Readers beware: Provocation, problematization and...problems

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This article is a continuation of the discussion of organizational discourse initiated in the September 2011 issue of *Human Relations*. Further commentary would be welcome.

Abstract

In their recent *Human Relations* article and subsequent rejoinder to three commentators Alvesson and Kärreman (2011a; 2011b) make a number of assertions concerning the development of organizational discourse analysis and the current state of research in this area. We believe their emphasis on provocation results in an unsatisfactory problematization of discourse based work with the result that there are significant problems with both their analysis of the literature and their solutions to the shortcomings that they believe exist. We discuss a number of reasons why we believe that readers should be wary of what they read about organizational discourse analysis in Alvesson and Kärreman's work. Drawing on our critique of their article and rejoinder, we propose some ideas that we believe will be more useful in developing studies of organizational discourse than those put forward by these authors.

Keywords

Organizational Studies, Organizational Discourse Theory, Discourse Analysis, Problematization, Language, Theory.

In their recent *Human Relations* article entitled: “Decolonizing discourse: critical reflections on organizational discourse analysis”, Mats Alvesson and Dan Kärreman (2011a), reflect on the contribution of their 2000 paper on varieties of discourse. In addition, they make a number of assertions concerning the development, since the publication of their original article, of organizational discourse analysis and the current state of research in this area. One assertion they make is that studies of discourse have become so “dominating” (p. 1121) that they have colonizing effects. A second argument is that studies of discourse have been conducted in highly problematic ways. A third claim is that these shortcomings cause sufficient problems so as to require “decolonizing” through a range of strategies proffered by the authors, including the use of counter-balancing concepts, being more open about discourse’s constitutive effects, and disconnecting little d “discourse” and big D “Discourse” (concepts introduced in their 2000 paper). Also evident in the paper and the subsequent rejoinder (Alvesson and Karreman, 2011b) is an attachment to, and advocacy of, ethnography as a superior way of conducting research that is sensitive to language.

Alvesson and Kärreman’s article – as with any journal article – makes a number of truth claims, some of which we would not disagree with. That organizational discourse “has emerged as a large research field and references to discourse are numerous” (p. 1121) is easily demonstrated. For example, Prichard’s (2006) study shows that between 1989 and 2005, 444 papers on discourse were published in organization and management theory journals and that during this period the number of such papers that were published steadily increased each year (also see Phillips et al., 2008 and Leitch & Palmer, 2010 on the number of papers using critical discourse analysis). However, the authors’ claim that discourse is a “dominating” approach, responsible for “colonization”, requires further scrutiny. A count of the number of articles published about discourse shows that of more than 4,400 articles

published in eight leading organization and management journals between 2000 and 2010 inclusive,¹ only 369 could be classified as having anything to do with discourse.² Overall, this number represents less than 9 per cent of all articles published by these journals, ranging from less than 1 per cent in *Administrative Science Quarterly* to 18 per cent in *Organization* (Table 1). Overall percentages remain low in the US journals and, as Table 2 shows, numbers of papers published about discourse appear to have peaked in 2005 – remaining steady since then and even falling back quite significantly in some years. So, is organizational discourse really the dominating, colonizing approach that Alvesson and Kärreman claim? We think not. And, if that is the case, do other claims made in their paper also require scrutiny? We think so.

TABLES 1 & 2 NEAR HERE

Tables 1 and 2 call into question whether organizational discourse studies are dominating the field; nonetheless, they do show that there are many papers on organizational discourse that have been published a range of journals. To our mind, one of the most disappointing things about Alvesson and Kärreman's article is that they cite so few of these papers. This lack of engagement with the recent literature calls into question many of their accusations. For example, the authors say discourse scholars need to “engage more with ethnographic approaches” (p. 1194) but fail to acknowledge those who have (e.g., Heracleous & Barrett, 2001; Learmonth, 2009; Ybema, 2010; Prasad, Prasad & Mir, 2011). They (p. 1198) argue that “there is a lack of studies that focus on how organizational members navigate social reality and create, use and mobilize discursive resources” which is inaccurate (see, for example, Hardy et al., 2000; Ng & De Cock, 2002; Essers & Benschop, 2007; Zilber, 2007; Gordon et al., 2009; Spicer & Sewell, 2010). They refer (p. 1130) to only Wetherell (1998)

for a “minimalist attempt to synthesize small d discourse and big D discourse,” whereas we point to work by Phillips and colleagues (2008), Taylor and Van Every (2011), Maguire and Hardy (2009), and Vaara (2010) among others that focuses on exactly this issue. They claim that Foucauldian studies draw mainly upon investigations of textbooks and other writings, and not on empirical studies of managerial practices, while failing to cite (Covaleski et al., 1998; Thomas & Davies, 2005; Riad, 2005; Maguire & Hardy, 2009; Kornberger & Clegg, 2011) and so on and so on.

