

Qualitative research ...

Science or pseudo-science?



Michael Morgan
returns to the
quantitative vs.
qualitative debate.

THE articles by Chris Stevenson and Neil Cooper, and by Carol Sherrard responded helpfully and accurately to the issues I tried to raise. This open discussion of an issue of basic importance to the future direction of psychology is to be welcomed.

Chris Stevenson and Neil Cooper were quite right to point out that qualitative psychology is a political issue. As they said, academic psychologists, at least in the United Kingdom, have traditionally taken great pains to have their discipline regarded as a normal science. 'Psychology as a discipline and profession therefore has a vested interest in maintaining the dominance of objectivity in relation to the production of psychological knowledge.' (p.159.)

The success of previous generations of psychologists in persuading their scientific colleagues that psychology is at least an embryonic science is reflected in the level of support given to psychology departments for carrying out research and teaching students.

From time to time, heads of departments of psychology have to beat off determined challenges to the status of psychology as a laboratory-based subject. All this will be at risk if psychology goes down the road which qualitative researchers would like to follow. Psychology would become, as it is in some parts of Europe, an arts-based discipline; its funding would decline; and scientific psychologists would leave to take up appointments in departments of cognitive science or neuroscience.

There is another political matter, which is tricky to express without giving offence. Several commentators in *The Psychologist* over the last few years have been worrying that psychology at university level is becoming an overwhelmingly 'female' subject. How, it is asked, can the subject make itself more attractive to 'boys'?

I put the gender terms in scare-quotes here because the issue obviously has nothing to do with biological sex. Many of the best experimental scientists in psychology are women.

But it is apparently a fact that the subject is failing to attract 'boys' in the same way that physics and engineering fail to attract 'girls'. And this trend will continue, I suggest, if the subject contrives to turn its back even further on the technical disciplines of science, and stresses instead the kind of issues that are concerned with the more emotional business of forming social relationships, and analysing 'discourse'.

I am reasonably certain that we can agree on what the political issues are, even if we take different sides on them. But political arguments will not solve the real question at issue: can 'qualitative research' lead to progress in our understanding of the human mind and of the brain on which the mind depends? The answer clearly is 'yes'.

Without the academic discipline of history we would know almost nothing worth knowing about the causes of our modern world. Historians change their interpretations from time to time, but we nonetheless know enormously more about, say, the events of 1789 than did the historians of the 19th century. But historians do not claim to be doing science.

The inevitable questions arise, therefore, why 'qualitative psychology' should claim to be a science, and why it should be carried out in a scientific department, rather than in an arts faculty, perhaps along with social anthropology.

This is not meant to devalue what goes on in arts faculties. If I were forced to choose between abolishing a university's history department or its psychology department, I would have the greatest difficulty making up my mind. But one does not mark one's equal respect for two things by pretending that they are the same. And I still wish to contend, despite the counterarguments in the April 1997 issue of *The Psychologist*, that there is a perfectly clear set of

The background

In March 1995, a special issue of *The Psychologist* was devoted to qualitative research. In an article in January 1996, Michael Morgan suggested that such methods took insufficient account of the need for scientific replication of data. Pursuing the debate in April 1997, Chris Stevenson and Neil Cooper looked for some common ground, while Carol Sherrard argued that Michael Morgan's article ignored the social sphere. Here, Michael Morgan argues that these articles have failed to make the case that the social sphere requires radical alternatives to traditional, objective science; Neil Cooper, Chris Stevenson and Carol Sherrard counterargue; and Michael Morgan replies.

minimal requirements which must be met by a science, and which are not met by most 'qualitative psychology'.

A science must use procedures for gathering data that are reliable across observers; and when scientists have disagreements they must know, at least in principle, how to decide the issue by data. They should accept that there is a difference between opinion and fact. They should also have accurate and precise definitions of all the technical terms they use. Thus, when I learn from Carol Sherrard that '[s]ome qualitative researchers do attempt to ensure reliable observations' (p.162) (italics added), I wonder what on earth the rest of them think they are doing.

