

Arguments for 'British Pluralism' in qualitative health research

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Aims of the study. This paper examines the argument that certain qualitative research methods can be used in 'pure' forms. Whilst rigid adherence to particular published procedures might be possible, we argue that in many cases this is neither necessary nor more likely to increase the validity of the research outcome.

Methodological purity. In examining the works of well-known claimants to particular research approaches such as grounded theory and phenomenology we show that purity of method is uncommon. In particular it is possible to demonstrate that all published qualitative methods are subject to their own underlying relativist philosophy. The implication of this is that all are social constructions and that their execution will necessarily be negotiated in time and context.

Conclusion. We conclude that analysis of varied examples of qualitative research shows methods to be more flexible than is often admitted. What we describe as 'British Pluralism' is an attempt to accept this reality whilst maintaining rigour through integrity, clear accounts, reflexivity and constructive critique of one's own work and that of others.

Keywords: ethnography, grounded theory, method slurring, phenomenology, pluralism, British Pluralism, qualitative research, validity, health care, nursing

Introduction

It is clear that there is quite a lively debate still current about the extent to which research methods, and the various qualitative ones in particular, can or should be mixed, combined, triangulated or otherwise used concurrently, wholly, or in part. This is not merely a 'theoretical debate' or we would let the matter rest. Rather, from time to time we have found research grant applications, research reports and students' dissertations foundering on the rocks of the 'purism' of examiners, reviewers and referees.

In this paper we will put forward an argument for what we describe as 'British Pluralism' in qualitative health research. We will show that from both philosophical and pragmatic viewpoints such an approach is not only sensible, it is increasingly inevitable. What we term pluralism in qualitative research has been somewhat disparagingly labelled as 'slurring' (Baker *et al.* 1992). In our contribution to this increasingly important debate, we will first review some of the earlier viewpoints expressed in this area. We will then focus more particularly on the arguments which have been made for a 'pure' approach to qualitative research. Often nurses

using qualitative methods include a strong plea for purism, that is, they argue that nursing as a 'unique' area of human activity even requires its own 'nursing research methodology' or, as some term it, 'nursing science'.

We will then suggest that both in general and specific respects this 'purism', whilst often well-meant, is not founded on any *a priori* or logical principles. It is, perhaps, a misplaced view that absolute conformity to procedure guarantees rigour. This, we might suggest, is a position commonly retreated into by we nurses (in particular) who seek the security of procedures in many areas of our work. We will demonstrate that many of the best examples of 'pluralism' come from those who claim adherence to one perspective, one method, or another. We will go on to show that what we will call 'British Pluralism' has something to offer in the context of the search for flexibility with rigour.

Maggs-Rapport (2000) has recently argued that, although the combination of methods of data collection and analysis in single research studies is 'commonplace', little has been written about this 'mixing'. In our view, a number of commentators have had something to say, which we would not especially want to rehearse excessively here. We are aware, however, of a certain mythology surrounding the existence of 'pure' qualitative methods which we would like to put into perspective.

Maggs-Rapport describes, in effect, conducting two studies (one ethnographic and one 'interpretive phenomenological') in the same setting, and then drawing on both sets of data to derive conclusions. She argues that the justification was that nursing is a 'wholistic' discipline and that this form of triangulation, presumably because it presented a more complete picture, was therefore appropriate and led to an outcome more 'complete'. She seems to suggest that, rather than using bits of each, she used each approach with integrity but then took conclusions which were supported by data from both methods to be more credible.

A paper by Rose *et al.* (1995) illustrates an issue of especial concern. In the paper, ostensibly about *Academic rigour in the lived experience of researchers using phenomenological methods in nursing*, one of the contributors details the problem raised when her 'observational data' and her 'interview data' did not correlate. We can assume from this that the nurses did not in practice do what they said they did. Perhaps surprisingly, but at least honestly disclosed in the paper, the researcher decided to reject observational data as legitimate data for the study:

I decided to abandon my observation data because I felt a moral obligation to my participants not to disturb or upset them because they had shared with me something quite vital.

