

CORRESPONDENCE

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We may return unduly long letters to the author for shortening so that we can offer readers as wide a selection as possible. We receive so many letters each week that we have to omit some of them. Letters should be typed with double spacing between lines and must be signed personally by all their authors, who should include their degrees. Letters critical of a paper may be sent to the authors of the paper so that their reply may appear in the same issue.

Correspondents should present their references in the Vancouver style (see examples in these columns). In particular, the names and initials of all authors must be given unless there are more than six, when only the first three should be given, followed by *et al*; and the first and last page numbers of articles and chapters should be included.

The controlled trial and the advance of reliable knowledge

SIR,—Professor H A F Dudley's thought provoking article (1 October, p 957) as well as being entertaining, is also timely, in drawing our attention to the epistemology of scientific medicine during a period of popular obsession with alternative remedies and the emergence of the British Holistic Medical Society. I respond to some of the challenges thrown down by Professor Dudley as another who is an "outsider" by nature. I have immaculate credentials as an iconoclast, so much so that as soon as I find that I share the majority viewpoint, I consider it time to re-examine my own beliefs. Yet it is my congenital iconoclasm that has attracted me to a Popperian view of science which finds its expression in the randomised controlled clinical trial.

To some extent, like Professor Dudley, I am of a Kuhnian mind. My own view of Kuhn's philosophy, however, is that he explains the history of science rather than its methodology whereby in retrospect it is possible to describe the spectacular corroboration of hypotheses which allowed the reinterpretation of existing data and the redirection of future research. There are a few such examples in orthodox medicine. For example, the corroboration of Harvey's theory of the circulation of the blood after the discovery of the capillaries and the corroboration of

Pasteur's germ theory of infection must rank as important milestones in the history of scientific medicine. The poverty of the Kuhnian paradigm is that it ignores the spectacular failures. Spectacular failures in the physical sciences are unlikely to harm the individual, but the obscene damage to countless patients resulting from the applications of treatments based on experience and intuition are countless and have been brushed aside in Hugh Dudley's polemic. The trouble with "experience" as a way of approximating to reliable knowledge is that all of us tend to reinterpret each individual experience in the light of a previously held conceptual framework. This is best illustrated within the subject of psychoanalysis. For example, in 1919 Popper reported a case history to the great analyst, Adler, who found no difficulty in analysing it in terms of his theory of inferiority feelings, although he had never even seen the subject. Slightly shocked, Popper asked him how he could be so sure, and he replied, "because of my thousand fold experience," whereupon Popper could not restrain himself from replying, "and with this new case, I suppose your experience has become a thousand and one fold."¹

Nevertheless, I would like to join forces with Hugh Dudley in accepting that it is

awareness of the reliability of the knowledge we use which must be the central issue in the distinction between scientific and alternative medicine. I am also inclined to agree with the visual interpretation of his scale of reliability with one important exception. We certainly start from ignorance but we never achieve certainty—the very title of Karl Popper's autobiography *Unended Quest* illustrates my point.² For that reason alone I strongly disassociate myself from his view that clinical trials are most easy to perform when they are least needed and vice versa. Surely the most difficult time to conduct a clinical trial would be at the right hand extreme of Dudley's linear analogue, as I know to my cost. Twice in my career I have been able to indulge my other congenital mental defect in exposing myself to medical martyrdom. Firstly, in challenging the radical en bloc approach to the management of breast cancer and latterly in challenging the new orthodoxy of adjuvant systemic chemotherapy.^{3 4} On both occasions our group were attempting to answer biological questions—firstly, concerning the role of the regional lymph nodes in the control or dissemination of cancer, and, secondly, in questioning the chemosensitivity of putative micrometastases.

I would love to embrace the cosy concept

of science described by Hugh Dudley and no doubt will in time, having exhausted myself with the scientific rigours of the randomised controlled trials, yet without wishing to appear sanctimonious what alternative framework for progress does Professor Dudley offer other than to continue muddling through?

The time is long overdue when we should be considering if medicine is or is not a scientific discipline. If we are indeed a scientific discipline then the General Medical Council could protect the public more efficiently by keeping a register of remedies that have been scientifically evaluated, rather than a register of doctors, many of whom have joined the British Holistic Medical Society. If we are not a scientific discipline, then I can see no demarcation between orthodox and the fringe, with the GMC register serving only to protect the status and income of doctors by operating a closed shop.

MICHAEL BAUM

Cancer Research Campaign,
Clinical Trials Centre,
King's College Hospital Medical
School,
London SE5 9NU

¹ Popper K. *Conjectures and refutations: the growth of scientific knowledge*. London: Routledge and Kegan Paul, 1963:34-5.