Our aim is to scrutinize some of the specific assertions made by Alvesson and Kärreman, as well as to encourage skepticism more generally regarding the way in which these authors have problematized research on organizational discourse. We believe their emphasis on provocation has resulted in an unsatisfactory problematization of the recent work on organizational discourse, resulting in significant problems with both their analysis of the literature and their solutions. If the solutions proffered by these authors are based on shaky foundations i.e., weak or mischievous claims about how research has been conducted, then they are unlikely to be a particularly useful guide as to how discourse researchers might develop their work in the future. Below, we discuss a number of reasons why we believe that readers should be wary of what they read about organizational discourse analysis in Alvesson and Kärreman’s article.

Beware truth claims...or watch out for cheap shots!

Alvesson and Kärreman make a number of assertions in their article. The fact that they are published in a leading journal does not automatically confer the status of “truth” upon these assertions; nor render them unproblematic. One such assertion, which we wish to explore in detail, is made where Alvesson and Kärreman state (p. 1194) that organizational discourse

analysis “sometimes seems to be used as a rationale for doing armchair research (or quick and dirty studies relying heavily on limited and thin material).” Although they do not cite any specific work, this comment seems to suggest that discourse scholars study texts simply because it is “easy” (p. 1125). This assertion appears to us to be closer to innuendo than to any plausible truth claim and, as such, should not be allowed to paint a negative picture of the organizational discourse literature without further scrutiny.

First, we would like to point out that there are many studies of discourse that are anything but quick and dirty, and yet which are not cited by Alvesson and Kärreman. To draw from one of the authors’ own experience, a study of a recent UN Convention on toxic chemicals was based on a wide range of empirical materials that took over four years to collect, including a large number of texts produced by a range of organizations, as well as conducting interviews and observing meetings in different parts of the world (Hardy & Maguire, 2010). In their discursive study of strategy, Mantere and Vaara (2008) studied 12 professional service organizations in different industries, conducting interviews with 301 individuals, while Laine & Vaara (2007) conducted a six-year longitudinal study, using participant observation of training sessions, company documents, and interviews. Heracleous (2006) engaged in retrospective data gathering going back 30 years, as well as engaging in over 100 interviews, participant and non-participant observation, and focus group sessions, in addition to gathering documents for his study of discourse and organizational change. In other words, there are countless “thick descriptions” of discourse that could have been cited as examples of how organizational discourse studies have been conducted.

Second, even when research does rely totally on the collection and analysis of texts, it is not necessarily “quick and dirty” or taking the “easy” way out. Nor does it suggest, as Alvesson

and Kärreman (p. 1194) would have us believe, that richer and more impactful studies would necessarily result were discourse scholars more predisposed to adopt ethnographic approaches (p. 1194). Some studies require textual analyses because other data sources are not available. For example, Maguire and Hardy's (2009) study of the deinstitutionalization of DDT focused on events taking place in the 1960s making it an unlikely candidate for an ethnographic study! In addition, as a study of institutions, it required empirical materials that showed changes in practices in multiple organizations which could not feasibly be observed ethnographically, even had time-travel been available to the authors (except, perhaps, if time travel had extended to allowing researchers to be in more than one place at once). Nor can we so easily dismiss the study of individual texts in the way that these authors do: some are indisputably consequential, such as the reports of public inquiry set up in response to major crises (e.g., Brown, 2004; Motion & Leitch, 2009). Even if their recommendations are criticized and ignored, these "non-effects" are still consequential and worthy of study.

Third, even when empirical materials appear delimited, their analysis can be sufficiently creative, robust, and insightful to justify "making large claims based on limited verbal exchanges" (Alvesson & Kärreman, p. 1194), despite what these authors say. Indeed, there is a corpus of work that clearly debunks their point. For example, Costa and Fleming (2009) focus on the experience of one individual to gain a richer and more intensive understanding of what self-alienation empirically means. Thomas and colleagues' (2011) study of a single workshop held in a telecommunications company involved finely-grained, intricate analyses to show the complexity of how meanings were negotiated. It is also of note that Alvesson and Kärreman (2001: 59) fail to reflect on how they have themselves used a single meeting to offer empirical insights and draw theoretical conclusions. While this paper is not specifically about discourse, it still illustrates how "the option of doing in-depth studies of micro events

as a way of making organizations visible.” Alvesson & Kärreman, justify this claim on the basis of it being part of an ethnographic study, but it nonetheless shows that, contrary to their assertions, paying attention to a small number of texts can justifiably be used as a basis for making “large” claims in discourse studies and elsewhere (p. 1194).

Alvesson and Kärreman do not mention any of the articles so far referenced in this section. This leads us to suggest that their representation of the body of work on organizational discourse might have been enhanced by greater engagement with the literature on which they purport to be reflecting. Another significant example of this lack of engagement occurs in the lengthy section, starting on p. 1128, critiquing the micro-discourse approach, which is framed entirely around Potter and Wetherell (1987) – a book written over 20 years ago. The authors find a number of faults with this work, one of which is that “Potter and Wetherell are unable to provide a theory that tells us what is not included in the context.” (p. 1129). That may, indeed be so, given when this work was written and the fact that it is situated firmly in the field of discursive psychology, where context was not a key focus at that time. Is it not possible that more recent work might have produced new ways of conceptualizing and studying context (e.g., Barry et al, 2006)? Only at the end of the section, is a single, more recent article cited (i.e., Taylor and Robichaud, 2004).

