Health research: need for a methodological revolution?

The polarization of research ideologies into two camps, qualitative versus quantitative, has been commented on by a number of authors. One of the more recent contributions to this debate comes from the journal *Addiction* in which McKeganey (1995) discusses the divide between the two underlying philosophies and concludes that this divide no longer serves a useful function. He then discusses a number of procedures for the analysis of qualitative data, before suggesting that qualitative research might benefit from a more systematic and rigorous approach in the interests of increasing the explanatory power of that method. The suggestion that the qualitative and quantitative camps might usefully come closer together has now become an established wisdom, and many researchers as a matter of routine suggest both quantitative and qualitative components in their grant applications. I certainly do this myself, as grant funding agencies seem to like this combination and in the present academic climate it pays to give people what they want.

However, as indicated in a number of following letters to McKeganey’s article, there may be a problem here. Basically, the philosophies underlying the two approaches are not, and in my view never can be, complementary. Whilst at a practical level we can ride roughshod over this issue in our research, by including some user-friendly group discussions along with our latest forced-choice questionnaire bashing exercise, the basic philosophical incompatibility of the two approaches remains unresolved. Since historically both schools of thought have at various times claimed superiority for their own method of pursuing ‘truth’ (see, e.g. *The Psychologist*, 5(10), 1992), differences in the data or information yielded create (insoluble?) problems as to which is ‘truer’; these usually being resolved by latter-day workers in terms of some more or less unsatisfactory linguistic sleight of hand. This typically takes the form of a story in which differences between data sets are explained on the basis that qualitative data shed light on how the individual person sees the issue, and thus do more justice to that person’s individual constructions; whilst, on the other hand, questionnaire data comprise ‘scientific measurements’ that are comparable across individuals, and thus provide the basis for statistical comparisons in terms of standardized measures derived from procedures of proven reliability and validity. If different pictures emerge, therefore, it does not matter since such differences can be explained away by the assertion that the two methods shed light on different ‘truths’, even if such truths are sometimes basically incompatible. Unfortunately life is seldom so straightforward [e.g. a recent study by Best *et al.* (1995) in which young drug users expressed negative ‘attitudes’ to drug use in a forced-choice drug questionnaire, but more positive attitudes in an unstructured group discussion, leaving the question ‘What is the true attitude?’ unanswered and unanswerable]. It goes without saying that by dint of scientific rhetoric it is usually possible to come up with a plausible reason why answers conflict; but surely it would be better to come up with an underlying philosophy that might subsume both approaches and thus render such linguistic gymnastics unnecessary.

Instead, however, we have competing, possibly incompatible philosophies, stemming from fundamental differences of opinion over what is ‘science’ and about the very appropriateness of ‘scientific method’ as a way of investigating human action. On the one hand, there is the classic physical-science approach to mental measurement, an approach pioneered by Thurstone and others (see McNemar, 1946) in the 1930s and still with us today in barely modified form. Mental entities in the head exist and are measurable on the same kinds of scales that physicists use, because they have properties of magnitude and temporal duration. Furthermore, that data obtained are scientific and objective (how anyone could ever conclude that people’s verbal reports of whatever kind, about anything, could be described as ‘objective’ eludes me totally) and provide indicants of actual states of affairs in the head unless the person is deliberately lying. On the other hand, the alternative approach asserts that knowledge of human phenomena comes largely from intuitions based on unstructured interactions in non-replicable contexts, that the validity
Editorial

of findings is impossible to determine, that insight is derived in ways which are subjective and may not even be communicable (Miles and Huberman, 1984), and that from a phenomenological point of view there may not even be any social reality 'out there'; only a personal social construction specific to a given context which has no finality in terms of meaning. Faced with such basic differences, to simply say 'Oh well, let's do both then' seems a strangely unsatisfactory solution. Rather like being a disciple and an agnostic at the same time.

In the area of psychophysics, it has been widely accepted for some decades that the answers one gets to questions about sensory processes (as in a signal-detection task for example) are dependent on the methods used to elicit them. Within that paradigm, for example, one simply cannot specify a sensory threshold; only a set of performance characteristics that describe an individual over a range of testing (criterion) conditions (Green and Swets, 1966). It is ironic that within psychology, signal detection theory should give full credence to the fact that the details of response elicitation and procedure account for variability in the answers received, since in the 'hard-nosed' and 'scientific' arena of the psychophysicist such difficulties are far from intuitively obvious. Meanwhile, mainstream social science research still gives credence to the idea that social constructs like attitude and belief can be measured accurately by means of a one-off questionnaire procedure. At risk of being obvious, it should be remembered that no amount of sophisticated multivariate wizardry applied post hoc can make sense of data which are inherently unsatisfactory (perhaps some older heads will recall the GIGO theory of factor analysis!).

