
In Defence of Paradigm Incommensurability

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Abstract

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This paper presents a rigorous defence of paradigm incommensurability as conceptualized by Burrell and Morgan, arguing that it is a necessary defence against scientific authoritarianism. Two recent attacks on paradigm incommensurability are examined which, although emerging from different intellectual traditions, are both seen to imply the restoration of the authority of scientism characteristic of the postmodernism of reaction. Both critiques base their attack on the work of Kuhn but this basis is rejected on the grounds, *inter alia*, of a clear absence of isomorphism between the paradigmatic model of Kuhn and that of Burrell and Morgan. Finally, the paper considers the relevance of paradigm incommensurability in the context of non-reactive postmodernism.

Introduction

It is now ten years since the publication of Burrell and Morgan's (1979) controversial contribution to organization studies, *Sociological Paradigms and Organisational Analysis*. The immediate impact of this book was to provide a framework which clarified the relationship between competing claims about organizations. Following on, and no doubt influenced by, the intellectual expansion of the 1960s, the 1970s had seen the emergence of thinking which, on the one hand, constituted a heterogeneous critique of the dominant Functionalism of the preceding era and, on the other, introduced new possibilities for understanding organizational structure and practice (cf. e.g. Silverman 1970). The publication of *Sociological Paradigms and Organisational Analysis* caused a frisson which signalled both its influence and its importance, but, whilst its influence has continued to be recognised in the widespread acceptance of its framework as a way of perceiving the work done in the discipline, its importance has gradually been attenuated through the prioritization of the form of the model of paradigmatic analysis at the expense of the theoretical imperatives of such a model (cf. e.g. Clark 1985) — in fact, has suffered precisely the emasculation and incorporation into the Functionalist problematic that Burrell and Morgan warned against (1979: 398).

Burrell and Morgan were quite specific that *a synthesis between paradigms cannot be achieved*, that they must remain discrete and

develop independently — in other words, that the paradigms are incommensurable. This incommensurability stems from the commitment to opposing beliefs in the nature of the foundational assumptions which make up the two dimensions, namely, ontological, epistemological, human nature and methodological assumptions on the objective/subjective dimension and regulatory/radical change assumptions on the other. Each pair of adjacent paradigms shares a common dimension but the differences in understanding on the other dimension are sufficient to make them as incommensurable as those pairs which do not share any common dimension. In present day parlance, incommensurability could be explained by the argument that each paradigm has a discrete language of its own — although they may, and probably do, share common signifiers, the signifieds entailed are radically different.

For those who see Functionalist science as an exercise in intellectual imperialism, dominating organization studies both epistemologically and politically, the paradigmatic understanding of organization theory offers a way of legitimating approaches whose probity would be denied by Functionalism. At the same time, the effect of paradigm incommensurability is to establish the integrity of each paradigm, obviating the necessity for interminable justifications of different ontological, epistemological and methodological approaches to the subject area, different beliefs about human nature and whether organization studies is primarily prescriptive or descriptive. What it implies is that each paradigm must, logically, develop separately, pursuing its own problematic and ignoring those of other paradigms as paradigmatically invalid, and that different claims about organizations would, in an ideal world, be resolved in the light of their implications for social praxis. In our world which is less than ideal, however, what has been witnessed is a more or less generalized attempt to synthesize the paradigms rather than to accept their inherent incommensurability. The effect of this is to render the paradigms into a technique for analysis. The influence of Burrell and Morgan's book has been achieved at the expense of that very quality of the model which gives the paradigms their utility, that which establishes them as an intellectual structure and makes their use as a mere technique not just wasteful, but actually inappropriate. It is paradigm incommensurability which is of central importance in Burrell and Morgan's theory and it is this particular aspect which has been the subject of attack, not only by Functionalists who, by denying it, open the gates to incorporation, but also by more adventurous thinkers. Yet incommensurability is not negotiable, it is a characteristic of the model — without it, paradigms are an unnecessary elaboration of what is already elaborate and complex.

However, our defence of paradigm incommensurability does not rest simply on a desire to retain the model in its pristine condition, or on the practical benefits bestowed by it on the non-Functionalist organization theorist, but has a more positive purpose. Paradigm incommensurability is a concept which has a practical historical (emancipatory) value. For, far from being an impedance to the development of genuine human under-

standing, it serves to protect actual plurality of modes of scientific enquiry from the imperialistic aspirations of an orthodoxy whose interests are rooted in performance and control, rather than the liberation and emancipation of the individual in society. Whilst the ultimate possibility of a genuinely unified body of knowledge remains an ideal goal, under the present conditions of an authoritative science acting in defence of a specific set of ideological values, the necessary conditions for such a body of knowledge cannot be said to exist. What we risk is the re-establishment of the dichotomy between science and non-science, determined by a totalitarian scientism which claims the exclusive right to define what is science.