We are not sure that the real source of this problem is 'method slurring'. Had the researcher only collected observational data the dilemma would have remained: should she report it or not, given that it might 'upset' the staff? The example does throw into the relief, however, the potential for difficulty when rival accounts or explanations are possible as a result of research.

In an interesting paper, which has a bearing on this problem, and which examines the relations of theory, method and philosophy, Kim (1993) suggests that one of the strongest arguments (although not obviously held by herself) for sticking to a particular approach is to avoid the possibility of competing explanations of phenomena. She also argues, however, for consideration of 'applied' and 'pragmatic' approaches to research which might be less convenient in their singularity of explanation but will advance 'nursing science' further.

Opposing this sort of pragmatism, the noted Janice Morse (1991) is particularly critical of what she describes as a 'sloppy mishmash' by 'self taught' researchers such as (she claims) Swanson-Kauffman (1986). Swanson-Kauffman's paper describes how she produced a 'unique blending' of phenomenological, grounded theory, and ethnographic methodologies. Clearly taking a dim view of this Morse argues that:

Such mixing, while certainly 'do-able', violates the assumption of data collection techniques and methods of analysis of all the methods used. The product is not good science... (p. 15)

Of course Morse is entitled to critique the paper, the method and the conclusions. We would like to see more of this in general ourselves. But many of our greatest researchers (and those of us with less claim) were 'self taught' as she puts it, working from the texts. Taking a more constructive line, Layder (1991) discusses the close relationship that exists between the various qualitative methods, arguing for a broader conception of 'grounded theory' than that set out by Glaser and Strauss. Layder, a British sociologist, is really arguing for an eclectic method drawing on methods from a range of qualitative approaches and which also has greater relevance to wider socio-political issues than ethnography, phenomenology and 'grounded theory' currently do, in order to enhance both theoretical and practical relevance. It is to the arguments both for and against these views that we now turn.

Pluralism and qualitative research procedures

For early anthropologists and sociologists 'method' was not so important. They generally either read widely and wrote, or they spent years 'in the field' taking notes and then 'theorizing' or 'recording' dependent upon their point of view. As a reaction to critics, however, researchers growing out of these

traditions, notably the 'Chicago School', attempted to make 'scientific' that which had commonly been accused of being 'mere journalism' or even 'fiction'. As a result, a good deal of useful energy has been expended on the development of data collection and analytic procedures for qualitative research. From this process we have benefited greatly.

Foremost among reactions to the then over-emphasis on 'armchair theory' and 'verification', Glaser and Strauss (1967) *Discovery of Grounded Theory* suggested sophisticated analytic processes and, provided one came to grips with quite a dense text, gave fairly clear procedures and actions which, if written down, might be followed by others. These procedures, whilst still arguably interpretive (Stern 1994), were developed by Glaser and Strauss explicitly to defend their work against critics who had, they suggest, labelled similar work as 'unsystematic', 'impressionistic', or even 'sloppy' and 'unsophisticated'. In their response, Glaser and Strauss argue that different degrees of plausibility might be needed for different sociological tasks (p. 223).

Originators of other qualitative methods also appealed to detailed processes to establish the rigour of their work. Among those claiming to be 'phenomenologists', Colaizzi (1978) and colleagues developed relatively clear procedures. Indeed, Crotty (1996) suggests that the plethora of 'methods' for phenomenology reflects a lack of certainty among nurses using these approaches about what they are doing.

On the face of it, these procedures are there to help the researcher come to grips with a complex social world and the data which might emerge from its study. Using each method carefully and independently of its 'competitors' may make a good deal of sense. 'Mixing' the methods in one project has been attacked as 'method slurring' (Baker *et al.* 1992).