In sum, some of Alvesson and Kärreman’s “claims” regarding shortcomings in the study of organizational discourse are more provocative than they are helpful because, as we have pointed out, they ignore so many relevant articles. Perhaps not surprisingly, Alvesson and Kärreman have taken steps to defend themselves from such criticism. For example, they say (p. 1141) that their aim is to “reflect on organizational discourse as a field, its ‘mainstream’ (fairly typical examples), and not to map all the variation in a fine-tuned way.” This gives

them license to dismiss any attempt to provide examples of exemplary work: “We are well aware that every time we try to say something with a broader relevance the informed reader may associate to a few texts that represent exceptions” (p. 1141). Predictably, when Iedema (2011) attempts to do exactly this in his commentary, the authors invoke their prepared defence:

Iedema, for example, seems to suggest that these problems are solved, or minimized, if one consults a select few works on discourse analysis, in particular on affect. Whether these studies really offer new hope ... we are more skeptical now, mostly owing to the evolution of organizational discourse analysis (ODA) that does not seem to converge towards a more useful and rich approach (p. 1193-4).

The implication is that, when they are being selective, the authors are representing the “mainstream” of the “field.” When someone else does the same thing, they are simply being selective! Is this merely a matter of language games or is there a more profound problem in that the authors are merely trying to construct a negative picture of a field that is self-serving and misleading; one that merely functions as a mechanism for justifying their “solutions”.

Beware of claims that matter matters...what matter means may matter more

Another area where Alvesson and Kärreman have failed to engage with existing work arises when they criticize studies of organizational discourse for ignoring materiality. They argue (p. 1125):

It appears as if there is an affinity between the ‘discourse constitutes’ assumption

and a discursive focus, and a relative dis-interest in practices, meanings, relations, materiality beyond and beneath discourse. If language, action, practices, cognition and meanings are inseparable – a key assumption in much discourse analysis – then it seems sufficient to study the language part, and not worry about the rest.

They are not the first to make such assertions (e.g., Thompson, 2004), but it seems to us that the critics have missed the point. It is not that discourse analysis ignores the material: discourse scholars, as with many social constructionists more generally, are interested in how the material acquires meaning and in how the material and the discursive are entangled.

[M]ateriality is discursive ... just as discursive practices are always already material (i.e., they are ongoing material (re)configurings of the world). Discursive practices and material phenomena do not stand in a relationship of externality to one another; rather, the material and the discursive are mutually implicated in the dynamics of intra-activity (Barad, 2007: 151-2).

In other words, if matter is to matter, it is the *meaning* of matter that discourse scholars find useful to examine by exploring the relationship between discursive practices and material phenomena. Mumby (2011: 1153) explains this point in his comments on Alvesson and Kärreman's paper, when he argues that "objects and activities are meaningful to us because they are constructed through a complex intersection of discourses/Discourses, practices, institutional forms, historical precedents, power relations, and so forth."

This focus on the interpolation of the material and the discursive is nothing new to discourse

scholars. In 1999, Trethewey published a paper that analyzed “how organizational and gendered discourses are quite literally written upon women's bodies in ways that often constrain women's professional identities” (p. 423). Her study went on to examine how bodies not only dressed, displayed, sculpted and transformed as a result of gendered discourses. Hardy and Phillips (1999) also addressed this issue when they argued that discourse produces concepts – ideas, categories, relationships, and theories through which we understand the world and relate to one another – which are attached to material referents or objects.

Objects are part of the practical order, which does not mean that they pre-exist as objects in some way that is revealed by the discourse. Rather, it means that some concepts are discursively attached to particular parts of an ambiguous material world; a world that has an ontological status and a physical existence apart from our experience of them (Hardy and Phillips, 1999: 3).

This study goes on to show how the material world is made meaningful in ways that have particular consequences in the case of asylum seekers. These individuals – material entities – took on very different meanings, such as refugee vs. economic migrant, with very different effects, depending on whether discourses of human rights or sovereignty were evoked. In this way, discourse may help to explain changes over time in the way refugees have been treated: with those arriving from Vietnam in the 1970s swathed in the protection of the discourse of communism, while many of those arriving today from countries like Afghanistan and Iraq are shrouded in the discourse of terrorism (also see Lynn & Lea, 2003; Tomlinson, 2005).