The intention of this paper is thus to examine the fate of paradigm incommensurability, and to call for its restitution as a central feature of paradigmatic analysis. We shall argue three major points. First, that there is a natural aversion, bordering on horror, to paradigm incommensurability amongst those who claim to be the purveyors and creators of knowledge, which aversion gives the impetus to attempts to synthesize the paradigms. One of the weapons used by those attempting synthesis is to claim that Burrell and Morgan's paradigms are a misconstruction of Kuhnian paradigms. Second, therefore, we shall argue that there is no basis for a comparison between the paradigms of Kuhn and those of Burrell and Morgan. Our third point is that the denial of paradigm incommensurability can be seen as part of what Foster (1985) calls the postmodernism of reaction. Burrell and Morgan's analysis created the potential to expand dramatically the scope of organization studies, the interests represented and those empowered to speak. This democratizing process legitimates the plurality of perspectives and the oppositional voice which challenges the hegemony of Functionalism. Yet this plurality sits ill at ease with the contemporary trend towards socio-political authoritarianism, of which science is clearly a constituent part. The characterization of this trend as the 'postmodernism of reaction' signifies the profoundly conservative and nostalgic response to models of society which emphasize its complexity and inherent ambiguity, and which thereby threaten the established order. In support of our arguments we shall consider two recent attacks on paradigm incommensurability, from writers of markedly different persuasions who, unsurprisingly, arrive at similar conclusions in terms of the implications of the denial of paradigm incommensurability, both thereby serving to reinforce the domination of the orthodoxy.

The Implications of a Paradigmatic View of Knowledge

In a hypothetical pre-paradigmatic world, knowledge was characterized as either science or non-science. The task of science was seen to be the discovery of law-like generalizations about the world, in the social world in a manner commensurate with the practice of science in the natural

world. The knowledge produced by science was understood to be factual, constant and objectively true. Non-scientific knowledge was knowledge constituted by, e.g., myth, opinion, value, norms, etc. — i.e. subjectivity, and thus was outside the realm of science. What constituted knowledge was determined by people called scientists, who were also those who had access to channels through which knowledge pronouncements were made. Because of the objective and factual character of scientific knowledge, scientists were neutral agents who merely made intelligible what nature had concealed. As such, scientists obviously could not be seen as morally responsible for what was the work of nature, and hence could not be held responsible for the effects of their findings.

Burrell and Morgan's arguments throw this model into confusion. The implications of the paradigmatic model are quite clear. First, there is no single homogenous body of scientific knowledge. Burrell and Morgan demonstrate that scientific knowledge is fragmented and contradictory. They suggest instead four macro bodies of knowledge, by definition distinct from each other because, although addressing ostensibly the same phenomena, they are based upon fundamentally different hypotheses about the nature of the world, the role of science and the problematics to be resolved. This understates the substantial differences *within* paradigms, which should not be ignored and which, at a subordinate level of analysis, might be seen to constitute further bodies of knowledge. It follows from this that a body of knowledge which claims to be true knowledge on the grounds of its scientific character cannot justifiably claim to be absolute knowledge, since claims to processual knowledge — i.e., that it is a particular *method* of discovering knowledge which assigns truthfulness to the knowledge produced — are only tenable within one paradigm. One paradigm, or even parts of more than one paradigm, may be absolutely true, but we do not as yet possess the capacity to identify such truths. Given the fragmentary and contradictory nature of paradigmatic knowledge claims, there can be no claims for transcendental knowledge, for hierarchizing knowledge. Thus science can no longer validate its claims epistemologically — if scientific knowledge claims are derived from beliefs about the nature of the world held by scientists, then clearly scientific knowledge is validated ideologically. This being the case, scientists are manifestly implicated in the consequences of their knowledge claims and can no longer be seen as innocent instruments of an impartial Nature.

Not surprisingly, scientists are very unwilling to accept this onerous burden of responsibility (Ravetz 1971). Such a responsibility demands an awareness of the dynamic social world which has hitherto been completely irrelevant to the practice of science (cf. e.g., Da Solla Price 1963; Polanyi 1958; Bohm 1983; Capra 1983, etc.). The effect of this is radically to proliferate the variety and ambiguity inherent in the environment of science. As argued elsewhere (Jackson and Carter 1984), the natural, and indeed necessary, tendency of human beings is to attempt to reduce the intrinsic variety in the world by appropriate means — the implication of

paradigmatic analysis for social science is to render the invocation of science as an attenuator of such variety no longer appropriate. The response to the discovery that a particular attenuator is inadequate must be *either* to find another attenuator which is appropriate, *or* to reject the claimed inappropriateness of the existing attenuator. To adopt the former course is to reduce the level of attenuation with a new understanding, creating a much enlarged version of what is the arena of, in this case, organizational science. To adopt the latter course is to deny the provenance of the paradigmatic model not on the basis of science qua science, but because such a denial constitutes an anxiety reducing strategy.

Extending the homeostatic analogy, the paradigmatic view of knowledge, by introducing an enormous amplification of the potential variety in the 'model' of what might be considered appropriate to organization theory, dramatically violated the stability criteria of the discipline. The problem was not the amplification itself, but its heterodox nature. The rise of the heterodoxy left the orthodoxy with a difficulty — either the adequacy of approaches to organization theory which reject the dominant view must be accepted, or that dominance had to be re-established. But selection of a response was not an evenly balanced choice. If a multi-paradigmatic view of science is accepted, then the orthodoxy can no longer claim to represent the one true science, and must deny the one article of dogma which allows the claim to be making authoritative statements. Thus the acceptance of paradigms can be interpreted as constituting a fundamental attack on the concept of selfhood of the orthodox scientist. This attack is not only at the psychological level, but also at the material level — it not only makes identity problematic but also, thereby, makes problematic the basis of domination. The problem for the orthodoxy was not so much the *presence* of new actors and new ideas, but what that presence *implied*. What was necessary was for these to be re-defined in terms of orthodox scientific dogma. A boundary needed to be re-established between orthodox science and non-science, not on the basis of subject matter, but on the basis of the criteria of science. As Bernstein says, to those who oppose the idea of paradigm incommensurability 'it opens the door to everything that is objectionable — subjectivism, irrationalism, and nihilism' (1983: 79).

