



HOT on the Discursive Limits of Organization Theory*

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review of: H. Tsoukas and C. Knudsen (eds.) (2003) *The Oxford Handbook of Organization Theory*. Oxford University Press. (HB: pp.664, £70, ISBN 0199258325)

Never give an academic a bad review, they'll never forget it. (Ingrid Black, *The Dead*)

Introduction

The Oxford Handbook of Organization Theory (HOT, for short) is the focus of this review. On initial inspection the material form of this text with its hardcover, embossed with gold lettering, communicating to the potential reader that this is *The* handbook of organization theory seems to testify to the importance of the material contained in the five thematic sections of this handbook. These cover: organization theory as science, the construction of organization theory, meta-theoretical controversies in organization theory, organization theory as a policy science and, finally, the future of organization theory. The scope of the material contained in this handbook draws from a variety of paradigmatic positions and charts issues such as the paradigm debate in organization studies, the production of actionable knowledge and the development of institutional forms that can effectively tackle global problems such as famine. Of course, there is a great deal of ground between these debates, but even this preview indicates the breadth of the material contained in the handbook and for which the editors, along with their contributors, must be congratulated for bringing together.

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Rather than adhere to the conventional narrative format associated with a book review, my goal is to bracket a large portion of these chapters and consider a very limited, but provocative sample. This will not be a point-by-point discussion of the contents of the papers, but rather, a selective and tentative excavation of points of interest that serve to problematise the function of texts such as this handbook. In structuring my review there are a number of issues that I want to interweave throughout and these coagulate around the consumption of theory, amnesia and the reproduction of the organization studies canon within the HOT. In conclusion I want to offer some tentative thoughts on the implication of these factors for the transgression, or not, of discursive boundaries in organization studies.

I will exert pressure on the assertion made by the editors that: “This handbook provides a forum for leading scholars in Organization Theory (OT) to engage in meta-theoretical reflection on the historical development, present state, and future prospects of OT...Notice that this is a *meta-theoretical* question: the object of analysis an debate in this volume...is OT itself.” (Tsoukas and Knudsen, HOT: 1). In doing so, I hope to offer an affirmation of the present status of organization theory by drawing attention to the limits and transgressive potential of selected chapters within the HOT.

As the volume prides itself on containing “meta-theoretical reflection on the epistemological status of OT” (Tsoukas and Knudsen, HOT: 3) I intend to utilize examples drawn from the history and philosophy of science and disciplines such as marketing and sociology to illustrate the argument that elements of the HOT are epistemologically fragile and thereby undermine the status of this text as a form of “meta-theoretical reflection on the status of OT by taking stock, on the one hand, of related debates in the past and, on the other hand, of new developments in OT and in the philosophy, sociology, and history of science” (Tsoukas and Knudsen, HOT: 3). My approach will be geared towards problematisation of the HOT and will, unavoidably, be partial in its analytic coverage. This is derived, in the most part, from my identity as a critical marketing researcher and the limited exposure I have had to the literature referred to in the HOT. This said, I start from the assumption that the HOT is produced, like other text, through the inclusion and exclusion of material, with the published textual construction achieved through the “violence of the body [which] reaches the written page only through absence...from which a presence has since been washed away, and through a murmur that lets us hear...that unknown immensity that seduces and menaces our knowledge” (de Certeau, 1988: 3).

It Begins...

In *The Structure of Scientific Revolutions* (1962, 1970a, 1996), Kuhn provides an interesting examination of scientific inquiry and the role that the textbook plays in the perpetuation of specific forms of scientific discourse and practice via the use of the, now notorious, concept of paradigm (Masterman, 1970; cf. Kuhn, 2000: 299-300). In his treatment of this concept Kuhn stresses that paradigm refers to the beliefs, norms

and values shared by members of a research ‘speciality’.¹ What is important for the present discussion is the role of the textbook (and academic paper) in the transmission of appropriate standards of scientific education to those who wish to be initiated into a scientific community (Kuhn, 1996: 178).

The astute reader has already noticed that the status of the discussion has switched from being about the function of *handbooks* to that of *textbooks*. This is a deliberate move and one made following the remonstrance by Jones (2003) that theory from various disciplines has a tendency to be invoked in a pedestrian fashion, often “imitative of the past rather than radically different” (Jones, 2003: 516). In equal measure it is necessary to avoid the use of theorists in ways that are “downright strange” (Letiche, 2004: 79). While not unproblematic, Kuhn’s discussion of the textbook as one medium for the transmission of the “rules and standards for scientific practice” (Kuhn, 1996: 11) provides a useful tool for prising open the HOT, particularly in relation to the construction of the identity and boundaries of organization theory. Throughout *The Structure of Scientific Revolutions* Kuhn argues that scientific education is taught via textbooks used as “pedagogic vehicles for the perpetuation of normal science [which]...have to be rewritten in whole or part whenever the language, problem-structure, or standards of normal science change” (Kuhn, 1996: 137). These changes occur within and adhere to the dictates of the dominant social or moral formations and the presuppositions held by scientists regarding epistemology, ontology, and so on, which direct their own academic work and the legitimising function scientists themselves play in evaluating the output of others.

