a two-way street unless capitalism has indeed collapsed by the osmosis of Hardt and Negri style globalization. Of course, workers have the freedom to be act on their own account beyond the realms of the working day, and some freedom to act independently during the working day. It is obvious that in the first case the constraint is the length of the working day itself and in the second it is the regime of capitalist control. There is little evidence that the working day is becoming shorter, or that the regime of capitalist control is weakening (for recent examples of how managers are increasing control through measurement as part of the neo liberal project, see Bohm and Land, 2007; DeAngelis and Harvey, 2006; Dowling, 2007). It is even less obvious that the freedoms in both cases are advancing to absolute levels, at the same rates, or are even direct substitutes for one another in the sense that categories such as 'affective labour' somehow transcend the working day.

By the time the third non-sequitur is reached, the logic is stretched so far that it becomes difficult to deal with without constantly reprising the inconsistencies in the first two. In view of these inconsistencies it is also difficult to form an opinion about what is meant by either of the concepts that Negri is trying to relate. For example, in this case the reader is left to interpret what is meant by "the subsumption of the social whole by capital". These related suggestions support Hardt and Negri's claim that under fully realized labour subsumption all human activity is productive. This achieves consistency with the claim that "the entire time of life is devoted to production". It is immediately obvious that not even the whole of the employed workforce is devoted to production (following Marx's categorization in the unquoted adjacent passages to Negri's citations), let alone the unemployed, the insane and the asleep. According to Hardt and Negri (2004: 130-131), the unemployed poor are indeed participants in

biopolitical production. In a chapter attacking Marx for tautology, in this statement the biggest and most nugatory tautology is uncovered, that life is production and production is life. It is on this preposterous fusion of idealism and materialism that the claim of immeasurability ('who measures whom') is advanced.

If all this seems harsh, there is one set of circumstances in which 'excess value' can be based on a logically consistent theory of value, and which follows from Hardt and Negri's notion of 'Empire'. Imperialism and monopoly capital are barriers to the realization of profit, following Lenin and Luxemburg (Hardt and Negri, 2000: 233-234), which capitalism destroys in the transition to Empire. Accordingly there is a point where the limits of capitalism are reached, when primitive accumulation ends and the whole globe is the subject of capitalist relations. Now, if as is possible, we extend Hardt and Negri's analysis further, so that at this point the conditions necessary for the realization of surplus value, that is, the incorporation of new sources of absolute surplus value through primitive accumulation, cease to exist, then so do the countervailing forces that prevent the law of the tendency of the rate of profit to fall. As the process of capitalist competition and the associated replacement of variable by constant capital continue, the rate of profit declines to zero, at which stage it becomes sensible to generalize surplus into an excess value attributable to the collective efforts of affective labour.

If this interpretation of Hardt and Negri's argument is correct in an ontological sense, then it is only their history that is at fault. It is obvious that the 'zero-profit, excess value' transition point from Imperialism to Empire has not yet been reached and indeed is some way off. Not only are large parts of the global economy still outside capitalist relations, the vast majority of its workforce do not possess basic literary skills, let alone access to the rhizomatic delights of the internet, or the luxury of self valorization through affective and co-operative labour. In 2007, 3.5% of the population of Africa had access to the internet (www.internetworldstats.com, 11th January, 2007). For the same reasons claims made about the spread of the 'general intellect' tend to be overstated. Yet Hardt and Negri are able to date the origins of the age of immaterial labour and immeasurability to the upheavals of 1968 (Hardt and Negri, 2000: 266; Negri, 2003: 77), the subsequent exchange rate crisis of 1971 and the resultant interweaving of political and economic relations producing the 'globality' of biopower and dissolution of every fixed measure of value (Hardt and Negri, 2000: 354). Implausible as they seem and notwithstanding the deductive problems discussed in this section, it is worth subjecting these claims to further scrutiny by examining how well they are likely to work as explanations of empirical trends.

Measure and immeasurability in the real world

Immeasurability arises from the loss of specificity arising from the processes as suggested by Negri (1999: 78-79). The international division of labour and post-colonial exploitation have not come to an end, but have lost their specificity, and therefore the possibility of reactivating the theory of value in concrete instances. Because these types of exploitation have become globalised, "the measure of exploitation has definitively declined" (Negri, 1999: 78).

Negri cites no empirical evidence to suggest that the use of such measures has declined. This is perhaps not surprising since if the measure of exploitation is taken to be Marx's s/v , there are few if any examples of capitalists using such measures in economic calculation, not least because of the conflation of price effects in ex post accounting measures and the theoretical and empirical difficulties associated with the computation of socially necessary labour for any given production process. In common with most Marxists therefore Negri (1999: 79) is content to blame conventional political economy for ignoring these problems. However the empirical side of the question remains important because if economists agree on the conceptual difficulties of measuring s/v and there is no evidence that capitalists have ever routinely included it in their calculations, it is difficult to see the purpose of saying that "the measure of exploitation has definitively declined".

Negri attacks this problem from a different angle, using a crude Periodization. He argues that labour power could be controlled, and measured, in days when it enjoyed relative independence. Subsequently capital succeeded in defining "the use value of labour power in terms of exchange value", first in the USA in the 1930s, then in Europe in the 1950s and then in the Third World in the 1970s (Negri, 1999: 82). No references are cited and the meaning is unclear, but if Negri is referring to the origins of exchange value as a theory of value and the displacement of labour power with utility as the source of inter-temporal value theory (Böhm-Bawerk, 1889 [1891]) then he surely means the development of marginalist economics from the 1880s. To disprove Marx's notion that the origin of profit lay in exploitation was an intellectual crusade in both Europe and America at this time (Desai, 2002: 61).

If the history is confusing, then so too is Negri's corresponding conceptual terminology. He suggests that Capital's requirement for an independent unit of measurement no longer exists, since today "one cannot imagine a definition of use value that could be given even partially independent of exchange-value" (Negri, 1999: 81). Although Negri is referring to cases of independent use-value creation by labour, there is no difference between this definition and Marx's definition of a commodity as its dual nature of exchange value and use-value (Marx, 1990: 138). However Negri is also arguing that use value has now been subsumed by exchange value, implying a transition beyond commodity production. Meanwhile, it follows from Marx's definition that if globalization (or 'Empire' for that matter) means the denomination of all relationships in money, as suggested by Negri, it also means the commodification of everything. Negri however sees what Marx calls commodity production at a moment in history, so that Negri is able to draw the conclusion referred to in the previous paragraph, that capital succeeds in defining "the use value of labour power in terms of exchange value", first in the USA in the 1930s, then in Europe in the 1950s and then in the Third World in the 1970s. So here we have the short history of commodity production according to Negri, which is born after the age of imperialism and dies in the post 1968 world of immaterial labour and immeasurability in which "labour is neither outside nor inside capitalist command" (1999: 80).

If Negri's argument makes little sense from an empirical point of view, how well do its conceptual components work as a blueprint for understanding the workings of the modern economy? The substance of Negri's argument, in the absence of empirical

grounding, is based on the marriage of Foucaultian biopolitics and Spinoza's power to act, so that "the more the theory of value loses its reference to the subject (...as a basis of mediation and command), the more value resides in affect..." where affect is the "power to act" (Negri, 1999: 79). It follows that affected labour makes labour power more important in production whilst at the same time making its measurement more difficult. One can certainly concur with this, as would the dynamic capabilities and knowledge-based view of the firm theorists, who show that as the firm invests in assets such as specialized production facilities, trade secrets and engineering experience (Teece et al 1997) over time (Dierickx and Cool, 1989), tacit knowledge is embedded in technically complex routines. Individual knowledge creation, which firms can then apply (Grant, 1996) leads to a tension between rent appropriation by individuals and team-based profit appropriation. For Grant the firm is set of incentives for co-ordinating team production. Therefore where a soccer team has complementary skills, the team members need to be tied together by long-term relations (Grant 1996: 113). If individuals understand their role in the team's effort bargain they will resist the appropriation of surplus by external stakeholders.

Hardt and Negri have ignored these and other insights from the management literature. Even so, none of this leads to the conclusion that capitalism has reached such a state of development that all is now beyond measure and that value theory should be abandoned. Indeed a knowledge-based theory requires the assumption (a physiocratic assumption in the words of Grant, 1996) that the source of value is attributable to human action within the productive process. Negri's paradox of labour's increasing importance but difficulty of measurement would appear to stem from the issue of control. Because the labour process occurs in conditions which are difficult for principals to monitor, control is a problem for capitalism.

However, the problem is a long standing one and pre-dates the age of 'Empire'. On the one hand agency theorists have constructed a theoretical world in which control is achieved through self interested behaviour of participants in 'efficient' capital markets (Jensen and Meckling, 1976), which are not dissimilar to Negri's world of dominant financial markets and exchange value. On the other there are managerialist mechanisms such as the cultivation of trust (Friedman, 1977), 'job enrichment' (Herzberg, 1966), and the mix between behaviour and output controls (Ouchi and Maguire, 1975). These and subsequent contributions have emerged inter alia out of a long-standing recognition of subjects' 'power to act'.

All of which suggests that Negri's affective labour contains categorical distinctions which require new attempts at solutions to old management problems. Affective labour as the power to act is defined as within the more general category of immaterial labour. Hardt and Negri (2000: 53) extend immaterial labour to include first, communicative labour of industrial production that has become newly linked in informational networks. Second, there is the interactive labour involving symbolic analysis and problem solving. Third, there is labour involving the production and manipulation of 'affects', for example the production of community, human contact, health service, entertainment industry, intangibles etc (Hardt and Negri, 2000: 292). Immaterial labour and affective labour accordingly produce a new collective subjectivity that post-industrial production depends on (Day, 2002: 1078).

None of these developments would however seem to exacerbate the perennial problem of capitalist control. Network embedded communicative labour implies information symmetry and knowledge sharing likely to attenuate the control problems of incomplete contracts and absence of trust identified by the agency and managerial theorists respectively. Indeed, co-operation would seem to be completely inherent in all forms of immaterial labour. Activities involving interactive labour and the production of community and human contact are likely to have similar effects. Whilst it is true that intangible assets pose more difficult valuation problems than tangible assets, the health, entertainment and similar industries, have long produced such intangible outputs utilizing human processes and there is no empirical evidence to suggest that the labour process is shifting towards less tangible activities. Even if taken together, these new classes of labour are symptomatic of the reconfiguration of global labour processes, there is no empirical evidence of a capitalist response to the resulting crisis of control. Immeasurability and its spread imply a specific crisis of risk transfer from labour to capital, to which the rational response of capital would be more intense monitoring. We might for example expect capital to shift away from output controls (which rely on ex post accounting measures) in favour of action controls (the dichotomy follows Ouchi and Maguire 1975), which involve direct surveillance, and to prefer labour contracts based on piece rates to time-based salaries, so that capital does not bear the risks of immeasurability. If anything the opposite has happened where immaterial labour is deployed and clock cards are phased out, suggesting that capital imposes control through quantifiable output measures in preference to the transaction costs imposed by close monitoring of time utilization (for a good example of the spread of this type of control in the education sector see De Angelis and Harvie, 2006). In other words it is equally plausible to argue that a shift to affective and immaterial labour involves increased control through measurable outputs against targets.

If the consequences of immeasurable affective and immaterial labour are debatable from an economic point of view, the sociological implications are also questionable. Hardt and Negri suggest that in these 'new forms' (2000: 29) of productive labour, exploitation immerses labour in social relations but at the same time activates critical elements that develop the potential of resistance. Here the 'potential of resistance' is the obverse of capital's problem of achieving control in the face of incomplete contracts or the absence of trust. In judging the behavioural reaction of the worker to capital's attempt to overcome these problems, an ontological leap of faith is necessary to suggest that the corollary is an attitude of resistance. The propensity to resist (or to comply) with the requirements of capital on the part of immaterial labour would seem to be contingent upon a number of conditions, including the position in the organizational hierarchy, the perceived effectiveness or otherwise of controls and the distribution of surpluses and rewards within the organization.

Armstrong (1991) shows that capital can more effectively overcome control problems by devolving trust and commensurate rewards through limited levels of the corporate hierarchy and denying access to the remainder. The idea is similar to the economist's notion of 'efficiency wages' (for example Katz, 1987), albeit applied to the top of the corporate hierarchy. Strategic jobs, which are difficult to monitor, in turn attract a loyalty dividend (Wright, 1985). The effect of such policies would be to create an attitude of compliance amongst an important minority of employees, which under a

reconfigured labour process would more likely include those engaged in the various categories of immaterial labour. Similarly, top management might employ ideology to mitigate apparently selfish utilization of tacit knowledge to consume wealth within the organization, consistent with Penrose's view that firms and their managers are essentially profit-orientated, and that managerial opportunism and the agency problem constitute only a special case (Lockett and Thompson, 2004). However, there is little rational basis for managerial pursuit of abnormal profit, since it derives from a contradictory appeal to the selfish interests of another group, i.e. the shareholders, and because normal rather than abnormal profits are a sufficient basis for the survival of the firm. In other words, for all these reasons, the potential for resistance is limited and occurs only within the parameters set out by the normal process of labour subsumption and the contradictory position of managers in capitalist social relations.

Conclusion: 'Resistance is useless'

Insofar as resistance is possible, it would appear to manifest itself in the form of self-valorization and self-valorization represents an extension rather than negation of the valorization component of the general labour process. However, for Hardt and Negri, self-valorization does not mean the private appropriation of rent by poorly monitored employees. Rather it is the virtual power of labour to create an 'expansive commonality', which (of course) is beyond measure. Why self-valorization should take this form and not rent appropriation is not clear, nor is the material basis for the shift from work-based resistance to virtual empowerment. Even so, we are expected to believe that because workers through their own actions are creative beyond the control of capitalism, that the concept of organization as a contested terrain can be abandoned.

Resistance is political as well as economic, and Hardt and Negri translate their understanding of the new organizational terrain into a programme of political action. Here is their equivalent 11th thesis on Feuerbach and possibly the reason why *Empire* has been compared to the *Communist Manifesto*. So what does the programme of action amount to? In the world of *Empire* "militants resist imperial command in a creative way", and "militancy...makes rebellion into a process of love" (Hardt and Negri, 2000: 413). *Empire*'s last few pages read less like a manifesto than a statement of absolution for the stereotypical 1960s hippy with a subsequent career in the 'creative' industries, whose angst at the working class defeats of the last two decades of the twentieth century has been mitigated only by disproportionate consumption of the world's resources and the inclusivity offered by the proletarianisation of culture. If the argument put here achieves anything, it will be to reduce the toxicity of 'militant' as a term of abuse when used by the right-wing press.

It is with the use of several important terms, including immeasurability, that the book has its most serious problems. As has been demonstrated, terms are poorly defined, are logically inconsistent and do not relate well to empirical evidence. For these reasons the immeasurability concept fails to undermine value theory and is itself valueless.

Meanwhile Marx is relied upon by Hardt and Negri and caricatured at the same time. Marx's triumph was that his work was consistent, philosophically, economically and

politically, his tragedy was that his work was nonetheless poorly understood by 'Marxists' ready to use his ideas for their own ends. In the case of Hardt and Negri, *Empire* will of course be misunderstood precisely because of its inconsistency. Tragedy will not however follow, since all that seems to be inspired is for the reader to carry on with what they are already doing. Resistance in this form will not strike fear into the boardrooms; it cannot be valued, it is immeasurable. It is useless.

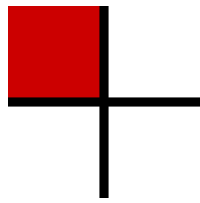
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Mary Follett on the Leadership of 'Everyman'

Nanette Monin and Ralph Bathurst

abstract

Mary Parker Follett wrote on business administration almost a century ago. Leading management scholars today generally agree that although often invoked and lauded her work has never been widely read or discussed. Our paper argues that it should be. Our close reading of *The Essentials of Leadership* demonstrates that Follett's ideas about leadership are not only seminal to current leadership theory, but are also more complex than readings to date have acknowledged. Follett argues that the primary responsibility of leadership is to discover the sense-making thread that structures understanding of the 'total situation', establish the 'common purpose' that emerges from this, and by leading, 'anticipating', make the next situation. But paradoxically, because time ensures that the situation is always transitory, it is always finally unknowable to the sense-maker. Leadership entails working with limited understandings. Reworking these abstractions, Follett also argues that leading and following are not antithetical because both should be lead by common purpose. Her arguments subvert the word 'leader', for her text ultimately suggests that a leader is 'Everyman'.

Introduction

In our forgetting of the foundational theory of Mary Follett (Graham, 1995)¹ there has been no forgetting of the leader-manager of today – that very late twentieth century invention, the material celebrity with the hefty remuneration package – because he² was absent, not there, never in Follett's writings to be forgotten.

Follett wrote of 'leadership' as a necessary management skill, and of those carrying out the functions of 'business administration' as 'managers', but never of a hero leader-manager. Although rhetorically ubiquitous notions of the hero manager in one guise or another are as widely taken for granted in established management theory as in guru books, our paper demonstrates that he was not conceived within the rich, enfolded

1 All the eminent management theorists who authored chapters in Pauline Graham's book speak in one voice of the on-going absence (since her death in 1933) of any substantial representation and/or discussion of Follett's contribution to management theory.

2 Although Follett often wrote women of diverse stature and interest into her theory she always wrote of her manager and leader subjects as 'he'. In this gender bias she was of course simply voicing the conventions of her historical time. While writing about her writings we have reflected this historicism.

layers of Follett's dignified prose. Notions of the celebrity 'leader-manager' so familiar today proliferated rapidly after Drucker (1955) had proclaimed the emergence of 'a new leading class', but where other theorists have represented their leader-managers in images that range from that of those born with a moral birthright to command lesser men (Taylor, 1911/1967) and the leader of a new leading class (Drucker, 1955); to the good cowboy out in the wild west (Mintzberg, 1973) or an American world-ruler (Kanter, 1984); Follett's manager is simply and humbly responsible to and for others (Monin, 2004).

That Follett did not assume that managers are born with, or should aspire to, the kind of status we have chosen to accord them today may well be one sound reason for our forgetting what Mary Follett's seminal work had to teach us about 'business administration'. Other comment has offered many alternative explanations for lack of interest in her theories: in the twentieth century climate of aggressive labour relations her texts were thought to be subversive (Drucker, 1995); her contribution was discounted because she lived at a time when women struggled for recognition in a man's world (Kanter, 1995); it is not her gender identity that has been problematic but that her ideas are innately 'feminine' (Stivers, 1996); she was a positivist espousing managerialist theory (Taced-Sherrif and Campbell, 1992; Newman and Guy, 1998); she was also an anti-positivist in a scientist world (Calas and Smircich, 1996); she was both reasonable and romantic (Parker, 1995); and although she is unpopular because she champions individualism, she is simultaneously misread as preaching the subjugation³ of the individual to the welfare of the group (Tonn, 1996). Debate of this order illustrates that recognition of plurivocality in management theory is alive and well.

There are though at least a couple of ironic twists to this excited argument: while we generate energy around our reasons for not reading Mary Follett's management theory we still do not read her; and because we do not read her, we neither discuss nor contest her theories. Yet amidst all of the debate noted above it seems to be a given that we do know of her work and that we ought to be reading it; for although they continue to be seldom read, her theories of what she called 'business administration', have been consistently lauded through generations of scholarship.