Confusion

The writers in the April 1997 issue argued that the objective methods of the traditional sciences are not always appropriate to the social sciences. Their argument seems to me to rest upon a core confusion between the subject matter of social science and its methodology. Carol Sherrard, for example, says that psychophysics can be an objective discipline because all our sensory systems are the same, while social psychology cannot be objective because people have different beliefs and values. She adds that we can agree as observers upon the height of a person but not the social class.

These points are neither true nor relevant. In the first place, our sensory systems are not identical, and some of the most interesting data in psychophysics come from individual differences. Colour deficiencies are an obvious example, where we now have the full causal story, stretching in time from John Dalton's original subjective observations to the sequencing of the abnormal genes involved. It is the uniformity of psychophysical *methods* that make it an objective science, not the uniformity of the human beings it studies. It is vital not to confuse the two, as I fear Carol Sherrard's article did.

Returning to the example of height and social class, no psychophysicist would measure the lengths of the lines used in an experiment by the observer's estimate of them, although they may well be interested in the way in which these estimates vary with the real length of line.

Similarly in the case of social class, I imagine one might well be interested in the way in which observers' estimates of social class depend upon their own social class; but this presupposes that there is a property called 'social class' which can be tightly defined, even though it cannot be accurately perceived.

So, to argue that there is no 'consensus' between individuals in society and therefore no possibility of studying soci-

ety objectively, is to confuse subject matter and methodology. Imagine if a geneticist were to refuse to study plants because their flowers have different colours!

The real issue is whether or not there are objective methods of studying people's social behaviour, not whether there are pre-scientific uniform beliefs about society. And here I continue to be puzzled that qualitative researchers make such little mention of ethology, the one novel scientific method that has emerged for studying social systems in the last 100 years.

Ethologists go to enormous trouble to standardise their methods across observers before beginning to study the complex interactions in a tribe of baboons. Why do qualitative researchers not accord the same respect to the complexities of human societies?

Carol Sherrard seemed to argue that a 'consensus' is not worth looking for, because a consensus can be wrong, as it is, for example, between members of the Flat Earth Society. But who has suggested that a consensus is what scientists should be looking for?

What scientists are supposed to do is to collect novel and interesting data, which other scientists can rely upon. After that, they are free to draw what inferences they like — the more the merrier. This is how science differs from a religion; consensus has no central place in it, however happily it may sometimes arrive. 'Flat earthers' arrive at their position by refusing to contemplate the facts. I know that this statement sounds naive to the post-modernist ear, but it is true.

Politics

Concerning the interpenetration of politics and science, there are two views: a strong and a weak. The strong post-modernist view is that a political opinion and a fact held to support that opinion are inseparable. Carol Sherrard dismissed this strong claim in favour of a weaker one: that the search for a particular kind of fact is often motivated by an opinion.

I am glad to concede this point. It is perfectly true that the search for anatomical differences between the brains of heterosexuals and homosexuals was guided in part by a hope to legitimise homosexuality as a healthy biological fact, rather than a morbid effect of society. But regardless of the motivation, the question about brains is an interesting one, and the work stands or falls on the strength of the anatomical research, not of the opinions that may have motivated it.

Chris Stevenson, Neil Cooper and Carol Sherrard argued that it is best in these circumstances if the researchers make full acknowledgements of their political opinions. Chris Stevenson and Neil Cooper went further, suggesting that scientists should write an ongoing

'reflexive' commentary on their personal feelings about their research as it progresses. This is an interesting idea, although I venture to hope that the writing of such diaries will remain voluntary.

Whether scientists have a political agenda or not is in the end not the issue: what matters is whether the observations made by researchers are reliable, and free from their opinions. The sense I got from Carol Sherrard's article is that qualitative researchers are split on this issue. *Some* researchers attempt to ensure reliable observations; others, we deduce, do not. 'Many qualitative researchers refuse to sacrifice validity (i.e. accurate representation of social perception) to a merely constructed reliability, masquerading as a "potential public consensus".' (p.162.) Others, apparently, believe that their research findings should be checkable by others.