Burrell and Morgan and Kuhn

Prior to the publication of *Sociological Paradigms and Organisational Analysis*, the notion of paradigms as a way of conceptualizing variety in the production of knowledge had already been explored by Kuhn in his book *The Structure of Scientific Revolutions* (first published 1962, 2nd ed. 1970). Kuhn's argument that a paradigm of 'normal science' is overtaken and superseded by a paradigm of 'revolutionary science' which, in time, itself becomes a paradigm of normal science, appears to lend itself to a way of understanding Burrell and Morgan's paradigms. It could be con-

strued, by mapping Burrell and Morgan's paradigms onto those of Kuhn, that the Functionalist paradigm is analogous to normal science and that the non-Functionalist paradigms together represent the revolutionary science paradigm, only one of which will eventually succeed in acquiring the status of normal science. Either way, given time, the four Burrell and Morgan paradigms would eventually dissolve into a new orthodoxy of which the prior Functionalist paradigm would obviously be a major component (see Kuhn 1970: 169). Clearly, this interpretation of the paradigms is very comforting to science, since not only is the contradiction between paradigms deemed to be healthy but it will also eventually resolve itself, without the need for action on the part of individual scientists. Furthermore, it implies that what will evolve from this resolution will re-establish that which is truly science — which has already been decided *a priori* — and discard that which is non-science.

Unfortunately, this attempted rapprochement between Kuhnian paradigms and those of Burrell and Morgan is untenable. Apart from the use of the word paradigm and the fact that both texts are concerned with the production of knowledge, the two theories are markedly different — there are no obvious reasons for conjoining them. This point might anyway be concluded by reference to Burrell and Morgan's text — there is virtually no consideration of Kuhn's work in the book, although with hindsight we might say that this was an unfortunate omission, as enlarged comment might well have obviated the confusions which were later generated. There are only two brief references to Kuhn in Burrell and Morgan's index, plus a fairly long footnote (Burrell and Morgan 1979: 35–6, note 1) which explicitly states that they are *not* using Kuhn's conception of paradigms as the basis for their own work. They do actually cite a number of schematists whose work 'fall(s), in a partial way, *within* the bounds of the scheme developed here' (1979: 37), but the list does not include Kuhn, which at least implies that they do not see Kuhn's work as an example of this approach.

However, in spite of this, attempts have been made to conflate the two paradigmatic theories. An example is the critique of Burrell and Morgan's text offered by Donaldson (1985) where there is considerably more about Burrell and Morgan's use of Kuhn than there is about Kuhn in their text. Donaldson claims that 'Burrell and Morgan base their notion of paradigm on that of Kuhn' (1985: 38). *Explicitly, they do not* — the comment Donaldson refers to is limited to the possibility of trans-paradigm migration. Donaldson then asks 'But is Burrell and Morgan's notion of paradigm consistent with Kuhn's conception?' (1985: 38). If Burrell and Morgan do not base their conception of paradigm on Kuhn's work, then this question, and the extended defence of Kuhn from the charge of relativism which follows, is not merely misplaced — Donaldson fails to make the distinction between the *process* of knowledge production and the *product* itself that Kuhn makes — but is actually irrelevant. (Ironically, the question would only be significant if the answer to it was yes.) Donaldson uses this assertion of isomorphism (apparently mis-

construed by Burrell and Morgan) between the two paradigmatic theories in the first place to dismiss the integrity of Burrell and Morgan's model. The main thrust of the claim of isomorphism, however, is to attack the concept of incommensurability between paradigms, an attack largely related to the assertion that Burrell and Morgan mistakenly see Kuhn as an Interpretivist (though this characterization of Kuhn in Burrell and Morgan *refers explicitly to Kuhn's theory of knowledge production* [Burrell and Morgan 1979: 255]). In Kuhn's theory, the normal scientific and revolutionary scientific paradigms are incommensurable, but this is a temporary condition resolved over time. However, arguably, Kuhn's model of knowledge (*rather than of knowledge production*), falls within Burrell and Morgan's Functionalist paradigm — i.e. both normal and revolutionary science are functional (see e.g., Bernstein 1983: 52) — and thus the extra-paradigmatic disagreement of Kuhn becomes an intra-paradigmatic disagreement in Burrell and Morgan. Kuhn's distinction between knowledge and its process of production make it perfectly consistent to claim that the former is Functionalist and the latter Interpretive. Clearly, it is contentious to separate out the product from the process in such a discrete fashion since it might be argued that this ignores the influence of the socially defined characteristics of the production of knowledge on that which is produced — thus Kuhn ignores the influence of power in defining problems, for example. But this problem is one which is inherent to Kuhn's argument, not one which has a bearing on the conceptualization of Burrell and Morgan's model.