Mary Parker Follett, a new-world woman who was as much at home in cosmopolitan European cities as she was in the rural seclusion of Vermont hill-tops, was undeniably a pioneer, a foundational management theorist. Our paper offers a close reading of her lecture on 'The Essentials of Leadership'. It was originally delivered in 1933 to the newly formed Department of Business Administration at the London School of Economics (University of London) as one of five lectures by Follett in a series entitled *The Problem of Organisation and Co-ordination in Business* (Urwick, 1949: vii). In 1949 Urwick published all five lectures as well as a sixth, 'The illusion of final authority' (delivered at a meeting of the Taylor Society in 1926), under the collective title *Freedom and Co-ordination: Lectures in Business Organisation by Mary Parker Follett*. This collection was reprinted in 1987 and published by Garland Publishing, New York and London; and this is the publication to which our references to 'The Essentials of Leadership' all refer.

3 Follett writes of the group in which an individual is self-actualised.

Like all of the other five lectures in this collection of Follett's work edited by Urwick, 'The Essentials of Leadership' was prepared for delivery to an audience of business administrators, but in this published version, though it does not claim the genre, it takes on the appearance of a formal essay, which can be defined as:

any short composition in prose that undertakes to discuss a matter, express a point of view, [or] persuade us to accept a thesis on any subject. [It does not pretend] to be a systematic and complete exposition, [is] addressed to a general rather than a specialized audience... [and] discusses its subject in non technical fashion... often with a liberal use of such devices as anecdote, striking illustration, and humour to augment its appeal. (Abrams, 2005: 103)

In this paper we will read Follett's lecture as an essay, and will explicate our close reading of it in a commentary that moves casually between dominant and critical approaches, an approach to text analysis described as 'togglng' (Monin, Barry, and Monin, 2003). But because it is easier we think to explain in a clever, readable way what a text lacks than to explain exactly what it achieves, and having done our share of the former – see for example Sayers and Monin (2007); Monin and Monin (2003, 2005) – in this paper we intend to focus primarily on the latter aim. In a poststructuralist world it is a given that every reading of a particular text offers just one of an infinity of possible interpretations, and that no one reading escapes the bias inherent in an individual reader's response. Since our reading is also bounded by a word limit we are self-consciously choosing to use the space we have to show why Mary Follett's essay on leadership is a classic, a work to which we may return again and again as we continue to mine its rich meaning.

As we complete our close reading of this text, we will also refer to Kenneth Burke's theory of logology to support the notion that the word 'leader' has evolved to a point where the identity that managers have had imposed upon them is now dependent on what Burke denoted a god-term; and we will suggest that if this is so, then we may have finally reached Follett's historical moment. In his logological theorising Burke argued that over time certain key words accrue more and more power: as language itself propels an upward drive the constructions that we attach to these words are weighted with ever-increasing status. Eventually a hierarchical end-point is reached and language doubles back on itself: it somersaults backwards through its own etymology, looking to recover its own historic meaning.

In both popular usage and leadership theory there is evidence to suggest that the word 'leader' has become a god-term. Over time we have seen, higher ranked administrators renamed managers and better managers denoted leaders. But now the best leaders are called leaders of leaders (Teo-Dixon and Monin, 2007), or 'exemplary leaders' (Heard, 2007), and in the rhetoric of business language the word leader has come to connote power, wealth, status, celebrity and even fame. Noting this historical development, in our paper we hypothesise that if 'leader' is now a Burkean god-term, and is therefore about to turn turtle and take all its connotative trappings of celebrity wealth, power and social status with it, we may at last be ready to return to Follett. She writes not of a class of leaders but of the leadership that is enacted in different situations and at different times by diverse people.

Our reading of Follett's chapter on 'The Essentials of Leadership' (Urwick, 1987) attempts to give the logological drive at work in 'leadership' a good shove. It demonstrates that almost a century ago Follett was denying the designation 'leader' all the celebrity status that has since accrued around it. Follett asked us to think less about leaders and more about the meaning of 'leadership'. If we are ready, now, to listen to her voice then we may already be enacting a logological leap back towards recognition that leadership is a communal, not individual, achievement.

And yet our reading also concludes that from within all the permutations of ambiguity and paradox that play through Follett's text, a composite picture of an ideal leader/manager/person (in Follett's text each term is embodied in the other two) does emerge. As she tells it, enacting leadership that seeks to best serve common purpose, entails acknowledging our limited understanding of both the situation in which leader leads, and of what the optimum group outcome might be. Follett's leader, even while he endeavours to enact the skills and traits of leadership that Follett imports from the contemporary literature and extensively discusses, also tries to understand, and work with two elusive abstractions, transitoriness and polyphony.

At first her discussion of the leader's need to understand the total situation is underpinned by her emphasis on the transitoriness of that situation (leaders must accept that even as the situation is being understood it is already changing). Then, within an all-encompassing contextual irony, she links the dilemma of living and working within the province of relentlessly passing time to her discussion of the purpose of leadership. As she describes it, the primary responsibility of leadership is to attempt to discover the sense-making thread that structures understanding of the total situation; but just as time is constantly dissolving the situation, so it never exists other than in its passing, so too polyphony, the play of multiple meanings, ensures that any final capture of the meaning of a situation is as elusive as the pursuit of time itself. Follett's paradoxical insistence that the unattainable is worth pursuing offers one more possible reason for our discard of her wisdom; and distinguishes Follett's voice from that of her own contemporaries as well as ours.

While working with these abstractions, Follett also argues that leading and following are not antithetical: both should be led by the common purpose discoverable, but never completely discovered, in the 'total situation'. Attempts to approximate as best the leader can what the ever-developing situation is, and then to determine the best outcome for those being led, is the leader's on-going challenge; and should determine the action outcomes that sense-making dictates when pursuing that which best benefits the greatest number. However, Follett does not advocate a reductionist view. The common purpose is not unified into a single narrative but is rather alive with possibilities; where multiple readings of the ever-changing situation allow for creative and new solutions not yet considered.

In all of this Follett's image of a leader is no more nor less than that of an ideal 'Everyman'.⁴ He does not exist because he is an ideal: but the potential to become an ideal leader is the birthright of the ordinary person, of every man. In the archaeological chip that contains almost all of the leadership theory constructed since Mary Follett first delivered her lecture on management leadership, there is an 'ideal' leader who is a bit like you and me.

Follett's Contemporary Voice

Follett's lecture, 'The Essentials of Leadership', opens with a challenge to contemporary established leadership theory. She tells us that the psychologists – whose leadership tests she has tossed aside with the scornful question "What on earth has all this to do with leadership?" and dismissive answer "I think nothing whatever" (p. 48)⁵ – that "These psychologists were... *assuming* that aggressiveness and leadership are synonymous", that "the leader is usually *supposed* to be one who has a compelling personality" (p. 48, our italics). As critical twenty-first century readers of her early twentieth century management theory, we settle in for a comfortable read, and it grows more comfortable as she continues to comment on 'long accepted' (p. 47) notions, and even, finally, the 'superstitions' (p. 58) on which accepted notions of management practice are based.

These accepted notions, these superstitions, she categorises as 'old-fashioned' (p. 47), the "old method of procedure" (p. 56), but her language also constantly depicts these 'old practices' as aggressive. Although her voice maintains its gentle, conversational tone, managers and management are portrayed in terms of dominance and militancy. On just one page (p. 48) she writes of both psychology and the business world assuming that leaders need to be 'autocratic', 'aggressive', 'masterful', 'dominating', 'compelling' and 'imposing': of their assumptions that leaders need 'self-assertion' and 'pugnacity' for they must 'militate', 'lay down the law' and 'give orders'. She writes of the young boss of a gang who as an adult showed no power of leadership in his community not "*in spite of* his dominating traits but *because of* them" (our italics). She goes on to write of managers who hold "the whip hand" (p. 58) and tells us of her intense dislike for the simile of a writer who "says that running a business is like managing an unruly horse" (p. 48).

Persuasion, as an attribute of leadership, though not as extensively indicted as domination, is Follett tells us, just as 'out of fashion' in the best run industries: *knowledge* is the first requisite of leadership (p. 50). Her prescience in prioritising knowledge is directly followed by her survey of what might still today be accepted as a summary of all the essential elements of leadership. Against all "the theory of the past" (p. 49) she juxtaposes her (then) very different view of leadership in the 'best-managed' industries (p. 49), naming and discussing as she progresses through her lecture, all the

4 Originally attached to a character in a sixteenth century play morality play, the term 'everyman' now suggests 'the man on the Clapham omnibus': a hypothetical ordinary person who is reasonably well-informed and responsible, but without special skills or status.

5 All references to Follett's text are to 'The Essentials of Leadership'.

main leadership theories promoted in the contemporary leadership literature of today. Situational, contingent, transformational and psychodynamic, path-goal and leader-member, skills, style, trait and team – the theories of leadership discussed in contemporary literatures are all introduced here.

Noting the skeletal listing above we might well conclude that although Follett may have achieved a prodigious feat of imagination and foresight, and may even have made an early contribution to the later flowering of elemental leadership theories, her text is very much just an interesting historical stepping stone. Reading through the sudden little flurry of comment on Follett that has followed hard on the heels of *Mary Parker Follett: Prophet of Management* (Graham, 1995) it does seem that courteous dismissal along the lines of: 'this is historically interesting but has little to tell us that we don't already now know', is still the predictable response of contemporary leadership theorists. We contend that where this is the case, it is a response based on a cursory reading, or worse still, on a second-hand knowledge of Follett's writings.

Our own readings of 'The Essentials of Leadership' reveal Follett pushing sense-making in all the familiar contexts noted above, way out beyond what twentieth century leadership theory customarily displays. Her leadership theory is as multi-faceted as the endlessly passing, and infinitely varied moments she positions at the core of leadership awareness when decision-making is called for. Yet through it all she argues that the leader has one over-riding task. The leader's job is to pull on the thread of meaning that seems to structure the situation and discover within it the action that will result in the most satisfying outcome for all involved while at the same time holding each resolution lightly.

Close Reading

Close reading as practised by Richards, and the North American movement that based their approach to reading on his so-called 'new criticism', can take us a very long way into the meaning of a text. With its emphasis on thematic ideas and imagery, paradox and ambiguity it may well propel us as far as we wish to go. But when it seems limiting it is most often because it does not encourage us to delve into the spaces, the unfinishedness, and the othernesses that intimate their presences as we read – and invite on-going exploration. Nor does it accommodate awareness of the role of the reader in meaning-making, and its adjunct, reflexivity; the role of inter-textuality in both construction and reconstruction; nor even, striving as it does for the one 'right' reading, the play of polyphony.

Noting the limitations of Richard's approach to reading, we have nevertheless worked with the emphasis on internal meaning-making it prioritises, while also acknowledging the mysterious realms of doubt and possibility that a more post-structuralist approach would accommodate. Close reading can tell us in great detail, about the woven themes, the pattern of imagery, the plays on meaning which both display and bind all the elements of Follett's text, but in order to open up 'The Essentials of Leadership' to deeper inquiry, to tease out something of the mystery of the amalgam of mystery,

wisdom, fame and discard that is the history of response to Follett's teaching, we must go beyond close reading to closer than close reading – to deconstruction.

In our close, and sometimes closer than close, reading of Follett's 'The Essentials of Leadership', we explore just three of the thematic strands that weave through it. We begin with comment on Follett's constant return to a juxtaposition of past, present and future time as she discusses from one angle after another the ways in which we must pursue understanding of 'the total situation'; we discuss some of the rhetoric that emotively charges her emphasis on the creative role of leadership (as distinct from 'leaders'), and the personification of the ultimate leader, the abstract notion of 'purpose' (p. 55); and we tease out Follett's play on the paradoxical notion that the better the leader the less the leadership required.

Time – and the 'Law of the Situation'

Because Follett had discussed the need for managers to recognise, and work with, the 'law of the situation' in an earlier lecture in this series, she refers back to that, her lecture on 'The Giving of Orders' (Urwick, 1987) when she says in the second sentence of her opening paragraph: "As I have said in the more progressively managed businesses an order was no longer an arbitrary command but – the law of the situation" (p. 47). In the earlier lecture she had argued that what she was choosing to call "finding the law of the situation", equated to the "depersonalising [of] orders", and is preferable to the "issue of arbitrary commands" (p. 24). Now building on this idea she argues that in order to find the law of the situation we must understand 'the total situation': and that the total situation: " includes facts, present and potential, aims and purposes of men" (p. 51). She argues that the leader's primary responsibility is to try to "*find the unifying thread* in this welter of facts, experience, desires, aims" (our italics) and to attempt to see "the relation between all the different factors", to see the situation as a whole, in its totality. For Follett this thread derives from the entire web of relationships that comprise an organisation, and rather than becoming a mechanism with which the leader can exploit and control outcomes, instead offers the paradox of the part representing the whole which in turn represents the contributing parts. Sense-making as 'the unifying thread' has never been described more succinctly.

Follett's emphasis on the need for leaders to be aware of the complexity of each unique situation is, in contemporary leadership theory, something of a cliché: but the knowledge basis on which she claims leaders must lead is not. Her lecture works towards the paradoxical acknowledgement that what she has just described as the central responsibility of the leader can never actually be accomplished. Because the total situation is never static, and is therefore only experienced in passing moments of time, it cannot ever be fully realised. The situation is gone, past in the moment that it is experienced, and replaced by a future moment. Leaders must attempt to make sense of the moment, recognising all the ramifications of its complexity, and simultaneously, as best they can, act *on behalf of* common purpose, the invisible leader discovered within it (p. 55), fully aware that the situation is changing as they act. In sum, leaders must act, and take responsibility for their actions, while dealing with their recognition that they do not, cannot ever completely understand the total situation on behalf of which they

act. Follett's humility (time is a mystery beyond our imaginings) and her 'negative capability',⁶ (she accepts that we must live in doubt, confusion, and limited understandings) establishes her voice as unique in the canons of management theory, where we are more often bombarded with assurance and assumed closure.

Although of course she never used the phrase 'social construction' Follett essentially argues that at no point does our socially constructed world have a knowable presence – every moment of construction is no more nor less than the outcome of the past moment and a prelude to the next. And yet, she insists, at a practical level we must operate within the context of both this limited understanding, and an on-going (doomed!) attempt to fully understand it. Even as we structure what we can of the total situation by looking for its unifying thread it dissolves into what was once future time, had become the present and has already passed. There is, ironically, no total situation: but the central responsibility of leadership is to understand it and act on behalf of the common purpose it reveals.

Images of time, past, present and future are woven into the entire fabric of this lecture. Again and again the past is juxtaposed with the present and both are set in place against the future: "old-fashioned theory" is being replaced by business as conducted "today" (p. 47); past theory is compared with "today" (p. 49); order-giving of "the old kind" is being ousted by that which is "coming to be" (p. 49); "domination is going out of fashion" as "wiser teachers say" (p. 50); "business is becoming", "men are learning" (p. 50) and by the time we reach the essay's mid point we are primed to learn that the leader must not only:

understand the situation, must see it as a whole, must see the inter-relation of all the parts... [but] must do more than this. He must see the evolving situation, the developing situation. His wisdom, his judgment, is used, not on a situation that is stationary, but on one that is changing all the time. (pp. 52–53)

This is the foundation from which Follett constructs the paradox at the heart of the leader's responsibility for making sense of, and making manifest, the total situation. Although the whole endeavour of the leader must be to attempt to understand all the inter-relationships that in total form a situation, and to then share this understanding with all those involved in it, there is no moment in which it can be fully experienced:

In business we are always passing from one significant moment to another significant moment, and *the leader's task is pre-eminently to understand the moment of passing*. The leader sees one moment melting into another and has learned the mastery of the moment. (p. 53, our italics)

Thus when Follett claims that the deepest "mastery of the moment" demands that leaders knowingly work with transitoriness – even as the leader is visioning a future from both past and present, moment melts into moment and the present is already the past – she also makes it clear that she is setting an impossible challenge. She justifies this by insisting that our attempts to understand, and to vision an ideal outcome, even as

6 In a letter written in 1817, the poet John Keats used the term 'negative capability' to describe the mind capable of appreciating that intellectual acceptance of mystery and doubt is preferable to 'irritable reaching after facts'. He instanced the mind of Shakespeare as supremely capable in this respect.

we simultaneously acknowledge this to be an abstract notion, will enable us to better accept the complexity of the transitory moment while meeting the demands of practical leadership. Ultimately this will make for better leadership practice. And *making* (as very recently 'discovered' and theorised by Senge, Scharmer, Jaworski, and Flowers, 2004 , 2004) is what leadership is about.

'Anticipating' we make, we create, the future – for, as Follett flatly points out “We usually have the situation we make” (p. 53). And the quality of this making depends upon the knowledge that resides in the leader’s recognition of the inter-relationships that structure the situation. The “welter” (p. 61); the “kaleidoscope of pieces” (p. 51); the “scattered forces” (p. 52); the randomness of “sometimes in one... and sometimes in another”; of “facts, present and potential”, (p. 51) that in combination make up all the “experiences and desires” (p. 51), the “aims and purposes and men” (p. 51) challenge the leader’s perception of the total situation. While it is the leader’s primary responsibility to recognise and understand “the *relation* between all the different factors” (p. 51, our italics) in the situation, this is not finally enough. The leader must do far more, must “see the evolving situation, the developing situation” (p. 52) for, as Follett repeatedly insists, the situation is not ‘stationary’.

Leading as ‘anticipating’ Follett argues, “means far more than meeting the next situation, it means *making* the next situation” (p. 53, our italics). The leader is a ‘maker’, *poetas*, the poet, subject of Aristotle’s *Poetics*, and as she writes of the creative force of leadership Follett’s own voice begins to merge with that of her depicted creative leader. As she creates ‘new theory’, Follett describes her ‘conception’ (p. 47) as something that is “forced on us” (p. 47) by business activity: business leaders “penetrate to the subtlest connections of the forces at their command” (p. 52) in order to “*make* all these forces available and most effectively available” to finally serve the one true leader, “the invisible leader” (p. 55), common purpose.

Follett’s Rhetoric

When she writes of ‘making’ Follett’s language takes on a fecund richness. From the straight-forward “conception” (p. 47) of new business practices that she describes in the early pages of the lecture, and the ‘becoming’ (p. 50) of its development she moves within two pages to comment on her own maxim – business success depends on our having the situation we make – that “no one sentence is more pregnant with meaning” (p. 53). Engendering the new is the theme she most intimately interweaves with that of time. When she combines this imagery with energy words such as ‘blaze’ (p. 53) and ‘explode’ and ‘dynamic’ (p. 55) her rhetoric promotes that which she really cares about, advocating a recognition that the ‘new’ way of business is very different, very creative, very energised and should draw everyone, everywhere into its paradoxical, all-consuming ‘making’.

Yet Follett the master rhetorician is also unique among foundational management theorists in overtly recognising that her own tools, ‘words’, can also be misleading, even treacherous, and never more so than when they box in theories. As an antidote to their deceptions and limitations Follett throws the ideas they perpetuate into stark

apposition with experience, with what actually happens in the work-place. Insisting (in 1926) that we have been too slow to see that it is followers who provide leadership, who enable leaders to lead, she says of authors who write only of the following role of followers: "these authors are writing of theory, of words, of stereotypes of the past" (p. 54). In the face of all her own attempts to make words work overtime for her, to call them into play as she tries to untangle the mysteries of time, being and action, she finally pushes all words aside as 'tired theory' and replaces them with experience, the practice of management 'told' by what happens.

