

## **Discursive Psychology: Between Method and Paradigm**

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## *Discursive psychology: between method and paradigm*



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**ABSTRACT.** Hammersley (2003) criticizes a particular style of discourse research for developing as a distinct paradigm, yet lacking the coherence a paradigm would require. He suggests a range of problems in relation to constructionism, reflexivity and the 'thin' model of the human actor, and argues instead for methodological eclecticism in which discourse analytic methods are supplementary to alternatives. This commentary highlights a range of confusions and misunderstandings in this critique. In particular, it highlights the way discourse analytic work is connected to a range of theoretical notions, most fundamentally in its theorizing of discourse itself as a medium oriented to action. It identifies important sources of incoherence that can arise when mixing discourse analytic and more traditional methods. It reiterates the virtues of constructionism, particularly when considering the operation of descriptions, stresses the value of exploring (rather than ignoring) reflexive issues, and emphasizes the rich and nuanced approach to psychology that has been developed in this tradition.

**KEY WORDS:** *constructionism, discourse analysis, discursive psychology, ethnography, methods, paradigms, reflexivity*

Martyn Hammersley (2003) has provided a detailed critical commentary on some aspects of some approaches to discourse analysis (DA) and conversation analysis (CA). The points are wide-ranging and there is space to deal properly with only some of them. I will attempt to correct the major confusions and offer arguments against his broader claims. I hope that my argumentative approach will be taken as collegial one, argument being a fine thing in the DA universe!

The central point of Hammersley's article (hence MHP) is that DA and CA have been developed as distinct paradigms, yet they lack the coherence needed for this because of a range of conceptual and philosophical reasons, and because of

a resistance to taking the (seemingly) sensible course of attributing distinctive qualities to categories of actors and using what people say as a source of information. DA and CA should instead be treated as useful but limited methods, which can be effectively used in conjunction with other methods.

Let me make some broad observations about the argument before addressing some of the specific points. Note that I am not proposing to speak for DA as a whole, let alone CA (although some of my points will have a wider focus).

### *1. Paradigms, methods and the discursive terrain*

The category discourse analysis is both a boon and an encumbrance. It is a boon as an enclosure in which a range of different kinds of work can come up against one another (as the current discussion illustrates). The success of the journal *Discourse & Society* is a testament to how creative that contact can be and how vibrant the debates have been. However, it is an encumbrance when treated as a singular thing without appreciating the consequences of its diversity (which is easily seen in the range of current overviews available: e.g. Jaworski and Coupland, 1999; Phillips and Jorgenson, 2002; Schiffrin, 1994; van Dijk, 1997; Wetherell et al., 2001; Wood and Kroger, 2000). For clarity, and to avoid trying to speak for the DA and CA community as a whole, I will develop my response to Hammersley from the particular variant of DA known as discursive psychology (henceforth DP; see Edwards and Potter, 1992, 2001; Potter and Edwards, 2001). Nevertheless, a number of the points will have a broader relevance.

MHP's arguments about paradigms may seem sensible when viewed from a distance, yet they fail to stand up to closer inspection. Let us first note that the notion of a paradigm is a tricky one. In Kuhn's original vision its role was to highlight the coordination of very different elements in scientists' conceptual worlds – values for theory choice, metaphysical models, symbolic generalizations and exemplars. According to Kuhn these are 'the objects of group commitment . . . and as such they form a whole and function together' (1970: 182). Yet there is much confusion as to how strong this 'functioning together' had to be and, famously, Kuhn specifically excluded social sciences from his analysis, doubting that they could ever be paradigmatic.

It is, therefore, confusing to talk of DP as a paradigm. Indeed, to do so risks stifling or evading debate with other approaches. One of the features in the development of DP (and much of DA) has been engagement at a theoretical, methodological and conceptual level with, for example, mainstream psychology and its approach to (and construction of) basic topics such as attribution, attitudes and social representations. This empirical and rhetorical engagement has been fundamental to DP's development; nevertheless, it is precisely the kind of thing that the well-known incommensurability of paradigms was supposed to rule out.

It is equally misleading to talk of DP as a method. It is not a free-standing set of data-generating and data-analytic procedures. It is an approach embedded in

a web of theoretical and metatheoretical assumptions. Theoretical advances in conceptualizing language were an important motor in its development. Constructing the research topic as *discourse* marks a move from considering language as an abstract system of terms to considering talk and texts as parts of social practices. It is not by chance that this journal is called *Discourse & Society* not *Language & Society*. Much of what is distinctive about DA/DP is a result of following through this move rigorously and, relevantly for this context, following it through in the arena of method.