Kuhn (1996: 167) links the rewriting of textbooks with the activities of the Ministry of Truth in Orwell’s *1984* and the Orwellian notion that whoever controls the present controls the past and those who control the past control the future. History represents “a palimpsest, scraped clean and reinscribed exactly as often as was necessary...Every record has been destroyed or falsified, every book has been rewritten, every statute and street building has been renamed, every date has been altered...History has stopped. Nothing else exists except an endless present” (Orwell, 1964: 128). Naturally it is a cause for concern that the Ministry of Truth rewrites history in order to suit present requirements. More relevant for the present discussion is that palimpsests retain traces of the truth, since they are comprised of the “myriad of events...they were formed” (Foucault, 1984: 81). Perhaps such revisionism is visible outside of fiction as well.

In a different context, Parker has written of the historical elision and amnesia endemic within the organizational culture and symbolism literature – “I do think that there was an increasing ignorance or marginalisation of much of this writing” (Parker, 2000: 139) and “It seems evident that much earlier work is being written out of the canon” (Parker, 2002: 46). Parker is not optimistic and believes that “this is a revisionist project likely to continue” (*ibid.*) because “where disciplines are concerned, stories about the past allow for legitimacy to be demonstrated through deciding what counts as proper – a tactic which both constructs and reinforces the discipline of discipline” (*ibid.*: 47). The production of

1 This definition is problematic but spatial limitations preclude any extended discussion of the paradigm concept. Needless to say, Kuhn does specify the ‘right’ inflection of this concept (see Kuhn, 2000).

textbooks and the inclusion and exclusion of content is similarly dictated by disciplinary, political, economic and cultural factors established via academic debate and “published within the political and economic constraints of markets, resources and power” (Apple and Christian-Smith, 1991: 2). It is important to acknowledge that a text whose marketing blurb professes to represent “a major new initiative in publishing...Each volume offer[ing] an authoritative and state-of-the-art survey of current thinking and research in a subject area” (HOT: back inside jacket), is similarly constructed via processes of inclusion and exclusion and consequently this review should focus on the material contained within the text itself and also what is excluded. This is because absence, along with making the heart grow fonder, “can really signify more profound political, economic and cultural relations and histories” (Apple and Christian-Smith, 1991: 3). As Thompson recognises, “a given piece of research reveals as much about a research community as it does about the phenomena under study” (2002: 142).

Absence is made manifest within the HOT via the assertion made by Tsoukas and Knudsen that this volume contains the ‘leading’ scholars in organization studies. Being aware that the “long march from research fellowship to full chair is a nightmare for many, containing the pitfalls of academic politics and fear of offending the powerful” (Burrell, HOT: 532) it is worth tempering the criticism I intend to make here. Nonetheless, it is very strange that a book that professes to contain leading minds, fails to acknowledge that the cognitive input into this text is all derived from a particular generation (if I am permitted to generalise a little) and that this selection is, possibly, emblematic of “the paraphernalia of power” (Burrell, HOT: 523) within the discursive structure of organization studies than it is of the extent to which the included authors are the leading minds of the academic community. Indeed, in view of the explicit meta-theoretical reflection that this title desires to stimulate, an obvious question to be posed to the editors is why they neglected to draw from younger members of the academic fraternity? Is it not the case that academic controversy is usually initiated by someone “new to the field” (Kuhn, 1996: 166). Maybe this is true, but I prefer to regard the young scientist thesis as an immodest reflection of Kuhn’s perception of his own self-importance at the time of writing *The Structure of Scientific Revolutions* (e.g. Kuhn, 2000: 255-323).

There are, however, numerous younger members of the organization studies community whose commentary frequently graces the pages of august organization journals and who could have been worthy contributors to this text, but who remain strangely in the shadows. Perhaps, I am being unfair to the editors, who may have contacted such people. But it would be interesting to see what impact the inclusion of such voices would have “had on the inventiveness, quality, and impact of the material published. Would it improve?” (Burrell, HOT: 533). These are issues that concern Gibson Burrell and who, in his chapter, explicitly acknowledges the inflection of power throughout the academy and the techniques of dressage and constraint that envelop the discipline. Stressing gerontological control of the ‘organs of publication’ Burrell proposes that “it is feasible to see editorial teams of Ph.D.s editing leading journals (on at least an occasional basis, if you so wish)” (HOT: 533). I, for one, look forward to seeing this policy implemented at *Organization*.

This said I appreciate that this text attempts to provide a summation of the current status of organization studies and will contain chapters that recycle previous research. Even so, a number of the chapters are minor extensions of previously published papers, with McKinley and Mone's chapter the primary example (as I shall discuss below). This makes the act of publication in such an expensive binding seemingly redundant, as those interested can obtain a selection of similar journal articles for a fraction of the price. In addition, this type of discursive re-jigging raises the question of how accurately this text can be described as representing the current state-of-the-art, as the marketing of this product would have us believe, rather than an exercise in the conservatism of intellectual capital among the contributors.

This phenomenon is not wholly unexpected if we share Fuller's view that the professional consciousness of the knowledge worker remains captive to an industrial mindset where "an expression as innocuous-sounding as 'knowledge production'...freely tumbles from academic lips, immediately calls to mind the manufacture of products -specifically books and articles- the more, the faster, the better" (Fuller, 2000: 82). The quest for uniqueness and career advancement necessitates that

Each of us in our careers invests considerable effort, energy, and work in building up a store of intellectual capital from which we hope to get good returns. These are the forms of investment that we make as scholars, and as investors we like to see these investments earn a good return. (Clegg in Boje, 1993: 192).

