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Methodolatry and Qualitative Health Research

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Abstract

The increasing turn to qualitative research in health psychology raises a number of issues about the appropriate use and relevance of qualitative methods in this field. In this article I raise concerns about methodolatry: the privileging of methodological concerns over other considerations in qualitative health research. I argue that qualitative researchers are in danger of reifying methods in the same way as their colleagues in quantitative research have done for some time. Reasons for the pre-eminence of methods are discussed briefly and their consequences considered. The latter include: a concern with 'proper' or 'correct' methods; a focus on description at the expense of interpretation; a concern with issues of validity and generalizability; an avoidance of theory; an avoidance of the critical; and the stance of the researcher. I offer some suggestions for avoiding methodolatry and some opinions on how we might develop and use qualitative research more effectively in health psychology.

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AS PART OF a broad movement within psychology, over the last decade we have seen an increasing turn towards the use of qualitative research within health psychology. This is readily indexed by the increasing number of published articles in health psychology journals, although this turn is not evidenced by all the leading journals in the field, and there is some resistance to the acceptance of qualitative research as legitimate in the field. As an example of this, a few years ago I tentatively sent a qualitative article to the editor of a leading health psychology journal with the somewhat audacious request that he give an opinion on whether it could be submitted there. He responded:

> Although I will not preclude you from submitting this article for review for possible publication in our journal, I must say that for several reasons I do not feel that its chances of acceptance after being sent to expert reviewers would be very good. First, although our readership is open to qualitative research methods, [journal name] is an empirical journal primarily interested in scientific issues relating to health and behavior. This in my view does not negate qualitative or other descriptive methods, but for this journal such methods should be in the interest of an empirical, scientific approach to problems. Secondly, although I realize that the methods you describe are often widely used in medical sociology and seldom (if at all) used in health psychology, this is for good reasons which have to do with the difference in methods, emphasis, and assumptions of these two disciplines.

The deconstruction was not difficult and the article travelled elsewhere.

However, it is not my intent to discuss the methodolatry of quantitative health psychology, although I will refer back to it occasionally. Nor do I want to go into the reasons for the turn to qualitative research in health psychology here, but it is refreshing to see health psychologists opening up new paradigms, raising a whole new set of questions and research issues focused around health and illness, and questioning how we can understand things psychological within that domain. This turn also raises fundamental questions about the meaning of psychological issues and how they can be investigated. To this extent health psychology is altering to keep pace with changing notions of what constitutes research, both in other areas of psychology and in other disciplinary domains of research into health such as sociology, anthropology and nursing.

Rather, I want to concentrate on the issue of methodolatry within the qualitative field. Although this term has not been used widely, it is not new. Romanyshyn (1971) commented on the consequences of privileging method over meaning in psychology, and Koch (1981) deplored the emergence and consequences of 'method-fetishism' in the discipline. Danziger (1990), in his history of the origins of psychological research, argued that 'preoccupations with the purity of method frequently deteriorate to a kind of method fetishism or "methodolatry"' (p. 5), and devoted a chapter to the move from quantification to methodolatry. Curt (1994) uses the term in a discussion of the special status given to methods in psychology, and the separation of methods from topics in research. In the qualitative arena, methodolatry was defined by Janesick (1994) as:

> a combination of method and idolatry, to describe a preoccupation with selecting and defending methods to the exclusion of the actual substance of the story being told. Methodolatry is the slavish attachment and devotion to method that so often overtakes the discourse in the education and human services fields. (p. 215)

Methodolatry in this sense clearly applies to the practices of health psychology today. In fact, methodolatry is characteristic of most psychology, and psychology has been overly concerned with methodology almost since its beginnings. Historical reasons for this relate to the dominance of behaviourism, a strong emphasis on being objective and the pre-eminence of measurement (see Danziger, 1985, 1990). The response of the journal mentioned above to qualitative research makes this point clearly. But we should note that throughout psychology's short history there have always been discontents with this focus. This can be succinctly captured by a reaction to psychological testing I came across many years ago:

> This business of intelligence testing reminds me of the way they weigh hogs in Texas. First

they balance a plank over a large rock, and then they place the hog on one end of the plank. Next they search around for stones and place them on the other end of the plank until they exactly balance the weight of the hog. Then they stand back and guess the weight of the stones.[1]

Within health psychology, as Michael Murray and I have argued elsewhere (Murray & Chamberlain, 1999), this focus on methods is enhanced by the location of health psychology within a medical context, or as Spicer (1995) has referred to it, as situated in occupied territory. This location tends to promote notions of objective reality, detached researchers, prediction and control, the measurement of atomized 'psychological' variables which can be unproblematically combined in studies with biomedical variables, alongside mind/body separation and a dislocation of the physical body from the social context.

So, it is my contention that when health psychologists move into qualitative research, they bring with them a strong tendency towards methodolatry, an overenhanced valuing of methodology. Within a quantitative framework, this tendency to privilege methods has a range of consequences (Spicer & Chamberlain, 1996). Within a qualitative perspective we find many of the same concerns arising (although they have different contributing factors and somewhat different outcomes), along with some new ones that are specific to qualitative research.

However, some caveats need to be raised before I continue. Although I want to consider a number of issues arising out of methodolatry, I do not want to suggest that there are no established, sophisticated users of qualitative methods in health psychology, as this is not the case. Rather, much of this argument is directed towards issues that surround people moving into qualitative research, struggling to find a way to do this competently and professionally and to produce high-quality research contributions. Further, a good deal of this arises from a reflection on my own progression from a tough-minded experimental psychologist who became interested in health psychology and, with a good deal of difficulty and tentativeness, adapted to became a tender-minded qualitative researcher: making the move from a 'prickly' to a 'gooey' as

a colleague would describe it. The developing interest in the use and application of qualitative methods in health psychology means that many researchers are making this transition, and implications around the use of these methods are important, more so because health psychology has engaged in only limited discussion of these issues to date. Consequently, I argue that within qualitative health psychology research there is an over-ardent concern with methodology, a privileging of methods over other considerations and a number of consequent problems with how such research is conducted. These are discussed in turn below.

An overemphasis on locating the 'correct' or 'proper' methods: the canonical approach to methodology

This is fundamentally what methodolatry is about, and it can operate at a variety of levels. Most obviously, it appears in concerted efforts on the part of novice researchers to find the 'right' way or, on the part of practised researchers, to criticize and demand 'good' practice. As an example of the latter, Wilson and Hutchinson (1996) in an article entitled 'Methodological mistakes in grounded theory' described a period when the 'methods police' serving as reviewers for articles demanded that any grounded theory published must include a visual diagram locating the categories and their relationships, and also 'proclaimed that a sample size of less than 12 is unacceptable' (p. 123). Admittedly, Wilson and Hutchinson were arguing against the rigidity of these rules as being contrary to the spirit of creativity underlying grounded theory methods. However, in doing so, they note that these 'cooked-up' or invented 'rules' were proposed by 'translators', people who have had 'no first-hand contact with grounded theory's originators or their students' (p. 123). From this we can deduce that there are 'proper' ways to be apprenticed and to learn how to conduct 'correct' grounded theory. In another section of the same article, they lament the 'muddling' of qualitative methods, and while conceding that there is methodological overlap between different methods, comment that the problem of muddling methods arises when the 'canons of a method are compromised'. So the

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inexperienced researcher is given the message that there is a 'right' way. It is possible to locate other examples of methods policing in respect of other methodologies.

At a different level, this issue is involved with the whole idea of what constitutes *qualitative* research. At some recent health psychology conferences, I have heard people presenting quantitatively oriented papers state that they have also collected qualitative data. What this most often turns out to be is the addition of one or more open-ended questions to the end of a questionnaire, or it perhaps involves the conducting of interviews with a small number of their participants. This material is organized in some form, usually examined rather perfunctorily, and 'themes' (which confirm the quantitative findings, but which are said to add depth, context, or some such) are presented as an adjunct to the 'main' findings. Qualitative research is more than this, but the term is used so broadly that it is possible to include almost anything under this heading that does not involve the statistical analysis of numbers.