But to point to this diversity of practice is to skirt around the issue; what I want to know is whether Carol Sherrard defends the researchers who reject reliability and objectivity as an aim. It is quite irrelevant to this question that there are other researchers who never rejected these values in the first place.

Compromise?

Chris Stevenson and Neil Cooper made a heroic attempt to find middle ground between the positions they call 'positivism' and 'constructivism', and which they reject equally as being too extreme for psychology.

I agree completely with their reasons for rejecting constructivism, but for some reason find their objections to positivism less convincing. 'Positivism,' they said, 'entails a narrow definition of good science which serves to distance the researcher from the researched.' (p.159.) This seems to be making the claim that the aim of psychology should be to *understand* people, and that one can only *understand* people by forming social relationships with them, to which scientific objectivity is a barrier. To this one could reply that we have been forming social relationships with one another for some half a million years, and have indeed got to understand one another quite well at a certain level as a result.

But it is unlikely that qualitative psychology will improve this intuitive understanding unless it brings some new technique to the job, and I have yet to be convinced that the techniques go beyond those of good investigative journalism. 'Forming relationships' with people is the job of everyone; I see no reason to pay researchers to do it. Something a bit different is expected from scientists.

There are some issues which can be solved by finding a middle way, and others where one has to make a choice. My opinion is that we have to reject post-

modernism from scientific psychology, if only to have a coherent teaching programme. We cannot have one set of lecturers explaining to students how to study psychology scientifically, and another set of lecturers telling the same students that when studying people, the methods of science are no use. It won't work, and psychology departments won't work.

References

Sherrard, C. (1997). Qualitative research. *The Psychologist*, **10**, 161–162.

Stevenson, C. & Cooper, N. (1997). Qualitative and quantitative research. *The Psychologist*, **10**, 159–160.

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'New science' and psychology



Neil Cooper



Chris Stevenson

**Neil Cooper and
Chris Stevenson
suggest that Michael
Morgan fails to
consider that science
is a social activity.**

MICHAEL Morgan's argument is based on the premise that in 'doing' science the researcher follows particular methods with rules agreed by the community of fellow scientists. For him, methodological rules within 'scientific' psychology facilitate coherent research and teaching, perhaps indicating a quest on his part for psychology to be seen as a discipline. Morgan appears to be concerned that allowing debate about knowledge will damage the discipline.

However, it is naive to think that psychology has always been a science in the sense of it having a single, uniting paradigm of what can be known about the world and how it can be known. Whatever the prevalent concepts and modes of analysis, an underlying characteristic of psychology is the coexistence of both humanistic and mechanistic understandings.

The existence of different ways of producing and interpreting psychological knowledge has created tensions which have been implicit within the discipline. However, in the endeavour to be recognised as an academic discipline, these tensions have been screened by psychologists striving for 'scientific' respectability through employing the methods of the natural sciences.

Dominant psychological knowledge is strongly associated with 'scientific' methods. Scientific knowledge in our society is generally *seen* as legitimate. 'Scientific' psychology simply attempts to tidy away discrepant forms of psychological knowledge by overlaying them with 'scientific' knowledge. Yet, such knowledge is not *necessarily* legitimate.

Shotter (1993) notes that when conversations become formalised into a particular discourse (regular patterning of conversation around some aspect of the world), ideological processes are likely to be at work to benefit some groups over others. The view that 'science = good' has served to bound psychology and mar-

ginalise other conversations. Had science not become the dominant discourse in psychology, conversations might have occurred more freely between differently oriented psychologists, or with other disciplines such as philosophy and sociology, where arguments regarding science are well rehearsed (see Chalmers, 1978, 1990).

The ideological processes involved in how we view the world and what we take as a research problem *is* the political issue which Michael Morgan misses. Drawing upon an example of neuro-anatomy and sexual orientation, he considers that the scientific findings of the research override the opinions which drive the study. Ignoring the political is to neglect the way in which science can construct meanings and the power inherent in this construction.

