ephemera | articles
theory & politics in organization

© ephemera 2008
ISSN 1473-2866
www.ephemeraweb.org
volume 8(4): 403-419

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 Otzas, 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 Otzas 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 undecideable 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. Modern*ism* 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 modern*ist* 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 passingly), 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.

references

Allen, P. (2001) 'What is Complexity Science? Knowledge of the Limits to Knowledge', *Emergence*, 3(1): 24-42.

Ashby, W. R. (1970) 'Analysis of the System to be Modelled', in R. Stogdill (ed), *The Process of Model-Building In the Behavioral Sciences*. New York: Norton/ Ohio University Press.

Beer, S. (1994[1979]) *The Heart of Enterprise.* Chichester: Wiley.

Boisot, M. and B. McKelvey (2006) 'Speeding up Strategic Foresight in a Dangerous and Complex World: A Complexity Approach', in G. G. S. Suder (ed), *Corporate Strategies under International Terrorism and Adversity.* Cheltenham: Edward Elgar.

Burrell, G. (1994) 'Modernism, Postmodernism and Organizational Analysis 4: The Contribution of Jürgen Habermas', *Organization Studies*, 15(1): 1-45.

Burrell, G. (1996) 'Normal Science, Paradigms, Metaphors, Discourses and Genealogies of Analysis', in S. Clegg, C. Hardy and W. Nord (eds), *Handbook of Organization Studies.* London: Sage.

Burrell, G, and G. Morgan (1979) *Sociological Paradigms and Organisational Analysis*. London: Heinemann.

Campbell, D. T. (1959) 'Methodological Suggestions from a Comparative Psychology of Knowledge Processes', *Inquiry*, 2: 152-182.

Carter, P. and N. Jackson (2004) 'Gilles Deleuze and Felix Guattari – A "Minor" Contribution to Organization Theory', in S. Linstead (ed), *Organization Theory and Postmodern Thought.* London: Sage.

Chia, R. (1996) *Organizational Analysis as Deconstructive Practice*. Berlin: De Gruyter.

Cilliers, P. (1998) *Complexity and Postmodernism – Understanding Complex Systems*. London: Routledge.

Clegg, S., C. Hardy and W. Nord (eds) (1996) *Handbook of Organization Studies.* London: Sage.

Clegg, S., C. Hardy, T. Lawrence and W. Nord (eds) (2006) *Handbook of Organization Studies*, 2nd edition. London: Sage.

Derrida, J. (1973) *Speech and Phenomena, and Other Essays on Husser's Theory of Signs*. Evanston, Illinois: Northwestern University Press.

Derrida, J. (1976) *Of grammatology.* Baltimore, Maryland: Johns Hopkins University Press.

Derrida, J. (1978) *Writing and Difference*. Chicago: University of Chicago Press.

Derrida, J. (1994) *Specters of Marx*, trans. P. Kamuf. London: Routledge.

Donaldson, L. (1985) *In Defence of Organization Theory*. Cambridge: Cambridge University Press.

Hassard, J. (1988) 'Overcoming hermeticism in Organization Theory: An Alternative to Paradigm Incommensurability', *Human Relations*, 41(3): 247-259.

Henrickson, L. and B. McKelvey (2002) 'Foundations of "New" Social Science: Institutional Legitimacy from Philosophy, Complexity Science, Postmodernism, and Agent-based Modeling', *Proceedings of the National Academy of Sciences*, 99 (supp. 3): 7288-7295.

Kuhn, T.S. (1970) *The Structure of Scientific Revolutions*, 2nd edition. Chicago: Chicago University Press.

Jackson, N. and P. Carter (1991) 'In Defence of Paradigm Incommensurability', *Organization Studies*, 12(1): 109-127.

Latour, B. (1993 [1988]) *The Pasteurization of France*, trans. A. Sheridan and J. Law. Harvard: Harvard University Press.

Lyotard, J.-F. (1984) *The Postmodern Condition*, trans. G. Bennington and B. Massumi. Manchester: Manchester University Press.

Lyotard, J.-F. (1988) *The Differend*, trans. G. Van Den Abbeele. Manchester: Manchester University Press.

Macey, D. (2000) *Dictionary of Critical Theory*. London: Penguin.

Maguire, S., B. McKelvey, F. Mirabeau and N. Otzas (2006) 'Complexity Science and Organization Studies', in Clegg, S. et al, op cit.

Massumi, B. (1992) *A User's Guide to Capitalism and Schizophrenia*. Cambridge, Mass: MIT Press.

McKelvey, B. (2003) 'From Field to Science: Can Organization Studies make the Transition?', in R. Westwood and S. Clegg (eds), *Debating Organization*: *Point-Counterpoint in Organization Studies*, Oxford : Blackwell.

McKelvey, B. (2007) '1st principles of efficacious adaption' [http: www.economics.uci.edu/~dbell/ 1st%20principles.doc]

McKelvey, B. and M. Boisot (2003) 'Redefining Strategic Foresight: "fast" and "far" sight via complexity science', paper presented at the *INSEAD Conference on Expanding Perspectives on Strategy Processes*, Fontainebleau, France.

O'Doherty, D. P. (2007) 'The Question of Theoretical Excess: Folly and Fall in *Theorising* Organization', *Organization*, 14(6) : 837-867.

Richards, I. A. (1964 [1929]) *Practical Criticism*. London: Routledge.

Sievers, B. (1993) *Work, Death and Life Itself*. Berlin: De Gruyter.

Westwood, R. and S. Clegg (eds.) (2003) *Debating Organization: Point-Counterpoint in Organization Studies*. Oxford : Blackwell.

the authors

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.
E-mail: carterjackson@carterjackson.karoo.co.uk

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.
E-mail: carterjackson@carterjackson.karoo.co.uk