The desire to maximise the return from these investments is manifested in the offerings of a number of HOT contributors. For example, Joanne Martin (HOT: 392) acknowledges that her "chapter is a revised version of chapter two, in J. Martin, *Organizational Culture: Mapping the Terrain*". Willmott (HOT: 88) draws upon two previous publications and likewise, Clegg (HOT: 536) declares that "Elements of this argument were developed from earlier joint work". In a similar vein, McKinley and Mone's chapter traverses familiar territory revisiting the issues of construct ambiguity, incommensurability and their ideal of a consensually organized dictionary for organization studies. 'Revised edition' are two words equally applicable to Gibson Burrell who raids his back catalogue for content, with the themes he indicates as prospects and limitations for organization theory remarkably similar to a recent *Organization* editorial (2003) and bearing the hallmarks of his early work on radical organization theory. I am sure that there are numerous arguments that can be made in defence of this policy, "especially if it reaps reasonable benefits for those pursuing it" (Fuller, 2000: 79) and the editors have dutifully provided a sample of these. Then again, what this signals about the intellectual craftsmanship of 'leading scholars in Organization Theory', I have no idea (Linstead, 2002).

Consuming Meta-theory

A turn to Foucault in organization studies and other disciplines has encouraged greater interest in understanding the production, maintenance and ordering of particular discourses and how knowledge claims are legitimated. It is in this spirit that the editors of the HOT see themselves and their contributors playing a role – "We keep learning by

keeping conversing, and reflecting upon how we do so. The present handbook is a modest contribution to the on-going meta-theoretical conversation in our field” (Tsoukas and Knudsen, HQT: 31). Demarcating the contribution of their volume from the extant literature in organization studies, they position the HQT as removed from early meta-theoretical analyses which sought to typologize the field of organization theory (e.g. Burrell and Morgan, 1979). Tsoukas and Knudsen propose an alternative meta-theoretical strategy that warrants application and entails the philosophical, historical, and sociological examination of “key categories implicated in organizational research” (Tsoukas and Knudsen, HQT: 2). Given this, the strategy for the invited contributors was to “engage in meta-theoretical reflection on the epistemological status of OT by taking stock, on the one hand, of related debates in the past and, on the other, of new developments in OT and the philosophy, sociology and history of science” (Tsoukas and Knudsen, HQT: 3). Maybe I shouldn’t dwell on the implicit view of theory development in organization studies that the editors hold, i.e. that those likely to read this text are so lax in their research practices that they have neglect to survey the interdisciplinary literature which may have some bearing upon their work. This is something that I initially found hard to believe, although, as I discuss below, the editors may have a valid point but possibly not in the way they were expecting.

In view of their espoused willingness to encourage an engagement with new developments in alternative disciplinary arenas I expected that the editors, in their co-authored chapter, would provide a detailed description of meta-theorization, drawing from available sources and outlining the benefits and drawbacks associated with such a pursuit. Alas, such description is not forthcoming, and in the interests of clarity it is appropriate that I briefly examine the relevant literature.

Practicing meta-theory involves extensive scrutiny of the presuppositions that underpin research (Morrow, 1994: 46). A meta-theoretical question being, as Calás and Smircich (1999) have pointed out: How can we think about and do theory differently? Drawing a parallel between meta-theorising and reflexivity, the editors of the HQT recommend that this involves “taking a step back from ordinary theoretical activity to reflect on what the latter should be aiming at and how it ought to be conducted – it is for this reason that such reflection is called ‘meta-theoretical’” (Tsoukas and Knudsen, HQT: 5; emphasis in original). Meta-theoretical reflection is undertaken “not to generate theory about particular organizational topics but to make the generation of theory itself an object” (Tsoukas and Knudsen, HQT: 5). The ontological, epistemological, methodological and view of human nature presupposed in any given social-scientific theory are delineated by the meta-theorist to ensure that any inconsistent assumptions which underpin organization analysis are avoided (Morrow, 1994: 47). Alongside this ameliorative role Burrell and Morgan (1979: ix, 398) have proposed that meta-theoretical awareness encourages the *negotiation* of social theory and diverse frames of reference in order to question paradigmatic orthodoxy and provide legitimacy for alternative forms of organizational inquiry.

No doubt, ‘publish or perish’ pressures can militate against meta-theoretical review², but the pressures the editors must have been under for them to ignore the extant meta-theorization literature must have been tremendous (e.g. Lewis and Grimes, 1999). This omission is unsatisfactory since their chapter is positioned as an introduction and overview of meta-theoretical reflection in organization studies. In addition, if the definition of meta-theorization provided by Tsoukas and Knudsen is recalled, then what is most disappointing is that the editors have studiously avoided citing and it can be supposed, neglected to examine meta-theoretical work produced by, for example, a sociologist such as George Ritzer whose meta-theoretical works include *Metatheorizing in Sociology* (1991) and *Explorations in Social Theory: From Metatheorizing to Rationalization* (2001).