Arguments against 'slurring'

The arguments against mixing methods seem at first very sound. How can students learn rigour if there is no clarity about the method used? Holloway and Wheeler (1996) summarize the criteria developed over the years to account for rigour in qualitative research. Among the four concepts, they focus particularly on is dependability. Drawing on Koch (1994) and Robson (1993) they argue that this should allow a 'checking process' of procedures used. They go on to say that:

If these follow acceptable standards and are clear, then the study can be found to be dependable. (p. 168)

Of course the idea of standards is sound. Rigour must in any interpretation be achieved by adherence to explicit standards of research conduct. Unfortunately, in our view, the wish to 'follow acceptable standards' has been interpreted as

following rigid procedures in the research process. This might be made clearer if we consider the analogy of wound dressing technique. The 'standard' has commonly been seen to be the conformity to a published hospital procedure rather than the avoidance of cross infection by different and complementary means.

Where a general account exists of the procedures to be adopted in a data collection or analytic process, then conformity with these seems to make sense. This view is confirmed when we look at the fourth concept used to establish rigour, 'confirmability'. In establishing this to be present, Holloway and Wheeler again quote Robson (1993) in requiring that the 'design strategies and procedures used' (p. 168) should be examined. The important point here is that according to Robson a clear 'decision trail' needs to include conformity with published analytic procedures and methods.

In a widely quoted and influential paper, Baker *et al.* (1992) argued that no method is credible if used inappropriately. Examining in particular the examples of phenomenology and grounded theory they imply that rigour is seen to be a virtue of compliance with the letter and spirit of the published procedure. They suggest that the purposes and assumptions of the two approaches are sufficiently different for them not to be sensibly combined. As many beginning researchers have difficulty in coming to terms with, for example, grounded theory and phenomenology a lack of 'purity' is sometimes assumed to be a demonstration of intellectual weakness. Given that many research projects are carried out as part of the requirements for a higher degree, failing this test is particularly dangerous to students and their supervisors, and sometimes students' work is referred because it took too pluralist an approach.

Perhaps the most profound argument against method slurring in qualitative, or any kind, of research is that of philosophical incompatibility. According to Baker *et al.* (1992) phenomenology is designed to 'describe psychological realities by uncovering the essential meaning of lived experience' (p. 1357). By contrast, they maintain, grounded theory explains social or psychological realities by identifying processes at work in the situation being investigated. We might naturally ask here what method we should choose when the phenomenon being 'experienced' is a 'process', such as an illness trajectory. Indeed, most of Glaser and Strauss's early work on awareness of dying was of this nature. A case in point is Morse and Johnson's (1991) *The Illness Experience: Dimensions of Suffering*. Explicitly a collection of graduate students' work using only grounded theory methods (supervised by the editors), the book gives something of the game away in the title, emphasizing individual experience rather than an abstract social process. One of the papers

(Chassé 1991) examines 'The experiences of women having a hysterectomy'. Without getting into too much semantic argument here about supposed differences in types of 'psychological reality', we feel that such studies do not easily fit Baker *et al.*'s (1992) perhaps simplistic classification.

In limiting the 'slurring' of approaches in the one (qualitative) paradigm of research, then surely Baker *et al.* (1992) exclude automatically the 'mixing' of approaches even further apart than these, such as experimental research and phenomenology. This is perhaps unhelpful in a current climate, in the United Kingdom (UK) at least, in which there is a need to 'change the culture towards so-called 'nontraditional' methods for research and development such as action research and 'realistic evaluation' (National Health Service [NCCSDO] 2000).

In a vein of pragmatism, it remains the case that funded research in most countries has to have a quantitative element for the policy makers to have some confidence in it. A number of studies, such as a majority of those commissioned by the English National Board for Nursing, Midwifery and Health Visiting in the UK, now combine a qualitative aspect or case study with a national correlational survey with large numbers. Senga Bond, Chair of the 2001 Nursing Research Assessment Panel in the UK, has argued that:

I do not subscribe to the view that research has to be either/or: qualitative or quantitative, dealing with meanings or with externally defined events, analysing naturally occurring or research-generated data... I believe that what is important is the credibility of research, the amount of confidence we have in the findings and not the particular methodological tradition that underpins it,...There is strength in diversity, as long as there is rigour (Bond 1992, p. 95).