I want to suggest that these people, in keeping with the religious nuances of the methodolatry argument, are in fact *agnostics*—they do not really understand what qualitative research is, and have little or no interest in finding out—they are merely dressing up and coming to church so as to look good to their community! They contrast with the idolatrists (methodolatrists) discussed earlier who are the *fundamentalists*—the literal interpreters of the one true way, the followers of the canons and commandments of *the* method. The methodolatrists that Wilson and Hutchinson are concerned with in their muddling critique are the *charismatics*—the users of an eclectic approach who claim that it is possible to draw on any methods or combination of methods unproblematically. In fact, I think these last people can be troublesome, but I do have some sympathy for their argument provided it goes beyond the methodological level. Finally, this raises the question of who are the *heretics*? Obviously, this title should fall to the poststructuralists and postmodernists.

So, what happens to new researchers trying to find their way into qualitative research? One issue here is that often the department in which they work, or their mentors or supervisors, will have a preferred set of practices. In many psychology departments this is commonly discourse analysis. Stepping outside psychology, almost all nursing research appears to be focused on the use of either grounded theory or phenomenological methods. So our new researcher may be directed to a method, or encouraged to use a method because it is 'what we do here'. Leaving aside this issue as a problem, it would appear, at least superficially, to be a relatively easy task to get on and find out how to use this methodology. In practice, it turns out to be much more complicated than that. If the choice was grounded theory for example, we find that there are debates within the field as to what is 'proper' grounded theory: the founders of grounded theory have fallen out, and there appear to be at least two versions: Strauss's version (Strauss & Corbin, 1990) and Glaser's version (Glaser, 1992). Reading further will locate other versions (e.g. Schatzman's [1991] dimensional analysis) and further debates about appropriate practice.

If our new researcher chooses discourse analysis, arguably the most dominant method in psychology, he or she can readily discover how to do it. Potter and Wetherell (1987) offer us a chapter in *Discourse and Social Psychology* entitled 'How to analyze discourse'. This discusses 10 stages in the analysis of discourse. Reading this and expecting to find clear directions, however, leads to disappointment—as prescriptions, these are empty, and Potter and Wetherell conclude their chapter with the comment:

> It is important to re-emphasize that there is no method to discourse analysis in the way we traditionally think of an experimental method or content analysis method. What we have is a broad theoretical framework concerning the nature of discourse and its role in social life, along with a set of suggestions about how discourse can best be studied and how others can be convinced findings are genuine. The ten stages we have outlined are intended as a springboard rather than a template. (p. 175)

Our researchers could instead turn to Parker (1992), who in *Discourse Dynamics* outlines seven criteria and three auxiliary criteria for distinguishing discourses. They are no further ahead here than they were with Potter and Wetherell's commentary, if what they were seeking was a prescriptive method. If they read

further, they will find not only tension between these two methods of discourse analysis promoted in psychology, but also the suggestion (e.g. Stainton Rogers, 1996) that there are other varieties out there as well.

If our beginning researchers were to take up phenomenology instead (or even in despair) they will find themselves little better off. They may feel that the seven steps for phenomenological analysis suggested by Collaizzi (1978) provide a sufficient guide, or they may prefer the six steps promoted by van Manen (1990), or again they may prefer to follow the guidelines suggested by Smith and colleagues (Smith, Flowers, & Osborne, 1997; Smith, Jarman, & Osborne, 1999) on how to do interpretive phenomenological analysis. Or they may stumble over phenomenography (Barnard, McCosker, & Gerber, 1999) and wonder how that is different—it is alternatively labelled empirical phenomenology—and if it would suit their purpose better. Once again, if they dare to read more widely, they will find debate and argument. Should their phenomenology be related to that derived from Husserl or the hermeneutic form proposed by Heidegger? Or can they use both in a layered way as proposed by Anderson (1998)? They may be led to consider whether the modern form of phenomenology used in psychology is truly phenomenological, a point that Crotty (1998) argues. Or even, is phenomenology a methodology at all?