Much of the rhetorical power that prompts response to this text stems from plays on juxtapositions and antitheses that mirror the play of parity and disparity in metaphor. Burke famously noted that metaphor is

a device for seeing something in terms of something else. It brings out the thisness of a that, or the thatness of a this. (Burke, 1989: 247)

and in rhetoric all yokings of the one with the other work in a similar way – word choices and placements emphasise, by connection that which is different even as they link that which is similar. In this text Follett again and again places a *this* against a *that*, the one against an-other. She sets images of aggression and domination against co-operation and common purpose; the old against the new; theory against practice; abstract against concrete; and unity and totality against fragmentation and chaos. All these yokings have similarly intensifying effects, but even as they clarify and explain the concepts and qualities about which she is writing, Follett's simplifying and excluding binary arrangements of her text simultaneously inspire an (otherwise unfounded) confidence in the surety of its meaning.

The persuasive impact of this kind of rhetorical balance – the seeming justice of acknowledging opposites – is notably called into play when Follett carefully sets 'difficult theorising' alongside her anecdotal story-telling. Her stated aim is to encourage better leadership at a practical as well as theoretical level; and when her anecdotes successfully encapsulate the theory, her story telling is almost Platonic. When Plato, in his dialectal treatise *The Republic* analogises the necessity for the philosopher king of his ideal state to have absolute authority, he likens him to a ship's captain: ships he stories, would run aground if democratically governed by all aboard. Follett, though she argues against totalitarian leadership, also illustrates the power of a captain's leadership – in a situation where delegated leadership (in this case to the boat's pilot) has endangered all aboard. Explaining that it is not for the leader to either dictate or delegate but to bring others to their own understanding of 'what needs to be done' she relates one of her many stories of personal experience. She tells of her experience aboard a ship that had run aground in rattlesnake infested waters. When the crew refused to enter the water to push the boat off, the owner jumped overboard: "Every member of the crew followed" (p. 57). Here Follett does not tease out the strands of the total situation, common purpose and leadership action that her anecdote illustrates. She simply shares 'what happened', telling the story and leaving her readers to draw their own conclusions. Complicity is a great persuader, and Follett draws her readers into her argument by making space for, and respecting, their readerly sense-making.

Leaders or Leadership?

Follett's lauding of such spontaneous heroism suggests that her text might be after all a prescription for a hero-leader of superhuman strength, intelligence and sensibility. Not so. Paradoxes within paradoxes provide her text with yet another kind of powerful antidote to this sentiment. Her imaging of the total situation has revolved around a paradoxical notion: that the leader's challenge is to manage within the transitoriness of a moment in which we both live our past and create our future even though it is impossible to fully experience that moment. An interweaving paradox of the leader's role plays through this image: in Follett's text there is, finally, no such thing as a leader. There is only leadership.

Although we are all always followers in certain moments, in particular situations, any one of us may be called upon to share our expertise. Within such a call to leadership we remain followers, for 'common purpose' is the invisible leader of both leaders and followers (p. 53); and "loyalty to the invisible leader gives us the strongest possible bond of union, establishes a sympathy which is not a sentimental but a dynamic sympathy" (p. 55). When Follett personifies 'Common purpose' as the ultimate leader her trope effectively argues for leadership, not leaders. This position leads her into the paradox that is central to her argument: because leaders are not leaders but the servants of 'common purpose', the better the leader the less the leadership required. Leaders and followers are both, essentially, followers who are "following the invisible leader, common purpose" (p. 55). The leadership of the leader of leaders (p. 57), common purpose, is enabled by 'leaders' of all positions, personalities and functions (p. 52); and these kinds (p. 58) of leadership can all be learned (p. 58). It is on the basis of this argument that Follett questions the notion that leaders possess any 'final authority', deeming it illusory.

It is her intention she tells us, to "explode" the "long-held superstition" that "leaders are born not made" (p. 58), and it is in this sense that her leaders are everywhere: everyone is a potential leader. And yet, she argues, the most effective of these leaders will have the least leading to do. Because we are all as leaders more essentially followers – ultimately looking to the leadership of the common purpose that sense-making reveals – effective leaders enable the followership of all, themselves included, to enthrone leadership and demote leaders. Follett constantly reminds her readers that the function of a leader is to implement the dictates of the 'leader of leaders': an abstract notion, common purpose (p. 57). As leaders we are responsible to point others to the 'leader of leaders'. Common purpose is endlessly waiting to be discovered in the total situation, and we must look for it in our 'mastery of the moment'. It is to this node of Follett's teaching that we are returned again and again: to her belief that if we allow the 'law of the total situation' to be the over-arching guide to action it will also guide the discovery of leadership itself as the only rightful leader.

In what sense is a metaphysical play of ideas such as this practical? As Grint (2007) has recently reminded us in a discussion of Aristotle's *phronesis*, wisdom is practical. If the result of attempts to understand the paradoxes and ironies of leadership and organisational experience is that we better understand the infinite web of relationships, desires and motivations at work in every situation; and if attempts to grasp the total

situation enable us to better understand the leadership that will work in the best interests of all, then wisdom does have a practical outcome.

Visioning the leadership that the future calls for, Follett cites Wells': "his hope for the future" she reminds us "depends on a still more widely diffused leadership" (p. 59). And if the time is right to not only remember Follett but to also begin to teach and attempt to put into practice our understanding of what she understood, then the logological drive (Burke, 1961) that is pushing along the increasingly weighty etymology of the term 'leader' may at last be about to take a tumble. Her lecture envisages a new era in which individual leaders, as we presently think of them, will no longer exist. The abstract notion of 'leadership' will replace them.

Follett is 'difficult'?

Attempts to convey this kind of ideality are never complete or even satisfactory. And since common purpose can only be supposed through our attempts to grasp the always transitory total situation, much as leaders are not, essentially, leaders, so too the total situation is never finally the total situation for it is always in the process of becoming another situation. It is its past and its future. It is also its present but the elusive present moments that it is the leader's 'pre-eminent task' to understand, are endlessly 'melting' into one another (p. 53). There is no present. Like the Heraclitean fire, we can know it only through its passing, and "the leader's task is pre-eminently to understand the moment of passing" (p. 53).

It is basic to our human condition that we do not arrive – being always in the process of becoming, paradoxically to become is to die. So Kanter (1995) is right when she claims that Follett is difficult – she may certainly seem so in the company of other management theorists. If Kanter is right Follett's 'difficulty' may explain why comment on her theories published within the discipline of management by those who have claimed to understand her, and to have made her theory their own, has tended to whittle down her texts into skeletal mis-representations. In contemporary comment on management leadership theories there is little trace of the complexity, irony, and ultimately the many layered paradoxes, that in Follett's teaching convey the mystery and metaphysics that inform the core of human experience.

But then again Follett is 'difficult' – though she is wise her ideas are impracticable – only if we assume two functionalist positions: that meaning is fixed; and that it is the role of the management theorist to provide firm, immediately and widely applicable, guidelines for action based on irrefutable truths. Given that throughout her texts Follett plays with paradox, irony, ambiguity and the juxtaposition of antithetical notions – and ultimately advises that we "act, whatever our theories, on our faith in the power of the invisible leader [common purpose]" (p. 55) – we can safely argue that Follett herself did not subscribe to either of these positions. Instead she challenged the fixed ideas that were bedded into the theory of her time, and offered a parade of anecdotal experience, to support the play of abstract theorising that challenges her readers to think about concepts such as time, situation, motive, purpose and the common good. That we endeavour to discover the common thread of the total situation, attempt to understand

its significance and then follow the common purpose that seems to emerge from the particular situation is her seemingly simplistic, but elusive contribution to management theory.

Certainly Follett's teaching is not easy to apply. Yet management practice at every level of the organisation informs and permeates her ideas. Changing and renewing these practices is her declared objective. In anecdote after anecdote she tells us that the leadership theory she has distilled from history, psychology and literature is also informed by her experience of business administration, the work place and her personal relationships. She draws on the lives and wisdom of people working at all levels of organisations whom she interviewed, observed and with whom she participated in numerous organisational projects.

Art or Science?

As she has done through this lecture, Follett promotes an image of management as both an art and a science and business as becoming a profession (p. 60). It is a science because as she has repeatedly insisted, we must analyse every situation methodically and consistently. That is how learning develops. Despite the mockery of psychology's leadership tests with which she sets the stage for the presentation of what were then avant garde ideas, she demonstrates elsewhere that she is well-read in and respects this discipline and the learning it offers management theorists. As she moves towards her conclusion, Follett states categorically that "leadership is not the 'intangible', the 'incalculable' thing" we have often seen it described as. It is "capable of being analysed into its different elements, and many of these elements can be acquired and become part of one's equipment" (p. 59). Her imagery at this point could not be more concrete: 'equipment'. We are back with function and the tools of management, but always in qualified terms: 'many' elements are 'capable of' being analysed.

Yet because, as she consistently argues, business is both an art and a science, if we are to manage it scientifically then we must understand it philosophically. As Follett herself attempts to call up the ideals and future we vision, the common purpose we must imagine, and a perception of the total situations that we experience, she repeatedly reminds us that we must work with both metaphysical understandings and learned methods.

Experience then provides Follett's knowledge of administrative practice, but anecdotally she returns to the arts when she attempts to convey the contextual play of irony, ambiguity and paradox within which she works and writes. She reminds us that in *Alice in Wonderland* "[Alice] had to run as fast as she could in order to stand still" (p. 53); and yet even as she is pulling in the authority of great writers to rhetorically support her arguments, in a more reflexive moment she defends this rhetorical practice. Having explained that "what might be called the consent of the governing" is "suggestions coming from below and those at the top consenting" (p. 54), she insists that she is "not trying to imitate Shaw and Chesterton and being paradoxical" (p. 54). In other words she is self-consciously aware that she may be accused of trying to be clever, intending a bit of intellectual fun. Not so. She argues that only through paradox

can she attempt to explain the complexity of the total situation and the web of the social networks that is integral to its evolution. Hence leaders:

must prepare themselves for business as seriously as any other profession... they must assume grave responsibilities [if] they are to take a creative part in one of the large functions of society. (p. 60)

They must also respond to the teasing challenge of the always incomplete. Follett notes that it is the 'left-over' in any decision-making situation that is most valuable. In the left-over, the 'carry-over', there is the wiggle room that all words, Follett's included, and our own, allow. The 'left-over' as we conclude this paper, is the elusive meaning of 'left-over':

It is the left-over in a decision which gives it the greatest value. It is the carry-over in a decision which helps develop the situation in the way we wish it to be developed. (p. 53)

This is the aporia that waits in the wings to be prized apart, explored and described. For now, as we conclude our reading, we are heeding Robert Graves *Warning to Children*: although we have dared 'to untie the [first] string', Follett's text is still 'a neat brown paper parcel'. But if with other readers we could

... dare to think

Of the fewness, muchness, rareness,

Greatness of this endless only

Precious world in which [Follett] say[s]

[We] live

then we might all 'untie [another]... string'.

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In Search of Seminality: David Knights and Glenn Morgan on Company Strategy

Peter Armstrong

Preamble

The purpose of this paper is to question the standards of scholarship now current in the discipline of organization studies. It does so through a close-reading of a paper which has a fair claim to seminal status, since it is widely cited within the literature and is described as seminal in a number of recent contributions. The paper is Knights and Morgan's Foucaultian analysis of strategic management (1991) and the present analysis is best read with that text to hand. Read in detail, Knights and Morgan's paper turns out to be deficient in almost every respect. Their claim that strategic management needs to be understood as a discourse which 'constitutes the subjectivities' of its practitioners amounts to an innovation in terminology only, since there is no suggestion as to what kind of evidence might distinguish that process from the conventional acquisition of technique. Their proposed history of the origins of strategic management does not fit the historical record. Their claims to theoretical and methodological advances over previous major works in the field are not substantiated and depend in any case on serious misrepresentations of those works. They present a drastically one-sided account of Foucault's views on power, the application of which to strategic management is claimed as one of their major innovations. The one positive and original thing they have to say about strategic management – that it confers psychological benefits on those who practice it ('power effects') – is supported neither by evidence nor convincing argument.

The analysis then proceeds to a 1995 paper by the same authors on the introduction of strategic approaches to IT and product management in a life insurance company. Interesting as a conventional narrative, the notable absence of 'constituted subjectivities', 'power effects' and, indeed, of any mention of Foucault in this study is *prima facie* evidence of the complete irrelevance of the 'genealogical' approach advocated in the 1991 paper. This conclusion is reinforced by two recent papers which claim to have provided empirical support for the original Foucaultian approach. Neither of them achieves anything of the kind. The analysis concludes by suggesting that Knights and Morgan's 1991 paper continues to be cited in despite of its evident shortcomings because of the feverish intellectual climate which now prevails within organizational studies. Under pressure to produce innovative work in fields dominated

by established reputations, young researchers may be attracted to any intellectual direction which promises to subvert those reputations and so provide space for new models of academic distinction. This, it is suggested, is the true nature of Knights and Morgan's achievement, and it is one which may account for the failure of the academic community to take due note of the deficiencies of their work. In this sense it may be symptomatic of a discipline in which standards of scholarship have been eroded in a frantic search for novelty.

Introduction: Seminality and the Pathologization of the Literature Review

Not many academic papers are described as 'seminal'. It is an accolade from one's fellow academics to which most of us can only aspire. As conventionally understood, its particular value lies in the fact that it is a gift unsolicited, and one given, moreover, on the basis of careful deliberation by minds which one respects. Knights and Morgan's Foucaultian analysis of company strategy (1991) is one such paper (hereinafter 'K&M 1991'). Over the sixteen years since its publication, the Institute of Scientific Information database lists 135 papers as having cited it as of January 2007. On top of that, it is actually described as seminal in recent papers by Samra-Fredericks (2005) (three times) and Laine and Vaara (2007) (five times).

An attentive reading of the paper, however, suggests another model of seminality. In this second model, seminality seems to lurk within the text itself, in the form of an unacknowledged desire. There is an impatience about K&M's paper, manifest in its insistence on its own novelty and the rough treatment meted out to the works discussed in its literature review. These are not so much drawn upon as resources but seemingly dragged into the discussion only for the purpose of being ejected from it again (c.f. Friedman, 2004). It is as if the writers cannot wait to get to their own unique selling-point. Tacitly but insistently, seminality is being argued for.

In this second model, the aspirant seminalist is no longer passive, as was the case in the first. Seminality takes on the character of a presentation of self, albeit one made through the medium of the academic text rather than face-to-face sociality. It becomes a performance, one which involves drawing attention to the differences between one's work and that of one's antecedents and presenting arguments purporting to show that the differences are improvements. In their nature such arguments will focus on the shortcomings of the work of these antecedents, the more illustrious, the stronger the claim to seminality. To the extent that arguments of this kind depend on the manner of description, it can be said that seminality on this second model is an accomplishment of the skilled academic performer.

Such performances depend on a kind of pathologization of the literature review. It is, legitimate and normal, of course, that an academic paper should make a case for itself. Inevitably this involves drawing attention to the manner in which the existing literature presents opportunities for extension, how it might be complemented either by additional evidence or theoretical reflection or even how it might stand in need of correction. In those aspects, the literature review necessarily depicts a present state of imperfection,

drawing attention in the process to the improvements which the author hopes to effect. Beyond a certain degree of insistence, however, an inventory of the shortcomings of previous research coupled with the claim that these will presently be made good invites the suspicion that a case is being made not so much for the existence of the new contribution, but for its superiority. Thus overstated, the rationale for a piece of research shades by degrees into claims for its seminality, a transformation which is recognizable once it has occurred though there is no way of identifying the precise point at which it happened.

It is not simply a matter of the stridency of expression. An over-extension of the critical process, both in respect of the number of works stacked up in the reject pile and in the density of the faults found in each can convey the same impression: that there is no conceivable prior research which could have satisfied our reviewer, that some form of distinction from it would have been sought whatever statement it had made, and that prior work is being criticized not for any shortcomings in its treatment of the problems to which it was addressed, but for its 'failure' to anticipate in every detail the approach for which seminality is now claimed. How far these remarks apply to K&M 1991 is, of course, a matter for opinion. The principle charges levelled against previous researchers in that paper are:

Alfred D. Chandler Jr., Oliver E. Williamson

Failure to acknowledge the mutually constitutive character of strategy and the problems to which it is addressed

'Intellectual legislation' - i.e. telling people what they 'really' mean

Anachronism

Andrew M. Pettigrew, Henry Mintzberg

Failure to question the view that strategy exists to solve problems in the relationship between the organization and its environment

Positivism

Determinism

Taking an unduly negative view of power

To these accusations Knights and Morgan (1995) add a further charge against all four that their work is a-historical. The extent to which these alleged failings are made good in K&M's own work is a matter which will be taken up presently. Meanwhile the question posed to the reader is what role they play in K&M's exposition. To what extent are they integral to its development and to what extent merely as a foil to the advances which are claimed?

As the world now recognizes, or at least that part of it which has read *Organization Studies*, 27(5), Knights is now a scholar of sufficient standing to have been persuaded by friends and colleagues to publish a *Vita Contemplativa*. Since his joint paper of 1991 on corporate strategy is one of the pillars of that eminence the qualities which mark it

out as seminal are of considerable interest. For young researchers who hope one day to achieve similar levels of seminality they bear on such questions as, ‘What kinds of advance must I make on previous work, how must I present it and with what kinds of supporting arguments and evidence?’ These matters may also be of interest to those who have no such ambition, but must still cope with the workings of the field of cultural production in which they find themselves faced with the problem of making a living.

Outline of K&M 1991 on Corporate Strategy

The basic argument in K&M (1991) is that the process of strategic management cannot be adequately understood as a rational response to market conditions nor as an outcome of a political process in which managers bring a variety of frames of reference to the strategic process. Nor, they further maintain, can its origins be properly comprehended as a search by company management for solutions to the problems of matching company resources to competitive conditions. In order to overcome what they see as the shortcomings of these traditional approaches, they draw upon the work of Michel Foucault to propose that strategy should be seen as a discourse which works by re-constituting managerial subjects as strategic actors who “secure a sense of well-being through participation in strategic practices” (K&M, 1991: 251). From this insight, it follows that the practice of strategic management must have originated in the discourse of which it is an expression and, beyond that, in the conditions of possibility of which favoured the emergence of that discourse.

On the Origins of Strategic Management

K&M (1991) do not claim that their approach supplements or adds a new perspective to previous accounts of the emergence of strategic management. They claim that previously accepted accounts, represented in their literature survey by Chandler (1962, 1977) and Williamson (1975), are actually wrong. Since Knights, by 1991, was an established luminary both of the labour process debate and of critical thinking on management, one might have expected the discussion of these works to begin with Chandler’s self-confessed neglect of labour relations (Chandler, 1977: 493) and the implicit functionalism of Williamson’s transaction cost economics. Instead, both Chandler and Williamson are summarily dismissed in a breathtaking coup of the logomachic imagination, the grounds being that “we cannot accept the legitimacy of imputing strategic intent to business practitioners where the concept of strategy has not yet secured a discursive foothold” (K&M, 1991: 255).¹

1 In this one sentence the authors would appear to have contrived a conceptual engine of awesome destructive power. If it ever became general currency, it could annihilate entire libraries: for example everything on ‘economic’ anthropology, on pre-modern ‘art’ and on ‘infectious diseases’ in the ancient world. To appreciate the sheer silliness which the ‘conditions of possibility’ template can attain in the wrong hands, imagine the following sentence in a magazine for vintage car enthusiasts: “We cannot accept the legitimacy of imputing motoring intent to the drivers of self-propelled vehicles where the concept of motoring has not yet secured a discursive foothold”. The error, of course, is to treat a context-limited vocabulary as if it were an all-encompassing discourse.