Theorizing language in this way means that the choice of discourse analysis is not like selecting one dessert from an array of different and equally tasty ones, or having two scoops of ice cream and one slice of chocolate cake. Rather discourse analytic methods have been developed (and still develop) to encompass and address this active use of language. Mixing them with methods that presuppose a very different view of discourse is a recipe for incoherence.

On the one hand, this involves a positive recognition of the primacy of discourse as a medium for action. It ceases to be sensible to separate a study of language from a study of behaviour as traditional social psychologists might. Many researchers (not just discursive psychologists, but people in different traditions represented in *Discourse & Society*) are involved in discourse analytic work because it involves studying one of the most pervasive, important and interesting things about human life.

On the other hand, this recognition of discourse as action oriented has been the foundation of a series of critiques of alternative methods. Typically, these critiques have shown how a particular set of claims are a product of embedding discourse in a method without considering the pragmatics of that discourse. Note that the critiques go beyond the basic claim that language is active and so methods that fail to recognize this are flawed. Rather they attempt to demonstrate *in specific detail* how *particular* claims in *particular* studies are flawed. For example, discursive psychological work on what used to be called attitudes has highlighted a range of pragmatic peculiarities and assumptions in the way attitude scales are designed and interpreted (Potter, 1998a; Puchta and Potter, 2002).

To take a simple instance, Wiggins and Potter (in press) considered attitude measures in the area of food and eating. They highlight the way those measures (i) predetermine the descriptive categories available to participants; (ii) effectively strip off any practical business that people might be doing in using food evaluation; (iii) recast conglomerated statistical findings in terms of an underlying universe of tastes, flavours and psychological states that are, in turn, further abstracted as 'preferences' and 'attitudes'. Wiggins and Potter went on study naturalistic records of food talk to show how particular distinctions (e.g. between 'subjective' and 'objective' assessments of food – 'I loved that pizza'/'that pizza is lovely') can be highly consequential and yet are blurred together in standard measures of food attitudes.

How should we respond to arguments and research findings of this kind? They raise very deep problems for MHP's argument for DA as a supplementing method

(nothing less, nothing more). Should we supplement an attitude scale with a naturalistic study of eating? Given the problems identified this seems to be a particularly incoherent thing to do. Of course, such research could be used to try and improve attitude scales. However, there is not much sign of attitude researchers responding in this way, probably because there is no easy technical solution to the problems raised.

There is a broader tension here between the different meta-theoretical assumptions of traditional attitude work and discursive psychology. The former typically uses a factors-and-outcomes logic that has been developed alongside notions of experimental manipulation and the associated multivariate statistics. This goes along with questions of the kind: *What is the influence of X on Y* (of health beliefs on diet, of family breakdown on education failure, and so on). DA/DP work does not, typically, ask questions of this form. Often they are more like: *What is an X? How is X done? How is X managed in the context of Y?* The logic of these questions is conversational and rhetorical; they emphasize action and construction. They do not mix easily with questions involving factors and outcomes. This is not a surprising or a particularly novel claim. Methods or analytic approaches do not tend to be freestanding – they are typically associated with broader principles and assumptions. It is only when such associations become embedded into research procedures over a long period that they become invisible. One of the positive contributions of an alternative analytic approach such as DA/DP is that it can highlight things that have become implicit and taken-for-granted. The practical corollary of this is the confusion that researchers' risk when trying to join together methods such as these without appreciating the tensions between them. I see this personally when refereeing articles and, frequently but more poignantly, in pleas for help from researchers who have tried to mix methods in the way advocated in MHP and have become, understandably, deeply confused.

Maybe Hammersley would see the methods of traditional social psychology as a rather soft target, as the kind of positivism that is often treated as discredited in broader traditions of social science. Yet the basic observations are not dissimilar from Sacks' critique of traditional ethnography (Sacks, 1992). A major element in that critique is derived from Sacks' general theorizing of the nature of description, and his specific observations about membership categories (cf. Silverman, 1998). Put simply, the argument is that ethnographic field notes already embody major elements of judgement and analysis in the formulating and categorizing that they inevitably involve, and that these are very hard to then make explicit, and harder still for readers to recover from ethnographic writing. Again a more sophisticated understanding of language raises questions of research methods that have traditionally assumed something simpler. Of course, in ethnography these kinds of problems have led to a range of developments and modifications, from the more reflexive tradition focused on ethnographic texts and their operation (see e.g. Atkinson, 1990), on the one hand, to the convergence of ethnographic methods with ethnomethodological CA in the work of Goodwin,

Heath and others (e.g. Goodwin, 1997; Luff and Heath, 2002), on the other hand. Instead of supplementing ethnography, this work (along with other complementary ideas) has been the basis for a thoroughgoing reappraisal of ethnographic methods.