The point of all this is that methodolatrists do *not* have a clear canonical path to follow, regardless of the method of choice. Even the most heavily prescribed methods, such as grounded theory, do not provide the guidance and reassurance that the fundamentalist methodolatrist seeks. This is fortunate, not a failing, as researchers must be free to develop and apply methods that are appropriate for finding answers to the research questions under consideration, and they should not be constrained in a methodological straightjacket. Further, the dominance of concern with methodology produces a range of concomitant problems, which are considered next.

A focus on description at the expense of interpretation

One of my concerns is that new researchers *do not* find their way. What they often do is find some methodological prescription (e.g. Collaizzi's seven steps for phenomenological analysis) and, believing they are following an agreed method, go on to produce a listing of thematic categories, under which they describe what was said by participants and illustrate it with example quotations. So, while they may think they are producing a phenomenological account, they offer a description of what was said rather than an interpretation that provides insight into the experience. Wilson and Hutchinson (1996) offer a related complaint about grounded theory publications, calling this failing 'premature closure'. To illustrate this, they quote an example from a study of men recovering from a heart attack, where the account was organized around key processes labelled 'disintegration of identity', 'dealing with broken identity', and 'reconstructing identity'. They rightly argue that this account is 'overly generic' and could be applied to any chronic illness or traumatic experience, and suggest that it does not 'contribute to theoretical knowledge of a specific substantive area' (p. 124).

Some time ago, I reviewed a manuscript that offered an examination of the experience of a particular chronic illness. The authors (reasonably) critiqued the previous quantitative research for, among other things, the way in which it dealt with coping: namely, as a list of strategies that could be itemized and used as necessary, and for avoiding the process involved in coping. Their phenomenological account would overcome that. They went on to produce a set of headings under which they described the talk of participants, and under a coping heading they enumerated a considerable list of differing strategies that their respondents described finding useful. They gave no recognition that they had fallen exactly into the position that they had initially criticized. Worse, they failed totally to convey the experience of coping through this presentation. However, they had achieved their goals of *categorizing* and *illustrating* the talk of the participants.

This is one of the commonest outcomes of the focus on method: methods produce data, and

data must be worked over and presented. It is easy to carry out an analysis that remains at a descriptive level (and it may also feel somehow closer and truer to the method—after all, much qualitative research claims this as a characteristic). Content analysis is certainly easier than interpretation, but it is usually not what we seek. Rather, we should be valuing interpretation over description. Interpretation takes us beyond description—answering the *how?* and *why?* questions, providing us with more than an account that tells us *what?* (or worse, merely categorizes what was said). P. Becker (1993) suggests a useful differentiation between working in descriptive mode and working in discovery mode: in the latter we might describe, but we go beyond that to offer an account that tries to 'get inside' what is going on and to represent it for readers. We try to connect the themes or categories and account for their interrelationships, we try to delineate the functions of discourses or how narratives are framed and build an interpretation of what is happening.

I do not want to suggest that we should routinely belittle description, as in 'it's *only* a descriptive study'. Description can be useful, and certainly has its place if your research question calls for that level of analysis. My lament is that so much research tends to stop prematurely at this level, a level that Donmoyer (1990) refers to as 'medium-rare'—'low-inference descriptions of behavior and excerpts from transcribed interviews' (p. 196). The researcher has 'cooked up' the raw data (to continue the metaphor), but insufficiently. This also creates a methodological paradox: we end up with a thematically described categorization of talk, almost without regard to the nature of the method used in the research. In contrast, developing a good interpretation requires thought and creativity, and its outcomes should be provocative and insightful. Codified approaches to method and analysis have a particular problem in capturing and presenting this. Further, we have great difficulty in communicating these notions to our beginning researchers. This is why Potter and Wetherell (1987) suggest that doing discourse analysis is somewhat like riding a bicycle: it is difficult to do, needs quite a lot of practice to get it right, and is hard to explain to someone but easy to carry out once one is accomplished at it. The *art* involved in analysis is not readily prescribed. Why so much of our research stops at the descriptive level is, I suggest, linked to two further issues: evaluating quality and the role of theory in qualitative research.