More recently, scholars have tried to develop this relationship between discourse and

materiality further. For example, Suchman (2005: 381) notes how the object “is an outcome of discursive practices that render it coherent and stable, rather than a property that inheres in it sui generis.” She also acknowledges the power of the material insofar as material entities “bite back” in different ways. Maguire traces the effects of changes in meaning of the pesticide DDT over time, as the materiality of this molecule has been subjected to different discourses and narratives (Maguire, 2004; Maguire & Hardy, 2009; Hardy & Maguire, 2010). Taylor and Spicer (2007) provide a framework for studying organizational spaces discursively. Halford and Leonard (2005) compare the distinctive spatial configurations of two hospitals to explore the relations between managerial discourse, worker subjectivities and organizational outcomes. Dale (2005) documents how changing spatial arrangements in a UK electricity company were consciously designed to differentiate the privatized company from its public sector past. Ainsworth, Grant and Iedema (2009) examine the role of space in the construction of middle management identity. Iedema and colleagues have employed videoing as a research method in order to capture aspects of materiality in their research (Iedema, Long, Forsyth & Lee, 2006; Carroll, Iedema & Kerridge, 2008). Tyler & Cohen (2010) discursively examine both space and body in examining how gender is materialized. Doolin (2003: 758), in his discursive study of organizational change, takes into account technological artifacts, arguing that “the ordering of the social is never purely social but rather is sociotechnical, in that the social and the technical mutually define one another.” Alvarez (2001) also examines the discursive strategies used to manage technological change, while Rasche & Chia (2009) argue for the investigation of bodily “doings” in addition to bodily “sayings”. Ashcraft, Kuhn and Cooren (2009: 34) explicitly acknowledge that organizational objects are composed of both material and ideational elements, leading them to re-define communication as “the ongoing, situated, and embodied process whereby human and non-human agencies interpenetrate ideation and materiality toward meanings that are

tangible and axial to organizational existence and organizing phenomena.”

In sum, the examples where discourse scholars have examined materiality and practice are legion, but they are overlooked in Alvesson and Kärreman’s article. Collectively, these studies offer considerable insight into the dialectical relationship of the discursive and the material, as well as providing space for the political interrogation of how materiality takes on particular meanings. There is certainly scope to develop these ideas further, as Mumby (2011: 1156) suggests, in calling for “a genuinely post-linguistic turn view of discourse” which avoids treating “the material as somehow bifurcated from the discursive in any meaningful sense.” However, this endeavour is best served by engaging with, and building on, the existing literature, not by ignoring it.

Beware reflexivity...or practice what you preach!

Given the interest of at least one of the authors in reflexivity (e.g., Alvesson et al., 2008; Alvesson & Sköldberg, 2009), we are surprised that they do not demonstrate greater awareness of how their original 2000 paper does not stand apart from the work on discourse that they seek to critique, but is very much part of it. A case could be made, perhaps, that the original article was separate from the work it was surveying insofar as the work being reviewed had not been exposed to the ideas in the article. Certainly, the thinking behind it is presented by the authors as looking down on a body of literature that failed to measure up:

We at first had the ambition to give a creative overview of the field through pointing at the metaphors for or images of discourse used in the various literatures. This ambition could not be realized, however, as we found most texts on the topic to be vague and incoherent. Authors on discourse could be all over

the place. Efforts to specify and stick to a coherent meaning of discourse were sparse and seldom very helpful in making us understand the points they tried to make (p. 1122).

The authors go on to suggest that it was the inadequacy of the literature being reviewed – rather than any authorial agency on their part – which resulted in the model that formed the basis of the 2000 paper.

An ambition when we started writing the 2000 paper ... was to point to a number of meanings (metaphors, images) behind organizational discourse. The texts in the field, however were so ambiguous that it proved futile and we had to be satisfied with the 2×2 model, which, perhaps to our surprise, was shown to be a source of great clarification for many in the area – a warning signal, perhaps, if anything (p. 1195).

The implication of this statement is that the model somehow evolved of its own accord from the shortcomings of the existing literature. But is this a persuasive account? Surely, the authors played some role in developing the model?

So, did the model emerge from the lackluster “data” or was it imposed on those data? This question is not trivial when you consider that, from a discursive perspective, texts seem to get taken up in other texts when complex ideas are converted into “a transportable, apparently neutral format”, which then becomes a form of shorthand that can “easily disseminated and which everyone ‘understands’, helping to simplify and reify” (Harley & Hardy, 2004: 392). And there is nothing quite as transportable as a 2x2 matrix! As the original text is

incorporated into subsequent texts, it may become an obligatory passage point through which all other researchers must pass – and cite (cf. Hardy, Phillips & Clegg, 2001). So, as much as the original paper is presented as an overview of a (separate and delineated) body of work by apparent outsiders, did it not also represent (consciously or not) an attempt to stamp a particular brand “on” a body of work and, in so doing, to shape the work that followed.

Whether this was the intention or not, the original article *has* become part of the very body of work that the authors are now critiquing. So, what has the publication of this article done for – and done to – the body of work referred to as organizational discourse analysis? Clearly, it did not solve the earlier problems noted by the authors, or Alvesson and Kärreman would hardly have written the follow-up article that they have. Apparently, the “field” is still getting it wrong. In fact, this time, even more wrong since those who did take up the original article have mistakenly focused on the D/discourse distinction rather than engaging with the other parts of the paper devoted to muscularity.

