



Commentary

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The Editor writes:

Burnard and Hannigan (2000) returned to the hoary old chestnut of the ‘qualitative vs. quantitative’ methodology debate, in the first issue of the new century. They applied their argument to mental health (sic) nursing in general, referring to both therapy (sic) and research, concluding that ‘a synthesis of the two, apparently opposing, camps’ might represent a prudent way to approach the challenges within the field. Burnard and Hannigan’s argument has drawn four responses, which I publish here unedited. I hope that other readers will feel motivated to join in this debate.

Reference

Burnard P. & Hannigan B. (2000) Qualitative and quantitative approaches in mental health nursing: moving the debate forward. *Journal of Psychiatric and Mental Health Nursing* 7, 1–6.

One law for the lion and the ox . . . A reply to Burnard and Hannigan

Without Contraries is no progression. Attraction and Repulsion, Reason and Energy, Love and Hate, are necessary to Human existence (William Blake 1793).

I am always suspicious when I see the word ‘synthesis’ used in connection with research, particularly when it claims to ‘move the debate forward’, since in my experience the usual result of synthesis is to kill all debate stone dead. Fortunately for the future of debate, the recent attempt by Burnard & Hannigan (2000) falls at the first hurdle by rather missing the point, both of the earlier debate on which they are commenting, and also of just what they are claiming to synthesize. As they note, ‘the debate about differences between quantitative and qualitative approaches to research have been widely

debated in the literature’ (sic). One might almost say that they have been over-debated (see, for example, Field & Morse 1985, Burns & Grove 1987, Chapman 1991, Bond 1993, etc., etc.), and we have to ask ourselves at this point exactly what another contribution is likely to achieve.

So what exactly are Burnard and Hannigan proposing? They continue by invoking a respected social scientist: ‘Bryman argues that qualitative approaches can be discussed from the points of view of epistemology and philosophy *or* from the point of view of methodology’. This statement left me more than a little puzzled, since for Bryman (1986), ‘the term “methodology” . . . will refer to an epistemological position’. It would appear, then, that, methodology is *precisely* the epistemology and philosophy underpinning the research approach, so that Burnard and Hannigan’s distinction between methodology and epistemology not only misrepresents Bryman’s view, but is clearly self-contradictory.

Parahoo (1997) has pointed out that ‘sometimes readers confuse the terms ‘methodology’ and ‘method’, and this would seem to be what Burnard and Hannigan have done here. This suspicion is confirmed if we examine the title of Bryman’s paper: ‘The debate about quantitative and qualitative research: a question of *method* or epistemology?’ (Bryman 1986, my italics). This, of course, suggests that Burnard and Hannigan are actually making a very different claim: to argue for a synthesis of methods is very different from arguing for a synthesis of methodologies. The former used to be referred to disparagingly as ‘method slurring’ (Baker *et al.* 1992), but is now widely accepted into mainstream research practice (indeed, it is very difficult to find any ‘pure’ research that does not, to some extent, borrow methods from other approaches). However, a synthesis of *methodologies* does indeed offer a considerable challenge, since ‘the two traditions reflect fundamentally dif-

ferent epistemologies concerning the sort of knowledge about the social world which it is possible to achieve, and different philosophies as to the nature of man' (Walker 1985). Walker went on to observe that almost every social scientist since Weber has grappled with the problem, and none has yet succeeded in resolving it. Indeed, such a synthesis goes far beyond a simple mixing of methods, and is for many philosophers (e.g. Kuhn 1962, Wittgenstein 1953) quite impossible, since the two paradigms (for that is what they are) are based not only on different belief systems, but on different language games, a fact which Bryman himself acknowledged.

Unfortunately, Burnard and Hannigan shun the challenge of a methodological synthesis in favour of yet another synthesis of methods. In fact (as I pointed out above) the argument is won before it is even started. As they rightly but rather tritely note, 'people in both groups *do* similar sorts of things', leading to the conclusion that 'it seems reasonable therefore to attempt to suggest a sharing of methodologies'. Note the language: 'to *attempt* to *suggest*'. It would seem that something radical is being tentatively proposed, and indeed, a sharing of methodologies (that is, of philosophies and epistemologies) would indeed have been a feat worth writing about. However, they continue with an example:

It is quite feasible (and well recorded in the literature) to use quantitative methods of analysis on qualitative data. It seems equally feasible that quantitative researchers could insert sections into their instruments for self reporting and for the recording of qualitative data – and, of course, this is often done. (Burnard & Hannigan 2000, p. 5)

In fact, Field & Morse (1985) pre-empted Burnard & Hannigan's conclusion by 15 years when they pointed out that 'often the strongest research findings are in studies that utilise both methods', and similar sentiments have been repeated at regular intervals ever since. As Burnard and Hannigan themselves point out, we all know about combining methods and we probably all do it; even, I suspect, Kevin Gournay and Ian Beech (whom Burnard and Hannigan used as exemplars of the quantitative and qualitative paradigms). Unfortunately, Burnard and Hannigan fall into the very trap that Bryman warned against, by conflating the concepts of method and methodology, claiming that 'it is particularly interesting, in our

opinion, to consider how close together the quantitative and qualitative camps already are with respect to a number of these key issues'. We should not draw the conclusion, so readily accepted by Burnard and Hannigan, that Gournay and Beech are engaged in the same project, just because they both categorize data or because they both generalize, or even because they both include self-reporting elements in their research tools.

Clearly, Burnard and Hannigan have misunderstood Bryman's basic thesis. His claim was that two researchers could be totally opposed philosophically and epistemologically, and yet could employ the same data collection methods without contradiction. Burnard and Hannigan attempt to turn that argument back on itself and argue that because the researchers were using the same methods, then their positions must be closer together than they think; indeed, that they might even be synthesized. This, unfortunately, was the exact opposite of the point Bryman was making.

The real, fundamental and (I would argue) irreconcilable differences between the two camps lie in precisely the issues which Burnard and Hannigan deliberately chose to ignore: their epistemology and philosophy. The similarity between Gournay and Beech is rather like the similarity between a sign writer and an abstract painter (and I do not wish to belittle either). They are both concerned with communicating a message and they both employ similar tools and methods to do so, but that is where the similarities end. However, if we were to do as Burnard and Hannigan are doing, and focus on method to the exclusion of philosophy and epistemology, then we are forced to conclude, as Burnard and Hannigan do about researchers, that there is indeed little difference between the sign writer and the artist.

I do not wish at this point to enter the futile debate about which methodology is superior, except to take exception firstly to being placed by Burnard and Hannigan in the 'qualitative camp' (my gripe with Gournay was not with his methodology, but with his claim that it represented the gold standard of research), and secondly to being used to support their argument for a 'synthesis'. When I claimed that (to quote Burnard and Hannigan quoting me) '... what mental health nurses really need is a broad spectrum of knowledge which can only be produced by an equally broad spectrum of research methodologies', I was certainly not calling for a methodological synthesis. Rather, I was

appealing for methodological *tolerance* in the face of diversity and irreconcilable difference. Indeed, I was celebrating difference rather than attempting to eradicate it.

For me, the *really* interesting question is not about the similarities between methodologies, nor even about the differences between them, but rather about why some writers feel the need to deny those differences. Clearly, some writers and some researchers feel uncomfortable with loose ends and a lack of resolution. For some, the need for a single end is part of a personality trait (defect?) of neatness and completion, whereas for others it is necessary for political reasons, so that we are all pulling in the same direction. The French post-structuralist Jean-François Lyotard contrasted the 'philosopher' as one who exposes and accepts difference, with the 'intellectual' who 'helps forget [difference], by advocating a different genre . . . for the sake of political hegemony' (Lyotard 1983), and added that the notion of a compromise is, in any case, illusory, since in any dispute it is the side with the most power that wins all the arguments. The project of the intellectual, in which Burnard and Hannigan appear to be engaged, is therefore to perpetuate the status quo by suppressing minority voices in the name of synthesis or compromise. Synthesis therefore implies closure: if we can only get everyone to agree that we are all doing essentially the same thing, then we have not, as Burnard and Hannigan optimistically suggest, moved the debate forward, but shut it down.