The evaluation of research quality

In evaluating the quality of qualitative research, issues of reliability, validity and generalizability arise. These are all highly contentious issues within the field, and different answers are offered depending on where one stands. What they do represent, certainly, is another quagmire for our beginning researcher to fall into.

As psychologists, we cannot help but be well versed in the diverse terminology of validity: external, internal, instrumental, theoretical, criterion, concurrent, cross-validation, consensual validation, conclusion validity, ecological validity. We are also familiar with the impossibility of separating the issues of validity, reliability and generalizability clearly and cleanly. It is also interesting to reflect back on the history of validity, which, in the beginning was linked specifically to psychological tests, then progressively extended to the results from psychological testing, then to measurement data, to research data, and finally to research itself. So today it is possible to talk about validity at a number of levels: as obtaining accurate data; as evaluation of data already obtained; as drawing valid meaning or interpretation from data; and as validating theoretical ideas.

However, although it has its roots here, the concern with validity in qualitative research extends well beyond psychology, and is perhaps as strongly driven by the applied social sciences such as education and nursing, which have whole-heartedly adopted (and simultaneously contributed to the problematization of) concerns with validity. Sometimes the traditional terminology has been retained, but more often new terminology has been developed in an attempt to retain notions of quality but to leave behind the empiricist connotations involved. So we find suggestions that, as qualitative researchers, we should be concerned about such things as trustworthiness, authenticity, saturation, meaning-in-context, recurrent patterning, and so on. Guba (1981) explicitly linked his four 'new' aspects of trustworthiness to the earlier

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concepts, suggesting that credibility was roughly equivalent to internal validity, transferability was equivalent to external validity and generalizability, dependability was equivalent to reliability, and confirmability was equivalent to objectivity. These new terms have subsequently been loosened considerably from these traditional links, and there are copious discussions of these issues and plenty of suggestions presented for adoption in the literature. A more extreme (and highly readable) example is provided by Wolcott (1990), who discusses some of the dilemmas raised by the search for validity and concludes that he has no place for it in his ethnographic research. He does, however, want to have criteria for evaluating his work and suggests that qualitative research may be better to set *understanding* as its goal.

However, I do not believe that psychologists will surrender the notion of validity readily, and this is particularly true of health psychology, given its history and its links into the biomedical domain. We will continue to require some form of 'validation' to warrant and legitimate our research and retain our acceptance as 'researchers' (even if not as 'scientists'). Also, we need to deal with this at one more level than our quantitative colleagues. Unlike them, we do not have the advantage of codified methodology so we do need to explicate how we collected and dealt with our data—the aim being to demonstrate the care, thoroughness and professionalism involved in our practices. Beyond that we need to offer an interpretation that is informative, compelling and provides something like Wolcott's understanding, or perhaps insight, into the issues under investigation. In the interest of brevity I have avoided discussing reliability and generalizability here, but both of these are equally problematic, involving a variety of meanings and applications, and similar arguments could be raised. For example, it is still all too regrettably common to find a qualitative study, working with a small 'n', apologizing or warning that the findings may not be applicable to other situations or to other groups.

These areas demand more work, but we must recognize that our historical legacy in relation to them, alongside the tendency to methodolatry, can constrain our qualitative research practice. As Denzin and Lincoln (1994) note, the perspective the researcher brings from his or her disciplinary framework or 'interpretive community' leads to particular views of the studied 'other' (p. 11). I think this is a particularly pertinent observation for most health psychologists. These are complex issues and are not easy to work your way through, especially in their current state of development. We need to recognize differences, not only from quantitative approaches but also between qualitative approaches, and develop better understandings of how to legitimate quality in our research. However, in doing this we need to extend the traditional boundaries that we are in danger of bringing with us: namely, that good methods and measurement make good research. We need to promote research quality in a much broader way than this. This *is* attempted by a lot of writing on this topic, but I suggest it is frequently drawn on too narrowly by health psychologists in the presentation of their research. While we may desire and value 'good' method, we should also desire and value 'good' interpretations and such things as the 'catalytic validity' (as Stiles, [1993], called it) of research that changes understandings and practices in the field.

