

The shifting boundaries of organisation theory across time: a demarcation perspective

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1. Introduction: the fluid nature of legitimate knowledge

Intellectual debates always take place in a context that renders them meaningful. Arguments presented in any controversy are shaped by internal and external factors: by what is immediately at stake, but also by wider philosophical movements within the social sciences. This paper makes a contribution to the ‘unification vs. pluralism’ debate (Knudsen, 2003) within organisation studies by examining over time changes in the nature of the arguments put forward within the debate. The aim is threefold: 1) to increase our understanding of how contexts mediate arguments; 2) to show how the contours of the field are re-drawn over time, and how this re-drawing contribute to sustain the debate; and 3) to bring attention to a number of lingering points of issues within the debate, and point out avenues for making advances within this central meta-theoretical debate.

In his historical studies of the demarcation between science and other forms of knowledge, sociologist Thomas Gieryn (1999) invites us to see legitimate science as a map drawn for us by actors contesting what is represented on the map. The maps that are drawn are variable, changeable and relatively inconsistent over time. Gieryn sees the signifier ‘science’ as a cultural space that has no essential or universal qualities. Rather, he says, “its characteristics are selectively and inconsistently attributed as boundaries between ‘scientific’ space and other spaces are rhetorically constructed” (xii). Gieryn posits that it is this flexibility that can explain the sustained authority of science. We can understand how science survives and flourishes when we see it as a cultural space that can encompass a relatively wide variety of practices and meanings. This calls for an analysis that does not try to affix once for all the content of the signifier ‘science’, but that recognises that, “What science becomes, the borders and territories it assumes, the landmarks that give it meaning depend upon exigencies of the moment—who is struggling for credibility, what stakes are at risk, in front of which audiences, at what institutional arena?” (x-xi). It is with that insight as a starting point that this paper endeavours to analyse how the unification vs. pluralism debate is sustained by the flexibility and changing character of the positions upheld in the debate. New arguments are rendered credible against different contexts and local contingencies, enabling the debate to carry on under different guises. The paper focuses specifically on how the ‘unification’ position changes over time, with authors pursuing different rhetorical strategies and designing arguments built on new rhetorical commonplaces.

The paper proceeds in three parts. *First*, I propose a reading strategy for the analysis of the changing character of arguments and rhetorical strategies deployed, and suggest that the

concept of *rhetorical commonplace* (or *topos*) is a useful analytical category to focus on. *Second*, I present a series of texts representative of the ‘unification’ position, ranging from the 1950s till the late 2000s, focusing on how they represent legitimate knowledge, and what project they advocate for organisation studies. The analysis reveals, in the *third* part, a number of themes on which the different authors show relatively different positions, suggesting that the ‘unification’ position has changed in important respects over time, and is far more heterogeneous than usually assumed. The discussion identifies also two recurrent preoccupations from the upholders of the ‘unification’ position: a concern for how ambiguity hampers advances in knowledge, and a concern for the generalizable character of knowledge. The chapter concludes that if the advocates of theoretical pluralism could address those two issues in a more satisfactory manner, it could contribute to taking the debate further.

2. A reading strategy: looking at rhetorical commonplaces

In his work on the rhetorical logic of science, Lawrence Prelli (1989) aims to reconstruct the informal but systematic logic underpinning arguments made within a given context. He shows that relevant topics of arguments are always guided (but not determined) by at least two things: the identification of the relevant issue to address, and the expectations about what constitutes a legitimate argument in the eyes of the audience. When a rhetor puts forward an argument, for it to have any influence on the audience, it must be underpinned by a principle or value which is considered legitimate by the audience. Prelli gives this example from a legal setting: “The kinds of audiences that see themselves as judging guilt or innocence will not be interested in expediency as a prime value in their situation” (36). Thus, as a general principle, Prelli says, “The typical normative principles accepted by the audiences in specific kinds of rhetorical situations prescribe the minimum basis of friendly rhetorical response” (37). When rhetors know what kind of response they want from their audience, they look for relevant specific *rhetorical commonplaces* (also called ‘topics of argument’ or *topoi*) that embrace the conventional ways of thinking of the audience.

Paying attention to rhetorical commonplaces is useful for the purpose of the analysis conducted here. In the first place, it is useful because *topoi* reveal how a given rhetor perceives the audience’s expectations and standards of reasonableness. Prelli (1989) says:

Every field of discourse contains shared preconceptions or patterns of thought that are so compelling that participants within each field do not have to justify the fact that they use them. Such common themes are located by referring to systems of special rhetorical *topoi* designating the characteristic line of identification that make a discursive community possible. Without clusters of commonplace ideas and familiar patterns of thinking, a field

of discourse would not be discernible; nothing would distinguish the range and forms of one field's discourse from discourse in other fields. (76)

Topoi are also a useful analytical category because of their relative stability over time. As Prelli notes, "while specific issues, ideas, and facts may change over time and across rhetorical situations, the perspectives or vantage points from which people choose to view their circumstances do not change rapidly" (78). While changes in the selection of *topoi* may reveal a change in how rhetors perceive the expectations of their audience, changes in the legitimacy of a specific *topos* can also reveal wider changes within the set of discourses organising the field. For example, while good research used to be defined as *objective*, nowadays many authors use instead the *topos* of *reflexivity*. This reflects not only a change in what peers expect, but also the influence of wide philosophical trends within the social sciences.

3. Fifty years trying to 'unify' the field: a reading of the 'unification vs. pluralism' debate

The debate about the intellectual structure of organisation studies has taken different forms in European and North-American journals. While, for example, the pages of *Organization Studies* have hosted various disputes on the theme of paradigm incommensurability, the *Administrative Science Quarterly* (ASQ) and the *Academy of Management Review* (AMR) have featured disputes centred primarily on issues around method and methodology. While the former set of disputes featured a plethora of positions, the latter set of disputes seems to feature two antagonistic positions that seem relatively stable and clearly defined around the project of unifying theories, or on the contrary of allowing diverse approaches to be developed within their own terms. The North-American dispute gives us the chance to analyse how within a seemingly dichotomous stream of controversies different rhetorical choices are made, reflecting changes in exigencies, contingencies and contexts.

The rise of constructivism as a theory of knowledge and the backlash against it provides a useful narrative against which we can read the various contributions made within the North-American debate. The constructivist argument for theoretical pluralism is a feature of many articles in the 1980s, and the qualitative research tradition associated with the constructivist position gets under many attacks. In the 1990s, in organisation studies and in the social sciences in general, we witness a different kind of attacks against what is seen as the excesses of a constructivist discourse labelled as 'postmodernism'. This forms the background against which new arguments for the 'unification' position are developed.

Here, I present eight texts that have been read as advocating the ‘unification’ position, or that are self-identified as such. These texts have been selected on the ground that they all have been published in major journals. This means: 1) that they must have been making an argument deemed reasonable by at least a journal editor and a number of reviewers; 2) that those texts are likely to have received more exposure than if published in a book, making them more likely to be subjected to refutations and critique; 3) that their higher level of exposure render their potential impact and effects more significant. Conveniently, they are also relatively spread in time, which enables us to a certain extent to identify trends and tendencies over that span. The texts are:

Litchfield, E.H. (1956) Notes on a General Theory of Administration. *Administrative Science Quarterly*, 1: 3-29.