The general point, then, is that DA/DP is neither a self-contained paradigm nor a stand-alone method that can be easily mix-and-matched with others. It is an approach with a range of meta-theoretical, theoretical and methodological elements. It does not tell us all we need to know about social life – nor is it intended to. However, it developed as an approach for a range of theoretical and methodological reasons that are not arbitrary. Combining it with methods that make different assumptions about discourse – grounded theory, content analysis, social surveys, etc. – is likely to lead to incoherence. Of course, DA/DP can be used on data generated through procedures such as open-ended interviews or focus groups, and it shades into ethnography when drawing on combinations of video/audio records and documents – but that is not the same thing.

This suggests that methodological eclecticism is unlikely to be a pathway to progress. On the contrary, it can generate muddle. Clarity and innovation is at least as likely to be a consequence of single methods being drawn on with a clear rationale and appropriately formulated research questions. This can be combined with rigorous arguments between approaches over matters of theory, data, philosophy and so on. Ironically, this is rather similar to what was suggested by Kuhn as the path to progress in his 1962 book.

## *2. Conceptions of construction, reality and reflexivity*

An important part of MHP is devoted to identifying supposed conceptual confusions in DA, and in particular in its constructionism. Constructionism is certainly a source of confusion and controversy, and has been one area of disagreement between CA and some styles of DA (for summary see Hepburn and Potter, in press). Indeed, there are probably as many different varieties of constructionism as there are varieties of DA. However, the relevant variety here is the one addressed by MHP, and the characterization is wrong.

MHP comes at constructionism from a philosophical angle and renders it a kind of idealism. For example: ‘the constructed character of social phenomena is taken to indicate that those phenomena do not have the kind of objective reality normally ascribed to them by everyday social actors and by most social scientists’ (MHP, p. 756). The constructionism in DP is certainly not an attempt to deny the ‘objective reality’ of phenomena, which would be as realist a move as endorsing that reality (Edwards et al., 1995; Potter, 1998b). Rather it is considering the role of those phenomena in terms of people’s descriptions, glosses, categories, orientations and so on. In doing this, it picks up from a rigorous tradition of studying knowledge developed by sociologists of science. Workers within that tradition have found that a form of methodological relativism is indispensable for managing a range of tensions and troubles (see Potter, 1996).

The significance of this issue goes beyond researchers specifically interested in knowledge. Take an example from ethnomethodological CA. Paul Drew studied the operation of examination of witnesses in a rape trial, he identified and explicated the development of accusations and defences. This did not require a unique (and godlike) access to the reality of events beyond the court case.

- C: An' during that eve:ning: (0.6) uh: didn't  
Mistuh ((name)) [*the defendant*] come over  
tuh sit with you  
(0.8)
- W: Sat at our table.  
(Drew, 1992: 489)

The competing (but not contradictory) versions produced by the Counsel and the Witness are the basis for different kinds of activities (roughly blame increasing and blame denying). While 'sit with you' suggests familiarity and prior relationship, 'sat at our table' de-personalizes and de-familiarizes the relationship.

In methodological terms, the discourse can be analysed for how it is put together to perform activities without knowing about the reality of W and Mistuh ((name's)) motives, or about the solidity of the table they sat at (or near). Those things are the job of the jury to assess; the discourse analyst's job is of a different order. That is not to say that the two things are not potentially related. As the sexual violence comes into the courtroom in terms of descriptions, items of evidence, images and so on then its constitution as a crime or not is studiable. Matoesian (1993, 2001) illustrates one way in which such an analysis might unfold. Wowk (1984) shows another possibility. Wood and Rennie (1994) show yet another. Some conversation analysts have suggested that interaction patterns might be linked to outcomes in a broader fashion (see Heritage, in press, on the relation of doctors' prescriptions of antibiotics to interaction in the examination). Put simply, an interest in discourse is both coherent and consequential, opening up a number of different analytic options, some more descriptive, some critical (Hepburn, 2003).