But the significance of the inappropriateness of the comparison between Kuhn's work and that of Burrell and Morgan lies in the fact that not only are Kuhn's paradigms convergent while Burrell and Morgan's are divergent, but also that the former are diachronic while the latter are synchronic. It is important to recognize that the two models are distinctive on a number of levels, including the most fundamental one of what the theory sets out to achieve. For Kuhn's theory sets out to demonstrate how enormous diversity of practice ultimately contributes to a unified body of science (convergence), that techniques which are eventually proved to possess inadequate explanatory power give way to those which are adequate in terms of their problem-solving capacity and that these together constitute universally defined good science and true knowledge. (Thus, such convergence does not operate epistemologically, but takes effect at the problem-solving, i.e. pragmatic, level.) Burrell and Morgan, on the other hand, intend to demonstrate that an apparent unity of (social) science is actually characterized by incommensurable diversity, leading not to one homogenous body of science, but to intrinsically different ones (divergence). What in Kuhn's terms might be a (universally) good problem-solving technique, in Burrell and Morgan's terms might well, from another paradigm, be seen as creating more problems than it solves. But the over-arching difference occurs at the level of the perceived time horizon of the two models. Kuhn's theory is diachronic — the incommensurability between paradigms is resolved on the temporal

dimension in a convergence of what were initially divergent theoretical perspectives. Burrell and Morgan's paradigms are, however, synchronic, they exist simultaneously and in perpetuity — the incommensurability which characterizes relations between the paradigms is irremediable. Even a glance at the characteristics of the dimensions shows that this must logically be the case, even were not incommensurability a given factor of the model. The passage of time in this model at best leaves the relations between the paradigms as relatively static, or, more likely, points to an increasingly significant divergence between the precepts of each paradigm. Furthermore, whilst Burrell and Morgan's paradigms are autonomous in relation to each other, sharing no necessary common antecedents, it is an implication of Kuhnian paradigms that they cannot be autonomous but must share a common base. The Kuhnian scientists may be in different paradigms but they are trying to solve the same puzzles (Kuhn 1970: 169, 205–6), but the 'inhabitants' of the different paradigms in Burrell and Morgan's model do not share the same objectives, and thus cannot move towards a common, unified goal — along with all the other facets of incommensurability between these paradigms, there is also an incommensurability of goals. For these reasons, incommensurability in the Kuhnian sense is apparently, as indeed Donaldson (1985: 39) notes, only temporary, but in Burrell and Morgan's sense it is a permanent feature of the model.

Kuhn's Concept of Paradigm Incommensurability

However, it is worth looking at the detail of Kuhn's paradigmatic model more closely, and to be more precise about the meaning of incommensurability in this context. Kuhn's model indeed represents a process of paradigmatic resolution but, most importantly, this resolution occurs not through synthesis but through appropriation. Quite clearly, there is nothing dialectical about the relationship between the paradigms in Kuhn's work. The popular conceptualization seems to be that there is a thesis (normal science) and an antithesis (revolutionary science) and these are resolved in a post-revolutionary synthesis, a synergetic abstraction of the best from each paradigm. However, this is definitely not Kuhn's model. Kuhn, adopting, effectively, the perspective of structural linguistics, argues that the protagonists of each paradigm in effect speak different languages, and that in the 'debate' which follows the nascence of the revolutionary paradigm one of two things can happen — either normal scientists switch paradigms as a result of the persuasive arguments of the revolutionary scientists or, given time, they become extinct — Kuhn comments that 'Conversions will occur a few at a time until, after the last hold-outs have died, the whole profession will again be practising under a single, but now a different, paradigm' (Kuhn 1970: 152). This is the mechanism by which the new normal paradigm is, in practice, the old revolutionary paradigm, still in pristine condition — it does not evolve from some rapprochement between the two paradigms, which is the one

form of paradigmatic resolution not possible. Since both the Kuhnian paradigms are judged in terms of their problem-solving capacity, any resolution which resulted from a combination between them would compromise the problem-solving capability of the revolutionary paradigm and thus detract from its claims to utility. Thus Kuhn avoids supporting the fallacy that the truth lies in the middle. This kind of resolution would effectively be a retrogression since, once the normal and revolutionary paradigms had synthesized, there would still be an incrementally revolutionary paradigm whose marginal problem-solving capability would be greater than that of the new normal paradigm. Paradigms become commensurable in the Kuhnian model by the ruthless extermination of the normal science paradigm, and by no other means: 'Revolutions close with a total victory for one of the two opposing camps' (1970: 166).

The misapprehension about paradigm incommensurability which abounds seems to stem from a lack of appreciation of what incommensurability actually means. Basically, what is incommensurable are the languages used by each paradigm, in a structuralist sense — again, Kuhn is quite specific about this. In the normal science paradigm there is a body of scientists using a language with a particular set of signifieds, and in the revolutionary science paradigm another body of scientists using a language comprised of the same signifiers but which indicate *different* signifieds (1970: 200). In other words, there are two completely different languages between users, of which meaningful communication is not possible. With the accession of the revolutionary science paradigm to the status of normal science, incommensurability ceases because one language has died out, not because a common signification has been achieved. Thus, commensurability is achieved synchronically through the elimination of one protagonist. However, if a diachronic perspective is taken, then the paradigms retain their incommensurability. If, after the accession of the revolutionary science paradigm, a group of scientists took up the language of the old normal science paradigm, for whatever reason, they would still be incommensurable.