Since K&M 1991 cite Williamson (1975) one must assume that they have at least dipped into it. They seem to have managed to do so without realizing that it is a work of institutional economics, not business history, and therefore one to which the accusation of anachronism is irrelevant. It is also a work which mentions strategy only in the 'internal' sense of monitoring divisional performance for the purpose of directing investment and only then in connection with the multidivisional organization. It is true that Williamson (1981), a later work to which K&M do not refer, *is* written as a contribution to business history, but Williamson's concern therein is to apply his transaction cost framework to developments in internal organization, not to consider developments in policy-making at the company level. Insofar as K&M's accusation of anachronism on the question of company strategy has any substance at all, therefore, it can apply only to the work of Chandler.

According to K&M 1991, Chandler was in error in imputing strategic behaviour to the managers of large US companies during the early years of the 20th Century because the 'discourse of strategy' had yet to appear. It is their contention that this discourse only began to evolve from the 1950s onwards and did so, not in the world of business practice, but in the academies. From there, it is said to have diffused outwards, through the vectors of management consultancy, the business literature and 'pro-active MBAs'. From the vantage-point of these insights, K&M 1991 then accuse Chandler (and Williamson) of 'intellectual legislation', on which concept they cite Zygmunt Bauman's *Legislators and Interpreters* (1987). By this, they mean that Chandler's attribution of strategy to management policies in advance of the existence of strategy discourse involves correcting these managers' own understanding of their activities.

First, the citation: a minor irritation but regrettably symptomatic. Bauman's book seeks to relate changes in the characteristic pronouncements of public intellectuals to a progressive disintegration of the moral order. In its early stages ('modernity' according to Bauman) the stance of intellectuals was basically 'legislative' in the sense that they sought to shore up what remained of a unified conception of the good life, refinement of manners and aesthetic judgment. In post-modernity, on the other hand, cultural diversity is supposed to have progressed to an extent which has undermined the legitimacy of any such attempt. Those intellectuals who have adjusted to the new situation now perceive their role as one of interpreting between cultures – hence the 'interpretive stance'. Those who have not made this accommodation persist with the legislative style but now practice it only within the boundaries of their own discipline, neither seeking, nor seeing value in, its imposition on a wider culture.

Notwithstanding the high regard in which Bauman's work is held, the thesis is unconvincing, not least because one could easily argue that there has been no secular decline in moral punditry from intellectuals over the centuries in question. Of more immediate relevance to K&M 1991's citation, the concept of intellectual legislation in the sense of insisting that a word should be used in a certain way forms no part of Bauman's argument: for him the legislative stance on the part of intellectuals always concerns a prescription for the 'good life', not the use of language. This means that K&M 1991's appeal to the authority of Bauman to lend gravitas to their argument is spurious. It is a citation which goes no deeper into the book than its dust-jacket, and it is not the only such citation in K&M's 1991 paper.

As for K&M 1991's substantive point, their claim that Chandler transgressed the mores of intellectual production in his use of the term 'strategy' to describe company policies which antedated the currency of the term is extraordinary coming from writers who are quite happy to tell us that managers are mistaken in believing themselves to be making rational decisions when they are 'really' acting out of a constitution by discourse. If the first amounts to 'intellectual legislation', so does the second. Not entirely unaware of the performative contradiction involved, K&M issue a disclaimer towards the end of their paper, though without withdrawing their original dismissal of Chandler. They are, they tell us, "not seeking to contradict or deny what practitioners do and say when they exercise power through strategic discourse" (p. 270), but how else can one describe their "rejection of the view that corporate strategy constitutes simply a rational response to the environment" (p. 268)?

Where to start?

Perhaps with K&M 1991's social ontology which might be summarized as a linguistic materialism wherein the usual relationship between the sign and its referent is reversed (Eagleton, 1978). In this particular case it is the description 'strategy' which is treated as ontologically prior to the practices which it describes (Gorman, 1993), the implication being that the latter cannot come into being in advance of the former. K&M's justification for this unusual view of the matter is that the discourse of strategy "constitutes the problems for which it claims to be a solution" (K&M, 1991: 255).

In the weak sense that managerial problems always appear as 'already interpreted', this is a notion with which the writer can only concur because it is also employed in his own treatment of management as inter-professional competition (Armstrong, 1984). In fact the idea that managers' functional backgrounds influence their interpretations of critical problems facing their companies has been around at least since the late 1950s (Dearborn and Simon, 1958) and it has inspired a considerable volume of research (Finkelsetin and Hambrick, 1990). None of this is cited by K&M and all of it is more sophisticated than their own work in that it provides a vocabulary wherein it is possible to discuss the *variety* of influences on the managerial perception of strategic options, something which K&M 1991's vaguely-defined 'discourse of strategy', however protean and various this is taken to be, does not. There is all the difference in the world, moreover, between the idea that problems can be variously interpreted and the crackpot notion that they simply do not exist before the evolution of the current manner of their description. This last statement of the issue, indeed, prompts the thought that it is K&M who read history backwards, not Chandler: this because they effectively assume that the practices which go to make up strategy *can only* be described in their present manner.²

2 Again K&M 1991 are not consistent in their scruples over "intellectual legislation", being quite happy to describe Fayol's *Administration Industrielle et Générale* (emphasis added) as a work on *management*. In fact Fayol's term "administration" continued to be preferred in the first English translation of 1930 and it only became a self-declared work on management with the new translation of 1949. On K&M's view of things, therefore, Fayol's work had nothing to do with management until 1949. They cannot counter with the argument that the two terms are synonymous without undermining their prohibition of the use of the term strategy to describe managerial planning before the 1950s, and also without committing an act of "intellectual legislation" of their own against the

In order to concretize this issue, let us take a brief look at the kind of managerial practices which Chandler is accused of illegitimately describing as strategic.

In the first and second decades of the 20th century the managers of the E.I. DuPont de Nemours Powder Company were routinely using the return-on-investment measure to evaluate the cost-effectiveness of integrating backwards along the supply chain so as to ensure the stability of the supply of raw materials (summarized from Johnson and Kaplan 1987: 82, 83). They had also integrated their pricing system and competition policy into a system which achieved the difficult feat of stabilizing production in the face of fluctuating demand, and with it, labour relations. In particular, prices were manipulated so as to keep small competitors in business whilst discouraging new entrants so that it would be the small competitors and not DuPont itself, who would absorb the fluctuations. (summarized from Chandler and Salisbury, 1971: 155).

Can it seriously be maintained that this was not strategic management, or at least something well on the way to it? If the discussion is to proceed, we will have to assume that it can.

The next problem with K&M's account of the origins of strategic management lies in their depiction of its discourse as originating in the academies³ and diffusing outwards thereafter into business practice. This is markedly at variance with what we know of the historical relationship between management practice and management thinking. From Fayolism to standard costing to divisionalisation to just-in-time, to 'flexibility', the historical record of managerial technique is overwhelmingly one of practice-led theory. Far from innovating, the role of management writers, consultants and academics has typically been to codify 'best practice' as they have encountered it in the field and from *there* to diffuse it into other companies. Indeed the fact that the latest 'best practice' is already in proven operation amongst a client's competitors has typically been a major selling-point.

Fayol's 'principles of management' are a case in point. K&M (1991) write that these were "developed in Europe [but were] only gradually and intermittently taken up in practice by European managements" (K&M, 1991: 256), a phrasing which suggests that the 'development' was somehow external to management practice. In fact Fayol's writings were the distillation of many years' experience as a manager in the French mining industry. Likewise, Taylor's innovations – very obviously given the manner of his expositions – were reports of practitioner developments in the management of production, made not just by himself, but by 'a hundred or more' industrial engineers employed in the workshops of North America (Urwick, 1953: 377). These precedents suggest that the assemblage of practices which we now call strategic management is the culmination of a long series of practitioner innovations, not the creation of decontextualised research and profound ab initio thinking in the academies. This, of course, is precisely Chandler's thesis and it directly contradicts that of K&M (1991).

Institute of Industrial Administration. Not surprisingly, that body continued to hold that the term 'administration' was more inclusive than 'management' until it was merged into the British Institute of Management in 1957.

3 Actually K&M 1991 specify the universities of the USA, because in Europe, they tell us, 'the ethos of universities was distinctly separated from that of industry and the world of work'. Whilst this was certainly true of the UK, they forget, or perhaps do not know, that it is certainly not true of the German Technische Hochschulen or the French Polytechniques and Écoles des Mines.

In sketching out the conditions which they say, constituted the ‘conditions of possibility’ for strategy discourse, interestingly enough, K&M (1991) reproduce some of the major building-blocks of Chandler’s history, albeit in drastically simplified form. The developments which they pick out are the separation of ownership and control, the rise of the multinational company and divisionalisation – all of which long predated their dating of the origin of strategy discourse.⁴ In thus replicating Chandler without acknowledging the debt in the relevant passages, and whilst simultaneously claiming to have surpassed him, K&M (1991) commit an academic malpractice which Friedman (2004) has called ‘strawmanning’. In this particular variant of strawmanning, the work of a previous author is criticized for weaknesses which it does not actually possess whilst elements of that work are simultaneously appropriated by the critics and credited to themselves. In this instance, in fact, the accusation may even be too generous because K&M (1991) offer no explanation of the evolution of organizational form. Instead they treat it as something which ‘just happened’ and which is related to the evolution of strategic management only as a condition of possibility of the appearance of strategy discourse. In this respect, theirs is a theorization which involves a considerable degradation of understanding as compared to Chandler’s subtle and heavily documented depiction of the interplay between managerial technique and organizational form.

Where K&M (1991) *are* genuinely innovative, though with doubtful merit, is in their emphasis, not on the concrete managerial problems of aligning company resources, internal design and control systems so as to respond to competitive conditions, but on the problems of reporting and accountability to which these new structures give rise – the ‘discursive spaces’ as they put it. Strategy discourse, ‘in their view’ prospered in this new environment because it provided a language in which management could describe their problems and propose courses of action both to ownership and to each other. But why *that* language? If, as K&M (1991) have already informed us, the problems to which strategy is addressed did not exist prior to the language in which they are now described, how could there possibly have arisen a demand for reports couched that language?

These difficulties aside, there remains the problem of why anyone should believe K&M’s account of the role of strategic management. What evidence would enable one to decide between strategy as a language of accountability and strategy as an attempt to direct the activities of a company? K&M (1991) do not see fit to provide any, but one would expect management reports to be couched in the language of strategy on either hypothesis – and what other possible evidence could there be? And if there is none, why should one accept an account of the origins of strategic management which leaves so much unexplained and throws up so many anomalies?

On the Strategic Process

K&M 1991 are no more prepared to co-exist with accepted views of the operation of strategic management than they are with accounts of its origins. The aggressive attitude

4 Brief as it is, K&M 1991’s excursus on business history provides ample opportunities for incidental embarrassments. In the course of it, they refer to “the huge conglomerates of Rockefeller, Morgan, Carnegie, etc.” (p. 257). Standard Oil and Carnegie Steel as conglomerates!

towards alternative theories is again to the fore. "Analysis of strategy" (K&M, 1991: 251), they write, "*cannot* be reduced either to rationalist accounts of markets and environments nor interpretive understandings of actors' frames of reference" (emphasis added). The deficiencies of these existing approaches, they claim, are made good in their application of Foucaultian methodology.

One of the difficulties in assessing a claim to a new approach written in language so different from that of conventional business research is that of distinguishing innovation of substance from innovation of terminology. In the particular case of *The Foucault Effect* (Burchell et al. 1991), the difficulty is compounded by its association with a broader linguistic turn which denies the legitimacy of any attempt to descry the matter of what is said behind the manner of its saying. Possibly that is one of its attractions: that it facilitates low-cost claims to originality. But unless the critical function in social science is to atrophy altogether, it is also a reason why the attempt must be made.

It is useful to begin by making the null argument: that there is no difference at all between the view of strategy as a rational response to market conditions and the view that it works as a discourse which constitutes managerial subjects. This would give some indication of the kind of counter-argument which K&M (1991) would have needed to make in order to sustain their claim to originality.

We start with the observation that, as K&M's own literature review makes clear, the consultants and academics who created the early strategy literature certainly viewed it as a process of rational planning. For K&M 1991 indeed, this presumption of rationality is sufficient grounds for dismissing that literature as a serious account of the strategic process. Since K&M (1991) also make the point that these writings occupied a key position in the 'genealogy' of strategy discourse, the image of the strategist as a rational planner must have carried into the discourse itself. If, as K&M 1991 further maintain, this is the discourse which 'constitutes' the manager as a 'strategic actor', that implies an actor whose self-conception is that of a rational decision-maker and whose idea of the proper performance of strategy consists of a rational assessment of a company's strengths, weaknesses, opportunities and threats (for example). If practices of this kind are the outcome of K&M's processes of 'discursive constitution', they would appear to be identical to those of managers who are simply following the prescriptions of writers such as Ansoff (1965) and Argenti (1968). The implication for K&M's 1991 theory of the strategic process is that it differs from the rational action approach only in respect of its predictions for the self-images of practitioners. As the proposers of what is claimed to be a distinctive new approach, one would have thought K&M 1991 would at least have considered what form these differences might take and how they might be observed in empirical research.⁵

5 Interestingly enough, this argument will generalize to all claims of discursive constitution. Since, as Foucault informs us, the discourses of the human sciences are of the form power-knowledge, they all incorporate a model of practice, a template of the proper relationship to the objects of practice and a self-conception of the practitioner. The result is that the practitioner constituted by such a discourse is subject to exactly the same internalized constraints as would be expected from professional education and socialization as this is conventionally understood.

A Superior Epistemology?

In the case of the processual and social constructionist approaches to company strategy – represented in K&M’s 1991 argument by Pettigrew, Mintzberg and their co-authors – they clearly *have* considered how their own approach might be claimed to be distinctive. As they put it, “It could easily be thought that our approach to strategy was not so very dissimilar from the processual perspective” (K&M, 1991: 267). Despite a complaint of ‘not enough space’, they find the thought sufficiently troubling to devote three pages to its rebuttal. In the course of these three pages it turns out that their claim to originality does not depend on any difference of substance in what they have to say about the strategic process. Indeed it could hardly do so in a paper which does not actually get to the point of saying anything concrete about it. Instead, K&M (1991) base their claim on their superior epistemology and theorization of power.

The odd thing about the first of these claims is that a paper which says nothing about the strategic process cannot be said to *have* an epistemology on the topic⁶. If no knowledge is offered there can be no operative theory of how it was obtained. What is offered in its place is an explication of Foucault’s methodological apparatus coupled with an attack on the alleged ‘positivism’ of Pettigrew, Mintzberg and their co-authors. The accusation is one which crops up frequently in the work of Knights and his co-authors, though not always in a manner which suggests that they have correctly grasped its meaning.

Pettigrew and Mintzberg commit positivism, K&M tell us, because they explain social outcomes in terms of interests and constraints – or (one senses an uncertainty of footing) that they are ‘inclined’ to do so. It is no act of intellectual legislation to observe that this is simply not what other people mean by positivism. Halfpenny (1982) identifies no less than twelve distinct meanings of the term whilst Keat (1981), less given to the classificatory impulse perhaps, lists only four. Not one of them so much as mentions either interests or constraints. Nor can K&M’s accusations be justified in terms of a more informal usage. Observing that out-and-proud positivism is comparatively rare, Alexander (1982: 5 ff.) suggested that much empirical research is still in the grip of a looser ‘positivist persuasion’. This tendency comprises a number of linked assumptions which are sub-articulate in that they are to be inferred from the actual conduct of research rather than announced in its proclaimed methodology: that there is a radical difference between empirical and non-empirical statements, that ‘philosophical’ or ‘metaphysical’ issues are irrelevant to an empirically oriented discipline, and that theory can somehow be constructed from observation by a kind of induction. Notably, neither interests nor constraints figure even in this in this less rigorous usage of the term.

Alexander’s (1982) observations suggest that the qualitative tradition in organization studies is overdue a period of critical self-examination. Such an effort of collective introspection would reveal that positivism is not the monopoly of quantitative research.

6 This comment is specific to K&M’s 1991 treatment of the strategic process. It does not apply to their remarks on the origins of strategic management since they at least offer a secondary analysis of Chandler’s data, albeit one which is unconvincing.

However commonly it may be believed, it is simply not true that 'soft' data from ethnographic work or unstructured interviews, and the interpretation of that data in terms of social action, automatically add up to a non-positivist methodology. If positivism is to be avoided, the theory-laden nature of interpretations needs to be acknowledged, but, beyond that, the actual lines of influence between theory and interpretation need to be made explicit. The implication is that the conclusions of a non-positivist research should, strictly speaking, be presented in a conditional format: 'if, such-and-such a theory is employed, *then* the data are consistent with such-and-such a conclusion'. By these means it is quite possible to interpret data in terms of interests and constraints without lapsing into positivism. Conversely, it is also possible to produce a positivist research which infers the discursive construction of subjectivities from its data, particularly when dogmatic truth-claims are made for such interpretations as they are throughout K&M's 1991 paper.

All this means that K&M's claim to superiority over other writers in respect of their anti-positivism would require that they consider the dependence of their own interpretations on the prior theory through which they are made. This, in turn, would require them to adopt a rather less dismissive attitude towards the work of other writers. They would need to acknowledge, for example, that the historical record might fit Chandler's version of the emergence of strategic management just as well as, or perhaps rather better than, their own; that managers 'doing strategy' might equally well be enacting the prescriptions of Ansoff as a constitution by discourse. This, however, is precisely what they cannot admit without abandoning their claim to have surpassed these writers.

A Superior Theory of Power?

K&M's claim to a theory of power superior to that of Pettigrew and Mintzberg begins with the accusation that these processual theorists treat power as a property of the individual rather than intrinsic to social relationships. Even if this characterization of processual theory were correct, both the critique of the 'property' theory of power and the improvement on it claimed by K&M (1991) were anticipated by the considerable, if unfashionable, figure of Talcott Parsons (1951: 121). To Hobbes' definition of power quoted as "a man's [sic] present means to any future good" (a property theory of power if ever there was one), Parsons proposed the qualification "that such means constitute his [sic] power, so far as those means are dependent on his relation to other actors".

K&M (1991) prepare the ground for their claim to theoretical superiority by criticizing the processual/social constructionist treatment of power on two grounds. The first is that these writers "cling to certain positivist [again!] attempts to identify and perhaps measure causal processes, as if action can be fully explained through references to interests and the opportunities or constraints surrounding their pursuit" (K&M, 1991: 267). The onslaught continues on the following page, partially contradicting itself in the process:

Despite the power being concealed behind constructions of reality that are seen to structure subordinates' wants, the conception being drawn upon is still the Weberian/Dahl one of getting people to do what they would not otherwise do in the absence of such power. It is still then a deterministic theory. (K&M, 1991: 267)

This is nonsense: no social scientist, not even one of the calibre of Pettigrew or Mintzberg, could simultaneously explain action in terms of interests and constraints on the one hand and social constructions of reality on the other. Besides which one can quote Pettigrew thus (1987: 656), “Neither, of course, given the dangers of simple determinism, should structure or context be seen as just constraining processes. Rather, this approach recognises that processes both are constrained by structures and shape structures”. On the supposed ‘determinism’ of Weber’s and Dahl’s views of power, here are the authors’ actual definitions:

In general, we understand by ‘power’ *the chance of* a man or of a number of men to realize their own will in a communal action even against the resistance of others who are participating in the same action. (Weber, 1947: 180, emphasis added)

the *probability* that one actor within a social relationship will be in a position to carry out his own will despite resistance. (Weber, 1978: 53, emphasis added)

A has power over B *to the extent that* he can get B to do something that B would not otherwise do. (Dahl, 1957:202-03, emphasis added)

Weber’s definitions are clearly probabilistic, not deterministic, whilst Dahl’s is either quantitative or probabilistic, according to whether it is envisaged that the ‘something’ which A is trying to get B to do can be done by degrees. If K&M (1991) still object to definitions of this kind, one is entitled to ask whether they have in mind a definition of power which makes it not even probable that it will prevail. Besides all of which, Pettigrew’s own treatment of power, throughout *The Awakening Giant* (1985), is explicitly anti-deterministic in that it consistently emphasises the part played by argumentative and tactical skill.