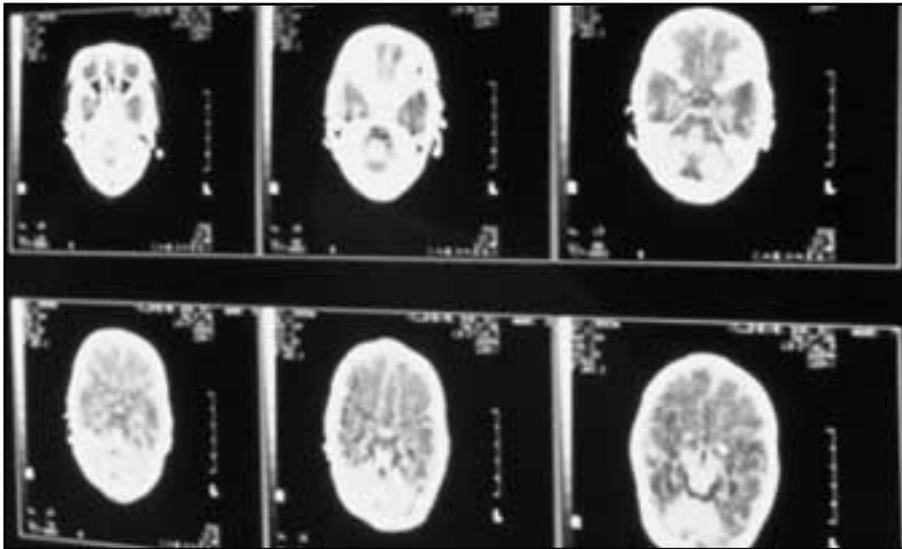
For example, the need to legitimise homosexuality as a biological fact rather than a societal effect is bound up with our understanding of our culture at a particular historical moment. As Gould (1981) has pointed out, the opinions of the researcher cannot be separated from the socio-historical culture in which they are expressed.

Interpreting meaning

In suggesting that without history we would know little about the *causes* of our own modern world, Morgan's article demonstrates the linear thinking and the view of knowledge as 'facts' which both lack a political dimension. While acknowledging that historians change interpretations from time to time, he fails to explore why such changes should take place, and the relationship between how we see our world now and how we interpret or recreate our past. He also fails to make the link between the production of history and the production of psychological knowledge,



Science = good?



Neuroanatomy and sexual orientation: ignoring the political?

especially knowledge of the social world. In describing the social world, psychology '... as part of a culture creates and re-produces the social phenomena it studies' (Parker, 1989, p.17) (original emphasis).

It is naive to compare complex human relations to those of other species and recommend the standardisation of observations as a means to ensure reliability. As Krippendorff (1980, p.129) stated: 'Two judges with the same prejudices may agree on what they see but be wrong by all other standards.' While reliability is usually stated to be evidence of objectivity, reliability often relies on the training of observers, and this training means that they are trained with the same biases as those of the trainer.

Stainton Rogers *et al.* (1995) draw attention to the problem of 'scientific' based studies in the social sphere, with its associated elements such as establishing reliability, the development of hypotheses and operational definitions:

For something to be measurable, of course, requires it to be objectively definable. But ... such definitions do not neutrally mirror an objective reality — all they do is 'replace the subjects meaning with the investigators' (Stainton Rogers *et al.*, 1995, p.26).

It is in the exploration of meanings where qualitative methodology is most frequently adopted. Morgan compares qualitative approaches with good investigative journalism; however, journalism does not attempt to offer deeper, theoretical understanding. Qualitative methods, such as discourse analysis and grounded theory, start with the same kind of material as journalists but allow the building of linkages and structures into theory. Theory adds an explanatory dimension to the analysis.

Qualitative research leads to progress in understanding the human mind in relation to the fluidity of the social world

and, due to the nature of the research methods, it also facilitates an awareness of the process of the construction of accounts and the relationship between the researcher and 'subject'.