The combination of some qualitative approaches with some applications of quantitative method is still troublesome to us. This may not be because they are 'philosophically incompatible' as such, but because, in some cases, the large scale survey is attempting to measure an unmeasurable concept, or one which seems to us meaningless without context, such as anxiety. Certainly such combinations would still be unthinkable to strict adherents to the philosophical principles of social constructionism from which grounded theory was derived or of interpretive phenomenology as Heidegger is claimed to have described it.

If the combination of qualitative methods with quantitative ones is problematic for some, then surely the use of qualitative methods together will be tolerable? Well, not really. Philosophical differences which may seem great between, say experimentalists and phenomenologists must seem smaller and more capable of coexistence among qualitative approaches as a whole. This seems, however, not to be true.

How 'pure' is phenomenology?

Of all 'qualitative' methods, phenomenology has the strongest claim to be something 'pure'. Writing of the place of his phenomenology in the early pages of *Ideas: General introduction to pure phenomenology*, Husserl (1931, p. 27) suggests that:

Philosophy can take root only in radical reflexion upon the meaning and possibility of its own scheme. Through such reflexion it must in the very first place and through its own activity take possession of the absolute ground of pure preconceptual experience, which is its own proper preserve.

Given the name of the book and the relative clarity of this passage (compared with many in that work) we can see that Husserl meant phenomenology to be some kind of pure approach to the understanding of the essence of things. This was supplementary to the 'mathematical' forms of 'science' which, though once a mathematician, he had rejected.

If slurring, which we take to be the use of methods in a less than pure and perhaps even 'mixed' form, is potentially problematic, we ought to be able to find good examples of the well described research methods being used in their 'pure' form. Benner's (1984) *Novice to expert* is widely described, not least by herself, as an example of Heideggerian interpretive phenomenology. As Crotty (1996) argues, her influential and widely read book is in fact 'a form of social enquiry that is 'in continuity with the American intellectual tradition but owes nothing to the phenomenological movement.' (p. 130). The point is that not only, in Crotty's opinion, are Benner and most of her nursing research colleagues misreading 'phenomenology', their use and interpretation of its methods are so inconsistent as to require a different label ('new phenomenology'). In Crotty's view, these differences are not subtle and need explicitly to be recognized.

Darbyshire *et al.* (1999) have responded vigorously to Crotty's critique, attacking principally Crotty's interpretation of Heidegger's position. In this paper they spend little enough space on the specific criticisms of nursing scholars that are so much the focus of Crotty's book. The detailed points they make are clearly aimed at those claiming to be 'Heidegger scholars' but the fact remains that many and varied interpretations of his work exist, both among 'philosophers' and nurses drawing on this perspective. The first of these authors has been an advocate of Benner's for some time, and in the paper a number of claims are made about the high quality of research and scholarship in both Patricia Benner's and Nancy Diekelmann's work.

Perhaps chief of these is the claim that they overturned a tradition which viewed theory as superior to practice. This, as regards Benner, seems difficult to dispute at first sight.

Benner, describing herself as an interpretive phenomenologist, certainly was the first influential 'nurse theorist' to collect substantial data to validate her theory. Herein, though, lies the problem. In her most famous work, *Novice to Expert: Excellence and Power in Clinical Nursing (1984)* she clearly sets out with the Dreyfus and Dreyfus theory of the development of expertise in fighter pilots and attempts to validate it for 'expert nurses' by means of the purposeful collection of data. This alone renders the work something other than 'interpretive phenomenology' and reifies theory above practice in a way that other, more 'inductive', approaches to phenomenology do not.

Given that Heidegger, Merleau Ponty, Sartre, and colleagues have, however, modified and deviated from Husserl's 'pure' form (not to mention Crotty's long list of defaulters like Benner) it seems fair to say that as currently practised, phenomenology is no longer *pure*, whatever procedures and steps seem to be in use.

Surely we can find 'pure' grounded theory?