Thompson, J.D. (1956) On Building an Administrative Science. *Administrative Science Quarterly*, 1: 102-111.

Koontz, H. (1961) The Management Theory Jungle. *Academy of Management Journal*, 4: 174-188.

Behling, O. (1980) The Case for the Natural Science Model for Research in Organizational Behaviour and Organizational Theory. *Academy of Management Review*, 4: 483-490.

Pinder, C.C. and Bourgeois, V.W. (1982) Controlling Tropes in Administrative Science. *Administrative Science Quarterly*, 27: 641-652.

McKelvey, B. (1997) Quasi-Natural Organization Science. *Organization Science*, 8: 351-380.

Rousseau, D.M. (2006) Is there such a Thing as “Evidence-Based Management”? *Academy of Management Review*, 31: 256-269.

Glick, W.H., Miller, C.C. and Cardinal, L.B. (2007) Making a Life in the Field of Organization Science. *Journal of Organizational Behaviour*, 28: 817-835.

Throughout the reading of these texts, I identify three distinct phases characterised by distinct rhetorical features. In the first phase, *The pioneers’ project*, we see efforts toward coalescing the field around a common project of developing a unifying theory of administration. We note however the inclusive character of that project. For the pioneers, developing a common framework was a mean of creating an identity for the field rather than a way of reducing theoretical diversity. In the second phase, *Achieving paradigmatic closure*, as the constructivist position is gaining legitimacy we see a reactionary attempt to realign the field with practices from the natural sciences. Finally in the third phase, *The new pragmatism*, we witness renewed attempts to achieve paradigmatic closure through ingenious strategies that criticise only subtly the type of research underpinned by a constructivist theory of knowledge.

3.1 The pioneers' project: coalescing the Field

The late 1950s seem to be a significant point in time for seeing the beginning of a unifying project for the emerging field. We start seeing various commentators deploring the lack of a general and unifying theory of organisation, and attempting to lay the foundations for one. The inaugural issue of the ASQ is a particularly interesting artefact in this respect as the majority of papers contained in it are deploring the dispersion of knowledge and the lack of such a unifying project.

Notes on a General Theory of Administration by Edward H. Litchfield (1956) exemplifies this well. Litchfield suggests that: “the years since World War II have seen an unprecedented increase in our knowledge of selected aspects of administration” (3). And while Litchfield goes on enumerating the already long list of fields and disciplines that have contributed to knowledge about organisations, he underlines two problems: “First, it will be noted that most of the new thought has come from the fields of mathematics, engineering, anthropology, sociology, or one of the emerging behavioural sciences. Relatively little has been contributed by academic students of administration per se...” (4). The author goes on: “Second, it is equally apparent that these additions to our knowledge have been concerned with selected parts of administration and not with the whole. Indeed, for the most part, their contribution to administration was incidental to another purpose” (4). Litchfield believes the field is at a “critical juncture”, and finds solace in the thought that, “Talcott Parsons and others [are] elaborating at least the beginning of a comprehensive theory of social action which might provide an over-all framework within which to develop a more specific theory of administration” (5).

How does Litchfield justify the need for the unifying theory he advocates? He begins by acknowledging that many fields have enriched the study of administration, but he says, “none of [them] is concerned with the larger problem of the total administrative process” (5). He adds:

Associated disciplines are helping us to learn a great deal about portions of the subject of administration, while others are adumbrating concepts of the totality of action of which administration is part. Flanked thus by singularly seminal movements, the question becomes, “What have we been doing to further our understanding of administration as a whole?” For, until we know the process and its setting, we can neither effectively integrate new materials others give us nor orient our process in a larger concept of social action. (5)

Litchfield opines that, “The answer to this question is not particularly encouraging” (5). He acknowledges that the understanding of administration has benefited from various developments in business administration, hospital administration, military administration, and

could potentially learn from educational administration. Litchfield says: “These have been significant developments. I do not minimise them. On the other hand, they have not told us much about the whole administrative process, its essential characteristics, its relationship to its environment, the way in which it becomes behaviour, or its function in modern society” (6). *Scope* and *fruitfulness* are crucial *topoi* this argument relies on.

About current knowledge, Litchfield says:

If we are lacking in comprehensive theory, we do at least have some thoughts which we may examine. It is scattered from field to field, seldom internally consistent and often unarticulated. Viewed in aggregate, it has a number of inadequacies which hamper the growth of administration as a science. (6)

The *first* problem—a recurrent theme, as we will see—is seen to be the confusion of terminology that makes communication across fields more difficult. ‘Management’, ‘business’ and ‘administration’ are thought of as interchangeable terms and, Litchfield says, “The consequence is that we are unable to speak precisely excepting in our own immediate circles where we have developed ephemeral professional dialects” (7). *Internal consistency* is a *topos* this argument relies on. The *second* and allegedly most important problem is that the emerging administration science, “has failed to achieve a level of generalizations enabling it to systematize and explain administrative phenomena which occur in related fields” (7). But that does not mean that the science of administration should end up simply showing the ‘one best way’. On the contrary, theory should account for the variations in practices required by different contexts. This is the *third* problem Litchfield alludes to: “if current thought fails to generalize the constants or universals in administration, it may also be criticized for its failure to accord a broad role to the variable in the administrative process” (9). *Explanatory power* is an important *topos* here. Finally, the *fourth* obstacle that hampers developments is the fragmentation of current knowledge. Litchfield relies on a *topos* that suggests that *synthesis* is better than fragmentation. He says:

(...) our present thinking has a fractured quality about it. Communication theory may be good, budgetary concepts may be entirely sound, and it may be that we have a reasonably clear concept of how policies are formulated. These and the other parts of administration have concerned us more than the whole. (10)

Litchfield is clear however on the fact that his project is not to divert attention away from those relevant areas. He says: “I am not suggesting that we should know less about the constituent elements in the process. I am insisting that in addition to knowing the parts we must understand the attributes and characteristics of administration as a totality” (10).

On Building an Administrative Science by James D. Thompson (1956) is another article part of the inaugural issue of the ASQ. Thompson argues that the major achievements of science have been accomplished under a minimum set of circumstances. First, a focus on *relationships*. Scientific theories, Thompson says, “are simplified models of relationships, which appear to account for experience” (104). Second, the scientific approach uses *abstract concepts*. Thompson says: “Science involves deliberate attempts to simplify understanding of relationships through use of abstract concepts which permit generalization” (104-105). Finally, the *development of operational definitions* is another major characteristic of the scientific approach. Thompson says: “Science requires that concepts be defined by a series of operations which permit the sensory perception and identification of the phenomena referred to by those concepts. Operational definitions make possible independent repetition of observations by scientists in many places and at many times” (105). The middle part of the article asks the question “How does the field of administration measure up against these requirements?” (105), for which the answer is: not very well.