In *Explorations in Social Theory: From Metatheorizing to Rationalization* (2001), Ritzer provides an extensive, critical review of extant meta-theoretical literature, drawing primarily, but not exclusively, from sociology. Defining meta-theorizing in a similar fashion to Morrow (1994), Ritzer asserts that meta-theorizing only takes place after initial theory development and proposes four broad types of meta-theorizing. The first, ‘Mu’, involves the study of extant theory. This immersion is used by the meta-theorist to develop “a more profound understanding of extant theory” (2001: 18). By ‘profound understanding’, Ritzer means the examination of similarities, differences, strengths and limitations that affect different theories and theorists. This, Ritzer proposes, is beneficial as the researcher gains a solid understanding of the theories or theorists involved. The second type of meta-theorizing, Ritzer explicitly positions as a prelude to theory development ‘Mp’. Extant theory is examined in order to produce new theory. The third type holds that meta-theorizing should be conceived as a source of overarching theoretical perspectives ‘Mo’ in order to produce a meta-theory that overarches some aspect of, for example, organization theory (e.g. Tsoukas, 1994). Finally Ritzer identifies ‘Om’, as overarching meta-theory. This he distinguishes from ‘Mo’ since meta-theorization takes place *prior* to initial theory development. Where ‘Mo’ is derived from the extant theory, ‘Om’ is imposed on the theory and does not involve the study of theory, but instead is a set of principles imposed prior to and presupposed by, the theorist or discipline (Ritzer, 2001: 15).

On my reading, the practice of meta-theorization has much to recommend it and I am greatly attracted to the idea(l) of encouraging people to immerse themselves in a broad spectrum of literature. This is not to suggest that the various meta-theoretical strategies enunciated and practiced by Ritzer are unproblematic, far from it. Ritzer’s argument that meta-theorists can effectively scrutinise theory because “they identify with no single theory...are able to distance themselves from all of them and render more impartial analyses of them” (1991: 302) is frankly implausible. Involvement with theory is never disinterested since it constitutes a form of political involvement in the discursive landscape. As a form of participation, the theorist cannot dispassionately preside outside of the field in which they seek to intervene, “so its failure to achieve the neutrality of fully knowing, fully objective gaze is one of its essential characteristics” (Davis, 2004: 4). Equally Ritzer’s desire for theoretical synthesis is improbable given the frequent

2 These will, no doubt, be somewhat tempered by recent calls in the *Academy of Management Review* for authors to challenge conventional wisdom via a ‘return to theory’ (Mannix, 2003: 543).

assertion that organization studies is characterised by “instability, fragmentation and heterogeneity” (Hassard and Kelemen, 2002: 333) and by the same token, I, like Ritzer, find ‘Om’ meta-theorization akin to wearing cognitive blinkers. This said, Ritzer’s contribution to the meta-theoretical debate is valuable, particularly his discussion of the role that poststructural and postmodern perspectives can play in the meta-theoretical destabilization and refiguring of a discursive formation and the methodological guidelines he provides for those intent on pursuing this strategy. At the same time, Ritzer does caution those interested in the production of meta-theory to be aware of the violence that the pruning and ordering of a field can cause, although such textual victimization is undoubtedly an inescapable element of academic life. It is the extent that victimization is performed that is, to some extent, within the control of the individual (Case, 2004).

Given the breadth and insight available in Ritzer’s contribution to meta-theoretical debates it is strange that the HOT editors chose to disregard this literature. This failure to acknowledge Ritzer raises questions about the violence performed in the name of symbolic boundary maintenance. Perhaps the ignorance of prior literature and re-packaging of ideas in this case “serves to demonstrate intellectual progress” (Parker, 2000: 139) and functions to define the nature of organization studies as distinct from progenitors such as sociology: “it is precisely by neglecting its prehistory that organization studies can come into being, as a contemporary management discipline which is *not* organizational sociology, or industrial relations and so on” (Parker, 2000: 140; emphasis in original). Organization studies, Parker says, “is made through the ramified micro-politics of citations, of whose ideas count and whose are left to molder unread – until they are resurrected by someone else under a new name, with, or without citation” (Parker, 2002: 48).

No doubt, there are benefits to be derived from boundary creation (Hernes, 2002: 103) but likewise, the ethnocentrism inherent in boundary maintenance fosters cognitive peripheral vision and can lead to the re-invention of the intellectual wheels. It would appear that the marketing of meta-theorising by Tsoukas and Knudsen is itself predicated upon epistemological amnesia or, at the very minimum, a highly tendentious reading of the history of meta-theorisation. The editors have on this score, served to provide an exemplar of the worst kind of academic practice, where the arbitrary specialization of academic boundaries closes off rich repositories of ideas and methods (Mills, 1959).

This epistemological amnesia is troubling, especially given that the market in which this text finds itself (and no doubt is offering itself as a palliative) is characterised by the time compression of PhD programmes and administrative responsibility curtails the length of time researchers can spend in the library (Hinings and Greenwood, 2002). These trends render such amnesia problematic since the marginalisation of historical antecedents can lead to the perpetuation of ignorance if material, such as Tsoukas and Knudsen’s chapter, is uncritically re-produced by students and academics alike. Although this would not be the sole responsibility of the authors. It does, however, reflect negatively on their scholarship when the shards of the discourse they elide are so easily recovered.