'New science' and reflexivity

We want to propose a 'new science' for psychology. The 'old science' is not broad enough to satisfy the appetites of different psychological perspectives. Contemporary debates concerning qualitative and quantitative research in the production of psychological knowledge are examples of a conversation that had previously been marginalised by the prevalence of 'scientific' discourse in psychology.

Our original article (Stevenson & Cooper, 1997) was in a sense a conversation we were having with ourselves about how qualitative and quantitative research, although underpinned by different philosophical approaches to the world, could be evaluated by the overarching criterion of researcher reflexivity. We proposed that adopting a reflexive approach could encourage researchers to locate themselves in relation to their studies, and to consider the political issues inherent in the production of knowledge. Researchers could do this by constructing a clear account of the way they have formulated the problem, used information and arrived at conclusions.

A lack of reflexive thinking leads to the uncritical following of research recipes. Once a definition of, say, homosexuality is accepted within a field, this definition becomes ascendant. As a result, researchers adopting established methods allied to this definition can do research without fear of being questioned by others who share their views about the methods' appropriateness.

Critics of the methods can be rebutted or ignored and the boundaries around the research are maintained.

The ability of researchers to reflect on their own research process, and the use of tools such as a reflexive journal, may open up problems and produce new ways of looking at questions. In this way, the 'new scientific' researcher will adopt or create the appropriate method of investigation. In using a reflexive approach to research, the usefulness of quantitative approaches may be appreciated and qualitative methods are seen to be much more than 'good' investigative journalism.

Cultivating open minds

Michael Morgan suggests that teaching psychology students that there are 'scientific' methods and 'other' methods is dangerous, and that we should 'reject post-modernism from scientific psychology'. We are arguing against worrying about whether psychology is an 'art' or a 'science' subject, but propose that the critical thing is to keep questioning the assumptions on which our knowledge is founded. It is this open-mindedness that is science and not the adherence to method. To tell students that there is only one 'scientific' way of looking at the world is to prevent them from entering into this questioning process. Teaching students that there are many ways to view the world is teaching psychology. Psychology has always been a post-modern discipline; perhaps we are just waking up to it now.

References

- Chalmers, A. (1978). *What is this Thing Called Science?* Milton Keynes: Open University Press.
- Chalmers, A. (1990). *Science and its Fabrication*. Milton Keynes: Open University Press.
- Gould, S.J. (1981). *The Mismeasure of Man*. Harmondsworth: Penguin.
- Krippendorff, K. (1980). *Content Analysis: An Introduction to its Methodology*. London: Sage.
- Parker I. (1989). *The Crisis in Modern Social Psychology — and How to End it*. London: Routledge.
- Shotter, J. (1993). *Conversational Realities*. London: Sage.
- Stainton Rogers, R.S., Stenner, P., Gleeson, K. & Stainton Rogers, W. (1995). *Social Psychology: A Critical Agenda*. Cambridge: Polity Press.
- Stevenson, C. & Cooper, N. (1997). Qualitative and quantitative research. *The Psychologist*, 10, 159–160.

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Social dimensions of research



Carol Sherrard
argues for the coexistence of qualitative and quantitative methods.

MICHAEL Morgan believes university departments must be either exclusively 'scientific' in the traditional sense, or convert entirely to qualitative methods. I know of no-one else holding such a polarised view. He rejects co-existence on the false ground that qualitative researchers reject scientific methods from all areas of psychology.

My own view is that laboratory work is appropriate for some areas of psychology, and qualitative researchers use much of the same expensive equipment. What is at issue is the appropriateness of traditional scientific methods to the study of naturally-occurring experience. It would be odd if psychology could never tackle this.

Morgan's 'minimal requirements' for a science include 'procedures for gathering data that are reliable across observers'. I said he was mistaken to make psychophysics the model for all psychology, because the (physical) objectivity of psychophysics is not relevant in all fields. I attributed the 'objectivity' of psychophysics to our identical sensory systems. He replies that individual differences, for example 'colour deficiencies', show that we do not have identical systems. True, but it does not demolish my point that there is majority possession of a broadly identical system, else how are 'colour deficiencies' defined?