Furthermore, Burnard and Hannigan claim that 'whilst polemic and polarization are stimulating and exciting, they can also lead to the entrenchment of positions and to "heroes" or "gurus" emerging'. This, surely, is a small price to pay for a little stimulation and excitement. And of course, what closure *really* does is merely to remove stimulation and excitement whilst replacing one set of heroes with another. For example, if the conflict in Northern Ireland is finally resolved, the heroes will not be the advocates of the entrenched positions on either side, but the arbitrators who hammered out the compromise. Perhaps Burnard and Hannigan see themselves as the next General de Chastelaine, or even as the next Peter Mandelson. Indeed, it might be instructive to extend the analogy: no one seriously believes that there will be a synthesis of republicanism and unionism into some new political movement. The debate will only move forward by each side accepting and tolerating difference

rather than by pretending that it does not exist 'for the sake of political hegemony'.

Burnard and Hannigan are, in my opinion, right in their conclusion that we should not try to use one approach to research at the expense of all others, but that is exactly what I was arguing in my original responses to Gournay (Rolfe 1997, 1998). However, the solution is not an *ad hoc* synthesis of methods into a quasi-eclectic and all-encompassing methodology, but a recognition that we are engaged in different projects with different ends, and that each has something valid but different to say. As Blake (1793) put it, 'One Law for the Lion and Ox is oppression', and it is perhaps foolish to suggest that we are able to 'move the debate forward' (forward to where; to a new (old) gold standard?) without oppressing some of the participants in that debate. Surely it is enough merely to ensure that different voices continue to be heard rather than to stifle debate in some illusory 'reconciliation and synthesis'.

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Synthesis and sterility: a reply to Burnard and Hannigan

Burnard and Harrigan's (2000) attempt to develop and resolve the debate on the relevance and need for qualitative and quantitative research in nursing seems to have misunderstood the point of the original debate upon which they were commenting. Their misunderstanding of the debate and the methodological confusion of their paper has been fully discussed by Rolfe. Therefore I will restrict my comments to my original contribution to the debate (Parsons 1997, 1998). This was to take issue with Gournay and Ritter's assertion that qualitative research was anecdotal and lacking in methodological rigour.

In 1997, I argued that qualitative research was an equal partner to quantitative research and that research that was correctly executed should not be seen as inferior simply because it has a qualitative basis. I contended that 'there are other questions which quantitative research does not address – fundamental questions such as: What does it actually feel like to live with depression? What do people need Mental Health Nursing for? What do Mental Health Nurses do?' (Parsons 1997). I went on to argue that quantitative research could not address these issues and that therefore a qualitative approach was needed. I am no longer sure about the assumptions that underlie my 1997 comments.

The need to ensure that nursing interventions are effective, efficient and affordable means that quantitative research, and especially the Randomised Control Trial, has a clear role in nursing research. Quantitative research is especially suited to deter-

mining and evaluating the outcomes of nursing intervention and if nursing is to be an influential discipline in modern health care we must have a solid evidence base to our practice. However, it is also clear, as I contended in 1997, that nursing is also an art that must address questions that arise from human experience of mental health problems; questions that touch upon issues of human spirituality. In 1997, I argued that these questions were not accessible to quantitative research but were only accessible through qualitative research; now, I am not so sure.

I have modified my position for two reasons: firstly, in recent years quantitative enquiry is increasingly successfully tackling questions that deal with human experience using a quantitative reductionist approach. For example, recent research has demonstrated that the learning of self-restraint and morals, as they are related to anti-social behaviour, can be linked to deficits in the prefrontal cortex of the brain (Raine *et al.* 2000); there are many similar examples. In 1997, I would have argued that philosophical enquiry alone could address questions of moral reasoning, yet, whilst not suggesting that prefrontal deficits contribute to all deficits in moral reasoning, a quantitative neuroscience approach is addressing a fundamental moral concept. Therefore it can no longer be correct to say that questions of human spirituality and moral reasoning are beyond the reach of quantitative enquiry. Secondly, researchers in other disciplines have questioned the usefulness of qualitative research in ways that I find compelling. The best example of this work is by Tooley & Darby (1998), who look at research in education. Education is another discipline that has used qualitative research extensively, yet Tooley & Darby (1998) found significant problems with the quality of the work. Of particular significance were failures to triangulate data within studies, therefore reducing studies to little more than anecdotes, and the failure to obtain even similar findings from the same data set when a different researcher reviewed the data. Although these criticisms can be accommodated within a qualitative paradigm, reliability and validity are traditionally, but not exclusively, positivist, quantitative concepts; they raise serious questions regarding the practical usefulness of qualitative research in daily practice.

If we are to continue to develop a useful and relevant body of nursing research, it is essential that all methods of nursing enquiry be evaluated for