Despite the unwillingness of Tsoukas and Knudsen to review the various forms of meta-theoretical analysis, one aspect of their chapter that does warrant attention (in a positive sense) is their considered examination of the role of paradigms in organization analysis. They suggest that research should be seen as “a practical social activity [which] makes us see more clearly than before that researchers rarely are idealistic paradigm warriors but, more realistically...remain open to borrowing from other paradigms and perspectives as they see fit and are subjected to normative institutional criteria regarding the evaluation of their work” (Tsoukas and Knudsen, HOT: 11). This is a refreshing acknowledgement and contributes to the destabilization of the anthropomorphism often attributed to paradigms within the literature (e.g. Kelemen and Hassard, 2003: 74). It also reiterates the point made by March (1996) regarding the intellectual malleability of organization scholars; something the paradigm literature so often seems to elide.

Notwithstanding this intellectual malleability, Tsoukas and Knudsen do emphasise that there will be disciplinary and institutional factors that impinge upon research practice. However I still wonder about the extent to which the theoretical consumerism implied in the assertion that there is “inevitable osmosis between paradigms” (Tsoukas and Knudsen, HOT: 12) will not be subject to censorship since even if there is public valorisation of alternative approaches such as multi-paradigm research, this material may fail to navigate the publication process because reviewers may fear “yet another round of paradigm battles” (Gioia, personal communication). As Marianne Lewis (1996) has remarked, there are explicit tensions in the production of research that seeks to operationalize interaction between different paradigms. The interested reader only has to turn to the remarks made by Weaver and Gioia (1994) concerning the issue of paradigm incommensurability to understand the degree to which the affirmation of an alternative perspective is a politically dangerous move and it may be expedient to remain ‘in the closet’ where such osmosis is concerned (Sutton, 1997).

Remaining with this theme of historical elision and the amnesia upon which the production of a discipline is predicated, Shenhav (2003) provides a stimulating excursion into the archives of organization theory. Drawing upon a similar approach to that of Roy Jacques in his book *Manufacturing the Employee: Management Knowledge from 19th to the 21st Centuries* (1996). In his chapter Shenhav attempts, in prophylactic fashion, to “provide an alternative historiography of the field of OT” (HOT: 184) and manages to mediate between the Scylla and Charybdis of authorised organization theory and postmodern historical accounts to produce a meta-theoretical analysis that traces two discourses, engineering and sociological, “in order to historicize the epistemological assumptions of contemporary OT directly back to the professional project of social engineering around the beginning of the twentieth century” (Shenhav, HOT: 186).

Immediately obvious from Shenhav’s chapter are the benefits to be derived from a poststructural approach to organizational research. Diverging from a traditional meta-theoretical emphasis where the focus is traditionally on the identification of central textual currents that exemplify the thought of invisible colleges or individuals, Shenhav’s chapter embodies the wealth of insight that can be sourced from the ossuary of university (and other) archives and the important yet neglected insight “that would not be new but would, however, have remained invisible” (Foucault, 2000: 309).

In reading Shenhav's chapter and his scouring of periodicals such as *American Machinist*, *Engineering Magazine* around 1900, it is hard not to appreciate the validity of Canguilhem's comments that "just as a layer of dust on furniture is a measure of the housekeeper's negligence, so a layer of dust is a measure of the carelessness of their custodians" (1994: 82). If I am to draw a parallel with the criticism I made of Tsoukas and Knudsen earlier, it can only be a positive reversal, in that, Shenhav attempts to reconstruct the historical fusion of engineering and sociological discourses rather than elide such antecedents. The archaeo-genealogical focus of Shenhav's chapter reveals the extent to which discourses of organization theory are constructed 'from below' and highlights the extent to which "People know what they do; they frequently know why they do what they do; but what they don't know is what they do does" (Foucault in Dreyfus and Rabinow, 1983: 187). Thus, Shenhav regales his audience with Parson's unintentional distortion of Weber's concept of rationality and ideal type of bureaucracy "as a prescription for formal organizations and a recipe for corporate managers" (HOT: 196) concerned with "value consensus and effective functioning than with the role of conflicting material interests in domination" (HOT: 195). This 'peculiar translation' of Weber's theory of rationality, i.e. minus critical elements, has had a "tremendous influence on the emergence of American OT [and] should be understood in this ideological and epistemological context" (Shenhav, HOT: 191).

This dismantling and construction of an alternative account of the emergence of organization theory and important concepts within this canon reiterates the extent to which a thorough understanding of the context in which complex constellations of individual factors strike together and spark, or not, is necessary to comprehend the development and mutation of disciplinary formations. The emergence of organization theory, Shenhav reminds us, occurred under particular social and political conditions and appeared and became established often, as the result of surprises, as in detective stories. Shenhav's account of this emergence functions as a palliative to the uncritical acceptance of particular facets of organization theory that still remains palpable throughout the discipline (Jones, 2003). In a disciplinary environment where the "easy satisfaction of expectations, the harmonic fusion of an image with reality itself and the elision of tensions without placing undue demands on the audience" (Linstead, 2002: 660) is an inherent feature of (some) theoretical discussion, such historical excavation and the role that such exercises can play in encouraging the reinterpretation of the present is to be welcomed (Jacques 1996: 13). One debate that does appear to reflect the easy satisfaction of expectations, at least within the HOT, is that of incommensurability.