One way of understanding this is to see that DP (and similar forms of DA) are taking a radically *emic* view of objects (whether they be motives, gravity waves, social classes or whatever). That is, those things are understood in relation to their involvement in participants' practices. For example, when Charles Goodwin (1997) studied the colour category 'jet black' (often treated as a cognitive universal) in the practices of scientists he was able to show the way membership of the category was accomplished in a variety of ways:

... for the geochemists, jet black (i.e. the most prototypical example of black) was not a context-free universal color category that pointed automatically to a specific set of color shades; instead, the term constituted a point of departure for a problematic judgment to be artfully accomplished through the deployment of a collection of systematic work practices. (Goodwin, 1997: 132–3)

Now note MHP's claim that ethnomethodology and constructionism both



effectively deny what seems to be a near universal feature of human experience, and one which has been the driving concern behind much conventional social science: that we are part of a causal nexus of physical and social events which shapes how we think and act, and what we are able to accomplish (MHP, p. 773).

MHP fails to understand research practice in this area. It is not that Goodwin's study, to take one instance, is effectively denying the claim that 'the causal nexus of physical events' (say the blackness of black) shapes how we think; nor is he endorsing it. Rather he is taking seriously what black *is* in *this* practice where black is highly relevant. MHP's plea for some kind of common sense – 'come on Charles, we know what black is?' – would simply obscure the relevant discourse practices.

Another way of understanding what is wrong with MHP with respect to this claim is that it is simply not justified. Where does the idea that 'we are part of a causal nexus' is 'a near universal feature of human experience' come from? It is a tendentious claim whose vagueness gives it a surface plausibility. Nevertheless, without being diverted by the extraordinary variety of cosmologies identified in classic anthropological work, we can note that close studies of particular practices in particular settings are often difficult to square with the idea that people are working with one simple coherent picture. For example, in Latour and Woolgar's (1986) study of the construction of scientific facts they note that at various points the scientists talked as realists, as conventionalists, as sceptics or even as relativists. Attributing an overarching, unitary common sense theory would have done violence to the practical and finessed way their scientific lives were organized.

MHP blurs together the style of DA discussed here with some kinds of critical discourse analysis that attempt to explain social actions in terms of social interests. The suggestion is that some DA takes a view of social life 'in which individuals and groups employ discursive strategies in pursuit of various interests' (MHP, p. 757). Some discourse work may take this view, however, DP is explicit in rejection of this approach. It is worth quoting at length to show how mistaken MHP is:

It is important to emphasise what I am *not* claiming here. The argument is not that social researchers should interpret people's discourse in terms of their individual or group interests. There are all sorts of difficulties with such an analytic programme, not least of which is that it is very difficult to identify interests in a way that is separable from the sorts of occasioned interest attribution that participants use when in debate with one another . . . The argument here is that people *treat each other in this way*. They treat reports and descriptions *as if* they come from groups and individuals with interests, desires, ambitions and stake in some versions of what the world is like. Interests are a participants' concern, and that is how they can enter analysis. (Potter, 1996: 110)

The difference between using interests to explain actions and treating interest attribution as a topic of study is a crucial one. Again, this clearly highlights what makes the approach discourse analytic rather than a more conventional social



science of groups, actors and interests. Moreover, MHP's general glosses on Wetherell and Potter (1992) are equally wide of the mark when they characterize it as dependent on a realist history of the development of New Zealand society, or on the correct identification of particular social groupings. Although versions of these things are important for understanding the significance of the analyses, and how they might be related to particular claims and conflicts, they are not a prerequisite for many of the analytic claims in that book. For example, Wetherell and Potter's observations about the way descriptions of culture can be used in both constructing and criticizing particular social groups, while avoiding the negative associations of traditional racist discourse, does not depend on a particular story of history or social organization.

MHP highlights a range of what it formulates as reflexive problems in DA. The argument seems to be that DA is both insufficiently reflexive about its own descriptive practices and *so* reflexive that it blurs the distinction between research and writing fiction (MHP, p. 765). DA researchers have taken reflexive issues seriously (most seriously in Ashmore, 1989) in considering the consequences of their analysis of fact construction for their own texts. These consequences have been explored in part through literary experimentation; but such experimentation is by no means a prerequisite for DA work. Nevertheless, they are generic issues that have been taken more seriously in DA than in the (rather under specified) conventional social science approaches that MHP advocates. Surely the widespread failure to consider reflexive issues in other analytic approaches is more of a worry for them than DA's consideration, however, far from an ideal it is.