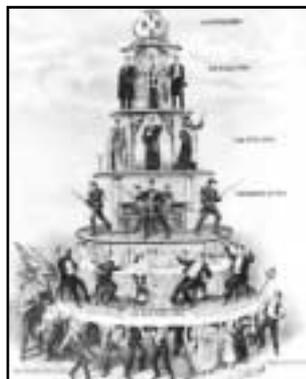
To my argument that only our shared sensory system makes sensory judgements reliable, he says I confuse uniformity of human beings with uniformity of method, since the stimuli in psychophysics are measured in objective, physical units (e.g. line lengths are measured by a ruler). It is still true that these units reflect the consensus arising from our possession of uniform sensory systems, and that psychophysical methods depend on these units. We can only use rulers because we have the same sen-

sory systems, and inhabit a world where most accept length as significant. Even so, different societies' use of different units (inches, centimetres) demonstrates the units' origin in social convention, and social interests.

This is not to deny that the physical world really exists, or that we can measure it in reliable units (a misinterpretation Morgan persists in). But its character for us depends on our being broadly similar. Beyond the physical sphere, differences of experience shape our social identities and perceptions.

Objectivity and subjectivity

Michael Morgan argues that social class is like length, even if (as for lines) there are differing perceptions of it, because it is a 'property', and 'can be tightly defined'. My point about class was that our differing perceptions of people's class reflect our differing experience. Morgan implies this is analogous to the different subjective judgements of line length in psychophysics; both can be set against an 'objective' definition. This would be physical length in psychophysics, but what would it be for social class? This case really is different, firstly because it is the subjective judgements in themselves that



Differing perceptions of class

are the focus of interest, secondly because there is no analogue of the physical line length. The judgements don't depart from the Registrar General's classification in systematic ways. It is true that we could reach a tight definition, but would it be 'objective' like length? We could find a group of people to frame one, but their definition would be challengeable by others, and all definitions would reflect the interests of the definers (as the Registrar General's does).

There is an apparent contradiction in Morgan's article. He denies that scientists should look for a consensus, but

later says 'what matters is whether the observations made by researchers are reliable'. This can only be resolved by inferring that he really believes that 'reliable observations' can only stem, not even from consensus, but from physical measures alone. If it prevailed, this belief would wipe out large parts of existing psychology.

Michael Morgan wonders why ethology is not seen as an alternative for the study of human social interactions. Ethology has virtues, but offers no way of analysing subjective understandings and experience. Morgan's low regard, from the methodological point of view, for empathy, and for subjectively experienced social relations (not the same as 'interactions') is evident. It is also convenient — it keeps alternative world views out of sight. Humans can tell us about their subjectivity, but baboons cannot. Why cut ourselves off from a direct source of information? Psychologists did in the past, because they considered personal accounts unreliable and unstructured.

Qualitative methods give us ways of analysing accounts so that within-respondent and between-respondent comparisons and contrasts can be made. The methods are increasingly well-speci-



Objectivity can be based on the universality of human faculties

fied and, therefore, repeatable as procedures. One's own observations and interpretations can be checked by using different information sources for the same topic of interest (e.g. interview, diary and personal constructs) and by seeking negative instances.

However, there is no insistence that different researchers' use of the same methods yields the same outcome for the same material (though totally different outcomes would be odd). This is because it is recognised that researchers themselves differ, bringing different concepts and experience to their understanding and interpretation of what respondents say.

Some analyses and interpretations are more convincing than others — they may be more complete, more internally consistent, or they may relate the material better to other bodies of knowledge. Because all intact (and many non-intact)

humans share some universal social faculties and are capable of empathy and intersubjectivity, researchers can make their analyses comprehensible to others even though they begin from different social positions.