The avoidance of theory

The second reason that much qualitative research rests at the descriptive level involves a failure to think theoretically—the flight from theory as I think about it. The dominance of method promotes obtaining and analysing data as the primary objective of research, rather than thinking about the phenomenon under investigation and how it should be theorized. Again, the legacy from traditional research and health psychology can be felt. The strong focus on selecting and operationalizing variables, dividing the person into psychological entities and giving these entities agency, influences qualitative research as well. Theorizing, in quantitative health psychology research, most often becomes model-building or 'cognitive algebra' as Stainton Rogers (1999) labelled it. This process of enclosing variables in boxes and connecting them with causal arrows has been described as the 'pathology of flow-charting' (Spicer & Chamberlain, 1996) and mitigates against considered theory construction in a number of ways (see also Stam, 2000). Although flow-charting is not much in evidence in

qualitative research (grounded theory being an exception), much published research with its focus on coding tends to atomize the person, separating them from their context. Themes can become dangerously like variables, discourses can be identified but never connected to their function, narrative typologies can be differentiated but without any consideration of the work they are doing.

Another issue that arises for qualitative researchers concerns the role of theory in research: whether research is a means for the verification of theory or for the derivation of theory. Wolcott (1990) labels these as 'theory-first' and 'theory-after' respectively. A common position is that theory-first (verification) is associated with positivist quantitative research, and theory-after (development and derivation) is what qualitative research is useful for (or *all* it is useful for, some would argue!). The theory-first notion carries with it the concept of hypothesis testing but, apart from grounded theory, this is usually dismissed as inappropriate within qualitative research. However, it also implies going into research with some theoretical ideas or presuppositions, even if these are implicit. On this issue, accounts of qualitative research can offer a further set of confusions to our beginning researcher, as various procedures are proposed. Grounded theory advises/prescribes that literature should *not* be read in advance so that the researcher is a *tabula rasa* on the topic. Phenomenology promotes bracketing (explicitly trying to recognize and put aside assumptions) in order to avoid this, and there is substantial discussion in the literature about reflexivity—how the researcher relates to the research and the researched, and how that shapes the interpretation. As *methods* these do not aid theorizing (e.g. I must be reflexive, which means I must describe what I am); as *concerns* they immediately begin to do so. Taking reflexivity seriously and considering how it shapes the interpretation inevitably forces attention to what is being constructed and offers some incentive for a move to a theorized account.

These pressures also take us more into the theory-after domain (although they also make us recognize the problematics of the before/after binary), and raise general concerns as to how accounting for the phenomenon is influenced by the values of the researcher. Extending this to consider how the phenomenon is locally situated, and shaped socially, culturally and historically, should provoke a more theorized account to emerge (see also Stam, 2000). However, a beginning researcher steeped in empiricism and positivism may prefer to withdraw from this and avoid the whole issue of reflexivity, a position that is less cognitively complex and dissonant.

A further pressure from qualitative research discussions can contribute to this—much that is written suggests that we should remain close to our data. Contrarily, Richards (1998) in a discussion of this issue suggests that closeness to data is overvalued and distance from data can be desirable. She comments:

> Getting closeness to data (or at least feeling you got close) is much less often a problem for researchers than getting distance. The most common problems researchers face when 'in' data is getting out of it again. (p. 322)

She suggests that distancing ourselves from data is difficult because: (a) we start close, often having some involvement or experience of the issues we are researching; (b) our data-production methods promote this—we come to know our participants, and we develop some allegiance to what they have told us; and (c) methods promote this—in proposing that we be able to offer fine distinctions, detailed descriptions, a thorough exploration gained through careful reading and rereading of our transcripts.