Thus Kuhnian incommensurability is, in effect, wholly consistent with incommensurability in Burrell and Morgan's model. Indeed, paradigm incommensurability in the latter case could be resolved — and it would be in exactly the same way as it is resolved in the Kuhnian model, i.e. by eliminating the opposition. *This is precisely what Burrell and Morgan warn against, and why paradigm incommensurability needs to be defended.* The one major difference in this respect between Kuhnian and Burrell and Morgan's paradigms is that in Kuhn the elimination of the weak is justified in terms of the better performance of the revolutionary paradigm. In Burrell and Morgan's model this justification cannot be made — there are no *a priori* grounds for deciding which paradigm has the better problem-solving ability and thus the right to supercede the other paradigms.

It has not been the intention here to claim that the two models are the

same after all. Although they possess similarities at the theoretical level, they are informed by different metatheories, specifically, in regard to what constitutes science. Hence our previous suggestion that Kuhnian paradigms can, to some extent, be contained within Burrell and Morgan's Functionalist paradigm. We have argued that the conceptualization of incommensurability in both texts is compatible, but the disparate qualities between the two models remain such that the attempt to resolve incommensurability in the Burrell and Morgan model on the basis of its isomorphism with the Kuhnian model must be bound to fail. Further, as previously noted, whereas in the Kuhnian model normal and revolutionary science both share the same goal — in effect, finding the best way to solve a common problem, in Burrell and Morgan's paradigms no such commonality of goal exists — the four paradigms represent four mutually exclusive and contradictory goals. Any attempt to synthesize paradigms must also constitute an attempt to synthesize goals. How might this be done? One obvious answer is that the paradigm with the most power — the goal which is most powerful — is prioritized over the others: power determines outcomes. However, scientists are reluctant to accept this view of how knowledge is created, and the preferred method would be that of pluralist consensus. The problem with this approach is the question of whose views should be considered as legitimate. In short, in both cases the question of what constitutes science is determined by an elite group called scientists. Two recent major works on organization studies have addressed this problem, though from widely differing intellectual positions — Donaldson's *In Defence of Organization Theory* (1985) and Reed's *Redirections in Organizational Analysis* (1985). Yet, although the two works set off in totally different directions to each other, it can be argued they end up at precisely the same position.

Paradigm Incommensurability Under Attack

For Donaldson, organization studies consists of those aspects of organizational life which it is 'proper' to study, in contrast to the totality of organizational existence/practice. He makes a strong distinction between what he sees as the province of organization theory and that which is, in his view, more properly the domain of 'the general sociologist, the political sociologist or the political scientist' (Donaldson 1985: 120), and argues that organization studies should not be conflated, or absorbed into, 'more general or more macroscopic-societal schema' (1985: 121) — Donaldson sees organizations as discrete bounded entities which must be studied as such. Reed, on the other hand, makes exactly the opposite claim for organization studies, seeing organizations as fundamentally involved in their social milieu and organizational analysis as 'a constituent element of that general body of socio-political theory which has been developed in Western society since the time of Plato. . . . (O)rganisation theory is part and parcel of an evolving grammar and vocabulary aimed at improving

our understanding of socio-political practices and the characteristic human dilemmas which they embody' (Reed 1985: 210).

Taking Donaldson's case first, and having established what he conceives to be the appropriate area of study for organization studies, it is pertinent to examine what he sees as an appropriate way of doing this work. Donaldson is strongly committed to a social science modelled on the natural sciences, and argues that the contention that organization studies depend on social theory is no more than a myth (1985: 74). He argues further that there is a language peculiar to science which is distinct and distinguishable from ordinary language. The language of science is that of hard data and 'scientific data are those on which there is a high degree of intersubjective agreement between trained scientists' (1985: 76). He goes on to say that 'scientific knowledge of social matters . . . will be superior to lay beliefs in that the former will better meet the criteria of validity according to the epistemological canons of science'. In other words, the epistemology of science determines what is valid data, and the epistemology of science is determined by people called scientists. Presumably, should someone who is called a scientist not agree with the established canons of science, such a person would be, *ipso facto*, a non-scientist. Such an argument creates havoc for Kuhn's theory of knowledge production, which Donaldson so ardently supports. In terms of Donaldson's argument, revolutionary science simply could not happen because it would be, inherently, non-scientific. Equally, anyone who is not an organization scientist as per Donaldson's definition is apparently excluded from making significant contribution to the debate — thus Donaldson's attack on Burrell and Morgan's concept of paradigm incommensurability is finally to claim that the paradigms themselves are simply irrelevant (for a particularly apposite example of paradigm incommensurability, see the *Organisation Studies* Symposium 'Offence and Defence' (1988), occasioned by the publication of Donaldson's book).

Reed, with his different model of organization studies, apparently has no wish to exclude any competent person from the debate. Whereas Donaldson was seeking to reduce variety in scientific debate by calling for a restitution of orthodox scientism, Reed seems to seek to widen the debate with his call for a pluralistic approach to the production of knowledge. Whereas Donaldson seeks to remove the rationale for paradigms by denying their relevance, Reed's more subtle approach is to seek to dissolve the need for paradigm boundaries. First, although successfully identifying the differences between Kuhn's version of paradigms and that of Burrell and Morgan, Reed then goes on to criticize the latter for misuse of the former's theory — he accuses Burrell and Morgan of basing their paradigms on Kuhn and then uses Kuhn's (different) concept of paradigm incommensurability to criticize Burrell and Morgan's concept of paradigm incommensurability (Reed 1985: 188). Since, as has already been suggested, the two concepts of incommensurability are significantly different, there seems to be little to be gained from criticizing them for being different, and it must be bordering on the fallacious then to claim

that one is wrong because it is based on a misconception of the other.