In the last part of the article, Thompson points out three areas where the future science of administration must develop. *Topoi* of *internal* and *external consistency* underpin the project proposed. *First*, he says, *concepts* that are already at hand must be operationalised, or must be revised and be made operational. Thompson says:

New concepts will have to be developed or they may need to be incorporated from related sciences. The basic social sciences have been wrestling with concepts for some time and have been operationally defining them. While they offer jargon they also offer concepts which promise to be highly useful in the study of administration. (109)

He adds: “To the extent that useful ideas have been developed in these related fields, it would be folly to ignore them” (109). Thompson highlights the risk of seeing the science of administration being developed in isolation. He says: “Effective channels have been built for funnelling new knowledge into medicine and engineering. By contrast, administration is relatively isolated from the basic social sciences” (110). The *second* challenge for the science of administration is *to develop a system that relates abstract concepts* together. Thompson argues that,

(...) systems of logic for relating (...) concepts are as urgently needed as the concepts themselves. In the physical science the service of mathematics for this purpose is obvious. Mathematics has not yet demonstrated equal power in the social science area, although new forms of mathematics may be developed for this purpose in the future. In any event, one or more systems of logic must be developed before administrative science can mature. (109)

Finally, the *third* challenge Thompson identifies is *to give fundamental research more importance*. He says:

The pressure for immediately applicable research results must be removed from a large part of our research. It is this pressure which, in part, leads to the formulation of common-sense hypotheses framed at low levels of abstraction, without regard for general theory. The focus of attention on results with immediate utility limits thought and perception and thereby reduces the ultimate contributions of the research to administrative science. (110)

Overall the conception of science Thompson upholds has at least three fundamental characteristics. *First*, it is concerned with theory. Thompson says:

Achievements in the physical and biological sciences, and in their sister applied sciences, have demonstrated most convincingly the practical value of theory—theory which is repeatedly tested against experience and modified accordingly. A science of administration will be distinguished from administrative lore by the methods used to build that knowledge of administration. (104)

Second, it aims to provide *generalisations*. Thompson says:

(...) the sole assumption required for the application of scientific methods to the subject of administration is now generally accepted. That assumption—that regularities can be identified in the phenomena under consideration—is the basis of every attempt to train people for administrative roles. (103)

He adds that, “It is no longer a ridiculous idea that regularities can be found in human behaviour. Previous impressions to that effect stemmed more from inability to perceive regularities than from their absence” (103). *Finally*, the science of administration must be *cumulative*. Thompson says: “Answers to questions of administration are more likely to come by increment than by the master stroke of one research project, and this requires a research sequence with each piece building on the knowledge gained before” (111).

The Management Theory Jungle by Harold Koontz (1961) acknowledges from the onset the emergence of an academic field dedicated to the study of management and organisations, but also deplores its fragmentation. Koontz says:

From the orderly analysis of management at the shop-room level by Frederick Taylor and the reflective distillation of experiences from the general management point of view by Henry Fayol, we now see these and other early beginnings overgrown and entangled by a jungle of approaches and approachers to management theory. (174)

The ‘jungle’ is made up of six perspectives. First, there is the ‘Management Process School’, which includes the works of the likes of Frederick Taylor and Henri Fayol. Koontz reviews their contribution and concludes: “Perhaps there are more useful approaches, but I have found that I can place everything pertaining to management (even some of the rather remote research and concepts) in this framework” (177). *Simplicity* is a *topos* Koontz appeals to. Second, there is the ‘Empirical School’ based on the comparative analysis of case-studies of real-life experiences. Koontz dismisses it on the ground that, “management, unlike law, is not a science based on precedent” (177), but also on the ground that the Empirical School

does not offer much more than the Management Process School. Koontz dismisses a third perspective, the 'Human Behaviour School', not on the ground that studying human behaviour is not important, but rather on the ground that there is, he claims, much more to management than human behaviour. The fourth perspective to be dismissed is the 'Social System School', which Koontz links back to the work of Barnard (1938). That perspective is dismissed on the grounds that sociology, the discipline that grounds that perspective, has a too wide object to be of any interest to analysts of management and organisations. *Scope* is an important *topos* here. The fifth perspective dismissed is the 'Decision Theory School', this time on the grounds that its focus is too narrow, and its problematic not specific to organisations. Management is not *only* about decision-making, Koontz says, and most of the theories of decision-making "can be applied to the existence and thinking of a Robinson Crusoe" (181). Finally, the sixth perspective left to be dismissed is the 'Mathematical School'. Koontz makes laughing-stock of operational researchers who, he says, "have sometimes anointed themselves with the rather pretentious name of 'management scientists'" (181). Koontz refuses to see mathematics as a truly separate school of management theory, but simply as a tool (albeit a useful one) subordinated to other perspectives. Koontz concludes: "I cannot see that mathematics is management theory any more than it is astronomy" (182). Koontz pursues his effort, trying to explain why such a 'jungle' exists, and how to clear it.

Koontz echoes Litchfield in suggesting that to overcome fragmentation, one problem the field has to deal with is the semantic confusion around the word 'management', which makes it difficult for researchers to work on common problems. Even more confusing, Koontz says, is the meaning of the word 'organisation'. Similarly, Koontz suggests that the boundaries of the field must be defined in a way that circumvents the realm of the phenomenon called 'management'. Koontz says: "With the plethora of management writing and experts, calling almost everything under the sun 'management,' can one expect management theory to be regarded as very useful or scientific to the practitioner?" (183).

Another problem the emerging field has to deal with is overcoming the prejudice against the work of pioneers like Taylor and Fayol. Fayol and other early writers on management are often seen as analysing the past on the ground of *a priori* assumptions. As if those writers are simply describing the practices that emerges from an *a priori* abstract model, rather than the model being the distillation of years of experience. Koontz says: "No one could deny, I feel sure, that the ultimate test of accuracy of management theory must be practice and management theory and science must be developed from reality" (184).

Koontz also points out that management researchers seem to be quite reluctant to engage with researchers drawing on different perspectives. Koontz says:

Perhaps this unwillingness comes from the professional 'walls' developed by learned discipline. Perhaps the unwillingness stems from a fear that someone or some new discovery will encroach on professional and academic status. Perhaps it is fear of professional or intellectual obsolescence. But whatever the cause, it seems that these walls will not be torn down until it is realized that they exist, until all cultists are willing to look at the approach and content of other schools, and until, through exchange and understanding of ideas some order may be brought from the present chaos. (185)

Koontz never concludes laying out his favoured candidate for a general and unifying theory. This is only done years later, when he starts championing the nebulous 'operational approach' (a term he borrows from Bridgman (1938)) as a candidate for providing a general and unifying approach to the study of management and organisations (Koontz, 1980). For now, all he does is laying-out the criteria by which we should judge any prospective theory, mobilising mainly *topoi* of *scope*, *fruitfulness*, and *usefulness* and *intellectual openness*:

- 1) The theory should deal with an area of knowledge and inquiry that is 'manageable'; no great advances in knowledge were made so long as man contemplated the whole universe;
- 2) The theory should be *useful* in improving practice and the task and person of the practitioner should not be overlooked;
- 3) The theory should not be lost in semantics, especially useless jargon not understandable to the practitioner;
- 4) The theory should give directions and efficiency to research and teaching; and
- 5) The theory must recognize that it is part of a larger universe of knowledge and theory. (188)

3.2 Achieving Paradigmatic Closure: Protecting the Field

In this second phase, what we observe is a reaction against the new and increasing legitimacy of research underpinned by a constructivist theory of knowledge. Qualitative research is dismissed for its epistemological flaws, and the project of an administrative science is narrowed to the development of refutable propositions expressed through a logical language from which any source of ambiguity is removed.