Richards (1998) terms the analysis most often created out of closeness as a 'garden path analysis' where:

> the researcher walks through the sometimes massive volumes of coded and retrieved data, commenting on and admiring each slightly different item or vista . . . The better we are able to describe the detail of the data, illustrating each different theme with appropriate quotes, the less concerned and less curious we may become about the task of synthesis. (p. 324)

Remaining at the descriptive thus works to avoid the theoretical. Considerations of reflexivity, sociocultural location, the legacy of empiricism and the use of theory can all function to enable a theorized, interpretative account that goes beyond the descriptive. As Stam (2000)

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argues, theory is not a luxury, although too often psychologists act as though this were the case.

Avoidance of the critical

A focus on methods raises a further important issue. Methodolatry, as I have argued, works to prevent us looking at the assumptions behind our research. Given this, it is not surprising to find that health psychology researchers give very little attention to the ideological base of their endeavour. Although some health psychologists have raised concerns of this nature (e.g. Marks, 1996; Stainton Rogers, 1996), they are few and far between. The debates surrounding these issues have been substantial in social psychology but appear so far to have had only a minor impact on health psychology. This is surprising given the degree to which social psychological theory and research provides a framework for health psychology. One reason for this is that in the turn to qualitative research, we have been overly concerned with *how to do it* (methodolatry). Concentration on these aspects has prevented concerns about the *implications* of what we do. Our beginning researchers may poke their noses into some readings on critical psychology and find that they should be undertaking research that is concerned with challenging psychology's individualism, scientism and inherent conservatism. Further, they may discover that they should be offering a conceptual critique of the philosophical underpinnings and assumptions of psychology, along with a political critique of psychology's socioculturally regulative role in maintaining social order and reproducing oppressive practices of power. This understanding challenges and problematizes the taken-for-granted practices that constitute so much of health research, and our beginning researchers have enough on their plate without wanting to add to their difficulties by engaging in the sort of perspective shift that this demands. Besides, it could be argued that it is possible to do good research and present it successfully for publication without engaging with this material. What this means is that a critical perspective continues to remain *external* to our understandings of research, even of most qualitative research, and there are plenty of directions to take without becoming involved with it. Until we have more debate and the critical is made more central in health research, I suspect that avoidance of the critical is likely to persist.

Taking a stance

Behind all of these issues and fundamental to them is the fact that the focus on methodolatry often presumes, or more often ignores, issues surrounding the philosophical position of the researcher: issues of where one stands in relation to ontology and epistemology, or 'O&E' as a graduate student neatly labelled this 'stuff' recently. Once again, for our beginning researcher, it is easy to see why this is ignored: these issues require a major mental relocation from, or dislocation of, the positivist viewpoint. Such a change is not made easily, and it is much less worrying to concern oneself with the correct details of method and methodology than to tangle with the philosophy of science. One can take heart from comments like that of Howard Becker (1993), claiming that if we haven't been able to settle epistemological debates definitively in 2000 years then we are probably never going to settle them, and that we should leave concerns about the philosophy of science to the people who adopt 'philosophical and methodological worry as a profession' (p. 226). This may be useful to justify the avoidance of O&E, but as we all know, and as Becker went on to point out, researchers always have a position on this even if it remains implicit. I fear that the underlying reason for keeping away from O&E in many instances is that a conventional focus on methods tends also to keep us focused on some notions of uncovering reality—that reality is out there to be discovered, albeit incrementally. To countenance ideas that reality is not quite as solid as we tacitly assume could be disturbing. Much of our published qualitative research continues to be flavoured with implicit positivist (and empiricist) assumptions. This is not a new problem for psychology by any means and was noted by Koch (1981), who suggested that psychologists were 'epistemopathic' (p. 258). I would go further and suggest that many qualitative health psychologists appear to be epistemophobic. This epistemophobic position is enhanced by many qualitative methods texts and articles, which present O&E-less discussions on methods. Qualitative research is beginning to develop its own methods cookbooks, which are,

like their cousins in the quantitative realm, O&E-free zones.

Looking ahead

In summary, I have essentially two major concerns: that our research is too anchored in a positivist legacy; and, related to this, that the focus taken in planning and conducting most of our research is largely in the wrong place. To overcome this I believe we need to do three things: first, promote more debate and discussion around these issues; second, promote a different, strategic, focus on research training and activity; and third, through activities like these, provide more guidance to those entering the field.