K&M’s second ground for criticism is what they describe as the processual/social constructionist assumption that power is “always about getting others to do what you want them to do” (K&M, 1991: 268). On this, they quote a paragraph from Pettigrew (1985) to the effect that power may be used either to defeat or prevent competition over strategic choice. Thus far, the criticism, if it *is* a criticism, holds. Pettigrew’s whole emphasis, however, is on a ‘multi-level’ approach which fuses cultural and political analysis and thus allows space for the understanding and tactical skill required to effect processes of cultural change, to construct effective symbols and to manage meaning (Pettigrew, 1987: 658-9). Whilst an eclectic approach to power of this kind allows for the comparatively straightforward business of ‘getting others to do what you want them to’ – as that of K&M (1991), one might observe, does not – it is certainly not limited to it. Against the approach which K&M (1991) have thus traduced, they counterpose a view of power which they attribute to Foucault:

power is not a property of individuals or groups but of all relations and, moreover, it is not principally about repression and constraint. On the contrary, it is more often positive and productive of subjective well being. Power is positive in the sense that it can transform individuals into subjects who secure their sense of what it is to be ‘worthy’ and ‘competent’ human beings through the social practices that it creates or sustains. (K&M, 1991: 269)

How far this is a fair representation of Foucault’s views on power is a question which will be taken up in a moment. For now, we note that K&M (1991) are proposing a view of power in which it is rarely or never about ‘getting others to do what you want them to

do'. In this they surpass even Talcott Parsons who at least recognised that "the capacity to get things done" might require "people to do things they may not presently want to" (1960: 182, 219-220).

Representations of Foucault

Much of K&M 1991's claim to theoretical sophistication involves setting themselves up as retail purveyors of Foucault to the management research community, a posture which later expressed itself in what must surely be one of the ultimate statements of hubris-by-proxy: *Writing Organization Analysis into Foucault* (Knights, 2002). Accordingly, much of K&M's 1991 paper is devoted to an introduction to Foucault's methodology and his thinking on power. Besides taking their percentage mark-up in the form of personal credit, however, retailers of this kind may make other claims on the wholesale product. They may market part of the range as own-label goods, as, for example, Knights himself (1990) is credited with the insight that through an engagement in discursive practices individuals can be "transformed into subjects who secure their sense of meaning, identity and reality" and Willmott (1990) with the discovery that power can "transform individuals into subjects who secure their sense of what it is to be 'worthy' and 'competent' human beings" (K&M, 1991: 269). A lesser variant of this tendency, and possibly an early stage in its development, is to add the retailer's own name to a citation of the authoritative thinker in question, as if to imply some parity of contribution. Other retailers may tinker with the goods in ways which affect their fitness for purpose whilst still others, as we will see in a moment, stock only that part of the range which they believe will appeal to their particular market.

The page references to Foucault for the view of power summarised in the above quotation from K&M (1991) are to two lectures of 1976 reprinted in Foucault (1980). It is virtually certain this citation was a mistake, and that K&M intended to refer to a brief passage on power in an interview of 1977 which is reprinted in the succeeding chapter. Of this latter passage, the quoted extract is largely a fair representation.

In the two adjacent lectures cited in error, however, Foucault expresses very different views on power, and at much greater length. In sum, the lectures are an extended meditation on how best to 'invert' the juridical doctrine of right which both legitimates and transmits power and so "give due weight to the fact of domination, to expose both its latent nature and its brutality" (*ibid.*, pp. 95-6). In these lectures Foucault does *not* say that it is 'incorrect' to think of power as repression. To do so, indeed, would contradict his characteristic treatment of truth as something internal to a particular discourse and an effect of power to boot. Foucault's point is that it is a mistake to conceptualize power as repression if the object is to get behind the ideology of which that conceptualization is a part – to unmask, as he puts it, 'the fact of domination', something which does not appear to be on K&M's list of things to do. His reasoning begins with the observation that the idea of repression implies that it is rights which are repressed – presumably because there is no place in Foucault's conceptual toolkit for essentialist notions such as natural rights or innate human expressiveness. He then argues that the notion of rights is embedded in a contractarian frame of reference, one in which powers are seen as surrendered to a sovereign authority in exchange for rights granted. This 'juridical' notion of power, he then maintains, has survived into the era of

disciplinary power wherein it now serves both to legitimate and obscure its real nature. From this chain of reasoning, Foucault draws the conclusion that any challenge to disciplinary power in terms of the ideology which sustains it is likely to be ineffective (*ibid.*, p. 108). He has a point of course, though the idea of active consumption (de Certeau, 1984, e.g.: 31) directs our attention to the many instances in which an ideology has been very effectively turned against itself.

There are other features of the discussion of power in Foucault's two lectures which are at variance with the views which K&M 1991 attribute to him. In stressing that power is a pervasive feature of human relationships Foucault is not de-emphasizing the powers exerted by centralizing authorities and nor (it follows) is he arguing that power can only be analyzed in relational terms, if that is understood in the sense of face-to-face sociality. Rather he is making the point that overarching systems of power can only be created on the basis of pre-existing local networks through which there is power already 'circulating' (Foucault, 1980: 98-102, see also Burrell, 1988: 227-8). Nor does he claim that disciplinary power operates primarily through the discursive constitution of subjectivities as K&M (1991) contend throughout their paper. On this point he explicitly states that it "*presupposes* a tightly-knit grid of material coercions" (Foucault, 1980: 104-5, emphasis added). Finally, there is nowhere in either of the two lectures, anything resembling K&M 1991 contention that power produces 'subjective well-being'.

It is not impossible to reconcile these two Foucaults. Power can be both brutal *and* technically productive. It can be productive both in the sense of enhancing human agency and in 'producing' the diminished inmate identities described by Goffman (1968). It can repress some whilst pleasuring others and, for some aficionados of restraint, it may achieve both at the same time. Such an exercise in apologetics, though, might be a misrepresentation in itself. From Foucault's interviews one gets the distinct impressions that he enjoyed toying with his interlocutors, and that he sometimes took up experimental positions in his published work as a way of developing his thought⁷. As Burrell (1988: 222) put it, "Foucault's iconoclasm takes him into positions which are not readily defensible and his refusal to retain one position for longer than the period between his last book and the next is certainly problematic".

It is certainly problematic for anyone who wishes to use Foucault's name to lend weight to a particular view of power, or, for that matter, of anything else. What is objectionable about K&M's importation of Foucault into business research is that it cherry-picks his writings so as to present a benign view of power which is unrepresentative of his work as a whole. There is a Bowdlerised Foucault, a Foucault stripped of his insurrectionary moments so as to fit him for consumption in the schools of management. It is ironic, therefore, that Knights (2002) later saw fit to excoriate the tendency of other (unnamed) writers to "appropriate the glitz [of Foucault] and gloss over the blitz" (p. 590).

7 This interpretation is supported by the very different attitudes towards the concept of ideology expressed in the chapters cited in the main text. In the lectures of 1976, Foucault uses the concept freely. In the interview of 1977 he considers it "difficult to make use of", in part because, "it stands in opposition to something else which is supposed to count as truth".

On the “Power Effects” of Strategy Discourse

As we have already seen, K&M (1991) do not show much interest in the processes by which managers decide upon a company strategy. Nor have they shown more in the effects which strategic management might have on the kinds of policies which companies pursue, a matter which has been hotly debated at least since Hayes and Abernathy's trenchant critique (1980). What they *are* interested in is the effects of doing strategy, or, as they put it, in the constitution of the 'subjectivities' of the managers involved by the discourse of strategy. They call these 'power effects' though actually they are nothing more than a list of the social and psychological benefits which K&M think might flow from the practice of strategic management. The theorization is as primitive as that of Maslow's hierarchy of needs, notwithstanding the invocation of yet another heavyweight thinker. This time readers are referred to J-P Sartre's *Being and Nothingness* (1969) on the 'existential void' which K&M believe to have confronted corporate policy-makers before the advent of strategic management. One searches in vain for any such sense of human futility in the biographies of the great entrepreneurs of the late 19th and early 20th centuries. If any existed, it would seem to have been filled by the collections of artworks and the endowment of charitable foundations rather than strategic management.

Seven 'power effects' are listed, ranging from the near-tautological "enhances the prerogatives of management" (K&M, 1991: 262) to the more arresting "reflects and sustains a strong sense of gendered masculinity for male managers" (p. 263). As a sample of the kind of evidence which might be offered in justification of this latter, K&M 1991 offer the phrase 'market penetration' – *penetration*, geddit? Here, for the reader's consideration, are some more phrases in which, by the same logic, the word 'penetration', "reflects and reproduces what may be termed a 'masculinist' conception of power" (K&M, 1991: 264)

What, then, would lead Cockburn who otherwise provides *penetrating* insights into the gendered labour process to resort to a compensatory theory? (Knights, 1990: 317)

Foucault's demand that subjects begin 'to refuse what they have become' remains something of an empty rhetoric unless there is some *penetration* of the way in which the attachment to a 'fixed' set of meanings is related to the pursuit of subjective security. (Knights, 1990: 329)

Lacking a theory capable of *penetrating* the complex constitution and reproduction of subjectivity, these studies are shown to be inattentive to the ways in which the contradictory, self-mediating relation to nature is also centrally implicated in its reproduction. (Willmott, 1990: 339)

Marx's analysis fails to *penetrate* the sensuous material reality of the interpersonal processes of production and circulation. (Willmott, 1990: 352)

Pollert's study provides a *penetrating* insight into why many women, occupying a subordinate position, are preoccupied with this particular identity. (Willmott 1990: 367)

(Italics added in all cases)

The practice of attributing secondary meanings to a text by reading it for innuendo seems to have entered the critical study of management with the work of Calás and Smircich (1991). The problem with this procedure is that it depends on the prior

assumption of what it seeks to demonstrate, as was pointed out with admirable brevity in Mintzberg's reply to Calás and Smircich (Mintzberg, 1991). By the logic of innuendo, for example, talk of 'market share' could quite reasonably reflect a gendered femininity. Perhaps too, the whole idea of adjusting management policies in response to market conditions, and even withdrawing from markets in which competition is too intense (Hayes and Abernathy, 1980), is a bit girly, or at least less macho than the generic Fordist strategy of mass-production backed up by hard-sell promotion.

There are other cases where the evidence presented by K&M pertaining to the existence of their power effects is decidedly thin. We are told for example that the self-conscious practice of strategic management serves to present an appearance of rationality to outsiders, as if this were an established fact rather than a hypothesis. In fact we already know that it isn't invariably true since Hayes and Abernathy (1980) and the many executives who requested reprints of the article from the Harvard Business Review were outsiders for whom strategic management appeared to be a recipe for economic decline. Even where they can be supposed to exist, K&M's 'power effects' could more plausibly be attributed to the delegated powers of capital attaching to senior levels of management than to a 'discourse' of strategy. Significantly, none of them are justified by reference to the prescriptive texts of strategic management which must surely be the ur-texts of that discourse. Though five pages are devoted to their explication, the question of how they might come about through ingestion of those texts or from the teaching and consultancy which derive from them is ignored, even though this posited process lies at the heart of K&M's theorization.

If we now fast-forward 13 years it is instructive, and depressingly so, to see how K&M 1991's speculations have been taken up in the subsequent literature. Thus Ross-Smith and Kornberger (2004) credit K&M 1991 (p. 264) with a *demonstration* that "strategic discourse and practice both reflects and reproduces what may be termed a 'masculinist conception of power'". So it is that the flower of knowledge blossoms from the green shoots of conjecture in the Disc(ourse)-World of social construction.

Consider now the implication of this mode of knowledge production for the supposed superiority of K&M 1991's epistemology over their 'positivist' antecedents. In relation to their 'power effects', K&M 1991 can truly claim to have transcended positivism, since they adroitly sidestep the problems of interpreting evidence by dispensing with it entirely. One might call this advance in methodology '(I am) positivism', or IA positivism for short – except that it is not new at all. Where positivism *simple* asks for the interpretation of data to be taken on trust, IA positivism goes one step further by asking for unsupported assertion to be taken on trust. Regrettably, IA positivism is far from uncommon in self-styled interpretive research, but the demonstration and exploration of this point would take us too far from the theme of the present paper.

Subsequent Developments

Knights and Morgan's 1995 Case Study

Knights and Morgan's 1995 case study of the advent of strategic management in a life insurance company (K&M 1995) is not explicitly presented as an application of the approach set out in their 1991 paper. Nevertheless it is not unreasonable to regard it as such since it makes extensive reference to that paper as well as to other works by the senior author. Looked at from this angle, the first sentence of their Introduction comes as something of a jolt:

Since its genesis in mid-nineteenth century America (Hoskin, 1990), the discourse and practice of strategy has proliferated throughout a majority of industries and the larger businesses and non-business enterprises in Western economies.

Welcome as it is, this retraction of K&M's earlier insistence that strategic management began in the academies of post World War Two, could have been more gracefully accomplished had it been accompanied by some recognition that it had in fact been made. As it is, we must be grateful for the ameliorative influence of Keith Hoskin, and hope that it leads to an eventual re-reading of Chandler.

One of the more striking proposals of K&M (1991), it will be recalled, was that the rise of strategy discourse was a response to conditions which created a functional need (a 'condition of possibility' as they prefer to call it) for a particular language of accountability. An increasing separation of ownership and control coupled with the advent of the divisionalised and multinational company, they say created a need (a 'discursive space') for managers to justify their actions in terms of the direction of the company as a whole. Possibly because only the first of these conditions applied to the most of the UK financial services industry, K&M's 1995 paper shifts their explanatory emphasis onto the intensified competition which followed various deregulatory interventions on the part of government and, to a lesser extent, onto an increased pressure from stock markets, at least in the non-mutual sector.

This change in the terms of explanation means that their 1995 paper effectively abandons their earlier claim that strategy discourse arose primarily as a means of communication. Though they continue to use the language of discourse, constitution and so forth, the substance of their explanation is now that strategic management seemed to offer a rational response to market conditions. Thus: "The discourse of strategic management was quickly embraced in that it constituted the problems of the market and the environment in such a way as to then be able to offer itself as a solution to them" (K&M, 1995: 199). Allowing for the fact that any means of dealing with any situation involves interpretation of that situation, this is a rational action theory. Contrast it with the statement from K&M (1991: 251) quoted earlier:

Analysis of strategy *cannot* be reduced either to rationalist accounts of markets and environments nor interpretive understandings of actors' frames of reference.

K&M (1991) claimed superiority over the 'processual' theories of Mintzberg and Pettigrew, it will be recalled, in respect of these writers' 'positivist' epistemology and

'determinist' theorizations of power. Attaching their own idiosyncratic meanings to these pejoratives, 'positivism' in K&M's usage refers to any explanation of social action in terms of interests and constraints and 'determinism' to any theory of power which suggests that it ordinarily prevails. Their 1995 narrative of the advent of strategic management in the life insurance company 'Pensco' manifests both of these characteristics in abundance. In outline, the story is that strategic management was forced into place by a new CEO, largely by the time-honoured tactic of installing allies in positions of power and confronting those who resisted with the alternatives of conversion or resignation. In contrast to K&M's earlier deliberations on the matter, the theory of power through which this narrative is presented is straightforward indeed: it is simply assumed to attach to the position of CEO. Notably absent is any reference to the Foucaultian circulations of capillary power-knowledge which featured so prominently in K&M (1991). Also absent is any demonstration of the 'power-effects' which supposedly attend the practice of strategic management. It is true that the converts are said to have become 'constituted' as 'subjects' by the new discourse (p. 208), but the claim is not accompanied by any evidence that this was the case, not that it is easy to see what would count as such. In sum, the account is straightforward, believable – and positivist in the sense that the facts are taken to speak for themselves.

K&M (1995) supplement this story of company strategy with one about the development of IT strategy in 'Pensco', driven by a 'belligerent' manager appointed by the new CEO. The failure of this strategy, overwhelmed as it was by the need to cope with a seemingly unanticipated surge in the public demand for IT services, is an interesting story in its own terms, but one whose relevance to the discourse of *company* strategy is not immediately apparent. In respect of its historical antecedents and its supposed constitution of subjects, there are fundamental differences, surely, between strategy as it applies to an entire company and strategy as it applies to aspects of its internal operation. At its lowest, the matter requires a discussion which K&M (1995) fail to provide.

As in their 1991 paper, K&M (1995) make much of their historical methodology. In the introduction to the later paper we read that both the 'rational' and 'processual' approaches to strategy are 'a-historical', a shortcoming which is contrasted with their own genealogical approach which, it is claimed, reveals that the "historical development of strategy discourses and practices are discontinuous and uneven across countries, sectors and companies" (K&M, 1995: 191). It so happens that Chandler's *Strategy and Structure* (1982) and Mintzberg and Waters' *Tracking Strategy in an Entrepreneurial Firm* (1982) are both referred to in K&M's 1991 and 1995 papers. Both studies give the lie to K&M's accusations. The former, certainly a work which treats strategy as a rational search for ways of increasing profit, is subtitled *Chapters in the History of the American Industrial Enterprise* and it has much to say about discontinuous and uneven development. Whilst this comparative dimension is necessarily absent from Mintzberg and Waters' case study of a single retail chain, history is certainly present since the research covers the evolution of company strategy over a period of 60 years, with particular emphasis, moreover, on its emergence as an accidental by-product of other management policies.

Turning to K&M's own 1995 case study: whilst it is true that they have interesting things to say about uneven developments in the financial services industry, those things in no way depend on the genealogical method. Instead, the late take-up of strategic management in life assurance as compared to other sectors is attributed to the stability of the clientele created by its tradition of personal selling and by trade association agreements which limited competition by limiting the rates of commission. In fact it is not entirely clear that K&M themselves have quite grasped the genealogical method which they advocate with such conviction. Quite rightly, they point out that it involves recognizing the role of accidental or historically discontinuous events (K&M, 1995: 196). In illustration of such in the case of strategic management, however, they list the growth of business education, the growing importance of marketing, the increasing separation of ownership and control, the threat of takeovers, the internationalization of business and competition and new organizational structures such as divisionalisation. Does it need saying that these are not accidental or unrelated events, and perhaps not 'events' at all in the sense that we apprehend them in their immediacy? Rather they are stories, told and retold: stories of highly interpreted and interconnected trends, interconnected because interpreted and interpreted so as to interconnect. They are, in fact, the elements of the theory of managerial capitalism and what K&M (1995) propose is that these elements should now be kicked apart, treated as mutually irrelevant, and then reassembled under the new brand-name of genealogy.