The Case for the Natural Science Model for Research in Organizational Behaviour and Organizational Theory by Orlando Behling (1980) is set against Morgan and Smircich's (1980) *The Case for Qualitative Research*). Behling advocates a form of research which is *transparent* (in opposition to *subject to fabrication*), *precise*, *replicable* (in opposition to *idiosyncratic*), *unbiased*, and *systematic* (in opposition to *subject to interferences*). This can be delivered by the so-called 'natural science model', and objections to that model can be dealt with if the requirements of the model are relaxed. Behling's overall argument can be schematised this way:

Premise: Objections made to the natural science model do not really hold if we relax its requirements.

Premise: There are far more problems associated with qualitative methods.

Conclusion: The natural science model is the least problematic and should underpin the development of the field.

Behling’s (1980) text starts by observing that research published in mainstream organisational behaviour and organisational theory journals and underpinned by what he calls ‘the natural science model’ has always been under attack, accused of merely verifying in elaborate and costly ways what managers already know, or “[splitting] hairs to benefit the egos of theoreticians” (484). But recently, Behling says, natural science has come under a new sort of attack, and different authors have questioned whether the natural science model is really suited to the specific and complex nature of organisational phenomena. Behling examines and discusses five major objections (see Table 1) and concludes that these objections do not call for discarding the model, but instead for a more thoughtful application of the natural science approach. But what does he mean by ‘thoughtful’?

Table 1: Five objections to the natural science model

1) Uniqueness	“Each organization, group, and person differs to some degree from all others; the development of precise general laws in organizational behavior and organization theory is thus impossible”.
2) Instability	“The phenomena of interest to researchers in organizational behavior and organization theory are transitory. Not only do the ‘facts’ of social events change with time, but the ‘laws’ governing them change as well. Natural science research is poorly equipped to capture these fleeting phenomena”.
3) Sensitivity	“Unlike chemical compounds and other things of interest to natural science researchers, the people who make up organizations, and thus organization themselves, may behave differently if they become aware or researchers hypotheses about them” (484).
4) Lack of Realism	“Manipulating and controlling variables in organizational research changes the phenomena under study. Researchers thus cannot generalize from their studies because the phenomena observed inevitably differ from their real world counterparts” (484-485).
5) Epistemological Differences	“Although understanding cause and effect through natural science research is an appropriate way of ‘knowing’ about physical phenomena, a different kind of ‘knowledge’ not tapped by this approach is more important in organizational behavior and organization theory” (485).

Adapted from Behling (1980: 484-485)

To the *first* objection—that the development of general laws is impossible—Behling replies that they are only an ideal to aim for. Making a distinction between *universal laws* and *empiric generalisations*, he says the latter is what is found in most research, and is useful for decision-making. To the *second* objection—that organisational phenomena are too instable to allow comparison and to arrive at general laws—Behling replies that if this objection was true, research and journalism would be undifferentiated. Behling reiterates that it is acceptable to aim for empiric generalisations rather than universal laws, and also that some phenomena

are slower to change than others. Organisation researchers should just avoid dealing with phenomena that cannot lead to empiric generalisations. To the *third* objection—that unlike with the study of chemicals, researchers, merely by their presence, can have an effect on the practices they study—Behling replies that it can be avoided. Participants, for example, may change their behaviour when they know what hypothesis is made about their behaviour. Behling says that it is not a bad idea to leave participants in the dark regarding the aims and purpose of a research, and to minimise interference from participants through the use of unobtrusive methods that minimise interactions between participants and the researcher. To the *fourth* objection—which can be broadly summarised as pointing out that laboratory experiments are artificial and do not inform much about real organisational phenomena as they happen—Behling’s response is two-fold. On the one hand, he says laboratory settings are much more flexible than it is generally assumed. On the other, it is possible to conduct *quasi-experiments* in the field, and their lack of validity and reliability is not as problematic as one may think. He says:

The criticism is invalid because it assumes that a flawed study—that is, one which does not control all threats to internal and external validity—yields no useful information. In fact, many of the conclusions drawn in the discipline are extracted grudgingly from the weight of evidence from dozens of studies, most of them flawed in one way or another. (488)

It seems at this point that *usefulness* is more important than any other *topoi*. Finally, for the *fifth* objection—that understanding in the social sciences call for different methods than in the natural sciences—Behling concedes that indeed it is worthwhile to think that the social sciences are not trying to generalise about *why* things happen by identifying causes, but are seeking to explain the significance or meaning of phenomena. The problem is, Behling says, that the methods to achieve that are so poor, biased and unreliable that any finding is deemed to be insignificant. For example, about in-depth observations, Behling says: “improperly performed, [it] is nothing more than a naive phenomenology that discards objective verification in favour of uncritical acceptance of the observer’s experience of reality” (488). Observer’s biases can sometimes be avoided, but Behling remains convinced that “the natural science approach has built in extensive means for protecting the researcher against personal biases and thus such biases affect the outcomes of natural science research less often than they do those of other methods” (489).

Behling concludes his text with the following remark that encompasses the structure of his overall argument: “My attitude toward the natural science approach can be captured in a paraphrase of Winston Churchill’s famous comment on democracy: it is the worst possible

way to study organizations—except for all the others” (489). Overall, Behling dismisses qualitative research on the ground that *it is likely* to encounter problems. But while arguments for and against the natural science model are given a fair hearing, those for qualitative research aren't.

Controlling Tropes in Administrative Science by Craig C. Pinder and V. Warren Bourgeois (1982), published in the ASQ, takes on the theme developed earlier by Morgan (1980), who suggested that the study of organisations had developed upon the basis of a too small number of metaphors that reflect the assumptions of the functionalist paradigm, and he suggested for the development of the field alternative metaphors that challenge the assumptions of the functionalist orthodoxy. Pinder and Bourgeois want to put in question, “the unconstrained use of tropes (such as similes, analogies, and metaphors) in the development and presentation of formal theory” (641). They believe in particular that, “continued use of metaphors in formal theory may be impeding the progress of administrative science toward the goals set for it by J.D. Thompson when he launched *ASQ*” (641). Pinder and Bourgeois suggest that,

(...) administrative scientists seem to have made much more deliberate and flagrant use of metaphors and other tropes than do people in general or scientists in other disciplines. Rather than create terminology and jargon for exclusive use in the field, organization theorists seem to have felt compelled to generate tropes, linking organizations and/or aspects of organizational functioning to a variety of other entities and concepts from other disciplines and facets of life. (642)

They add,

There has been a tendency in the field to derive comfort and feelings of accomplishment from producing clever metaphors, as if linking part or all of organizational phenomena with outside referents via metaphors somehow describes what organizations are and explains why they are like they are. (642)

There comes a point, Pinder and Bourgeois say, where metaphors stop being of positive heuristic value and start to become misleading.