Grounded theory is well known to most nurse researchers and has a long history of use, not least in the UK. We believe that one of the first claimed uses was in British nurse Felicity Stockwell's (1972) *The unpopular patient*. Stockwell's earlier structured approach to data collection had proved of some value in identifying those likely to be unpopular. Her real aim, however, was to develop criteria of quality of care and so she pioneered an elementary form of constant comparison to analyse data she collected by nonparticipant observation. Like Kratz (1974) not long after, for practical reasons Stockwell took liberties with the method as described, not least because the data were analysed retrospectively rather than concurrently with their collection. As a result, theoretical sampling as we now understand it was not possible, nor could 'theoretical sensitivity' be developed to anything like the same extent. These researchers, though, broke new ground contributing to a new perception of these approaches as rigorous and useful.

On the mystery of 'theoretical sensitivity' even Glaser and Strauss (1965), in their early work using grounded theory in relation to 'awareness contexts', admit to being interested in communication with the dying as result of personal experiences. This, despite the officially 'open agenda' approach of grounded theory, they justify as being 'sensitized' rather than 'biased'. So how 'pure' is grounded theory? Even as early as the first text (1967) Glaser and Strauss were stating their general premise that (theory):

..is likely to be a better theory to the degree that it has been inductively developed from social research (p. 5)

whilst also saying that:

However, it is possible to formulate formal theory directly. The core categories can emerge in the sociologist's mind from his (sic) reading, life experiences, research and scholarship (Glaser & Strauss 1967, pp. 89–90).

Looking wider we might reasonably analyse all the theses and research reports written from an explicitly grounded theory perspective and conclude that each was carried out with greater or lesser emphasis on the approach documented by Glaser and Strauss (1967). The best evidence that there is no such thing as the 'right way' to do grounded theory comes from the authors themselves. Notoriously they fell out in the early 1990s when Glaser (1992) lost sympathy with what he felt to be the unduly 'procedural' emphasis of Corbin's collaboration with Strauss which resulted in *Basics of qualitative research (1990)*. Reading between the lines we can hypothesize jealousies, personal or professional, which cause this kind of rift in a truly great partnership, but the key point is that there is no 'final' version, no one approach which we 'must' follow to achieve rigour.

So what of ethnography?

We make no claim to the originality of our case. Rather we acknowledge that British sociologists Hammersley and Atkinson made similar pleas for 'naturalism' in ethnography in the 1980s. Despite the high degree to which their book *Ethnography: principles in practice* is influential, their plea for flexibility seems less well recognized:

A first requirement of social research, according to this view, then, is fidelity to the phenomena under study, not to any set of particular methodological principles, however, strongly supported by philosophical arguments. (p. 6)

Hammersley and Atkinson (1983) are clearly rejecting the idea of methodological purity, and a good number of UK ethnographers have indeed utilized a kind of pragmatic pluralism. Nevertheless we still find the subtle distinctions espoused by Morse and Field (1996) such as 'ethnoscience', 'qualitative ethology', 'ethnomethodology' and of course 'ethnography' troublesome to our students. Frequently the standard thesis methods chapter consists of students rejecting each of these by turns in a sentence or two but without really knowing why.

So the case rests: of the examples of the qualitative research traditions that we examined, neither phenomenology, grounded theory nor ethnography are in any sense 'pure' or capable of having their procedures rigidly adhered to, whatever the

claims of those who abhor 'slurring'. Furthermore, ethnography, which given its long history ought to be more pragmatic and 'natural' has itself been 'purified' by nursing academics like Morse and Field into artificially separate 'methods' with 'scientific-sounding names' like 'ethnoscience'.

Indeed, the relativist perspective which arguably underpins most of these approaches in various forms, is itself a threat to claims to 'purity'. Symbolic interactionism and interpretive phenomenology, to which Strauss and Benner, respectively (and many others) are claimed adherents, holds that all social phenomena are products of a negotiated reality within which many (or even infinite) versions of events or strategies for dealing with and interpreting the social world are possible. As such, 'research methods', especially those of the social and behavioural sciences, are flexible and mutable products of the imagination. They are not based on 'scientific laws' such as those of physics or chemistry. This, if it is a philosophical position worth defending, is an argument both for 'method slurring' and against the view that some claimed 'philosophical inconsistency' will result from the mixing of methods like grounded theory, phenomenology and ethnography.