The 'enlightenment' view that science deals in certainty is challenged in the physical sciences by the theories of Einstein and Heisenberg who introduced notions of relativity and uncertainty into physical measurement; principles which, even if only by analogy, ought to be of central concern to psychologists and social scientists. If any subject area is confronted daily with demonstrations that what people report depends on (is relative to) what you ask and how you ask it; and that once you have asked the question you can never ask it again in the same way (the act of 'measurement' changes what has been 'measured') it is the psychological and social sciences. Similarly, it must be obvious that verbal behaviour is motivated and serves purposes for the speaker. It is not simply unmodified 'data' retrievable computer-like from 'storage' whenever the researcher presses the appropriate buttons. At a time when even physics encounters problems with specifying the enduring and absolute properties of matter, for psychologists to insist that a given method applied on a single occasion is sufficient to specify the nature of an existing attitude, belief or whatever seems somewhat behind the times.

Small wonder then, that the health literature is crammed with references to the failure of attitudes to specify behaviour. Or that the undergraduate curriculum still requires its victims to recall dim and distant studies in which unknown American hoteliers serve meals to legendary Chinese couples despite having said on a previous occasion that they would not, in order to bolster a distinction between verbal and behavioural attitudes. Is it really necessary, faced with examples of this type, to build elaborate models with multiple components and to construct intricate webs of intervening variables whose epistemological status becomes more and more problematic, just to explain the fact that what someone said at time 'a' did not semantically reflect what they did at time 'b'?

A central UK figure in the above argument is Potter, who in a number of publications has sought to suggest major philosophical problems with what have become normative procedures for dealing with answers to questions. Central to the present argument is the contribution on attitudes from Potter and Wetherell which is summarized in the book *Discourse and Social Psychology* (1994). They write '...if a certain attitude is expressed on one occasion it should not necessarily lead us to expect that the same attitude will be expressed on another. Instead there may be systematic variations in what is said...'. The problem with Potter and Wetherell's approach, however, is that the type of discursive approach they advocate rests on
Editorial

localized and intuitive interpretations of speech-in-context, from which it is difficult (impossible?) to extract any finality.

This bleak state of affairs appears to offer a choice between a 'scientific' method based on structured data-collection methods, within which answers are highly cued by the researcher and thus to a large extent reflect the researchers own agenda, and a discursive approach, which appears in principle to make the structured and replicable 'analysis' of 'data' at best difficult and at worst undesirable according to certain underlying philosophies. It remains only to say that within the area of psychophysics, a person's verbal responses, even on such socially irrelevant matters as whether they can hear a sound or see a light, are found to be as much a function of subject criterion (a motivational variable) as of the loudness or brightness of the stimuli. Consequently, from the signal detection theorist's standpoint, the subject's 'real threshold' can never be specified. Instead, an ROC curve is constructed which describes the subject's responses over a range of conditions which vary in terms of signal strength and subject motivation. Consequently, comparisons between individuals are made in terms of their performance over that range of conditions, with some individuals producing more HITS (correct detections) over a wider range of conditions than others.

Isn't there a lesson to be learned here? Does it still seem reasonable to suppose that an attitude measured by questionnaire 'a' under circumstance 'b' measures it once and for all and reveals the 'true' attitude that underlies all other attitude statements that could have been made contemporaneously in all other circumstances? Maybe the relative failure of attitudes to predict behaviour in a number of health circumstances arises simply because we never assess them properly in the first place and perhaps our fascination with constructing ever more complex models of intervening variables of uncertain epistemological status is an adaptation to that failure more than anything else. By contrast, a view of attitude as a process variable that manifests itself in a variety of ways in different circumstances, as a dynamic thing that people DO for their own reasons, instead of as a static (at any moment in time) entity in the head, raises a whole new range of possibilities. It also implies a number of added complexities to the issue of data collection, which is why we need a methodological revolution.

In the meantime, it is not necessary to search for alternative 'truths' via qualitative and quantitative methods with conflicting philosophical bases. The only basis necessary is one that asserts that (1) any two approaches can be subsumed under a common philosophy of language insofar as they make use of verbal behaviour of whatever type in an attempt to predict, understand or control behaviour; (2) differences in cueing and context are the only features of importance, since these lead to the production of differing reports; (3) any data collected are relative to the methods used to collect them; (4) truth resides in no single data collection exercise; (5) if truth is a possible construct, it will emerge from the overviewing of samples of verbal behaviour taken in a variety of contexts and situations, employing differing methodologies; and (6) the measure of an attitude resides in its pattern of consistency or variability in different contexts and under different methodologies. This is indeed a challenge, but it may not be impossible to meet. Some concrete suggestions of what social research methodology might look like in the future have recently been discussed elsewhere by Davies and Best (1996). It is surely high time for such a revolution, given that the methods in most common use derive from procedures developed by stalwarts in the 1930s, based on a physical model of science which has long since been abandoned by physicists themselves (see Koch, 1964). On the other hand, viewing the collection of verbal report data of different types as an exercise in understanding the properties of dynamic and active systems which manifest differently under different cueing and criterion conditions offers at least the hope of a common philosophical basis for all such exercises, and suggests integrated research designs which go beyond the cobbled together of incompatible ideologies.
Editorial

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References