The relative muscularity of discourse was a key idea in the Alvesson & Kärreman (2000) paper, which was thoroughly explored in the latter two thirds of the paper. In fact, we pointed to no less than four types of muscularity ... One reason for revisiting the paper was that this differentiated understanding of discourse and language was never really picked up, and the paper has been mostly misrepresented as arguing for two types of discourse (p. 1200).

The authors would not be the first to quibble with how their ideas have been taken up by other scholars, although “misrepresented” is an interesting term to use for two scholars who claim to be “sensitive” to language use. Discourse studies can, in fact, help to explain why

one part of their template survived, while other parts did not, because it involves an examination of the *consumption* of texts, not just their production and distribution. An individual text is “a link in a chain of texts, reacting to, drawing in and transforming other texts” (Fairclough & Wodak 1997: 262). Meanings intended by authors of the original text do not necessarily survive this process, but may change significantly as they are taken up and translated in subsequent texts (e.g., Maguire & Hardy, 2009).

Given the failure of consumers to engage with muscularity first time around, the authors propose it again, as a “solution” to the intransigent problems of an entire “field”, arguing that to accept the distinction between small d discourses and big D discourse is also to accept the relative muscularity in different discourses. But, should we accept the distinction between D/d discourse?

It could be argued that this distinction was a useful classification for what was then a nascent approach, and whereas some researchers used the term discourse to describe language-in-use or talk-in-interaction (see Putnam & Fairhurst, 2001 for a review of this literature); for other researchers it meant a system of thought, à la Foucault (e.g., Knights & Morgan, 1991; Deetz, 1992; Townley, 1993; Covalski et al., 1998). Papers submitted to journals were invoking the term “discourse” and assuming that readers on both sides of the Atlantic – and across a range of disciplines – knew what each other were talking about.³ The original framework did, therefore, help to provide some clarity. However, clarity does not come without a cost. From a reflexive perspective, the very existence of these categories serves to construct research that populates them. The framework is neither “neutral” nor simply descriptive. A different language would have produced different constructions of discourse: researchers would have conducted different analyses; made different interpretations; and written up their research in

different ways. The language of d/Discourse means that researchers are apt to interpret their data in accordance with one or the other category. Even if they then go on to look at how these two categories of discourse interpenetrate each other, two different “levels” of discourse have nonetheless been constructed by the researcher. The use of the convenient “shorthand” language provided by the framework thus serves to simplify, reify and divide; directing attention towards the insides of the boxes instead of problematizing the boundaries that separate them; the proposed framework all the more so. Given that discourse scholars are particularly interested when language becomes taken for granted, it is now time to interrogate this framework more closely; examine its “truth” effects; explore how it has become “naturalized”; and consider whether alternative language would allow us to address more complexity in our research?

We suggest that this framework has now outlived its usefulness and may even give rise to some of the problems of which the authors now complain. The bounded categories give rise to an “either/or” dualism i.e., either discourse or Discourse, whereas, metaphorically, it might be better to consider discourses as operating on a continuum – more or less pervasive, more or less local – rather than be situated inside boxes. But even the metaphor of continuum is problematic because discourses interpenetrate and overlap each other or, put another way, because the “local and global mutually condition each other” (Foucault 1980: 94).

In sum, it appears that the original framework may be no longer helpful to the development of work on organizational discourse. The new and “improved” version of D/d discourse advocated by Alvesson and Kärreman risks reinforcing boundaries further by “disconnecting discourse and Discourse” (p. 1121) with the help of a new template and new terminology – TFS and PDS. Is this what the study of organizational discourse really needs – a even more

simplified, rebranded model to travel around the “field”? And, if so, is this not itself a discursive manoeuvre in colonization?

Beware definitional (im)precision...or avoid being hoisted by your own petard

Alvesson and Kärreman (p. 1121) argue that the term “discourse” is used “in vague and all-embracing ways, where the constitutive effects of discourse are taken for granted rather than problematized and explored,” as a result of which have emerged the errors of “overpacking” and “colonizing”. The problem is, according to these authors, overly broad definitions of discourse. This issue of definitional focus is complex. Accordingly, we seek to examine this charge – and add some clarity to the discussion – by addressing the following questions:

1. Is there a lack of definitional focus in the organizational discourse literature?
2. If there is a lack of definitional focus, do Alvesson and Kärreman offer any insight into why might this be the case and what the effects are?
3. Is the narrower definitional focus advocated by these authors likely to be helpful in the study of discourse?
4. Would Alvesson and Kärreman’s critique benefit from some focus of its own?