Incommensurability

Incommensurability is an issue that McKinley and Mone (HOT) have seen fit to return to within their paper in HOT. McKinley and Mone provide a useful review of various schools of thought within organization theory characterising these as either micro or macro perspectives. Their chapter represents a laudable attempt at meta-theorising, where the overriding objective is "to obtain a more profound understanding of extant...theory" (Ritzer, 1991: 17). On the side of the micro theoretical perspectives, McKinley and Mone discuss neo-contingency, resource dependence, and transaction

cost theory. Macro perspectives include, population ecology and neo-institutional theory. It is their argument that “all these schools³ are founded on ambiguous theoretical constructs. While ambiguous theoretical constructs foster creativity in empirical research...they also preclude conclusive empirical testing of the schools that are organized around them...and make the schools incommensurable with one another” (McKinley and Mone, HOT: 346).

This argument appears to be perfectly plausible and indeed the definition of incommensurability utilised by McKinley and Mone reinforces this: “By ‘incommensurable’, we mean that there are no widely accepted standards by which the relative validity of different schools with competing claims can be judged” (HOT: 346). Extending while at the same time tempering their previous arguments for the development of a democratically built construct dictionary (e.g. McKinley and Mone, 1998: 176), akin to that proposed by Campbell (1920, 1957) for physics in the 1920s, McKinlay and Mone set forth outlining their thesis of the incommensurability of the various schools of thought, the general argument being as follows: “We submit that any efforts to assess the relative validity of these three explanations...would be problematic, due to construct ambiguity...Thus we reiterate the conclusion we arrived at above: construct ambiguity and incommensurability are major problems in any effort to compare organization theory schools of thought empirically” (McKinley and Mone, HOT: 357).

For example, in their examination of neo-contingency theory and transaction cost theory, they assert that concepts such as ‘translation’ or ‘efficiency’ “are formulated at such an abstract level that...their meaning is unclear and multiple interpretations are possible. This situation could be considered good for transaction cost theory in the sense that no individual empirical study, or even a body of studies is likely to constitute a falsification of the theory...However, it is not evident that the situation is good for organization theory as a whole, because construct ambiguity makes it difficult to assess the validity of transaction costs theory relative to other schools” (McKinley and Mone, HOT: 356).

The criticism I am going to make will be contentious, operate at an abstract level,⁴ and is difficult to specifically tie to McKinley and Mone’s chapter due to their frustrating tendency to assiduously avoid the citation of any philosophers of science whose views are associated with the issue of incommensurability (for example, Davidson, Feyerabend, Hattiangadi, Kuhn, Putnam, Shapere and so on), and who might have sensitised and advanced McKinlay and Mone’s discussion of this issue; especially with regard to their own conceptual clarity regarding the incommensurability thesis, i.e. whether they are referring to semantic, methodological, or meta-incommensurability

3 I am assuming that schools of thought are commensurate with a research speciality and that a research specialty clusters around a paradigm. Research specialty is not equivalent to a discipline (Kuhn, 1970a).

4 Engaging in a nuanced discussion of the various forms of incommensurability and attempting to compare the content of different theories would require a separate paper and is currently being undertaken as part of a larger project. It is recognised that this endeavour is made more difficult in view of organization studies being multi-paradigmatic and synchronic in nature (Jackson and Carter, 1991) rather than mono-paradigm and diachronic.

resulting from referential, ontological difference between realism and non-realist (cf. Hoyningen-Huene, 1993). Nor do they explicitly locate their views in relation to Burrell and Morgan (1979). This renders it difficult for any major criticism of their views to be put forward and once more, indicates a lack of meta-theoretical analysis on the part of McKinley and Mone. In spite of this, it is worthwhile to reflect upon McKinley and Mone's understanding of incommensurability. Incommensurability is an issue which has vexed numerous scholars and has eventually found itself in a kind of theoretical limbo, although worthy commentary such as Jackson and Carter (1991) has advanced debate surrounding this topic. Brown (1998) manages to capture this sentiment nicely when he admits: "I'm still not quite sure if we finally sorted out the issues of incommensurability" (Brown, 1998: 374).

In order to engage with McKinley and Mone's argument I want to consider the extent to which McKinley and Mone hedge their thesis with qualification. For example, they hold the view that there are no widely accepted standards by which the relative validity of the claims of different schools can be made and that ambiguous theoretical constructs "preclude *conclusive* empirical testing of the schools that are organized around them" (McKinley and Mone, HOT: 346; emphasis added). Fine. There are no widely held standards against which different claims can be evaluate. Nor are we likely to ever have conclusive empirical testing of various theories as there will always be conflicting viewpoints, I would hope, in a discipline as variegated as organization studies. So, while we may never have conclusive empirical testing this does mean that such comparisons cannot be made, only that each group will be politically motivated to support their research speciality via *ad hoc* manoeuvres and qualifications. Incommensurable theories produced by incommensurable schools of thought make different claims about a specific subject of research and these will not be literally translatable into one another. If we want to compare two theories, it is necessary to learn the language of the theory and become bi or multilingual (Oberheim and Hoyningen-Huene, 1997), the difficulties of which, are well documented (Kelemen, 1995; Lincoln, 1990).