Nor is it true that the genealogical method can be unproblematically contrasted with what K&M call 'positivist' history. It is simply not true that the positivist assumption that the facts speak for themselves automatically correlates with a neglect of the accidental, the trivial and the seemingly irrelevant. A.J.P. Taylor's history of the First World War (1966) is only the most prominent counter-example, belonging as it does to a whole genre colloquially known as the 'cock-up theory of history'. A case can be made, on the other hand that the genealogical method, as described by Foucault himself, *is* positivist:

Genealogy is gray, meticulous and patiently documentary . . . it rejects the metahistorical deployment of ideal significations and indefinite teleologies. (Foucault, 1984: 76-7)

What else is this but a declaration that the historical record itself is to be pressed into service as a means of subverting imported schemes of theoretical interpretation?

In summary, K&M (1995) is a poor demonstration of the approach advocated in K&M (1991). Where the earlier claims are not quietly abandoned they remain unsubstantiated by the case material presented, not that this dents K&M confidence in the advances they believe themselves to have made.

Other Empirical Tests

If K&M's 1995 paper provides no confirmation of the value of their approach, perhaps that can be found in the work of those later writers who have declared their indebtedness to the approach. Two such are Samra-Fredericks (2005) and Laine and Vaara (2007), both of which declare K&M (1991) to be 'seminal'.

Samra-Fredericks positions her paper as a response to what she calls the theoretical and empirical ‘challenges’ posed by K&M 1991. That these challenges exist is not in dispute. An important theoretical task, one would have thought, would be to show how the constitution of subjectivities by the discourse of strategic management differs from rational action guided by its precepts and to indicate how the former might prove superior in explaining the substance of strategy in particular cases.

This is not how Samra-Fredericks sees matters. Regarding the core theory as already accomplished she sets out to consider how the subjectivities of ‘strategic actors’ – including the power effects listed by K&M (1991) – might be constituted in everyday interaction. To this end she embarks on a commendably energetic tour through Habermas’ universal pragmatics and speech act theory in search of what she calls a ‘complexified notion of competence’. Thus prepared she proposes to ‘meet the empirical challenge’ by attending to conversations between strategic actors or, as she puts it, “audio recording their naturally occurring talk-based interactive routines over time/space” (Samra Fredericks, 2005: 803). On the nature of that empirical challenge, there is more agreement. In drawing attention to what she takes to be manifestations of K&M’s ‘power effects’, she tacitly accepts that there is a need for some such evidence.

In fairness to Samra-Fredericks, she herself draws attention to the brevity of the two⁸ ‘transcribed strips of interaction’ which comprise her data and to the fact that neither of them featured ‘overt strategy talk’. She is not overstating on either count. The following, in its entirety, is the second of the extracts (reproduced here because it is more substantial than the first). ‘Six white male strategists’ are discussing the employment of an IT consultant during the development of an IT strategy in response to the cost advantages thought to be gained from IT by a competitor:

Speaker C – but [name of Managing Director] I’m (.) I don’t want to sound as if I’m always defending [name of IT consultant used] but I I am =

Managing Director – = but you are yeah

Speaker C – he can do in two days what a rookie programmer =

Managing Director – = right =

Speaker C – = would would take five days =

Speaker D – = but for [that argument [for six to nine months but [not forever! and if

Speaker C – [yes

Speaker A – [don’t take the step you’ve got [surname of IT

[Speaker D] – [and thats

Speaker A – consultant] forever

8 It is Samra-Fredericks herself who describes her data as consisting of two extracts. In fact her conclusions feature a third extract which she includes as a “last word” from the managing director.

Speaker C – um

Speaker D – [yes quite

Speaker A – [sometime you've gotta bite the bullet otherwise you're frozen [rigid in time

[[interrupted]

In line with her theoretical excursus, much of Samra-Fredericks's commentary is taken up with an identification of the various Habermassian validity claims involved in this exchange, and with the use of pronouns as indicative of membership claims relevant to the power-play between speakers. Considered as an analysis of negotiation, this exercise looks as if it might form the basis of a useful approach. Concerning the empirical validation of K&M's 'power effects', however, her results are more questionable, claiming as she does, that all seven of them are observable in the foregoing exchange. They are:

1. the provision to managers of a rationalization of their successes and failures
2. the sustenance and enhancement of the prerogatives of management and the negation of alternative perspectives on organizations
3. the generation of a sense of personal and organizational security for managers
4. the reflection and sustenance of a strong sense of gendered masculinity for male management
5. the demonstration of a managerial rationality to colleagues, customers, competitors and government
6. the facilitation and legitimation of the exercise of power
7. the constitution of the subjectivity of an organizational member as a particular category of person who secures their sense of reality through engaging in strategic discourse and practice

There is no reason to dispute that Samra-Fredericks sees in her data what she says she sees. To count as evidence, however, other people have to see it too. In the more meticulous traditions attaching to the quantitative analysis of discourse, this is recognized as the problem of 'rater bias' and attempts are made to mitigate its effects by the employment of independent judges. How many such, one wonders, would see what Samra-Fredericks sees in her brief 'strips of interaction'? This critic cannot see any of the listed power effects and nor, more generally, can he see any sign that the subjectivities of the speakers have been constituted by the discourse of strategy. They are simply trying to weigh the advantages of rapid progress accruing from the continued employment of the consultant against the disadvantages of perpetuating the consequent dependency.

The data offered by Laine and Vaara (2007), or at least the excerpts quoted from it, are more substantial and, as well as an empirical validation, they are presented as an extension of the work of K&M (1991) to include cases where strategic priorities are contested. Material obtained in the course of a six-year teaching relationship with an engineering and consulting group are used to examine inputs into and interpretations of the strategic process at three levels: that of the group as a whole, that of the business

unit and that of the engineering consultants themselves. Whilst not quite the 'ethnography' it claims to be, the material is well-documented and significant. The picture of the different and conflicting priorities as they appear to the different levels of management is vivid and convincing. Corporate management see the need to move into alternative markets, managers at business unit level see the value of drawing upon the skilled and experienced of their staff in formulating policy whilst the engineers themselves see the need to preserve a reputation with clients for the competent and reliable completion of projects. Of particular interest is the manner in which middle managers had learnt to legitimate their own agendas in terms of the necessarily abstract formulations of group-level strategy, and even to 'educate' senior management into a 'mature' understanding of (their own?) strategies.

Contested understandings of the strategic options facing a company are, of course, very much the territory of the processual approach and one would have expected Laine and Vaara to pay more attention to the work of Pettigrew and Mintzberg than the perfunctory acknowledgment it actually receives. Instead, reflecting the influence of K&M (1991), their material is presented as a struggle over *subjectivity* and *identity* and that framing of the data, runs right through their paper, from abstract to conclusions. This being the case, it is pertinent to ask how Laine and Vaara's data would look to a reader not convinced in advance that changes in subjectivity and identity are integral to the strategic process. Because of Laine and Vaara's generous presentation of verbatim quotations, it is possible test this to some extent by lifting that data from its context of prior framing and subsequent interpretation, though space does not permit this exercise to be performed here for the material from all three levels. On the basis that the quotations from the corporate level are the most likely to manifest the constitution of actors as strategists, that material is chosen here. In its entirety and in the order of its presentation, it looks like this:

The renewed strategy process was implemented with Consulting Company Ltd, making use of their strategy model. The objective was to introduce a new method for developing the Group's strategy for the period 2001 —2005, and at the same time identify new business ideas. Hundreds of pages of bullet point presentations were generated in the course of the strategy process. The complete strategy documentation amounts to more than 100 pages. A condensed version of the strategy will be cascaded throughout the company in the form of presentations. Because of the confidential nature of the strategy documentation, it cannot be presented in detail in the In-House Magazine. Therefore this article is limited to presenting the mission slogan and the values in brief. The strategy will be cascaded throughout the Group to every employee. (In-House Magazine 12/2001)

Side by side with the strategy process, a separate innovation stream was pursued with the aim of creating new products and business ideas transcending the borders between business groups. Representatives of all business groups participated in this process. (In-House Magazine, 12/2001)

We [top management] can only anticipate the expectations of the clients and personnel. The only issue that we do not have to guess are the expectations of the owners. That we know; it is a fact. The owners want earnings per share. That means profitability and growth and above all no surprises. Whatever we say we better live with it. (President, speech in a management training programme, 2004, participant observation)

The definition of the strategy is relatively easy but the implementation is the hardest issue... The most important things in strategy are actions. (President, management training programme, 2004, participant observation)

Significantly, Laine and Vaara's (2007) talk of constituted subjectivities and power effects temporarily thins out in their immediate commentary on the above passages. The extracts, they say, reproduce managerial hegemony in the form of a traditional top-down model of strategy formation. The emphasis on preparation and documentation, they continue, "underscores the central role of those who supposedly have the best knowledge" (p. 41). Similarly, the insistence on confidentiality "underscores role of corporate management as the key strategists and the passive role ascribed to the organization" (*ibid.*). That other employees are given only a condensed version which is 'cascaded down' further positions these others as the passive recipients of company strategy. In summary, "The strategy discourse of corporate management has promoted their own status as strategists whose decisions and actions determine the future of the organization" (P. 43).

This, of course, falls a long way short of support for K&M's 1991 theory. One can readily agree that the passages do indeed express a view of strategy formation as the prerogative (and responsibility) of senior management (§2 and possibly §6 of K&M's 'power effects'). However, they also contain elements at odds with this 'top-down' model of the strategic process which Laine and Vaara do not mention: notably the participation of business unit representatives in the product review and the recognition that the formulation and implementation of strategy are not easily separable. Of K&M's other six 'power effects', §3 (a sense of personal and organizational security) would appear to be contradicted by the strategists' recognition that the only thing they can be certain of is the expectation of profit. Of the remaining three 'power effects' there is no sign.

As evidence, even the most convincing of the above interpretations – the claim of an exclusive senior managerial prerogative to determine strategy – does not amount to much. An expression of the idea that it is the job of senior management to decide on company strategy can only be interpreted as a manifestation of the formation of subjectivities and identities by assuming what is supposed to be demonstrated. And as the paper progresses, that is exactly what happens. As the commentary proceeds further from the quoted material of which it is supposed to be an interpretation, mentions of subjectivity and identity re-appear so that by the time we reach the beginning of Laine and Vaara's Discussion we find the following statement of the 'key argument' which they believe themselves to have substantiated:

in these struggles, strategy and other discourses assign particular kinds of subjectivities for organizational actors, with empowering or disempowering effects. (Laine and Vaara, 2007: 50)

In reality this hasn't been shown at all. To do so would firstly have required information on what kind of person the strategists believed themselves to be and, ideally and more conclusively, how, if at all, their view of themselves had changed with the assumption of a role in the strategic process. This is not asking for the impossible, as has been demonstrated in the case of less exalted levels of management by Harding's (2003) interviews with nursing staff who had found themselves having to take on a managerial role and Parker's (2004) introspections on taking on the job of Head of Department. Secondly, and more onerously, it would have required some indication that the strategist identity had been formed by imbibing a discourse of strategic management and not, for example, through an autonomous process of individual adaptation. Given the lack of

attention to the empirical dimension characteristic of the 'discourse approach', this would present more problems, but researches on the influence of the mass media (e.g. Hall, 1980) would seem to provide a useful precedent.

What is dispiriting about all this is that the need for empirical validation of this kind does not seem to have been appreciated during the sixteen years' considerable attention which K&M's approach to strategic management has received – not even by those who believe themselves to have provided it. It is as if K&M's work has persuaded its adherents by a kind of textual charisma deriving from its deployment of what were then theoretical exotica, the counter-intuitive character of which seems to have convinced talented but impressionable young researchers that here at last, in a hitherto stagnant field of study was an exciting new direction.

No doubt these reflections, with their insistence on matching evidence to the assertions it is meant to test will strike the more avant-garde of those who 'write organization' as faintly retro and even gauche. If so, that is a measure of the state into which much European research on the subject of organizations has fallen. How much more pleasant it is to retain the creative freedom to think at a never-quite-defined angle to the empirical world, to have that speculation transformed into socially-constructed 'truth effects' through the collaborative agency of friends, colleagues and students, and so to enjoy a reputation as the producer of seminal 'truths', always in scare quotes as befits a conceptual universe in which only the 'positivists' are ever known to be mistaken.

Conclusion: Power-Effects, Paradigms and Seminality

It should now be evident that the standards of scholarship in K&M 1991's paper do not stand up to serious scrutiny. In the continued absence of evidence pertaining to the discursive constitution of subjectivities, their account of the practice of strategic management must be considered as innovative only in its terminology. Their proposed history of its origins does not square with the evidence and is, in any case, the subject of an unacknowledged retraction in their later paper of 1995. They are able to present their work as an advance on previous approaches only by serious misrepresentations of them. They lay claim to a theoretical and methodological sophistication superior to that of previous writers in the field whilst making accusations against them which give the lie to that very claim. On the question of power in particular, they present a grossly one sided view of Michel Foucault, their major intellectual source. On the one positive and original thing they have to say about strategic management – that it confers social psychological goods on those who practice it – they offer little in the way of supporting argument nor evidence.

Now no part of the examination of K&M 1991's paper which led to these conclusions is beyond the capabilities of a graduate student in the social sciences. The question then arises of how it is that such a piece of work has become a citation classic. What overriding virtues does it possess that have led so many intelligent and well-educated people to overlook its evident failings? Alternatively, what have they been able to take from the paper on a reading so superficial that those failings have not even become apparent? Wherein lies its seminality? Amongst the possibilities which suggest themselves, there

is one relating to the material conditions of academic production which seems particularly apposite: that the paper opens a way forward for the press of young researchers who find themselves required to publish something original, appropriately 'academic', and on a managerial topic. What follows is offered as an hypothesis, of course, not as accomplished fact.

Consider the situation of the young researcher with an interest in strategic management as it stood before the publication of K&M 1991's paper. On the question of its origins, there was the massive presence of Alfred D. Chandler Jr. and behind him, that of other business historians, some of them of almost equal eminence. On the question of the strategic process too, there were substantial bodies of empirical and theoretical work by Pettigrew, Mintzberg and other social constructionist writers. The production of original work in either tradition required the mastery of a great deal of material combined with the creative flair needed to detect, and find ways of making good, any points of weakness within it. Once produced, the contribution would then have to run the gauntlet of a refereeing process dominated by the gatekeepers and custodians of those traditions, some of whom would no doubt welcome fresh insights, but others of whom, for very understandable reasons, might be less inclined to do so.

Under those circumstances, the promise that there exists a new way of doing research on corporate strategy which not only by-passes these formidable obstacles, but also creates new criteria of academic acceptability which destroys their credibility *as* obstacles, is not something one would be inclined to scrutinize too closely. Conditions of this kind, it is suggested, create a seller's market for accusations of epistemological, theoretical and empirical inadequacy against the dominant figures of any tradition. This pull of demand might well work to lower the threshold of credibility which those accusations would have to attain in order to be believed. K&M (1991), we are suggesting, has 'power effects' of its own: it provides a rationale, never mind how ramshackle, for a new generation of researchers to disregard and so subvert the existing order of authority in their discipline.

K&M's instance on the centrality of discourse too has convenient implications for the processes of academic production, especially for those researchers who lack the resources and industry contacts required to carry out empirical research on the activities of senior managers. If the subject matter of strategic management is displaced from its practice to the discourse which 'constitutes' that practice, it becomes possible to study it through the traces of that discourse in books, reports and other documents. And if, in the course of writing studies of this kind, empirical researchers can be berated for a failure to observe the protocols of the discourse approach, a display of outraged virtue can be added to benefits of convenience.

Viewed in this light, the signal virtue of K&M 1991's paper, the one that over-rides all of its many deficiencies is that it holds out the promise of what is miscalled a 'paradigm shift' in the social sciences relating to management. Its seminality, argued for in its treatment of established reputations and granted in the enthusiasm with which younger researchers have tried to read confirmation of its assertions into their data, is born of a desire to erase the past so that the game of reputations can begin afresh. The hypothesis, in sum, is that K&M (1991) is symptomatic as well as seminal, its seminal status

symptomizing a discipline in which standards of scholarship have been eroded in an over-riding drive for novelty.

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The Philosophical Friendship of Gilles Deleuze and Félix Guattari

Anna Helle

review of: Dosse, François (2007) Gilles Deleuze et Félix Guattari: Biographie Croisée (Gilles Deleuze and Félix Guattari: Parallel Biographies). Paris: La Découverte (HB: pp. 648, €29.50 ISBN: 9782707152954)

Introduction

Gilles Deleuze (1925–1995) expressed his suspicions of biographical ways of explaining his work in an interview with Raymond Bellour and François Ewald in 1988. He said: “If you want to apply bio-bibliographical criteria to me, I confess...” by which he probably meant that if he was in the place of the interviewers he wouldn’t want to do so. Referring to his own life, he also said that the academic lives are seldom interesting (Deleuze, 1995: 137-138).

François Dosse’s ‘crossing biography’ of Deleuze and Félix Guattari (1930–1992) is, however, highly interesting since it connects the incidents in the lives of Deleuze and Guattari to their thinking without falling to a trap of explaining things causally in a too naïve way. As a matter of fact, it is exactly the combination of Deleuze AND Guattari that makes the biography so interesting, because the way the two – otherwise so different men – worked together was quite exceptional and certainly worth documenting.

In France the book has been received positively, even delightedly. *La Quinzaine Littéraire* emphasizes the increasing importance of Deleuze and Guattari’s thought in a world that is otherwise ‘a little flat’. *Libération* praises especially the chapters in which Dosse tells about Guattari’s life. *Lire* writes that one has to read Dosse’s book about Deleuze and Guattari’s thought that some would like to liquidate and calls Dosse “an excellent specialist” of French cultural history. Only *Le Monde de Livres* requests more serious historiographic and comparative study.

Dosse is a French historian and professor who is internationally best known for the massive opus *History of structuralism I–II* (1991–1992). He bases the biography on unpublished archives and interviews with people who have known Deleuze and

Guattari, but he also exploits a great number of writings about Deleuze and Guattari, not to mention the books they wrote themselves. The black-and-white photographs of Deleuze, Guattari and their families are not at all intrusive but supplement the already warm-hearted viewpoint to the lives of the philosopher and the psychoanalyst.

The biography is composed in a way that the lives are presented separately except for the times when Deleuze and Guattari worked or spent time together. Though the historical persons and events are at the forefront of the book, Dosse carries the thinking of Deleuze and Guattari along with them. He begins the biography from the early years of Guattari instead of Deleuze who was born before him. This procedure is an homage to Guattari who has often been left to the shadow of Deleuze.

Guattari Finds Lacan

Pierre-Félix Guattari was born in 1930 in Villeneuve-des-Sablons, northern France, and he was the youngest of three brothers. Though he is often said to be Italian, to be precise, only his parents Louis and Jeanne were Italians living in France. Guattari grew up and was educated in Paris region. Dosse draws a picture of a sensitive and sentimental child and young man who changed his name from Pierre to Félix because the latter reminded him of happiness, *félicité*. Guattari's parents weren't intellectuals – his father owned a small chocolate factory and his mother took care of the household – but they had artistic passions: father was a music enthusiast and mother was interested in literature and museums.

Like many other intelligent young ones of his time Guattari was heavily inspired by Jean-Paul Sartre in his youth. He also became a political left-winger in quite an early age. Guattari was known as a Trotskyist activist and he was a member of several groups that were against Stalinism and the war in Algeria. Guattari himself has described his political orientation in an interview in 1972: “I'd come from the Communist Path, and then the Left Opposition. Up to May '68 there was a lot of activism and a bit of writing...” (Deleuze, 1995: 14).