It is ironic that, given the reaction of qualitative research against notions of variables, measurement and statistics, we continue to perpetuate many of the problems from the quantitative research arena through a focus on and privileging of method. Methodolatry promotes security of a sort, but for the reasons I have discussed, it is a false security. Not only that, it can lead to inflexible and inappropriate guidelines for conducting research in a domain where no codified practice is readily available, and where in fact *all* practice tends to be contested and debated. Much of my comment has been centred around the beginning researchers—the people trying to make sense of qualitative research as they make their initial forays into the area. I suggest that practised researchers have a dual responsibility to improve this situation, in their roles as *publishers* of material, as authors of journal articles and book chapters; and in their roles as *mentors*, reviewing manuscripts and supervising students.

In the former role, I believe that we need to promote more commentary and debate centred around these issues. I acknowledge that some journals in health psychology have been supportive of publications of this nature. However, the amount of discussion has been limited and we need more debates within health psychology about the relevance of these issues for our research into health and illness. The mentoring role can be more problematic. Manuscript reviewers should work to enhance qualitative research publications in health psychology, pressing for more theorized accounts, for O&E

assumptions to be made more explicit, and to promote interpretation over description where that is relevant. Supervisors and research project coordinators face a more explicit dilemma. Should they be expected to be specialists, knowledgeable about particular approaches (e.g. discourse analysis), or generalists, knowledgeable about qualitative research at large? I do not really believe that the latter is a realistic possibility, given the diversity and flux within the arena of qualitative research. However, we need to seek a balance between these positions, recognizing the limitations of being overly specialist alongside the impossibilities of being a true generalist. Whatever the balance, I am certain that it should not be dependent upon *methods*. Knowing about methods certainly has value, but we should never lose sight of the fact that methods are *tools* rather than ends in themselves.

Further, I also want to suggest that we should adopt a more strategic process to research planning that places methods in their rightful context. Denzin and Lincoln (1994) discuss qualitative research as a process involving different phases, and in doing so offer an organized, sequential structure for planning and conducting research. They distinguish between five phases that constitute a research project, each with a different emphasis and goal:

- the researcher as a multicultural subject, involving reflexivity, ethics and politics of research
- theoretical paradigms and perspectives, or O&E considerations
- research strategies, identifying the research methodology
- methods of collection and analysis
- interpretation and presentation, including considerations of quality.

Thinking about research in this framework locates method as subsidiary to more fundamental and important issues.

Crotty (1998) in a related, but perhaps more functional, analysis, suggests four questions as basic elements of the research process:

- what methods do we propose to use?
- what methodology governs our choice and use of methods?
- what theoretical perspective lies behind the methodology in question?

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- what epistemology informs this theoretical perspective?

Crotty notes that these issues are often combined in a confusing way in many texts, and argues that it is important for them to be identified and separately considered in turn. The appropriate sequence is the *reverse* of the order stated above, as each level serves to constrain the one below. Considering these issues in turn, and in the appropriate sequence as suggested, should ensure the demise of methodolatry. Deciding on the epistemology (e.g. constructionist) prior to selecting the theoretical perspective (e.g. phenomenology or feminism) prior to choosing the methodology (e.g. grounded theory) and then the specific methods (e.g. focus groups) puts methodology and methods firmly in their place. Further, adopting a strategic approach to qualitative research planning of this sort should ensure that O&E considerations become explicit and promote a more theoretical approach to research.

Finally, in presenting these arguments I recognize that I have constantly fallen into a trap—of talking about qualitative research as though it was an entity while simultaneously claiming that it is diverse, contradictory, and should be so conceived. I hope that in responding to these arguments you will allow me licence for this and agree that these arguments are important, albeit in different ways for different qualitative orientations, as we struggle to overcome the dominance of notions that guessing the weight of stones tells us much at all about hogs.

Note

1. Unfortunately, I have been unable to locate an accurate reference for this saying and can only quote it from memory. I am also uncertain as to the attribution, but suspect it may be John Dewey. If anyone knows this saying I would appreciate having an accurate reference to it.

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