Sacrificing understanding

Great researchers in social psychology, such as Fritz Heider and Erving Goffman, did not place reliability and physical 'objectivity' before understanding. Their objectivity was of the intersubjective kind, depending on universal human faculties, analogous to our shared possession of a language, and clearly recognised by them as such. They hardly need me to defend them.

If psychologists demand reliability and physical 'objectivity' everywhere and before anything else, they sacrifice human understanding and adventurous research. It is possible, with tolerance and effort, for psychologists to grow up and accept that humans are both physical and social. Otherwise, they may continue doing a kind of science, but it won't be psychology.

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Postscript

Michael Morgan
attempts to summarise the main questions that divide the participants in the debate about qualitative psychology.

THE Editor and the other participants in this debate have kindly allowed me to add a postscript. I shall try not to introduce new arguments, but instead to summarise the main concerns about qualitative psychology that I had at the outset, and leave it to the reader to decide whether these concerns have been addressed.

I began this debate by suggesting that qualitative psychologists pose a threat to traditional notions of psychology as a science. The problem is not that they use single case studies; it is not that they may find no use for statistics; it is not even that they reject the goal of quantitative laws. The real problem is that they reject the notion of something 'out there' which can be studied objectively. The articles in this issue by Cooper and Stevenson and by Carol Sherrard seem to me to establish that my concern was well-founded.

Both articles imply that I am being naive in believing that psychologists should develop agreed methods of observation. It is also, apparently, naive to think that observers should try to keep their personal prejudices out of their science. Cooper and Stevenson go so far as to agree with the formidable Krippendorf (Krippendorf, 1980) that it is pointless training scientists in methods of observation because judges can agree and still be wrong. What they do not explain is how in the brave new post-modernist world we could ever know that anything is 'wrong'.

I have also been accused several times of having an over-restrictive definition of 'science'. I hope this is not true. I suggested merely that a minimum attribute of good science is that it should be based upon observations that can be repeated by other competent scientists. It is clear to me, and I hope to others, that Cooper, Stevenson and Sherrard reject this simple idea.

I have challenged the view of qualitative psychologists that the study of social interactions and of mental experiences demands methods that necessarily go beyond those of 'traditional science'. Carol Sherrard asserts that this is so but does not seem to me to give convincing reasons. The core of her argument appears to be that people are all different, while the objects of science are not. I do not understand this argument, since she does not specify the degree of difference that there would have to be between people before they

were accepted as fundamentally different. The many wonderful and subtle differences between our sensory systems are dismissed as irrelevant because we are all fundamentally the same; while the many obvious similarities in people's social behaviour are minimised because we are all fundamentally different.

Experimental psychology and its sister discipline neuroscience have developed methods that have given us undreamed-of insights into our mental processes, and those of animals. Have qualitative psychologists really made their case that these methods will ultimately fail? Even if they have, is it proven that talking to other people must succeed, while other methods meet a barrier?

Sherrard says that 'humans can tell us about their subjectivity, but baboons cannot'. There is a strong assumption here that language is a very special form of behaviour which gives us a direct insight into the minds of other people. This proposition is not self-evidently false, but neither is it self-evidently true. It is just as likely that our feelings of empathy for our fellow human beings depend on smells and visual signals. (But then, it may be a personal failing that I have always found it hard to empathise with people on the telephone.)

As for baboons, Sherrard may be correct that it is hard to know what they are thinking and feeling, but she does not explain her objections to the many intricate experiments by which the 'theory of mind' experimenters have tried to investigate exactly this issue in animals, and indeed in autistic children. If Sherrard is right, all attempts to see inside the minds of infants are pointless.

My quarrel is not with the methods and procedures of qualitative psychology. The subjects studied by qualitative psychologists are often fascinating, and in many cases it is difficult to see how they could be replaced by more objective methods. But I argue that it is potentially misleading, to both students and the public at large, to invest these methods with the authority of objective science.

References

- Krippendorf, K. (1980). *Content Analysis: An Introduction to its Methodology*. London: Sage.
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Are all attempts to see inside the minds of infants pointless?