However, Reed's attack on paradigm incommensurability does not end with Kuhn. He further asserts that what he calls the paradigm mentality 'grossly overstates the philosophical and ideological incompatibilities between different conceptual frameworks', as well as encouraging isolationism which 'severely circumscribes the potential for creative theoretical development' (Reed 1985: 205). He goes on:-

'In addition, this mentality also generates a relatively static and myopic vision of organization theory's historical development in which the subtle and complex interaction between intellectual change and social context tends to be ignored in favour of a single-minded concentration on internal factors. Finally, in denying the possibility of rational comparison and assessment across paradigm boundaries this mode of thinking facilitates an extreme form of cognitive relativism in which intellectual production and evaluation are seen to be subordinate to ideologically mediated interests that contend in a wider social arena.' (1985: 205)

In order to bypass the problems of the 'revelatory imagery of imperialism and the relativistic leanings of isolationism' (1985: 201), which, Reed claims, attenuate the development of organization theory, he proposes a different model of knowledge production, which he calls a pluralist strategy and which is based upon the notion of practical discourse, drawing specifically on the work of Bernstein (1983). However, Reed's appeal to pluralism does not provide an antidote to the incorporation of non-orthodox science by the orthodoxy once the concept of paradigm incommensurability has been abandoned. (It should be noted that Morgan has latterly espoused a similar argument, using the Rortyian concept of 'reflective conversation' and encountering similar problems [see Morgan 1983; Willmott and Jackson 1985; Jackson and Willmott 1987].) Unfortunately, Reed is not particularly explicit about his conceptualization of pluralism, although it is undeniably a highly contentious concept (see e.g., Wolff 1968; Playford 1971; Carter and Jackson 1987). The main argument against pluralism *per se* can be very briefly stated in the point that there is, under pluralism, no mechanism for judging between contradictory claims, either in terms of their moral implications or even in terms of their status as knowledge. Within the precepts of pluralism, outcomes are simply determined by the consent of the majority — which, under present conditions in organization theory, would be the Functionalist orthodoxy, generating effectively the same results as Donaldson's strategy. But, according to Reed, 'the primary mission' of the pluralist strategy is 'to steer a middle course between structural determinism and cognitive relativism' (1985: 204). Yet, within the precepts of pluralism, such a goal cannot be determined *a priori*, since outcomes must be consequent upon the degree of support for any given position. Equally, if the science of the 'middle ground' is to become dominant, then, arguably, this would constitute a paradigm in its own right, subject to the same characteristics as any other paradigm, and representing yet another form of domination by a particular interest group. But, in any case, on the one

hand there is no basis for assuming that the middle ground has more truth value than the extremes, and on the other, there is no basis for assuming that, in any group of people, the consensus will lie in the middle ground.

How this middle course is to be maintained against the attractions, for example, of determinism, Reed does not explain. Reed's analysis — and indeed that of pluralism generally — ignores the issue of power, presupposing equality, not only of participation between these groups, but also within the groups themselves. This assumption is very difficult to substantiate. Very few members of organizations would suggest that power is equally distributed within these organizations — equality of power might even be a fundamental contradiction of the concept of organization. Within what Reed terms the intellectual community it is equally clear that some members have considerably more power than others, and, indeed, in a way this is presumably the very thing that Reed is trying to circumvent. For, further, Reed sees his strategy as being anti-Functionalist (and, later, as also excluding extreme relativists and extreme objectivists [1985: 204]) — in other words, he seems to want to exclude, from the very start, a particular group of organization theorists who might reasonably claim to be members of the intellectual community (and/or technicians, and/or members of organizations [see later]). This process of exclusion is fundamentally the exercise of power. Is it possible to set up a process of knowledge production based on equality of power, as is implicit in pluralism, which apparently establishes itself by the exercise of power against a particular group? If the middle ground is to be held through strategies of exclusion, this underlines the potential for a return to conditions of domination.

Certainly, one potentially liberating feature of Reed's argument seems to be the intention to extend the arena of debate to include 'technicians' and 'members of organizations' (see e.g., 1985: 202), as well as 'the intellectual community', though clearly, as these are not discrete and mutually exclusive categories, there are some logical problems entailed in this proposition. But, from Reed's comments, it seems reasonable to conclude that — again, as is the case with Donaldson — the primary producers of knowledge remain this 'intellectual community' of 'organization theorists'. Who are the members of the intellectual community? In the absence of further distinction, we must conclude that it is identical with the already existing group — i.e., a self-validating professional elite who themselves decide upon criteria of access to the profession and, more importantly, who control that access. Given, also, Reed's commitment to a 'cumulative growth in knowledge' (1985: 201), it is difficult to escape the conclusion that, though implicit rather than explicit, in this model too, science is what scientists do.