In addressing the first question, we would argue that there is evidence of a focus, at least within the discipline of organization studies (OS) where, as the authors are compelled to admit, studies are predominantly characterized by social constructionist assumptions. Many researchers have, as a result, drawn on Foucauldian ideas, as Alvesson & Kärreman acknowledge (e.g., Knights & Morgan, 1991; Townley, 1993; Harrison 2003; Meriläinen et al., 2004; Thomas & Davies, 2005; Ezzamal & Willmott, 2008; Kornberger & Clegg, 2011). Alvesson and Kärreman do not, however, approve of this particular convergence around

Foucault's ideas, claiming his "framework" has been over-used. A related point of convergence reflects the influence of Fairclough's (1992, 1995) work (see Leitch & Palmer, 2010), and is found in the many empirical studies that involve the systematic study of texts – written texts, spoken interactions, visual representations, artifacts, etc. (e.g., Hardy & Phillips, 1999; Zanoni & Janssens, 2004; Vaara et al., 2004, 2010; Laine & Vaara, 2007; Phillips et al., 2008; Greckhamer, 2009). However, again, Alvesson and Kärreman do not approve: as they make painfully clear, they do not trust studies that rely on the collection and analysis of texts.

Therefore in answering the first question as to whether there is a lack of definitional focus, we would argue that that, while we would not want to claim that there is a single definition of discourse in OS, most research adopts social constructionist assumptions and has converged in using insights from Foucault and/or Fairclough.¹ The "problem" is that Alvesson and Kärreman don't appear to approve of the particular focus that has emerged.

While there is a considerable degree of convergence in how discourse has been theorized within organization studies there is, perhaps not surprisingly, less focus if one casts a wider net. Organizational discourse is studied in a range of different academic communities, as a result of which research has been informed by a range of theoretical positions (Prasad & Elmes, 2005: 852), including "Gramscian socio-political analyses (Van Dijk, 1997), Cultural Studies (During, 1993), Habermas' work on communicative action (Deetz, 1992) and Postcolonialism (Loomba, 1998)." Similarly, Vaara and colleagues (2004: 4) note differences associated among scholars of different disciplinary backgrounds: "linguists seem to understand discourse as language use, psychologists as cognitions, and sociologists as social interaction." There are also the differences noted earlier between the study of discourse as

language-in-use and as systems of thought. Thus a case can be made that the concept of discourse has been used in all-embracing ways, if one looks across a broad range of disciplines. This then brings us to our second question: if there is a lack of definitional focus in the study of discourse, does Alvesson and Kärreman's critique offer any insights into why this might be the case and what the effects are?

Despite referencing scholars in organizational studies, organizational communication, linguistics and discursive psychology, Alvesson and Kärreman seem uninterested in how these different academic traditions might account for diversity in how organizational discourse is studied, let alone how they might form the basis for a more nuanced interrogation of how discourse is defined (see van Dijk, 2007 for a comprehensive coverage of different disciplinary influences in the study of discourse outside organization studies). If there are differences in the definition and mode of using discourse when the range of academic communities that study organizational discourse is taken into consideration, then it would be more helpful to explore the source and consequences of this diversity. However, Alvesson and Kärreman have little to say about the contributions to knowledge made by different disciplinary traditions or how the particular ways in which these communities interweave have led to particular trajectories in how organizational discourse has been studied over time. As a result, the authors have missed an opportunity to provide a deeper reflection on the current state of this multi-disciplinary domain.

Our third question asked whether a narrower definitional focus within the organizational discourse literature along the lines advocated by Alvesson and Kärreman, is likely to be helpful in studying discourse. To answer this question, let us first differentiate *assumptions* from *definitions*.

As far as assumptions are concerned, we would argue that the variation noted above among academic communities does involve different theoretical assumptions, which researchers should articulate. However, Alvesson and Kärreman (p. 1196) dismiss this form of precision as “obsessively ontologizing” which should be avoided at all costs. We find this a strange thing to say. If one believes the world to be socially constructed, shouldn’t one say so? And if there are variations in the degree and manner to which one believes the world to be socially constructed, shouldn’t one articulate this also? Some ontologizing would seem to us to be part and parcel of reflexive research.

Definitional straitjackets are, however, another matter. Diversity is an important means of generating new ways of thinking. As Prichard (2006: 4) points out, there is a “lack of agreement over just what discourse analysis in organizations might be about and how one might go about investigating it ... what counts as discourse analysis depends a lot on one’s academic biography and the particular location in which one works.” He goes on to argue: “We have no way to resolve these differences; indeed, most would agree they are an important part of ensuring the development of the field.” Narrow, convergent, institutionalized definitions can be dangerous, as Van Maanen (1995) eloquently pointed out in his criticism of the “Pfefferdigm.” In the case of other concepts, such as power, simple definitions have proved overly constraining and have had to be “challenged, amended, critiqued, extended, and rebuffed over the years”, as a result of which there is, today, “a multitude of different voices that speak to and of power and a variety of contradictory conceptualizations” (Hardy & Clegg, 2006: 754). However, this has enhanced – not hampered – the study of power as insights have been garnered over time from the growing awareness of multiple dimensions (Lukes, 1974), the interest in disciplinary power (Foucault,

1977), and the acknowledgement of power/knowledge (Foucault, 1980). In other words, we would argue that we are far more likely to gain greater understanding of complex organizational phenomena if we are willing to break through the constraints of a single precise definition.