Of course, there are differences between translating a language and understanding it, as where it is possible to fail to translate from a foreign language to the mother tongue, it does not necessarily follow that it is impossible to *understand* the language (Feyerabend, 1987). Both Kuhn and Feyerabend have argued that comparing two incommensurable theories is difficult and more complicated than comparing commensurable theories. This is because commensurable theories share a similar apparatus of concepts and, as such, the empirical precision of the predictions made by each theory will be relatively clear. In the case of incommensurable theories this comparison via superimposition, as Kuhn's (2000) example of 'phlogiston' illustrates, is rendered complex because incommensurable theories conceptualise the same phenomena differently. Incommensurable theories cannot be compared through the adjudication of some common measure because different paradigms are complex constructions and any comparison will, of necessity, be multidimensional in nature. This does not mean that they cannot be compared (Sharrock and Read, 2002).

However, the question remains whether comparison via 'empirical' testing is desirable in light of recent suggestions that the paradigm debate needs to be 'managed' if the incommensurability associated with theory pluralism is to be neutralised for the benefit

of managerial decision-making (McKinley and Mone, 1998; Scherer, 1998; Scherer and Dowling, 1995). Empirical comparison in this environment might lead to a Stalinist purge similar to that fantasized about by Pfeffer (1993) and used to discipline emergent forms of inquiry. What I hope is clear is that the empirical and political argumentation surrounding incommensurability and commensurability equally merit extended examination as they in the case of McKinley and Mone's commentary deficient since it fails to account for recent advances in the philosophy of science or question the politics of the 'management' of consensus on the issue of incommensurability that they desire and assimilation of paradigm debate into the mode of production of advanced capitalism (Fuller, 2000).

In contrast to the version of incommensurability that McKinley and Mone advance which assumes that conceptual ambiguity precludes empirical testing and thus, by implication, that conceptual clarity and definitional consensus is required and will facilitate comparison and evaluation, thereby ridding organization theory of incommensurability (HOT: 353, 357, 360, 365, 367; see also McKinley and Mone, 1998: 171). Feyerabend has argued that the issue of incommensurability "*turns up* when we sharpen our concepts in the manner demanded by the logical positivists and their offspring" and "*disappears* when we use concepts as scientists use them, in an open, ambiguous and often counter-intuitive manner. Incommensurability is a problem for philosophers not for scientists, though the latter might become *psychologically* confused by unusual things" (Feyerabend, 1993: 211, emphasis in original). In addition to this reversal of McKinley and Mone's argument, Feyerabend bemoans the uncritical acceptance of precise concepts and rules that, if followed slavishly, may appear to be the only correct representations of thought. These, he argues, are only adhered to because they represent the criteria against which research is evaluated. This, he argues, serves to stifle the imagination and creativity of a researcher (Feyerabend, 1987) and contributes to the production of research "devoid of ideas, and full of fear" (Feyerabend, 1999b: 189).

Of course the systemization McKinley and Mone seek is understandable since conceptual clarity is useful for the puzzle-solving activity of (hyper)normal science and allows new researchers to develop linguistic and practical proficiency in technical language-games relatively quickly (Thoenig, 1982). Equally though, the conceptual clarity that McKinley and Mone desire, has the potential to become a suffocating straightjacket since the "more we try to be precise and exact, the less we are able to say and the harder we try to follow a rigorous theoretical system, the more we are tempted to fill it out with uninspired observations" (Van Maanen, 1995: 139). Let us hope that rigor and clarity functions to highlight anomaly and stimulate the destruction of restrictive theoretical systems "by new emancipating, and at the same time, enslaving conceptions" (Berlin, 1962: 19). Even so, I'm not optimistic about subscribing to anything close to a falsificationist view, no matter how sophisticated.

Let me be clear about the incommensurability thesis that McKinley and Mone ambiguously draw upon. It does raise legitimate questions about conceptual change in science, even though the implications for the philosophy of science are less extreme than is typically argued. "Reference change is not so radical as to preclude referential connections between theories. Translation failure prevents neither communication nor

comparison” (Sankey, 1994: 220). Provided that is, those associated actually want to communicate with each other, rather than talk past one another (Morgan, 1983: 17). These tendencies are, of course, not restricted to organization studies debates within the natural sciences provide reassurance that incommensurable differences can form the basis for profitable communication. Take for example, the paradigmatic transformation of classical physics to that of quantum physics, a radical transformation in the history of science. The protagonists in this debate, Bohr and Einstein (among others), discussed every stage in this debate: “Einstein raised an objection; Bohr was mortified, thought intensely, found an answer, told Einstein, and Einstein accepted the answer. Einstein raised another objection; Bohr was again mortified, thought intensely-and so on” (Feyerabend, 1999a: 267).

However it would be wise not to view this example as the norm for academic discourse since the ideological nature of paradigmatic affiliation will ensure that a reconciliation of differences is not easily forthcoming (Jackson and Carter, 1991). Communication will be, at best, partial. To understand why this is the case, we have to remind ourselves of the rationale behind Kuhn’s original formulation of the incommensurability thesis which he saw as a challenge to a realist perspective and the assumption of ontological convergence. This, he contrasted to what can loosely be termed ‘non-realist’ view with these two different parties holding distinct metaphysical assumptions (see Hoyningen-Huene, 1993: 34; translator’s note, 62-66, 121-125, 267-271). Incommensurability between these parties is most usefully seen as a form of meta-incommensurability and is a consequence of the referential differences between terms such as ‘world’ or ‘reality’ that exist for these groups. This throws into doubt the relationship seen to exist between theory and reality and the ontological status of the object referred to and results in “partial or incomplete communication, the talking-through-each-other” (Kuhn, 1970b: 232) with “debate about paradigm choice...necessarily circular” (Kuhn, 1970a: 94). The only way out of the argumentative circularity that inhibits meta-theoretical and meta-incommensurable theory choice is, as gestured to previously, to become bi or multilingual. This, Fabian (2000), reminds us consumes more resources than a conventional research education or project and is likely to discourage new researchers, particularly when the various pressures I have already mentioned are factored in.