Reed supports his arguments regarding the potential of the pluralist strategy by reference to the work of Bernstein (1983). Although, in the first instance, Bernstein might be seen as offering a degree of support to Reed's position, on closer examination this is thrown into doubt, and the

appeal to Bernstein in this context seems rather disingenuous. As he notes, Bernstein is talking about post-empiricist philosophy and his particular concern is with the hermeneutic approach, exemplified in the work of Gadamer, Habermas, Rorty and Arendt (the appropriation of whose work as organization theory by Reed seems somewhat idiosyncratic [see Reed 1985: 201–202]). Bernstein's arguments are rather more complex than Reed's usage would suggest — and he is certainly not making the classic arguments for pluralism *per se*. In the philosophico-hermeneutic tradition the intellectuals, whilst key actors, do *not* occupy the role of expert, with all the attendant power of such a role. Genuine discourse in this tradition can only be achieved in the absence of conditions of domination, which clearly do not pertain in contemporary society, *and only when participants in the discourse have a commitment to it as a means to achieving the goal of emancipation can the discourse be fruitful* (Bernstein 1983: *passim*). The quasi-ordinary language of Reed's model, rather than being an example of Bernstein's thesis, should be clearly distinguished from the emancipatory discourse which Bernstein champions.

Finally, Reed appears to be unaware of the conditions which make paradigms incommensurable, i.e. language. He claims that the 'paradigm mentality' puts an end to dispute and debate, yet this is clearly not the case. On the contrary, distinct paradigms should encourage debate, and, should that debate bring about resolution of differences, so much the better, *provided that such resolution is not achieved through the exercise of power*. But, more importantly, the major impact of paradigm incommensurability is that no paradigm should be compelled to justify itself *in terms of any other paradigm*. As each paradigm is sustained by a different language, defence in terms of another paradigm means adopting the language of that paradigm, which is the route to paradigm domination. All that the paradigm model demands is that each paradigm should be allowed to develop without the constraints of defence in terms of other paradigms. Furthermore, Reed's attempt to resolve the issue of paradigm incommensurability by emphasis on intercommunication between different perspectives founders on his explicit stress that the development of the pluralist strategy takes place on the middle ground between the extremes of relativism and objectivism — this makes science the prerogative of the scientist of the middle ground, which then, as noted, itself becomes a dominant paradigm.

It has indeed been a popular interpretation of the concept of paradigm incommensurability that it signifies the prohibition of communication between paradigms. Yet common sense indicates that there *is* communication between different paradigms. The problem is with a conflation between senses of communication. What inter-paradigmatic communication tends towards is not an inter-paradigmatic dialectical synthesis of ideas but to an intra-paradigmatic development where the essential incommensurability is maintained. (Sometimes, as a result of inter-paradigmatic communication, there is a migration from one paradigm to

another, but this is a personal paradigm shift — an individual moves paradigm but the paradigms themselves remain intact.) Indeed, we would argue that inter-paradigmatic debate is not just inevitable but also essential. The ‘inhabitants’ of a paradigm do not develop in cognitive isolation from all other activity in a discipline. An awareness of what is going on in other paradigms is a fundamental characteristic of intra-paradigmatic development. What is at issue is the difference between inter-paradigmatic debate leading to intra-paradigmatic synergy, and that which leads to inter-paradigmatic assimilation. Competing paradigms constitute for each other an Otherness which gives each paradigm its specific identity. Loss of paradigmatic differentiation constitutes loss of identity. *It is identity which is the source of the agonistic energy which is the proper effect of inter-paradigmatic debate, rather than the pseudo-harmony of the unitarist authoritarian view of science.*

The Implications of Abandoning Paradigm Incommensurability

What we have attempted to show so far is that attempts to abandon paradigm incommensurability lead, apparently inexorably, towards epistemological authoritarianism. Both Donaldson and Reed imply that their own strategies are necessary to increase correspondence with ‘the real world’, and this is to be achieved by casting scientists of their own persuasion as the only legitimate interpreters of this world. (Ironically, it is arguable that, because of their various definitions of what constitutes legitimate interpretation, each would probably exclude the other as competent to contribute.) Both authors end up by saying that science is what scientists agree it is. But, because of this argument, what actually happens is that both knowledge and its process of production move away from any *necessary* relation with something which might be called ‘external reality’.

We see the urge to deny paradigm incommensurability and to restore authority in science as being exemplary of what has been called the postmodernism of reaction (Foster 1985). Briefly, the term postmodernism is used to describe the response to the perceived failures of the ‘preceding’ modern period, and has two basic dichotomous forms which can be seen as both critique and extension of modernism. The postmodernism of resistance is identified, generally, with the critical tradition in social theory, and the postmodernism of reaction with the conservative tendency. The former is seen as a liberating tendency, seeking to transcend the profoundly amoral nature of modernism and its prioritization of form over content, using the insights of the (post)structuralist understanding of the immanence of power in language which the excesses of modernist development have stimulated. The postmodernism of resistance has fundamentally questioned the basis of rationality, emphasizing the ineluctable individuality of cognition. However, it has much in common with critical modernism, which seeks to retain a rational

basis for action, albeit one rooted in the philosophical humanistic, rather than the scientific, mode (see Cooper and Burrell 1988). The pernicious postmodernism of reaction seeks, on the other hand, to resolve the proliferation of variety and uncertainty in the modern age by a return to a golden age of simple authoritarian structures. This is an illusory quest for foetal certainty which clearly divides the world into those who are empowered to speak and those who are not.