Let us now turn to our fourth question: would this critique of organizational discourse benefit from some focus of its own? We suggest that a clearer focus on key concepts used in Alvesson and Kärreman's paper – particularly the term “colonization” – would have provided greater insight into the basis of their critique, and provided an interesting reflection on how the study of discourse has developed. Colonization is apparently an important concept for Alvesson and Kärreman, given how often it is italicized. It is also a problem since the title and tenor of the paper advocate *de*-colonization. However, nowhere is this concept actually explained. The terms colonization and colonizing are used seven times in the paper but, for the most part, the term is simply stated with no explanatory text, except to indicate that it is a problem. For example, the authors refer to “difficulties following from a vague and broad, even colonizing, mode of using discourse” (p. 1123). Only at the end of their paper (p. 1142), do the authors associate a “less colonializing approach” with “cut[ting] the concept of discourse down in size, assum[ing] less, cover[ing] less, reveal[ing] more and allow[ing] a clearer space for other approaches and vocabularies”. However, nowhere do the authors explain in any detail what they mean by colonization and how broad or varied definitions of discourse might have caused it.

The authors also fail to explain what is being colonized. It is possible that they wish to indicate that the “field” has been colonized but, even here, their lack of precision makes it difficult to ascertain exactly what the field is. The title refers to organizational discourse, but

later on there is reference simply to the “discourse field” (p. 1121; 1122; 1123), which the authors frame as problematic (p. 1142): “Part of the problem is the constitution of a field of discourse studies.” If there is a “field of discourse”, as the authors repeatedly mention, it is hardly surprising that it has been colonized; otherwise, it would presumably be a field of something else. If the study of organizational discourse has colonized *other* fields, then it would be helpful to know what which fields are involved and how colonization occurred.

In sum, we acknowledge that, when the range of different disciplines that study organizational discourse is taken into account, there is variation in how these studies have been conducted, although we also note a considerable degree of convergence within organization studies. We are skeptical about whether future research would benefit from tighter definitions (notwithstanding a need for clarity regarding theoretical assumptions). In fact, it may be that overly precise definitions are more susceptible to colonization in the event that subsequent research is shoe-horned into the discursive equivalent of a Pfefferdigm. Having said that, we believe there are a number of occasions where Alvesson and Kärreman might have made a greater contribution to the future study of discourse had *they* been more precise. The authors make much of the claim that organizational discourse studies are colonizing but, without an adequate understanding of what colonization is – how it has occurred and what its effects are, particularly over the last ten years – we are not in a position to engage in de-colonization even if we wanted to.

Beware...much ado about nothing!

One must ask, what is the point of this latest article by Alvesson and Kärreman? Certainly, it is to celebrate the original 2000 article which has been highly influential on discourse scholars. But, other than being provocative, what do the authors really intend to achieve? In

their rejoinder to the commentators, they state their aims of encouraging discourse scholars to do more ambitious empirical work and weaning us off Foucault. A slavish commitment to ethnography also runs through both the article and the rejoinder: “Perhaps one way of measuring the success of this forum would be the extent to which this forum helps to persuade organizational discourse analysis scholars to engage more with ethnographic approaches” (p. 1194). However, as Watson (2011: 212) points out, it is all too easy to be smug about ethnographic research, “we must take great care here not to over-privilege the experiential aspect of our investigative work and insist that ‘I know better than you because I was there and you were not.’” As Alvesson and Kärreman seem to emphasize, especially in their rejoinder to their commentators, they do know better because they are always there. In contrast, we discourse researchers are never there – we are stuck in our armchairs, producing “quick and dirty” research from our “thin and limited” materials, while making “large claims based on limited verbal exchanges” (p. 1194). Such a problematization of the “field” of discourse is, indeed, provocative; but whether it deserves to be taken seriously is quite another matter.

And finally...

We wish to conclude on a more constructive tenor than Alvesson and Kärreman by proposing some ideas that we believe that will be more useful and less colonizing than those put forward by these authors.

Do problematize the way in which discourse is studied. As Alvesson himself has argued, challenging the assumptions underlying an existing theory is an important way to develop it (Sandberg & Alvesson, 2011). Alvesson & Sandberg (2011: 260) even provide us with a typology for carrying out problematizing, as well as a set of principles for identifying and

challenging assumptions. However, problematizing how current research is conducted requires a willingness to engage with existing literature to the extent that one can, with some degree of confidence, identify the assumptions to be challenged and discern problematic patterns.

Don't do ethnographies simply because Alvesson and Kärreman say that discourse scholars ought to engage more with this particular mode of inquiry. Collect the data that make sense in the light of the way in which existing research has been problematized and the particular research questions that are to be answered. Consider ethnography, traditional discourse analysis, other forms of textual analysis, interviewing, shadowing, videotaping – in fact, any appropriate method that helps to shed light on the issues under investigation (cf. Phillips et al., 2008).