Pinder and Bourgeois' argument implies a view of legitimate science as one which shares at least three characteristics. *First*, legitimate science puts forward claims that are *falsifiable*. That requires language that is clear and literal. The problem is this: “hypotheses stated in terms of [metaphors] do not have enough clear content to be falsifiable” (643). Similarly, they say, “The danger in using metaphors is that we may not notice that the object being metaphorically described does not share many, if any, *defining* characteristics of the object used metaphorically” (643). For them, “Metaphors must be eliminable, and inferences made in metaphorical terms should still hold when one speaks literally, if they are to be of use in

science” (643). On this point, they conclude: “honest science ultimately puts itself on the line in literal enough terms to show us the conditions under which we should reject [a model]” (644).

Second, legitimate science uses concepts that are tried and tested, mobilising the *topos* of *corroboration*. Metaphors seem to either spring from imagination, or come from dubious borrowings. Pinder and Bourgeois say: “We believe that the enlightened and constructive borrowing of concepts requires high levels of competence in both the field that imports imagery as well as in the areas of knowledge from which images are borrowed” (645). Here, the crux of the problem is this:

(...) to be competent in any discipline requires years of training, much more than is normally possessed by organizational theorists who enter them for the sake of borrowing. So, while borrowing is relatively easy, *informed* borrowing is not, because it requires sufficient competence to appreciate the nuances of both (or all) of the sciences involved in order to understand the *limits* of the applicability of the concepts of one science to another. (645)

Pinder and Bourgeois say that they worry that, “too much of the content of administrative science consists of low-quality adaptations of concepts from other fields...” (645).

Finally, a legitimate science of organisation has the potential to be *useful* to practitioners. But for that, it needs to be *precise*:

We believe that the goals of an applied administrative science, like the goals of any applied science, should include (but not be limited to) the provision of advice to practitioners that is useful, precise, and predicated on scientific grounds. In addition, we believe that the spirit with which the science is conveyed to practitioners should reflect the degree to which it is limited in its usefulness, precision, and justifiability. (...) [A]ny practice that systematically impedes the capacity of the science to be precise necessarily also impedes its potential to offer applied advice that is either justified or useful. (650)

Overall, that is not to say that metaphors should be entirely excluded from science. Their role simply needs to be circumscribed to the early pre-scientific stages of enquiry:

(...) because of the impossibility of avoiding metaphors and other tropes in everyday language, they are bound to play a role in the early stages of inquiry, guiding speculation in a heuristic manner. But the ideal of scientific precision is literal language, so, to the extent that it is possible, administrative science must strive to control figurative terms in the development of formal hypotheses and theory. (647)

Morgan (1983) replied that Pinder and Bourgeois have misunderstood the status of metaphor, “as primarily a figurative device for the embellishment of language and discourse, rather than as a basic structural form of experience through which human beings engage, organize and understand their world” (601). The ideal of a science purged of metaphors, Morgan argued, “is simply unattainable” (606). Defending their original position, Bourgeois and Pinder (1983) responded that Morgan’s critique, “arises from the fact that he adopts fundamental positions in the philosophy of language, linguistics, epistemology, and

metaphysics on these issues that are different from those that we embrace” (608). Morgan’s position, they say, “is as much an artefact of his own philosophical perspective as ours is of our philosophical assumptions” (608). Bourgeois and Pinder are comfortable with their position being labelled ‘conservative’, as they see much value in the traditional nonconstructivist conception of knowledge they rely on. Following Ortony (1979), they say that the traditional perspective on metaphor, “holds that the description and explanation of physical reality can be conducted with precise scientific procedure that makes use of unambiguous language processes” (609). They add: “Ortony notes that this set of belief has been a dominant presupposition in our culture in general as well as within science in particular” (609).

Overall, what we note with both Behling (1980) and Pinder and Bourgeois (1982) is a general hardening of the position against any form of research underpinned by a constructivist theory of knowledge. We note also how the belief in the suitability of the natural science model is preserved through a series of compromise about the principles that need to be followed.

3.3 The New Pragmatism: Intellectual Hegemony with a Velvet Glove

In this last phase, we observe renewed attempts to achieve paradigmatic closure through ingenious strategies that criticise only subtly and implicitly the type of research underpinned by a constructivist theory of knowledge. While McKelvey (1997) enjoins constructivists to see that there is much in common between their project and his, both Rousseau (2006) and Glick *et al.* (2007) follow a different strategy. Research falling outside the mainstream is not criticised on epistemological grounds, but a call is made to aim for paradigmatic closure nevertheless on the ground of a set of pragmatic criteria.

Quasi-Natural Organization Science by Bill McKelvey (1997) endeavours to show that the reason why the ‘paradigm war’ persists is mainly because both proponents and critics of the natural science approach imply in their reasoning an antiquated conception that misunderstands what scientific knowledge is really about. In a long and dense article, McKelvey presents the tenets of the approach he suggests should underpin the developments of a new science of organisations, drawing on *scientific realism*, a view that recognises the complexity of organisational phenomena and aims to account for the fact that organisations are at the same time the product of human intentionality *and* subject to forces that determine them.

Set in the aftermath of the Pfeffer/Van Maanen controversy (Pfeffer, 1993; Van Maanen, 1995), McKelvey's article starts by telling readers that, "Even a hermit in bleakest Antarctica must be aware of the organization science paradigm war by now" (352). He hopes to contribute, not to a temporary truce, but to genuinely 'resolve the war' by suppressing the 'misunderstanding' behind it. Defending his thesis, McKelvey goes as far as suggesting that, "A quasi-natural organization science could end the paradigm war" (374).

McKelvey starts with a diagnosis of the situation: both the 'anti-positivists' and the 'positivists' have a mistaken understanding of what contemporary science has to offer. While the anti-positivists fail to see that the social is ultimately subjected to background laws that can be established with the help of sophisticated mathematical tools, the positivists fail to acknowledge adequately the seemingly complex and idiosyncratic nature of organisational phenomena. This failure, especially on behalf of the positivists, is seen as the underlying cause of the paradigm war. Who among the positivists and their foes has the right view appears difficult to tell because, McKelvey says, "current organization science method does not foster easy refutation of false paradigms, due to subjectivist epistemology and lack of testability" (353). Falsification of "false paradigms" in the field is difficult for various reasons:

First, and somewhat trivially, the science is relatively new, many paradigms are quite recent, and it simply takes time for the weight of incremental refutations to finally scuttle strongly entrenched claims, paradigms, or schools. Second, there is cause to believe that the testability criterion has failed in organization science, for two reasons: (1) prediction, falsification, and generalization, the basic elements of normal science, do not work with organizational phenomena because much of it is idiosyncratic; and (2) subjectivist thinking is increasingly prevalent. (354)

McKelvey acknowledges that human intentionality is not acknowledged within a natural science approach. In fact, he says, both positivists and their foes are erroneous in the way they conceptualise organisational phenomena and especially in the way they define 'good' science. About positivists (called 'organization scientists' in the text), McKelvey says:

Organization scientists have a truly archaic eighteenth century view of science, a worst case scenario really, in that it is a linear deterministic Newtonian mechanics epistemology without the power of mathematics: this is the "normal science straightjacket" alluded to by Daft and Lewin (1990). Now, twentieth century natural science is dramatically different from the eighteenth century version. (357)

McKelvey is somehow kinder to the anti-positivist crowd. The concerns of the postpositivists, for example, are seen as very legitimate:

Postpositivists focus our attention on the idiosyncrasy problem, instead of glossing over it as do the realists, and for this we are indebted. Postpositivists are scholars who take a closer, richer, thicker, more subjective view of organizational phenomena, coming to appreciate its fundamentally complex, idiosyncratic, and multi- and mutually-causal

nature (...) They conclude that the prevalence of idiosyncratic phenomena precludes the use of conventional realist methods, calling instead for subjective, richly descriptive, natural history style case analyses (...) It is also clear that they make no pretense of worrying about testability, that is, valid self-correcting justification logic. (354)

Postpositivists are right to want to acknowledge the complex and idiosyncratic character of organisational life, but they have simply misunderstood what ‘real science’ has to offer them. The same goes for the ‘other’ anti-positivists:

Phenomenologists, social constructionists, interpretists (sic), and postmodernists hold that individual actors in firms have unique interpretations of the phenomenal world, unique attributions of causality to events surrounding them, and unique interpretations, social constructions, and sensemakings of others’ behaviours... (356)

McKelvey recognises the importance of ‘chance’ and random behaviour. Idiosyncrasies or ‘microstates’ should definitely not be ignored, but they can be explained through a truly scientific approach.

Microstates, McKelvey says, “are defined as discrete random behavioral process events” (356). He adds: “they form the lower bound between organization science and more fundamental sciences, such as psychology, decision science, physiological psychology, biochemistry, and so forth, which might discover uniformities among microstates” (356). Here, McKelvey opines, we are touching at the crux of the problem afflicting organisation studies:

The dilemma is that it seems impossible to simultaneously accept the existence of idiosyncratic organizational events while at the same time pursuing the essential elements of justification logic defined by realists—prediction, generalization, and falsification—which requires nonidiosyncratic events (...) The dilemma is significant since idiosyncrasy will not disappear and realism is the only scientific method available that protects organization science from false theories, whether by distinguished authorities or charlatans.

McKelvey’s solution and resolution of the paradigm war calls for the study of firms as *quasi-natural phenomena*. ‘Quasi-natural’ is meant to define, “the intersection of *intentionally* and *naturally* caused behaviour” (353). He says:

There are two critical features of this view. First, firms are composed of numerous structures and processes amenable to *natural* science methods of inquiry and justification logic, including prediction, generalization, and falsifiability. But they also comprise behaviors directly attributable to human *intentionality*: behaviors and causes that may not be fruitfully understood in terms of natural science methods (357).

The new science of organisation is defined as one that focuses on the problem of discovering uniformities in microstates, and a large part of McKelvey’s article presents four perspectives for approaching this problem, all grounded in *scientific realism*.

In order to deal with potential objections, McKelvey puts emphasis on the fact that treating firms as *quasi-natural* phenomena rests on similar assumptions than those of the ‘flawed paradigm’ of postpositivism:

Guba, Clark, Huff, Lincoln, and Weick (...) have taken assumptions similar to those of complexity theorists as the basis of the postpositive pseudoscientific (sic) methods they advocate. Their assumptions (...) are not unlike those of complexity theorists: complexity, negentropy, contextually emergent simplicity, multiple causality, nonlinearity, self-organization, adaptive learning (...) Physicists, chemists, and biologists have taken these same assumptions and have developed a modernized twentieth century natural science that still upholds the traditional tenets of ‘good’ science, namely: objective measurement, replication, prediction, generalization, falsifiability, and most important of all, self-correction... (357)

For the future of the field, McKelvey holds the following view: “[its success] depends in part on finding more fruitful applications of computational and analytical methods to intraorganizational explanation, a trend recognized by the founding of an important new journal, Computational and Mathematical Organization Theory” (375). McKelvey concludes with what summarises more succinctly his view of ‘good’ science:

Organization scientists necessarily live in the midst of the idiosyncratic details. Nevertheless, this proximity should not dissuade us from searching for background laws. We need more focus on background laws, not less, as the ‘thick description’ postmodernists prefer. Without a strong scientific realist epistemology, organization science will continue to ‘churn’ paradigms like stockbrokers churn the accounts of distant customers. Strangely, organization science would probably be more successful to ‘consumers’ of its findings if it appropriately distanced itself from microstates by the organizational equivalent of microscopes, telescopes, and earth, and searched more intensely for background laws explaining naturally occurring order in organizational phenomena. (375)

Is there such a Thing as ‘Evidence-Based Management’? by Denise Rousseau (2006) is a developed version of her 2005 Presidential Address to the Academy of Management. The article is structured along a narrative that suggests that the great hopes for management research to improve concrete practices have been disappointed. She exemplifies this with her own experience:

I have nurtured my great hope—that, through research and education, we can promote effective organizations where managers make well-informed, less arbitrary, and more reflective decisions. My great disappointment, however, has been that research findings don’t appear to have transferred well to the workplace. Instead of a scientific understanding of human behavior and organizations, managers, including those with MBAs, continue to rely largely on personal experience, to the exclusion of more systematic knowledge. Alternatively, managers follow bad advice from business books or consultants based on weak evidence. Because Jack Welch or McKinsey says it, that doesn’t make it true. (257-258)

Rousseau is concerned by the fact that managers seem to not make use of the knowledge available to them from research. Rousseau advocates narrowing the gap between existing

research and concrete management practices through *evidence-based management* along *evidenced-based teaching*. Evidence-based management, Rousseau says, “derives principles from research evidence and translates them into practices that solve organizational problems” (256). She adds: “[it] is a paradigm for making decisions that integrate the best available research evidence with decision maker expertise and client/customer preferences to guide practice toward more desirable results...” (258). Proponents of the approach, Rousseau says, “are skeptical about experience, wisdom, or personal credentials as a basis for asserting what works” (258). Characteristics of the approach include:

- learning about cause-effect connections in professional practices;
- isolating the variations that measurably affect desired outcomes;
- creating a culture of evidence-based decision making and research participation;
- using information-sharing communities to reduce overuse, underuse, and misuse of specific practices;
- building decision supports to promote practices the evidence validates, along with techniques and artifacts that make the decision easier to execute or perform (e.g., checklists, protocols, or standing orders); and
- having individual, organizational, and institutional factors promote access to knowledge and its use. (259-260)

The idea of evidence-based practice is not new, Rousseau says: “Chester Barnard (...) promoted the development of a natural science of organization to better understand the unanticipated problems associated with authority and consent” (260). However, she adds, “Since Barnard’s time (...) we have struggled to connect science and practice without a vision or model to do so”. Evidence-based management provides such model.