To conclude this section, since “so few of the radical claims associated with the incommensurability thesis are warranted by the phenomenon of conceptual change [or ambiguity]...it is not clear that there is anything left for the word ‘incommensurability’ to stand for...there seems little point in saying that theories are incommensurable” (Sankey, 1994: 221). Such conclusions are not simply the preserve of the philosophy of science literature but can also be found within the marketing literature (e.g. Hunt, 2003: 229).

Conclusion

These insights drawn from the history and philosophy of science serve to highlight the epistemological fragility of the argument presented by Tsoukas and Knudsen that this volume contains contributions that have actively sought to meta-theoretically reflect

upon the present status of organization theory. McKinley and Mone's conceptualisation of incommensurability has drawn attention to the lack of any substantive attempt by the authors to engage in meta-theorisation, i.e. through the inclusion of "new developments...in the philosophy, sociology, and history of science" (Tsoukas and Knudsen, HOT: 3), in their analysis.

In reading the papers reviewed here I repeatedly found myself asking whether the discussion of metatheoretical analysis or incommensurability was interesting. If the criterion of interesting is conceived in terms of challenging taken-for-granted assumptions, I would say not. Less interesting ideas, Smith (2003) proposes, and I am inclined to agree, tend to be those that are consistent with and reaffirm already held beliefs and there does seem to be, a willingness within the HOT to perpetuate ideas that offer comfort, rather than cast doubt, on what is a politically contentious issue. Yet in spite of the conservatism inherent in Tsoukas and Knudsen's attempt to distance organization theory from its predecessors and McKinley and Mone's reaffirmation of incommensurable disciplinary categories, there are chapters present within this text that seek to question the historical formation and perpetuation of these tendencies.

Undeniably the inclusion of Shenhav's chapter is due to the increasing capital associated with such an approach and the availability of exemplars of this style of research against which potential contributions can be evaluated. It is also the result of adhering to the tacit rules of the academic game. Shenhav minimises criticism of the institutional form and location of organization studies by carefully noting how business schools in the United Kingdom differ from their American counterparts: "they allowed for other voices to be heard and a wider range of theoretical options" (Shenhav, HOT: 203). But equally, Shenhav refuses to consider his account as an authoritative conclusion, instead preferring to see it as a contribution to the critical literature "with the objectives of unmasking power relations and control structures" (Shenhav, HOT: 204). This is a task, Burrell argues, should remain high on the agenda of organization studies especially in view of the "three billion people on the planet who remain outside of conventional notions of organization" (Burrell, HOT: 534).

It is perhaps not unexpected then, that McKinley and Mone fail to extend their analyses further into potentially provocative areas, as this may incite calls of hereticism and heretics within both the natural and social sciences are "still made to suffer from the most severe sanctions this relatively tolerant civilization has to offer" (Feyerabend 1999b: 182). Just ask Immanuel Velikovsky (see De Grazia, 1978; Mcaulay, 1978), Bjorn Lomborg (2001; cf. *Grist*, 2001; *Kuro5hin*, 2003), or Copernicus (Zaltman and Lawther, 1979) and others whose work has posed potential challenges to established beliefs. The foregoing scrutiny of either Tsoukas and Knudsen or McKinley and Mone's chapters should not be taken as a purely negative summary. I prefer to view it as an affirmation of the benefits that can potentially accrue to organization theory when new perspectives on issues such as incommensurability are drawn from other disciplines in order to encourage the transgression of current categories of thought. Transgression then, can be seen as the affirmation of difference, with transgressors "those whose words and images are picked from familiar forms, but who twist and distort those forms in order to place before us a monster that is nevertheless born of our flesh and blood"

(May, 1992: 14) and this, Giddens has maintained, is “the very life blood of conceptual development” (1993: 1).

After reading the HOT my view is that it is an edifice whose marketing imagery, in the limited number of cases considered here (with the exception of Burrell and Shenhav’s chapter), triumphs over substantive content and represents little more than a parody of efforts conducted within, for example, sociology. This is especially disappointing as the HOT has demonstrative value, derived from the symbolic capital of its contributors and as such these efforts at meta-theoretical analysis may be taken as indicative of the appropriate standard for such analysis. Which, they most assuredly, are not. The overriding lesson to be derived from the HOT is that, “*ideas and arguments should be evaluated independently of their origins, [and] we must first learn of those origins...Ignorance may appear...due to the surface clarity of relatively contemporary texts, which effectively discourages any probing of their sources*” (Fuller, 2003: 71-72; emphasis in original). I can only hope that this text does not function as an exemplar of meta-theoretical analysis but rather as indicative of the problems associated with trusting that which purports to be meta-theoretically informed.

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