After having attempted to study pharmacy, which he thought was deeply boring, Guattari found the thinking of Jacques Lacan with the advice of Jean Oury (b. 1924). Oury founded the famous La Borde clinic in 1953, and a few years later he invited Guattari to work at the clinic, too. In the Lacanian psychoanalysis Guattari seems to have found what he had been looking for. Guattari attended the courses of Lacan for years and assisted him in organizing seminars. Dosse also brings out an interesting detail: as early as in 1954–1955 Guattari had written in his notes about the concept of machine as follows: “The subject as a machine-individual has unconscious manifestations that couldn't be introduced in the concreteness without a special treatment” (Dosse, 2007: 53). Years later Guattari would systematize this notion that was to become crucial in the works of Deleuze and Guattari.

The Clinic of La Borde

La Borde, officially called La Clinique de Cour-Cheverny, is a psychiatric clinic located in an old castle southwest of Paris and still in use today. The original strategy was to apply Lacanian psychoanalysis to psychiatric treatment and to organize the clinic according three principles: democratic centralism that corresponds to Marxist-Leninist principles, a rotation system of responsibilities corresponding to the communist utopia, and anti-bureaucracy that ensures equality in tasks, responsibilities and salaries.

Guattari had a crucial role in La Borde; Dosse writes that from the day on when Guattari came to the clinic they formed a 'two-headed machine' of the clinic with Oury. While Oury was the one in charge of the whole clinic Guattari took care of the everyday functioning of La Borde. Guattari worked as a psychoanalyst, and according to many patients, he was very intelligent and attentive in his relationships with the patients. He had an extraordinary ability to see what was special in each patient.

An important concept that Guattari worked with in La Borde was 'transversality'. He introduced it at the first international congress of psychodrama in Paris, 1964. The transversal approach opposes both the vertical axis of an organigram, formed in a structure of a pyramid, and the horizontal axis, where there are many juxtaposed groups with no relationships between each other. With the transversal analysis practice Guattari wanted to make the patients leave themselves behind for a moment, and as he said: "The transversality is the place of the unconscious group subject that is beyond the objective laws that it is based on; it is the group's support of desire" (Dosse, 2007: 81).

Life in the Clinic

In 1951 Guattari had moved together with a young woman called Micheline Kao. They had met in 1946 when Guattari was 16 years and Micheline only 14 years old. When Guattari decided to go to La Borde, Kao followed him there. After having lived there only for one year, however, the couple broke up, and according to Dosse one of the reasons was the promiscuity of the life in La Borde.

Not a long time after the separation Guattari met a girl called Nicole Perdreau, who was to become his wife and the mother of their three children, Bruno, Stephen and Emmanuelle. The marriage lasted about 10 years, but Guattari was not very much present at home. He had a lot of work to do in the clinic and besides he also spent some time in Paris where he had a flat, too.

The biography does not tell whether the marriage was happy or not, but in the end Félix and Nicole separated because of a young woman called Arlette Donati with whom Guattari had fallen in love. While Félix and Nicole had lived with their children in a modest apartment in the La Borde area, Félix and Arlette moved into a vast castle in Dhuizon, near La Borde, where they lived together for seven years. At the same with the relationship with Donati, Guattari had other relationships, too. He was looking for ways of life outside such institutions as marriage or family, and this was one of the ways in which he criticized the bourgeois family.

Guattari had always wanted to write. In his youth he had thought about writing novels but later he began to feel he had a lot to say about political and philosophical problems. The years he had spent in political activism had, however, estranged him from writing, much to his chagrin. He felt he didn't know how to start writing anymore.

The Brother of a Hero

Gilles Deleuze was born in Paris in 1925. He was the younger of the two sons of Louis and Odette Deleuze, the first-born Georges being two years elder. Father Louis was an engineer and mother Odette a housewife.

Dosse emphasizes the importance of the brother in Deleuze's life. In the Second World War Georges joined the Resistance in France and he was arrested by the Germans. He died during the journey to a concentration camp. According to Deleuze's close friend, writer Michel Tournier (b. 1924), Deleuze's relationship to the dead brother was always a complicated one, because in the family there was only one hero, Georges, whereas Deleuze always had to be the second or even the mediocre one. Dosse sees here a connection to Deleuze's opposition towards bourgeois family.

The war reached Deleuze at the age of fifteen. He was sent away from Paris for one year to Deauville, Normandy, where the family used to spend their summer holidays. To Deleuze the year was to become a turning point. Until then he had not been especially good at school, and otherwise he had tried to fight boredom mainly by arranging his stamp collection.

In Deauville he had a teacher called Pierre Halbwachs, who encouraged Deleuze to read French literature. Young Deleuze was immediately impressed, and later he described their relationship by saying "I was his disciple. I had found my master" (Dosse, 2007: 114). When he returned to Paris in 1943 he started studying philosophy and from the beginning he knew that philosophy was exactly what he wanted to do.

At the age of 18, when he was still a student in a *lycée*, Deleuze started following the philosophy courses in Paris. Because he was a student he did not participate in the Resistance. In 1943 there was a major philosophical event in the French intellectual milieu, the publication of Sartre's *Being and Nothingness*. Deleuze admired the work greatly as did many others, since Sartre was the brightest star in the French sky of philosophy at the time. Tournier also shared some of the enthusiasm with him, but he wasn't as eager as Deleuze. In an interview Tournier has told to Dosse that Deleuze used to call him every day to talk about Sartre's book and that he could also read passages from it by heart.

After having graduated Deleuze went through the preparatory classes *hypokhâgne* and *khâgne* to make his way to École Normale Supérieure, but he failed in entering although he was already known for his exceptional skills. However, he was excellent enough to be granted a scholarship in Sorbonne where he began his studies the following year. Because of his ability to discuss philosophical problems creatively, he became admired by the other students and also by a few teachers in Sorbonne. By the time he was about

to get his *agrégation* degree Deleuze had already problems with his health. He suffered from asthma and he had not been able to attend the last year's courses.

Teacher of the History of Philosophy

After having finished his studies Deleuze taught philosophy in various *lycées* in 1948-1957. As a teacher he was encouraging and he knew how to interest his pupils in philosophical problems. He also took good care of them and tried to help the ones he found talented. For example, a pupil called Claude Lemoine had moved from the province to Paris to study, and therefore felt a little lost in the capital city. That is why Deleuze invited him to live with Deleuze's mother in Paris.

In 1956 Deleuze got married with a certain Fanny Grandjouan, who worked for the fashion designer Pierre Balmain. Later she was better known as a translator. The wedding took place in the basilica of Saint-Léonard-de-Noblat where the Grandjouan family estate was located. A few years later their children Julien and Émilie were born.

In 1953 Deleuze published a book on David Hume called *Empiricism and subjectivity*. Dosse calls the years between the Hume book and Deleuze's work *Nietzsche and philosophy* (1962) a 'latent phase' in Deleuze's life. Deleuze himself has told in an interview that he can recall what he did at the time but he sees it as an outsider. Deleuze said: "It's like a hole in my life, an eight-year hole" (Deleuze, 1995: 138). Dosse shows, however, that though Deleuze did not publish a lot at the time, he continued working. First he taught in the *lycées*, and the years 1957-1960 he worked as an assistant in the history of philosophy at Sorbonne, where he was greatly admired by his students. One of them, Marc-Alain Descamps, remembers Deleuze having talked about irises and how their roots can form a net. Years later he would conceptualize it with Guattari as 'rhizome'.

Besides teaching, Deleuze wrote several works on philosophers. Later he called the works 'portraits'; while painters like van Gogh had had to begin the painting with portraits before being able to become a colorist, such was the case with a philosopher, too. In addition to the books on Hume and Nietzsche, he published works on Kant (*Kant's Critical Philosophy*, 1963), Proust (*Proust and Signs*, 1964), Bergson (*Bergsonism*, 1966) and Spinoza (*Expressionism in Philosophy*, 1968), and finally two more personal works: his thesis *Difference and repetition* (1968) and *The Logic of Sense* (1969).

In the end of the 1960's, however, Deleuze had come to a kind of dead end because of at least two reasons. In 1968 he had suffered from serious pulmonary problems and his other lung had been extirpated. The operation had exhausted him, and he had had to recover for a year in Saint-Leonard-de-Noblat in Limousin. Another problem was alcohol: according to Dosse, Deleuze was very close to alcoholism at the time. Deleuze needed to change.

gillesdeleuzefélixguattari

Deleuze and Guattari had lived their lives without knowing each other and in very different environments: Deleuze in the academic world and Guattari among the political activists and psychoanalysis. They only met each other after the events of May '68 in 1969.

While Guattari had actively taken part in the action of '68, Deleuze had maintained a certain distance to the events. He lived in Lyon at the time, and though he was not an activist he was delighted by the student radicalism, and the fearful reactions of other professors irritated him. According to Dosse, 1968 was a significant turning point to both Deleuze and Guattari alike. Guattari said in an interview:

May '68 came as a shock to Gilles and me, as to many others: we didn't know each other, but this book [Anti-Oedipus], now, is nevertheless a result of May. (Deleuze, 1990: 15)

A certain Jean-Pierre Muyard played a major role in bringing Deleuze and Guattari together. Quite interestingly, Dosse pays attention to the fact that in the stories about the encounter of Deleuze and Guattari, Muyard has, nevertheless, disappeared. Deleuze has told that it was Guattari who came to look for Deleuze, and also, according to Guattari, it was him who went looking for Deleuze but Deleuze was the one who suggested that they work together.

Muyard had studied medicine in the 1950s in Lyon and participated actively in the left-wing student politics. He met Guattari for the first time in a seminar of the left opposition in Poissy in 1964, and Guattari made a great impression on him because of his intelligence and energy. Muyard became a part of the group Dosse calls "Guattari's band". Muyard had heard about Deleuze who taught in Lyon in the faculty of Letters. In 1967 he had also become interested in Deleuze's new book *Masochism: Coldness and Cruelty*. Because Muyard occasionally spent time in Lyon, he and Deleuze became friends. Muyard was fascinated by the contrast in Deleuze's relationship with mental illness: he talked about madness but at the same time madmen terrified him. When Deleuze was recovering in Saint-Léonard-de-Noblat Muyard took Guattari and François Fouquet to meet Deleuze, and Deleuze and Guattari were immediately charmed by each other.

Muyard suggested that Deleuze should make a visit to La Borde, which he did, and in the castle in Dhuizon, where Guattari was living at the moment, Deleuze, Guattari and Muyard discussed a book that would become *Anti-Oedipus. Capitalism and Schizophrenia I* (1972). According to Dosse, Deleuze would have wanted Muyard to work with him and Guattari with the book, but Muyard felt Guattari did not feel comfortable working with him.

In Dhuizon Deleuze and Guattari discussed together creatively and intensively, but later they would mainly work through correspondence. Guattari lived in the castle that was often very crowded – at times there were around 50 people staying in – and Deleuze for his part did not feel comfortable in crowds. According to Arlette Donati, *Anti-Oedipus* was written in such a way that Guattari wrote intensively in Dhuizon and sent his notes to Deleuze, who corrected and revised the texts. At the time they wrote the book

Deleuze had said to Donati that Félix was the one who finds the diamonds and he himself was the stonecutter.

From the beginning Deleuze and Guattari felt it was not the two of them as persons writing together. The place for creativity was, rather, in between them, as Deleuze would later tell Robert Maggiori:

we didn't collaborate like two different people. We were more like two streams coming together to make "a" third stream, which I suppose was us. (Deleuze, 1995: 136)

Around the time of the publication of *Anti-Oedipus* Guattari also finally managed to publish a collection of his writings called *Psychanalyse et transversalité* (1972) that he had dreamed about for such a long time. It was published by Maspero, and Deleuze wrote a preface to it.

Four Books Written Together

Anti-Oedipus was an immediate success: the first review was published in *Le Monde* three days after the publication. Since the very beginning, however, *Le Monde* saluted and criticized Deleuze but tended to ignore Guattari – later it would even ‘forget’ to mention Guattari’s name when referring to the books written by both of them. Amongst many others, Jean-François Lyotard and René Girard wrote about *Anti-Oedipus*. Lyotard expressed his admiration towards the work, emphasizing the affirmative nature of the book, but Girard was more critical because of the way *Anti-Oedipus* refuses to admit any significance to myths and tragedy. Of course, there was a harsh critic, too. Jacques Lacan was irritated by the book: he advised the members of École Freudienne to remain silent, and according to a Lacanian psychoanalyst Catherine Millot, Lacan saw the book as a personal attack.

In 1975 another book by Deleuze and Guattari was published, *Kafka: Toward a Theory of Minor Literature*. Both Deleuze and Guattari were great admirers of Kafka. *Kafka* is at least as important in its original ways of reading Kafka, as it is in the way it experiments with such concepts as rhizome that they would later use in the second part of *Capitalism and Schizophrenia, A Thousand Plateaus* (1980). A little essay *Rhizome*, in which was introduced Deleuze-Guattari’s new way of thinking was also published the next year. The essay was later republished as the introduction to *The Thousand Plateaus*.

The reception of *A Thousand Plateaus* in 1980 was much more unpretentious than that of *Anti-Oedipus*. According to Dosse, behind the celebration there was also a lot of confusion. While *Anti-Oedipus* was immediately a best-seller, *Thousand Plateaus* was published in a climate of indifference. It was accused of being too difficult and bewildering. If the reception of *A Thousand Plateaus* was rather tame in 1980, Dosse and many of the critics writing about Dosse’s book today now emphasize its increasing importance (see Aeschimann, 2007b; Ghosn, 2007).

What is Philosophy? was published in 1993. Dosse writes that the book was almost entirely written by Deleuze, but it was published under the names of Deleuze and

Guattari for two reasons. Firstly, Guattari's friends had asked Deleuze to revise the book with Guattari, because they hoped it would relieve Guattari's serious depression for a moment. According to the friends, Deleuze did not hesitate a minute. Besides, Deleuze felt that it was only fair to add Guattari's name to the book although he had only given a few comments on it, because the concepts used in the book had been created together with Guattari in the earlier years. Dosse writes that some people have wanted to 'deguattarise' the late Deleuze, but in an interview he stresses that it is not appropriate (Aeschimann, 2007).

In addition to the books written together, both Deleuze and Guattari published books on their own. Guattari wrote about such topics as ecology and politics, and Deleuze for example about cinema and literature.

Different Environments

If Deleuze had experienced the May '68 somewhat far away from the center, in 1970–1971 he dived right into the heart of its aftermath. In 1969 he had been appointed university lecturer at the department of philosophy in the new experimental university in Vincennes (Paris VIII), but because of his health problems he came to Vincennes in the 1970–1971. The head of the department was Deleuze's friend Michel Foucault, and the idea of a new university had been created by French intellectuals such as Roland Barthes, Jacques Derrida, Jean-Pierre Vernant and Georges Canguilhem, to mention only a few names.

The university aimed at being different from the old academies; Dosse calls it a sort of 'anti-Sorbonne'. Vincennes was also a kind of refuge to many of the activists of '68, and its basic idea was to be multidisciplinary and modern. According to Dosse, Vincennes was planned to be a 'little MIT', an American-style university. What was special in Vincennes, too, was the possibility for the non-bachelors to enrol and the great number of international students from all over the world.

In Vincennes, Deleuze worked close to another world famous philosopher, Jean-François Lyotard, but even closer to him was Foucault. They had met already in 1952 but they became better acquainted in the beginning of the 1960s when they worked together with the edition of Nietzsche's *Completed Works* for Gallimard. Dosse illustrates the friendship from various points by setting forth the mutual respect as well as the disagreements; in the end of 1970s there was an increasing number of matters on which they disagreed, especially in politics. Tragically enough, Foucault didn't have a possibility to reconcile with Deleuze before his premature death in 1984 though, according to Didier Éribon, it was Foucault's eager desire. Deleuze who hated symposia made for his part an exception and participated in an international conference organized as an homage to Foucault in 1988 to show his respect for Foucault's work. In 1985–1986 he had also given a course on Foucault in Vincennes and published a book called *Foucault* in 1986.

Guattari continued the political activism. He worked in a group CERFI formed in 1967. The idea of CERFI was to be an associating group among other autonomic and free

groups. The members of it headed towards a communal life together: they wanted to join their work force together, sell the work and then share the money. This way they wanted to create new forms of subjectivity.

In the 1970s Guattari became familiar with the Italian left-wing activists such as Franco “Bifo” Berardi and Toni Negri. The situation in Italy was difficult because of a severe social crisis: for example, there were about 2,000,000 unemployed and the rate of inflation was 25% per year. The fascist party MSI for its part radicalized the political situation in Italy. Many found in *Anti-Oedipus* – translated to Italian already in 1975 – a new way of thinking and speaking about political matters. The left-wing activism had two different forms in Italy; while others were directed towards creativity, some others turned towards terrorism. The acts against terrorism made it difficult to all the left-wing groups to work, and many had to move away from Italy to avoid imprisonment. This is why for example Bifo and Negri became political refugees in France. Guattari’s relationship to terrorism was controversial. He knew many of the Italian activists and he never condemned terrorist acts publicly.

Friends, Not Mates

According to coevals, the friendship of Deleuze and Guattari was exceptional in many ways. One interesting detail is that they never began to use the French pronoun *tu* (instead of the more formal *vous*) when speaking to each other (which would have been more predictable in the context of the 1960s and considering how long they knew each other). Dosse explains this decision by referring to the mutual respect and quotes Guattari’s words: ‘Gilles is my friend (*ami*), not my mate (*copain*)’.

Deleuze and Guattari were very different from each other. Guattari was quite mobile, like quicksilver. According to Deleuze, Guattari jumped from one activity to another, slept a little, travelled a lot and never went off. Deleuze himself did not much like to move and he said he could not run two enterprises at the same time. Deleuze also described their working by saying that Félix’s ideas were like real flashes whereas Deleuze was more like a lightning conductor.

In later years Guattari suffered from deep depression, and Dosse describes in a quite touching manner how Deleuze – himself exhausted and incapable of breathing properly – tried to take care of his old friend. At the time, Deleuze and Guattari were not as close as they had been before. Guattari was having a party in his house and a friend of both Deleuze and Guattari asked Deleuze to come over, too. When he arrived to Guattari’s house Félix was watching football on the television and said hardly anything during the evening. Though Deleuze did not feel himself comfortable, he stayed the whole evening. Dosse writes (quoting Michel Butel):

Félix was completely hieratic, sitting on the floor watching television, the football finals. And beside him sat Deleuze, who would undoubtedly have given a finger of his hand for not having to be there, before the football, in the party. Deleuze for whom two people were already a crowd. (Dosse, 2007: 29)

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In Need of Medication?

David Sköld

review of: Emma Bell (2008) *Reading Management and Organization in Film*. Basingstoke: Palgrave Macmillan. (PB: pp. 240, £23.99, ISBN: 9780230520929)

As I am reading through *Reading Management and Organization in Film* by Emma Bell, my thoughts constantly waver off in all kinds of directions. But rarely do they linger over any of the films discussed in Bell's little book, or, for that matter, over the ideas that she tries to illustrate through the many film interpretations she engages in. And I can't help wondering what it is that makes my mind flicker and the reading so tedious – and whether or not I am in bad need of that ADHD medicine recently promoted in *Wired* so as to amp up my brainpower.