Rousseau’s article proceeds with three tasks. *First*, it analyses why managers are unlikely to rely on research evidence to make decisions. Rousseau identifies many reasons, for example, she suggests that part of the reluctance to use research evidence in practice, “stems from the belief that good management is an art—the ‘romance of leadership’ school of thought (...) where a shift to evidence and analysis connotes loss of creativity and autonomy” (261). But most of her criticisms are toward her own profession:

(...) the most important reason evidence-based management is still a hope and not a reality is not due to managers themselves or their organizations. Rather, professors like me and the programs in which we teach must accept a large measure of blame. *We typically do not educate managers to know or use scientific evidence*. Research evidence is not the central focus of study for undergraduate business students, MBAs, or executives in continuing education programs (...) where case examples and popular concepts from nonresearch-oriented magazines such as the *Harvard Business Review* take center stage. (262)

The *second* task Rousseau sets out to do is to discuss how the Business School does not prepare students for the practice of evidence-based management. She portrays management

education as being often dumbed-down, with Business Schools more preoccupied by student ratings than by assessing real learning. Rousseau says:

We frame, and perhaps even slant, what we teach to make it more palatable. Can it be we are on that slippery slope of avoiding teaching the most current social science findings relevant to managers and organizations, from downsizing to ethical decision making, because we fear our audience won't like the implications? (264)

Rousseau accuses academics of acting irresponsibly when they teach things like “Herzberg’s long discredited two-factor theory” (263) when more updated knowledge is available. Academics have a duty to help students becoming fluent in navigating vast bodies of research literature, but, she says, “Neither students nor managers have clear ideas of how to update their knowledge as new evidence emerges” (264). *Finally*, Rousseau proposes actions that can be taken to close the gap between research and practice. Some examples are: to introduce to students role-models of successful managers who rely on research evidence; to show students that evidence is available and to teach them how to use it; and to create networks of academics interested in teaching from an evidence-based perspective. What is clear from this text is that Rousseau does not see the modernist ambitions of management as flawed; they just haven’t been adequately fulfilled yet. What the science of management must aim to be and to deliver is captured succinctly in this quote: “Managers need real learning, not fads or false conclusions. When managers acquire a systematic understanding of the principles governing organizations and human behavior, what they learn is valid—that is to say, it is repeatable over time and generalizable across situations” (261).

Making a Life in the Field of Organization Science by William H. Glick, C. Chet Miller and Laura B. Cardinal (2007), published in the *Journal of Organizational Behaviour* picks up over a decade later where the Pfeffer/Van Maanen controversy left, and endeavours to provide a reflection on the main issue raised by Pfeffer. The authors make no secret of their sympathy for his standpoint. They say: “Although ridiculed by many for his comments on weak paradigm development, Jeff Pfeffer helped to raise our collective consciousness regarding the difficult road we travel” (832). The article is presented as an essay that aims to help young academics having a successful career in the field. It is part of a special issue on the topic of ‘Careers in organization science’ (Ashkanasy, 2007) that contains four responses.

The article is long, but its argumentative structure is simple. Glick *et al.* identify a number of features of the field—features attributed to a low level of paradigm development and regarded as negative—and conclude with recommendations to help aspiring academics. Let us jump to those.

The *first* conclusion is that aspiring academics should have a relatively narrow research agenda, and should select over time no more than two research platforms from which they will derive all their projects.

A *second* conclusion is that aspiring academics should make sure they have the right personality traits and aspirations before embarking into this career. Glick *et al.* say, for example: “emotional stability is an important factor because it relates directly to how a person copes with stressful situations and heavy demands. An individual who scores low on this trait typically (1) is not relaxed, (2) is quick to feel anger, (3) often becomes discouraged, (4) often becomes embarrassed, (5) has more difficulty resisting unhealthy urges associated with addictions, and (6) does not handle crises well (...) In the stress filled world of organization science, an individual prone to these behavioral outcomes is at risk because of fixed deadlines, ambiguous performance standards, and periodic, random negative feedback on manuscripts and presentations” (829). *Sensitivity to inequity* is another important factor to consider: “Individuals who are sensitive to inequity are at risk in our field because equal inputs from talented individuals can yield very different outcomes through random forces. (...) Individuals who manage to enjoy life as an academic in this field probably have learned to deal with inequity” (829).

Aspiring academics are also told by Glick *et al.* that wanting the following things is a very bad idea: a social life (“An adult social life will become a distant memory”), marriage (“Being single may provide both the time and the flexibility to work nights and weekends”), and family life (“Individuals with children and a desire to spend a great deal of time in their children’s development need not apply...”). Aspiring academics should also make sure they are healthy because the job will take its toll: “Histories of heart disease, suppressed immune systems, and other diseases exacerbated by stress are signals that an academic career in organization science may not be the best choice. Long work hours, periods of little sleep, and randomness in key outcomes yield stressful lives” (830).

Finally, another set of conclusions is addressed to senior academics, who are given suggestions to help their junior colleagues avoiding being set on doomed paths, among which: to select doctoral students and junior faculty partially on personality factors; and to hire only seasoned scholars into research oriented positions.

A priori this article is the least suggestive of a hegemonic project designed to eradicate or suppress heterodox forms of research, but one may argue that it is because it does so in a subtle manner. Glick *et al.* are clear on the fact that they *are not* suggesting organisation

studies should establish a stronger paradigm. Not that establishing a strong paradigm would be a bad idea, but being realistic led them to recognise it is an unattainable fantasy: “Despite Jeff Pfeffer’s stature in the field and despite the strength of his arguments, there have been no broad-based movements for greater paradigm development in our field. Indeed, if there has been any change at all it has been in the opposite direction” (819). In fact, Glick *et al.* are keen to emphasise that they do not entirely agree with Pfeffer:

We do not agree with all of Pfeffer’s prescriptions for the field, but we have become more sympathetic over the years having watched many of our colleagues, friends, and students struggle with the implications of weak paradigm development. Rather than calling for a stronger paradigm, however, we believe that it is time to take actions that will help us cope with dissensus. (832)

Those actions—in the form of recommendations to junior academics and their senior colleagues—all give the same message: one can stray off established paths, but only at great personal cost.

4. Change and Continuity within the ‘Unification’ Position

Here, I discuss six themes that emerge from this analysis. Four themes put emphasis on the discontinuities in the eight texts, while the two last themes pay attention to two things these texts all have in common.

1) *From an open-ended project to a unitary field.* In the rhetoric of the texts presented here, it seems that over time we can observe a gradual narrowing of what constitutes the legitimate domain of organisation studies. This is counter-intuitive because as common-sense and research evidence¹ suggest, over time the domain of the field has been much enlarged. What is crucial to note is that in the early contributions to the debate, authors proposed a project that was much more open-ended than generally assumed. Litchfield (1956), Thompson (1956), and even Koontz (1961)—a fierce critic of fragmentation—were convinced that it would be suicidal to cut the ties between the emerging science of administration and the other social sciences. For them, a science of administration that would be developed in a completely autonomous manner would be bound to be impoverished. What these authors wanted was not so much unifying the field by eradicating anything outside a narrow mainstream, but developing a new and distinct stream of theories that was relatively inexistent at the time; a stream of theories that would be underpinned by their ideal form of

¹ For example, a bibliometric analysis of references in the ASQ from 1956 to 1985 shows that authors have increasingly been citing works from outside the immediate realm of the administrative sciences (Déry, 1990).

science and that would exist along other perspectives. Koontz in particular enjoined researchers to recognise that the general theory of administration that will be developed will be part of a larger network of theory. Perhaps for these authors, the constructivist perspective was not yet representing a challenge.