Can reflexivity be an argument for pluralism?

Reflexivity has been seen recently as a means by which qualitative researchers can make their effect on the research more explicit, and in turn give some account of the study's effect on themselves, or more accurately, to make known the 'researcher's' differences from the 'researched'. Koch and Harrington (1998) have extended this analysis to include what might be called the politics of location, a deeper understanding of the context of the study and the influences upon it. This aspect of reflexivity, if this should be its name, certainly invites consideration of wider issues than the personal or the microcosmic. Qualitative methods like grounded theory and phenomenology certainly risk, in their current conceptions, ignoring the political and contextual explanations of phenomena (Porter & Ryan 1996).

For Pierre Bourdieu, reflexivity was more than this. It encompassed a whole 'critical attitude' within which the methods and theory of both oneself and one's colleagues should be subjected to constructive criticism. Bourdieu was 'viscerally opposed' to the dogmatization of thought and 'methodological and theoretical fetishism', to which much social and behavioural science and lately nursing research may be party.

For Bourdieu the precondition of true rigour is the reflexive critique of research techniques and procedures, which makes clear that none are in any sense permanent or perfectly

formed as the purists would have us imagine. He makes clear that there is currently insufficient criticism of most theories and methods. We would echo this, noting that nursing seems especially vulnerable to fads, fashions, and the 'Emperor's New Clothes' phenomena.

To summarize the Bourdieu (1992) position, first the primary target of the methodological and theoretical critic is not the individual analyst or theorist but the *social and intellectual unconscious* embedded in analytic tools and operations used. It must be a *collective enterprise* rather than the burden of the lone academic, which we take to mean that we all share a responsibility to develop the 'critical attitude' if the profession is to progress to true maturity. This attitude seeks to buttress, not to assault, the epistemological security of [in his case] sociology or, in our case, nursing knowledge.

Some implications of pluralist qualitative methods

Perhaps close to the hearts of some of our students, there will be no need to 'refer' dissertations where students draw upon different methods provided that they justify the combination or modification of methods. This is illustrated by Taylor (1993) whose paper is a justification that she felt it necessary to provide having combined methods which at first seemed unsuited. Where methods are in combination, such as 'participant ethnography' and 'phenomenological interviewing', there remains no less appeal to rigour according to acceptable understandings of validity, honesty, avoidance of deception and the provision of relevant evidence (Long & Johnson 2000). In our view, there remain both the researcher's and their colleagues' responsibility to avoid, insofar as it is possible, the otherwise common pursuit of hobby-horses and the grinding of axes. These are vices of the so-called 'purists' which it would be counter-productive to adopt. Some methods will be even more than usually impractical, or theoretically inconsistent, and hard to combine sensibly and in such cases reason must prevail. An important implication for graduate education is that programmes of associated study should be eclectic to the extent that those choosing to adopt a method are aware of its strengths, weaknesses and those of the approaches with which it might be 'slurred'.

Conclusions

The arguments for pluralism in qualitative research are to us overwhelming. They are epistemological, pragmatic and political. There are no 'pure' qualitative methods. Of course we do not deny the rigour which may be evident when the steps individuals take to collect, analyse and theorize from

data are made plain in writing in a report. But it must be clear from the outset that modifications of approaches and 'mergers' say between Crotty's (1996) 'new phenomenology', ethnography and grounded theory are not only possible but may add to rigour if well articulated.

Fundamental to this position is the view, to us self evident from any interpretive, interactionist, social constructionist or hermeneutic perspective, that there are no 'real' natural laws concerning socially derived knowledge and therefore no possibility for a 'pure' method for the social or interpersonal sciences. There is, we believe, no possibility of such a method unique to nursing which is the discipline most commonly making this claim. We believe that effective debate about methods, alongside our best efforts to implement and evaluate critically and consistently our approaches can only assist in the evolution of the human sciences in general and nursing in particular.

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