The contribution that the paradigmatic model made was to provide a legitimate channel of communication and development for belief systems which do not accord with the dominant ones. The period of intellectual liberation of the 1960s highlighted recognition that the ontological and epistemological controversies depicted by the paradigms could not be resolved through the scientific mode — it raised, for example, issues of oppression that had not been previously recognized as legitimate and which could not be resolved under the existing hegemony. Although this paper is primarily focused on the arena of organization theory, it is clear that all aspects of science are profoundly affected by the socio-political context in which they have existence, and the paradigmatic model both represents and is a representation of this. It was, for example, in this period that general theories, not originating in organization theory but perceived as relevant to organizational existence, were brought into the arena of organization theory, e.g., the work of Freud, Satre, Castenada, Laing, etc. Thus the paradigmatic model can be seen as a way of regularizing the non-orthodox interests to allow them to develop unimpeded by the weight of justification solely in reference to the dominant framework. The liberalist gains of the modern period are now enveloped in an authoritarian reactive socio-political culture, and this is, of course, mirrored in developments in the world of science. The attempts to refute paradigm incommensurability can be seen in this light.

Obviously, as proponents of the paradigmatic perspective we take as given that we represent a particular paradigmatic position. Central to this position is the understanding of language as problematic, both as a medium of communication and as a constructor of reality. This conceptualization of language has long-term standing in European thought but has recently seen rapid further development in the areas of Poststructuralism and Critical Theory. This development has had general significance and application but has also penetrated the realm of organization theory (e.g., Cooper 1986; Jackson and Willmott 1987; Alvesson 1987; Carter 1987; Jackson 1987). We have already suggested that the denial of paradigm incommensurability is commensurate with seeing language as unproblematic, but if the post-Saussurian interpretation of language is accepted, such an assumption becomes untenable. This interpretation is one which recognizes the prioritization of meanings as attended by power. Habermas (1978), from the critical rationalist point of view, has discussed the concept of systematically distorted communication, where 'real' meanings are presently in the service of power, but argues that the

potential exists to establish meanings which are consensually arrived at in the absence of power. Indeed, this should be the primary aim of 'emancipatory' science, but is one which, as yet, remains unprivileged and which will continue so as long as there are those who exercise arbitrary power. Habermasian consensus is, however, profoundly different to the consensus of the pluralist — the first depends on the absence of power, the second on the disguise of the presence of power (Carter and Jackson 1987). Those of the postmodernist persuasion, on the other hand, see the relativity of language as irreducible and unregenerate. But, from either point of view, the present problematic condition of language is incontestable.

In Lacanian terms, language is a primary constructor of selfhood and, as such, language meanings in use are contaminated by their immediate past history as interpreted by the subject. If this understanding of language is accepted, its perception as a disinterested medium of description of and correspondence to 'true states' is revealed as simply an attempt to prioritize particular interests among other interests. In Lacan's (1980: 86) phrase, 'the function of language is not to inform but to evoke', and is thus inappropriate for 'objective' description. Even acceptance of this, however, has not prevented theorists from hypothesizing the existence of a putatively objective language, a language free from the contamination of individual cognition. But Barthes, for example, shows language as inescapably figurative and, as such, instrumental in its evocation — 'language is never innocent' (1967: 22). All this implies the problem with the concept of objectivity. Lyotard (1977: 96) is much more specific: 'Language is not meant for telling the truth'.

If all, or any, of this is to be credited, where does it leave science? Essentially, it leaves it as 'just' another Foucauldian discourse. Foucault's eternal triangle of power, knowledge and discourse, whereby it is established what is proper to a discourse in terms of what can be said and who can say it, is highly reminiscent of attempts to deny paradigm incommensurability in favour of a central authoritative science, either science by prescription or science by consensus. In the model of discourse which is the subject of Foucault's critique, a discourse is maintained primarily through immanent processes of exclusion (Foucault 1971), a notion well-represented in the cited, and other, examples of attempts to refute paradigm incommensurability — the chastity belt approach to science. Foucault's argument is that, on the contrary, we should be opening up the discourse, accepting as valid anything which supplements our understanding of the world, that this should be our genuine aim and that the processes of exclusion operate, not to the benefit of the production of knowledge, *but to that of the producers*, sustaining their power and authority at the potential expense of knowledge itself.

Ironically, one of the much vaunted strengths of science is debate and dispute — both Donaldson and Reed, for example, emphasize this. But theirs is an essentially parliamentary model of debate, where outcomes are a function of numerical majority, not of truth content. If debate is to

be meaningful it is essential that there are meaningfully different positions engaged in the debate. Lyotard's (1984) agonistic model stresses not merely the virtue of the struggle but the fundamental need for contention in order to give the produced knowledge its dynamic vector. Recalling our previous emphasis on the argument that paradigm incommensurability does not, and is not intended to, preclude communication, as opposed to domination, the attempted refutation of paradigm incommensurability thus denies the impetus which gives the fuel to the debate, by denying the fundamental difference which gives the energy to the advancement of knowledge. There is a fear of heterogeneity, and comfort sought in homogeneity — the interminable 'debate' within the dominant discourse is focused on internal wrangling which seeks to deal with local dissent whilst denying validity to dissent from outside the 'family'. Though obviously contentious, it might be argued that this debate amongst the orthodox in organization theory has manifested its sterility in the imputed failure to produce either any significant new perspective, or any recognizably universal statements based on existing perspectives, about its subject. A classic example is to be found in the area of motivation to work, which has produced nothing new since the 1960s but which has been forced to recognize that everything produced up to and including that time has failed to explain the phenomenon. The recognition of paradigm incommensurability provides the best defence for radical perspectives against the encroachments of the orthodoxy, and offers the best conceivable stimulus to genuine agonistic debate.

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