In comparison, in later texts, authors are much more comfortable with the idea of dismissing large sections of the field. Behling (1980) dismisses 'qualitative research' for it represents a too high risk of producing research that lacks rigour; and McKelvey (1997) thinks that constructivist researchers will have no purpose once researchers realise that sophisticated computational methods will explain all the idiosyncrasies featured in their 'thick descriptions' of life in organisations.

2) *An appeasing rhetoric.* Intuition would suggest that as the hostilities of the 'Science Wars' were raging on, something similar would be happening in organisation studies. We could have expected that texts from the 1990s would have featured a much more antagonistic rhetoric. It is quite the contrary that happened.

McKelvey (1997) dismisses a large body of research associated with constructivism, but also puts much efforts in trying to show that little distinguishes his position from the constructivist one, and he enjoins constructivist researchers to embrace a project presented as inclusive. Rousseau (2006) makes clear that there is no *a priori* reason to exclude qualitative research from the field. What is less explicit is that qualitative research is perhaps legitimate only as long as it is contained to a subordinated role to more rigorous research, and as long as it meets a standard of evidence that is almost impossible to reach. Glick *et al.* (2007) show no hostility to any form of research whatsoever, and that is perhaps what makes their text so cunning: qualitative research is the elephant in the room.

3) *The relaxing of the natural science model.* Common understanding suggests that over time, organisation researchers have progressively distanced themselves from the natural science approach, as other approaches grounded within other traditions gained increasing legitimacy. The texts presented here tell a different story. As the legitimacy of the natural science approach gets eroded, authors propose some sorts of 'compromise' in the form of new interpretations of the approach. While Thompson's (1956) text mobilises the approach in its most traditional form, Behling (1980) rescues it by modifying the meaning of almost every element of it. Behling looks at the standards set for each element of the approach, and lowers that standard. As for McKelvey (1997), he does not 'relax' the standards set by the approach, but argues that the approach is grossly misunderstood. In his view, his realist predecessors

have been relying on an antiquated version of the natural science approach that allegedly bears little resemblance to the real work done by contemporary natural scientists. This, he says, represents an enormous misunderstanding that has prevented constructivists, postmodernists and phenomenologists seeing the real potential of the approach to explain even the most idiosyncratic phenomena.

4) *The emergence of a new pragmatic criterion.* Over time, it seems that a new pragmatic criterion emerges as a mean of assessing developments within the field. In the early texts, authors justify their project using epistemological criteria. For example, Behling (1980) advocates the use of a natural science approach on the ground that other approaches are problematic, and that the natural science approach produces knowledge that is valid and reliable. Arguments that make references to theories of epistemology more or less disappear over time. Following Pfeffer (1993), Glick *et al.* (2007) never even insinuate that, say, interpretive research is less insightful than other forms of research, or problematic in respect to its epistemological foundations. Instead, their argument is the following: life for a junior academic is already hard as it is, why making it harder by following an obscure path outside the mainstream? In Rousseau's (2006) text, the pragmatic criterion does not completely replace the epistemological one. Rousseau (2006) still appeals to epistemology in order to defend a conception of knowledge that, for example, quite clearly excludes the management advices of quacks and so-called 'gurus'. The legitimacy of knowledge is assessed *first* through an epistemological criterion: knowledge must meet certain standards in regard to the quality of the evidence gathered and in regard to the procedure used to interpret it. But there is a second test: whether it is of any use to anybody.

5) *A constant preoccupation: reducing ambiguity.* This is a theme that recurs in almost every text, and its importance hints at one direction in which the debate can go. The problems associated with *ambiguity* of different kinds and especially *semantic ambiguity* have preoccupied most authors. Litchfield (1956) feared that confusion of terminology would make communication across fields more difficult. Thompson (1956) held that concepts that are ambiguous cannot be operationalised, and are therefore useless. Koontz (1961) believed that theory "lost in semantics" would not be understandable to the practitioner. Pinder and Bourgeois (1982) put at the centre of their argument the need for semantic clarity as a pre-requisite for falsification. Glick *et al.* (2007) saw ambiguity as a symptom of the problems associated with a 'weak' paradigm. They followed Pfeffer who thought that the field is characterised not only by ambiguity, but also by a higher level of ambiguity: ambiguity about

how to overcome ambiguity. As for Rousseau (2006), she suggested that evidence has to be able to speak for itself in an unambiguous manner.

Many commentators (e.g. Gergen, 1998; Van Maanen, 1995) have suggested is that ruling out ambiguity is not only an unattainable fantasy; it is also potentially undesirable in regard to advances in knowledge. Similarly, rhetoricians influenced by Burke's (1945/1969) conception of rhetoric (e.g. Ceccarelli, 2001; Gusfield, 1976) have suggested that rather than trying to eliminate ambiguity, analysts should try to understand its workings, for they believe it does play an important role in boundary-work and in knowledge-making in general. What this suggests is that the topic of *ambiguity* is one where significant discussions could still occur and stimulate new developments within the unification vs. pluralism debate.

6) *Another constant preoccupation: establishing generalisations.* This is another theme that hints at where the unification vs. pluralism debate could go. With the exception of Glick *et al.* (2007)—whose text presents less usual argument than others—every author suggests or claims that good science/research implies producing knowledge that can be *generalised*. What is worth noting however is how the *topos* takes on different meanings. For example, for Litchfield (1956), generalisable knowledge is knowledge that has *explanatory power*: it does account for all the organisational variables at play. For Thompson (1956), producing generalisable knowledge will require adopting a logical language by which concepts can be related together. For Behling (1980), generalisations are not universal laws. In order to be useful, generalisation can be merely about narrow populations, as long as the populations are defined systematically rather than casually. For McKelvey (1997), organisation researchers should define aim to identify background laws. He believes this is entirely possible because new computational techniques can enable researchers to see order where they previously only saw chaos. Finally for Rousseau (2006), generalisable simply means: usable in various contexts.

The theme of *establishing generalisations* is not one that is often discussed by advocates of the 'pluralism' position. This could be seen as something akin to a 'point of commensurability'—a point of entry into a more productive discussion. More efforts could be put to propose approaches that address the issue of generalisation in a satisfying manner. Approaches that address the problem of generalisation without necessarily relying on a realist theory of knowledge exists within the social sciences (e.g. Flyvbjerg, 2001), and discussing them could be one way of unlocking an unproductive debate.

By focusing on rhetorical commonplaces and on how standpoints are justified, we get perhaps a better measure of the heterogeneity characterising the ‘unification’ position. The absence of a reliable and comprehensive historical account of the field renders it difficult to relate rhetorical strategies to what we can suppose is the changing context of the field. However, what this analysis shows is how the ‘unification’ position is far from being homogenous, and how it changes in important respects over time. What the analysis conducted here also shows is the extent to which the field is actually distant from the natural science approach. On the one hand, most authors either use a ‘relaxed’ conception of the approach, or do not use it at all.

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