Do consider how ethnographies might enhance our understandings of discourse; but also consider how understandings of discourse can enrich ethnographic findings. For example, Alvesson and Kärreman dismiss discourse on the grounds that managers in one of their ethnographic studies “demonstrated great use of leadership vocabulary” but “it did not seem to influence their practices very much” (p. 1132). A more interesting avenue to explore is *why* did managers use the language of leadership if they did not adopt the practices? For example, were there organizational barriers that prevented managers from adopting the leadership practices they had learned; or had they been taught leadership theories that failed to address the organization's problems. Either way, the willingness to pursue links between discourse and practice – and not simply dismiss the former – offers considerably more potential for a richer and more useful understanding of organizations.

Do remember that *all* research is partial, regardless of whether it is ethnographical, discursive or something entirely different again. Accordingly, bear in mind that individual studies inevitably follow a particular line of inquiry – and journal articles even more so – depending on the disciplinary backgrounds, theoretical assumptions, and empirical choices of researchers.

Do also remember that studies of discourse and even organizational discourse occur in multiple research communities with different traditions and institutions that produce different concerns and assumptions; and, further, within academic communities, scholars often develop different positions over time.

Do therefore ontologize – even a little obsessively – in order to understand the limits and assumptions of one’s research. Ontologizing helps to situate research in relation to other studies of discourse, the work of other academic communities, and the use of other methods which, in turn, helps to identify and articulate the contribution to knowledge. As Cunliffe (2011: 651) has observed, our metatheoretical assumptions “have very practical consequences for the way we do research in terms of our topic, focus of study, what we see as ‘data’, how we collect and analyze the data, how we theorize, and how we write up our research accounts”.

Don’t close down thinking with overly narrow definitions. Research is likely to make more of a contribution if it considers complex phenomena such as discourse, context, and other key concepts as “epistemic objects” which can be understood more fruitfully through openness rather than closure (cf. Chouliaraki & Fairclough, 2010).

Don't divide discourse into artificial categories but **do** think about whether and how more local discourses are “imbricated” (Taylor & Van Every, 2011), “re-textualized” (Iedema & Wodak, 1999), or “articulated” (Jian, 2011) leading them to “scale up” (Hardy, 2004) and transmute into discourses that are more organizational, national or global (and vice versa). In short, think about ways of collapsing, transgressing, erasing and eradicating the boundaries constructed in Alvesson and Kärreman's (2000) text.

In conclusion, a rich and diverse array of “plurivocal” discourse studies (Grant et al., 2004) have to date made substantial contributions to our understanding of a variety of organizational phenomena. If the study of organizational discourse is to continue to generate new knowledge, then establishing boundaries between discourse and Discourse, between discursivity and materiality, and between discourse and practice is more likely to hinder us than help us. Alvesson and Kärreman's position would result in narrowing down and reifying the focus of organizational discourse. We believe we need to work to bring down the barriers to studying organizational discourse, not engage in discursive manoeuvres that reinforce them.

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Notes

¹ The journals are *Academy of Management Journal*, *Academy of Management Review*, *Administrative Science Quarterly*, *Human Relations*, *Journal of Management Studies*, *Organization*, *Organization Science*, and *Organization Studies*.

² An article was identified as being about organizational discourse if it the term “discourse” or “discursive” appeared in the title, abstract or keywords according to EBSCO. If anything, this count overstates the number of papers on organizational discourse since sometimes these words were used only in passing or in very general terms.

³ We are grateful to the reviewers for pointing this out to us.

⁴ It is worth pointing out that many OS scholars have combined Fairclough’s interests in texts with a Foucauldian perspective in adopting a strong social constructionist position that can nonetheless accommodate agency and structure (e.g., Phillips & Hardy, 2002; Grant et al., 2004; Hardy & Phillips, 2004; Bergström & Knights, 2006; Mantere & Vaara, 2008; Phillips et al., 2008; Maguire & Hardy, 2009; Motion & Leitch, 2009). Applications of Foucault’s critical discourse analysis framework are, however, to be found without reference to Foucault (e.g., Vaara & Tiernari, 2008; Pälli, Vaara & Sorsa, 2009; Siltaoja, 2009) and, more recently, Fairclough (2005: 916) has distanced himself from what he calls the “extreme” poststructuralist work carried out in OS, and located his approach closer to critical realism and a more “moderate” position on social constructionism.

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Table 1: Number of discourse-related articles by journal (2000–2010)

Journal	No. of discourse-related articles	Total no. of articles*	Percentage
<i>AMJ</i>	13	792	1.64%
<i>AMR</i>	14	560	2.50%
<i>ASQ</i>	2	202	0.99%
<i>HUM</i>	94	706	13.31%
<i>JMS</i>	60	378	15.87%
<i>ORG</i>	93	511	18.20%
<i>OSC</i>	10	590	1.69%
<i>OSS</i>	83	699	11.87%
Total	369	4438	8.31%

Table 2: Total number of discourse-related articles by year

Year	No. of discourse-related articles
<i>2000</i>	34
<i>2001</i>	20
<i>2002</i>	25
<i>2003</i>	28
<i>2004</i>	40
<i>2005</i>	42
<i>2006</i>	33
<i>2007</i>	36
<i>2008</i>	<i>39</i>
<i>2009</i>	<i>33</i>
<i>2010</i>	<i>39</i>
Total	369

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