Medicine aside, I do believe I've figured out why.

What Bell sets out to do in this book, is to explore how different aspects of organization and management are represented in popular film. And she takes what she terms a thematic approach to the endeavor “whereby themes related to management and organization that occur frequently in film are analyzed in depth” (p. 8). In fact, more than a hundred films are referenced, analyzed and discussed over the two hundred plus pages of the book, and related to seven different themes comprising 1) the organization of the film industry, 2) the negative portrayal of organizations, 3) the presentation of ‘organization man’, 4) the shift from modern to postmodern worker, 5) women as the excluded Other of organizational life, 6) a preoccupation with the meaning of work, and 7) spectres of organization. In a somewhat roundabout fashion, this approach is said to rest (partially at least) on a recent development within organization studies by which narrative approaches have been gaining ground. Drawing, for instance, on the work of Barbara Czarniawska and Pierre Guillet de Monthoux (1994), Bell argues that interpreting and evaluating any kind of narrative exercises the reader's skills of criticism and aids the understanding of the complexity of organizational life. And drawing on the work of Nelson Phillips (1995), she argues that a narrative account, if successful, to some extent contributes to validate theory, or theoretical facets, residing in the narrative.

Writing for advanced students of business and management, at an undergraduate or postgraduate level, popular film should thus make up a plentiful source for this audience to further the understanding of organization and management in different times and contexts, and with regards to practical aspects as well as theoretical ones. And working with the assumption that film plays a decisive role “in producing systems of discourse which help to shape our collective perceptions of management that continue to inform our experience of organized work” (p. 202), it could also serve as a means for reflecting upon the impressions and opinions one holds in relation to these matters. All that needs be done to reach this ‘deeper’ understanding, is to take a deconstructive approach to the operation, Bell seems to argue (one influenced also by intertextual and semiotic readings of the material in question).

And this is what Bell claims to do in the book. She has asked around among students, colleagues and friends what films have told them anything about the subject at hand, and she has watched these films closely. She has taken notes, identified themes and worked with these themes “in a way which enabled them to be related to theory” (p. 8). And she has extended the research material and claims to have found related films by consulting services like Amazon’s ‘Customers Who Bought This Item Also Bought’, in what appears to be an effort to reach a fair representation of films dealing with organization and management. Throughout the book, she then elaborates on the different themes, introducing the reader to how they have been discussed within the field, and bringing up numerous examples of how these themes are manifested in film – in the body text as well as in film focus boxes containing more detailed accounts of scenes from particular films (and the approximate running times for these scenes, should we want to check them out ourselves). Aspects of organization and management which usually are repressed in narratives on organization and management are also, in good deconstructive order, explored and exemplified in the last two thematic chapters. And by the very end, Bell concludes by reminding her readers that the specific themes she has been discussing (mentioned only briefly in the above) keep recurring in very similar form in film after film, year after year, and in varying contexts. Somehow they appear to have a universal dimension which stands the test of time and withstands significant external variations in the socio-economic constitution of society. And supposedly, it is their recurring representation which makes them “more acceptable within the social context in which they are located” (p. 202).

Not without its merits, I believe, however, that the approach taken by Bell – and the insights she delivers from the endeavor – are somewhat problematic. And I am quite convinced that this contributes to the reading experience.

To begin with, I’m not entirely certain of whether Bell sees this book as an introduction to (critical) management studies through film, or as a thought provoking piece of research in itself. That is, I am not sure of whether the book strives to introduce and present themes and topics frequently discussed in the organization and management field in an elucidating manner, and point to some of the merits with the kind of reading undertaken. Or, whether it strives to bring about new understandings of the topics addressed, cast new light on the phenomena encountered in the film material – understandings and shifts of perspective that make a strong contribution to the way we think about organization and management practices by challenging prevalent

movements within the field.¹ Or rather, if she sees her work as a little bit of both, whereby the novelty and contribution of the book first and foremost lies in directing the attention towards a somewhat unconventional source of empirical material. But, where the ‘theoretical’ themes are not in themselves explored or challenged much further, but merely collected from a kind of mainstream critical management discourse – with the author’s identification of them only serving to validate their existence and their relevance also in the realm of popular film. For much of new understanding of these themes and how they have been represented through the film medium over time – of their inner workings, of their interplay with the film medium as such, and of slight but important changes in their representations, for instance – is indeed hard to see here. And to claim, as the author does, that the book deals with in-depth analyses of the many films encountered, really is a bold statement in this regard.

Rather, it appears as if Bell, by this only halfhearted attempt to explore the power of theoretical notions and make use of any specific conceptual apparatus, contributes to a slight confusion. This is a confusion one often seems to come across in interdisciplinary fields where philosophical/theoretical perspectives are used somewhat haphazardly and coupled in peculiar and even careless ways (with such variegated weaves of concepts and conceptualizations sometimes even being posited as theoretical bases for a certain field), and it concerns how differing theoretical positions actually relate to one another. In short, a confusion concerning the way in which one treats theoretical notions and concepts.² On the very first page of the book, Bell presents the reader with a quote taken from Slavoj Žižek’s *Interrogating the Real* (2005). Žižek writes:

Why do I resort so often to examples from popular culture? The simple answer is in order to avoid a kind of jargon, and to achieve the greatest possible clarity, not only for my readers but also for myself. (Žižek, 2005: 56)

One can only assume that clarity, rather than confusion, is what Bell also appears to be striving for in her book. But is it not near necessary, for the sake of clarity, to spell out what theoretical position one at least tentatively subscribes to in taking on an endeavor such as Bell’s? Especially, if the analysis circles around ambiguous notions such as reality and ideology, and one sets out by quoting Slavoj Žižek’s work which is infamous for addressing such notions in a somewhat unconventional manner? I find little of this sort, however. And it most probably contributes to making me a rather disinterested reader.

It might seem somewhat superfluous to add, but I am far from convinced that such a move would be opposed to any of the two distinct approaches suggested above – that is, neither to the teacher’s nor the researcher’s approach to analyzing organization and management issues in film. I am not even convinced that they at all need to be

1 Certainly, Bell explicitly states that this is introductory reading for students. But doesn’t she at the same time posit the work as a scientific and explorative endeavor, in her emphasizing that the analysis rests on an allegedly scientific methodological approach, whereby she has been striving for an exhaustive screening of the themes related to organization and management figuring in popular film, and whereby she has been aiming to gather a fair representation of films by means of certain sampling techniques?

2 Not to mention confusion concerning what these theoretical takes actually give to the analysis.

understood as distinct approaches. But for the sake of clarity to any one reader, I suspect that a more fruitful way to carry out an organization and management reading of film, would have been to attempt a symptomatic reading of some of the films treated in the book. That is, to attempt to use philosophy or a theoretical apparatus in order to bring a *new* understanding to how we, through these popular films, relate to management and organization (and not only to validate what we thought we ‘knew’ already); to intervene in the field by *re-interpreting* how popular film might act on our conceptions of organization and management (cf. Kristensen, Pedersen and Spoelstra, 2008). In other words, attempt to use philosophy in order to look for *other* mechanisms at work in ‘film texts’ than merely themes that we are already used to hearing and talking about within the field, and to explore how these mechanisms might work in detail.

That is to say, instead of yet another attempt at deconstructing management discourse and laying bare supposedly stable symptoms, themes, truths (?) of the representation of organization and management, would it not have been a more fruitful project to perform, for instance, an ideology critique of some of the films in question in a Žižeko-Lacanian vein; exploring different ideologies at work in these films, and how these ideologies might structure various kinds of enjoyment regarding organizational and management practice? And, not to forget, exploring how these ideologies might have changed ever so slightly throughout the time-period explored and discussed (i.e., mainly the twentieth century)? Seriously subscribing to a Žižek-inspired theoretical framework – which the author shows some aspiration towards doing, judging again by the opening quote – implies that such an analysis would be able to capture beliefs at work in this particular realm, and in society more broadly, by treating popular film as a materialized unconscious. And, hence, not as something separate from our immediate perceptions of organization and management but as a part of the apparatus which creates the fundamental screen through which we view and perceive in the first place (i.e., ideological fantasy) (cf. e.g. Žižek, 2007: 52).

Such an endeavor would, to my mind, be truly interesting – as material for teaching advanced students of business and administration, and for researchers alike. It would, however, most certainly require a dramatic narrowing down of the film material addressed. And thus not offer as many analyses of particular film scenes, and as many films to point a larger group of students to for further exploration, as the book now does. But I believe that the benefits certainly would overshadow such drawbacks. For potentially, at least, it would give to the text and the analysis that which it most acutely is lacking in its current state. Namely a strong guiding idea of what is to be demonstrated throughout the book; a strong idea guiding the exploration and analysis undertaken – and not least guiding the reader.

Moreover, such an approach might well have done away with some of the anxiety which marks the text, now manifesting itself in the author attempting to legitimate and explain her endeavour far more than I believe is necessary, or even sound. And, instead let her concentrate on getting the analysis to speak for itself, and demonstrate that the project is one worthwhile.

Such an approach might also hold in itself a potential for explaining one of the major mysteries surfacing in this work, and which has been hinted at in the above. Namely, that although film is proposed to contribute to producing and re-producing systems of discourse which shape collective perceptions of management, and thereby the themes discussed throughout the book, these same themes – according to Bell – appear to demonstrate a “historical endurance despite significant socio-economic changes in the period since film making began” (p. 202). And, in case they should disappear for a bit, Bell concludes that they merely “recede until societal conditions change in a way that permits their revival in a revised form” (*ibid.*). But – forcing the argument just a bit – if film discourse plays such an important role in shaping society, and society’s view of organization and management, how come discourse can remain (thematically) stable while society is changing significantly? – And in case it is not that stable, but dependant on ‘societal conditions’? What explanatory power lies in such a conclusion? Could this insight not, rather, suggest that our collective perceptions of management aren’t indeed, to any greater extent, shaped by these seemingly stable discourses or themes, but that you always will be able to find examples of such themes if you are looking for it? Or, on the contrary and more plausibly: that there actually have been important changes in these discourses which we haven’t yet paid enough attention to, and properly understood?

Is it not also in order to better understand the somewhat paradoxical conclusion of Bell, that we must proceed to a Žižeko-Lacanian framework for carrying out symptomatic readings of cultural texts such as film? I am convinced it would make medication a less tempting option.

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Managerialism and Development

Sam Dallyn

review of: Sadhvi Dar and Bill Cooke (eds) (2007) *The New Development Management: Critiquing the Dual Modernization*. London: Zed Books. (PB: pp. 245, £17.99, ISBN: 9781842779224)

Does development need management? Or what is the relationship between development and managerialism? These are questions that might immediately occur to one when taking a first glance at Dar and Cooke's rich collection of essays entitled *The New Development Management*. But Hugh Willmott's foreword and Arturo Escobar's afterword (senior thinkers from the critical management studies – CMS – tradition and the critical development studies – CDS – tradition respectively) already begin to address such questions by placing the book in context. Dar and Cooke's first chapter establishes an important link between CMS 'alternative' perspectives, key thinkers of which being Alvesson and Willmott (1996), and post-development thinking and its central thinkers, such as Ferguson (1994) and Escobar (1995). However, this book is characterised by a broad and diverse range of theoretical approaches brought together with insight and critical acumen from post-colonial theory (Kenny), with particularly valuable and insightful ethnographies (Srinivas, Kenny and Dar), theories of elite formation and ideology in terms of managerialism (Murphy, Cooke, de Vries), critiques of the prevalence of audit culture and log frames in development (Kerr, Dar) and critiques of NGOs as complex subjects and purveyors of development and administration management (Lewis).

In many ways the book succeeds in situating critiques of Development and Administration Management (DAM), a subject of Cooke's (2002; 2003; 2004) earlier work, within a wider CDS tradition as having an important and valuable contribution to make, which I will begin by discussing. I want also to draw on some of the strongest aspects of the book when it engages with development actors ethnographically and critically. Finally though I want to bring out some tensions between this broad range of different theoretical approaches and studies of development which in turn raise questions about where critiques of DAM go from here.

In regard to what precisely critiques of DAM are against it is Parker (2002) that seems to set the terrain of the book and is drawn on in several of the contributions. As Parker (2002: 10) famously directs his critique at the market driven ideology of managerialism,

this fits the critiques of DAM since one can associate DAM with a certain aggressive neoliberal ideology that casts itself as technocratic. Through matrixes of development (Cammick, 2002) to log frames and the project cycle which present themselves as sciences of universal applicability (Kerr, p. 97) but are rather political instantiations of a neoliberal ideology.

The book is at its best in the earlier chapters where Lewis (p. 50) makes this kind of intervention in a historical analysis of NGOs and nongovernmentalism which he defines as a “policy ideology of comparative advantage that privileges ideas about efficiency and flexibility of nongovernmental actors”. The discussion around the role of NGOs is neatly situated around broader debates within managerialism and different more complex manifestations of this are brought out on the ground in particular micro contexts by Kenny, Srinivas and Kerr. One of the richest case studies is provided by Srinivas where she draws out the complex interaction between modernisation and tradition and their reconfiguration in the context of a close examination of the autonomous handlooms sector in India. In an insightful examination of the creation of spaces of tradition that represents a strange modern instantiation of tradition (Srinivas, p. 78), she draws on a modern and self-conscious carving out of a traditional space of a model village of craftspeople, Shilparamam. In this nuanced analysis questions of modernisation, managerialism, state government responsibility in protecting exclusive aspects of tradition and commercialism are all read through the narrative of the changing handlooms sector in India.

Similarly in a grounded, post-colonial, reflexive and ethnographic analysis Kenny examines the practices of a small, non-profit United Kingdom NGO, EWH which builds and designs information technology products. Using post-colonial theory she draws attention to the Western NGO’s characterisation of a non-Western Other in need of help (Kenny, p. 61) and as inferior. She also highlights what she characterises as silent discourses, which one might in a Foucauldian sense refer to as the nature and formation of the sayable (see for example: Foucault, 1991: 60; 1990). The two silent discourses refer to the unquestioned idea that EWH can save the Other and that the Other is in need of being saved, the second is the position of the EWH itself which was never seriously debated (Kenny, p. 67-68). Furthermore, she closes by noting the pressing need for an examination of the ‘micro-processes of power’ within contemporary organisations in a development context (Kenny, p. 72). It is this task which the book at its best performs in these two chapters and others through being faithful to the lived experiences of subjects and employing theoretical tools which are sensitive to and draw out some of the disjunctures, the gaps where things don’t fit in terms of policy models, between different levels of discourses and practices.

The significant tension in the book is between different forms of grand theorising (Murphy, Thorne and Kouzmin) and the more rooted ethnographic approach of particular NGOs and projects which draw out complexities and discontinuities on the ground in particular contexts. If there is a weaker part of the book in my view it is in some of the grander theorising that neither does justice to development experiences nor to the positions that are critiqued. While Murphy’s chapter does provide a clearer sense of what the book is against, global managerialism as an elite formation and ideology, there is a rather too easy totalising critique in operation here. Focusing on the World

Bank as the principle purveyor of development managerialism and its ideology he focuses on the interesting example of the Millennium Development Goals and the particular commitment to education for all (EFA) which originally came from UNESCO. He argues in the context of Niger that the EFA goals, at the World Bank's stipulation, led to the reductions in salaries of teachers and the creation of a group of underpaid contract teachers as well as a directing of resources away from secondary towards primary education (Murphy, p. 36-37). This critique of a transnational global managerial elite is nicely aided by the EFA example, however what is missing in this grand characterisation is the potential for disjunctures and differences within this elite ideology, perhaps between a more socially oriented development model and a more purely neoliberal one. These points of tension have often characterised the history of the World Bank's development as an institution (see for example Wade's (2001) account of the issues around the making of the 2000 World Development Report and Kanbar's resignation) but are missed when one paints too homogenous a picture of the ideology and practice of a global managerial elite.

Later in the book Thorne and Kouzmin (p. 134) equate postmodernism with a certain neoliberal agenda and then rather carelessly equate postmodernism with Foucault, which seems all the more pressing since in de Vries's following chapter care is taken to distinguish postmodernism from poststructuralism. This disavowal of the postmodern tends to seem at times like pulling apart a straw man since one is often not sure who or what is being referred to. Furthermore equating postmodernism with neoliberalism and globalisation raises the question of whether one is referring to a particular constellation of thinkers, or to a sociological condition, as can be found in Jameson and often Baudrillard. However, there is a lack of clarity in the argument here, if postmodernism tells us something about neoliberalism and globalisation, and I'm not clear what it is or whether it does, then it is at least sociologically valuable in that it reveals something significant about the way capitalism operates in the present world. Alternatively they might be using postmodernism to refer to a particular group of thinkers that might be associated with certain 'postmodern' developments in management theory, in which case what these developments are needs to be clearly established in relation to particular thinkers who reflect this, but the chapter does not provide any detail or evidence of this. Thus the critique of postmodernism in Thorne and Kouzmin's chapter seems beset with a confusing ambiguity because of a lack of definitional clarity.

De Vries's following chapter is theoretically intriguing and makes a radical almost disjunctive gesture where he calls for the 'promise of development' drawing on his experiences of Andean villagers and their unfulfilled desires for concrete improvements in areas such as infrastructure (de Vries, p. 172). De Vries misses the mark however with a pretty unreasonable characterisation of David Mosse's work. The de Vries critique runs that Mosse argues that analysis of development should be ethnographic and that the complexities of practice as it interacts with policy formation should be more closely examined. De Vries (p. 162) plays fast and loose here declaring that this is ultimately a legitimisation of the workings of global managerial elites. What Mosse (2004) I think is calling for here and throughout his work rather is much more grounded theory and complexifying the processes of policy legitimisation and the disjunctures between policy and practice in development. Indeed, I think this sort of critique is vital

for CDS and is most evident in his co-edited book with David Lewis *The Aid Effect* (2005). It is precisely this kind of critique that *The New Development Management* at its best achieves. As Dar and Cooke (p. 17) acknowledge at the start in reference to Dar's contribution, the task is to examine the "dispersed and ambiguous discursive field that is malleable, reconstructing and continually being real-ized by development workers in different ways and with very different effects in the field".

It is here too in the margins of practices and its consequences on given populations at the micro level of the field, that alternatives might emerge. These can only be engaged with I think through applying a grounded theory that is sensitive to subject actors and populations. Indeed while the book does not profess to have the intention of addressing alternatives (Dar and Cooke, p. 17) and rather focuses on bringing to light the problems with development managerialism at the end of certain chapters there are instances of disjuncture of how the managerial development script is not quite played out in practice. Kerr (p. 109) for example closes his analysis of log frames with a notion of the carnivalesque and humour as development workers mocked instrumental log frames in development through creating songs. While he also acknowledges the limitations of this kind of resistance casting it as anti and not counter hegemonic he points to it as an illustration that "what is represented by the powerful is not the final word on representation" (Kerr, p. 110). Indeed, it is potentially in the marginalised and disjunctive (Van den Berg and Van Ufford, 2005) practices of development in resistances on the ground in particular contexts to managerialism that the most damaging critiques of DAM as well as the beginnings of alternatives might emerge. Furthermore it is precisely this kind of theoretically grounded, ethnographically situated micro analysis and its complex interaction with bigger discursive pictures in development that I would argue is the most promising line of critique and methodology against DAM. Slaughter and Rhoades (2004), Harvie (2006) and Martin (1998), contributes to the growing leftist critique of corporatized Higher Education in advanced capitalist countries.

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