The suggestion that DA is, through its consideration of reflexive issues, moving toward aesthetic rather than epistemic criteria is wrong. Any reading of methods writing in DA and DP over the past 15 years will note a range of criteria specifically offered for good work, as well as considerations of issues of validity, reliability and discussions of sampling (contra MHP's claim on p. 764); yet *none* of these criteria are specifically aesthetic (for recent examples, see Potter, 2003, in press). Indeed, MHP does not pay much attention to the empirical work in this field where such criteria are in play – the most recent actual research *study* in DA he discusses is Wetherell and Potter (1992), which is more than ten years old – not surprisingly, there has been considerable theoretical and methodological progress in the intervening years. MHP's points are abstract rather than grounded in cases.

MHP also develops the claim that DA has a rather thin model of the actor (specifically thinner than its thin, but perhaps inconsistently thick, model of society – MHP, p. 766). On the one hand, a certain kind of thinness is what is required. The thicker DA's model of the actor, the more it would obscure the models used in peoples' practices. DA is itself dependent on neither a developed notion of society nor of human beings (nor physics, furniture, G3 mobile phone technology and so on). Its focus is on discourse, and these other things enter into it in terms of descriptions, orientations and formulations. Yet, on the other hand, MHP's observation seems to involve a highly traditional approach on these

things, where society and its actors are understood as structured sets of causal entities.

Let us take psychology and focus again on DP. Although DP is not developing a model of what a person is in the classic psychological mode, it nevertheless offers an approach to the most intimate, subtle and complex of psychological phenomena. Edwards' (1997) book on discourse and cognition, for example, includes work on the way constructions of particular emotional states in relationship counselling are bound up with certain kinds of attributions of blame, which are further bound up with counselling practices and practical upshots such as who needs to change. Billig (1999) offers a different, but complementary, take on psychology in his rhetorical reworking of Freudian repression. It is a rather weak idea of what is thick or thin that treats these intimate, consequential studies of psychology in practice as somehow lacking in comparison with traditional models of agents with inner motors.

By not starting with a predefined model of the human actor DP allows a broader and more culturally embedded set of possible constructions and relevancies to be identified. Moreover, this approach does not contradict the inner motor view. In taking psychology in terms of its constructions and orientations it does not *straightforwardly* contradict behavioural, cognitive, humanistic, psychodynamic or neuropsychological views.

There is contradiction, however, but it is much more nuanced than MHP implies. It is a consequence of DA studies of the way discourse is conceptualized in the methods of human research. Particular models of the person may be dependent on certain assumptions wired into method. Edwards and Potter (1992) develop this argument in detail with respect to a range of methods in social and cognitive psychology. Other relevant studies of method include Schegloff's (1999) study of the administration of a test for 'pragmatic deficit' and Antaki's (1999) analysis of the delivery of tests for assessing people's 'quality of life'. The point is that critique of psychology comes less from developing an alternative model of the actor, as would be the traditional psychological way, than through developing an alternative understanding of language and its role in the machineries of psychological research and assessment.

### *3. Discourse and society (and psychology)*

In MHP's characterization DA has a highly restricted topic of study. It studies discourse leaving psychology, society, social processes, etc. to other approaches. This characterization goes with the call for complementary methods. However, the power and broad relevance of DA comes from the centrality of discourse. Discourse is the vital medium for action. It is the medium through which versions of the world are constructed and produced as pressing or ignorable. For social scientists the study of discourse becomes a powerful way of studying mind, social processes, organizations, events, *as they are live in human affairs*.

One of the features of MHP is its conceptual approach to DA. It is attempting to

identify coherence from a perspective that is itself untroubled by the messy business of doing research (apart from vague allusions to sensible, traditional approaches). It does not, therefore, have to compare DA with other specific kinds of research. However, one of the features of the development of DA and DP has been a rigorous cross-comparison with other kinds of studies (from a range of traditions). The key point of these comparisons here is that they highlight the way that other kinds of research are also centrally dependent on discourse of various kinds (in experimental protocols, interviews, vignettes, ethnographic descriptions) while failing to recognize the importance of this centrality. The business being done in the answer to a question in an ethnographic interview is not theorized as such; the constructive work of an experimental vignette is overlooked. We are not in a situation where there is discourse analysis and non-discourse analysis. Rather there is analysis that is highlighting and attending to the role of that discourse and analysis which is ignoring it. It is this final point that makes MHP's call for a traditional mix of methods ill judged.

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