² Popper K. *An intellectual autobiography—unended quest*. London: Fontana Collins, 1976.

³ Baum M. Should lymphadenectomy be discarded? *J R Coll Surg Edinb* 1973;18:351.

⁴ Baum M. Ethics of British study: criticisms answered. *Oncall* 1980 Jan 22:12-13.

SIR,—The problems of acquiring truth in clinical medicine might have been dealt with more sympathetically in Professor H A F Dudley's discourse.

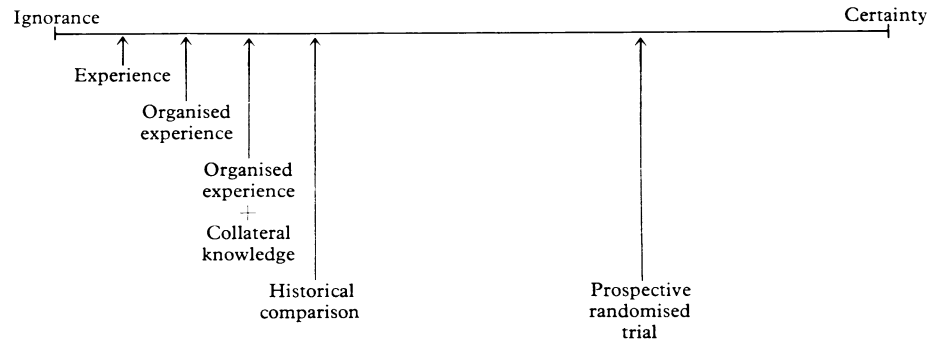
The biologist's model comprises an indeterminate number of preformed interdependent axes, whereas the axes of the physicist's model are reasoned before their incorporation. The acquisition of clinical truth (as in other biological systems) requires disassembly of those axes that we gradually recognise. We become cognisant by reference to basic measurement and thus reject the shifting sands of uncertainty. This argument suggests that we clinicians should accept that we know nothing and work towards truth with humility and ensure objective analysis of each axis rather than obfuscate truth with empiricism.

NICHOLAS J KAY

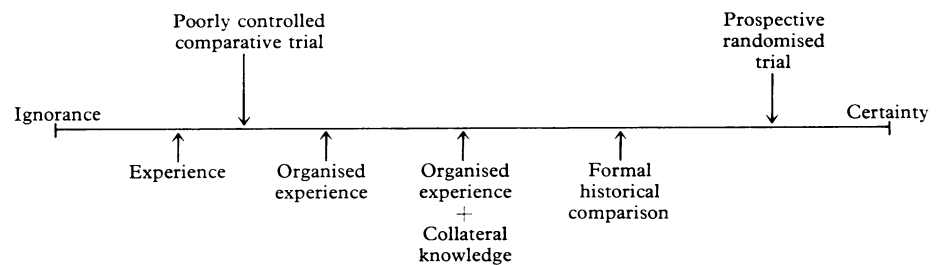
St James's University Hospital,
Leeds LS9 7TF

SIR,—It is perhaps a pity that Professor H A F Dudley's article for debate on controlled clinical trials appeared at almost the same time as an article by Dr A McGlashan (24 September)¹ and a letter from Dr P Pietroni (24 August)² in *The Times* specifically attacking the controlled trial.

Professor Dudley's scale is an example of a well known graphical method³ where labels have been given to arbitrary points on a line; this is a nominal scale.⁴ The implication of the drawing is, however, that it is an interval scale with equal intervals and that the prospective randomised trial comes at a late stage of the development of knowledge when the result is almost certain and the trial by implication unnecessary. The illustration could be redrawn (since the intervals are arbitrary and may be expanded or contracted at will).



Or better, since the scientific part of medicine should properly be separated from the history and art of the subject, could appear as follows:



It is not true that an experiment is harder to conduct when there is less knowledge. Any well designed experiment will provide a result. The fact that the result is a verdict of "not proved" does not mean that there is no result. If there is less knowledge in a specialty the results of a trial are the more valuable.

Many doctors are unsophisticated in measurement theory, and I know of instances where important clinical decisions have been taken on the basis of measurements that had not been adequately validated. Professor David Kerr (30 July, p 355) refers to the doctor who chooses measurement data that accord with his own ideas and asks if this is fraudulent; it is certainly an unscientific attitude, as it aims to achieve conformity with preconceived ideas rather than allowing refutation. If I understand correctly this is the path Dr McGlashan and Dr Pietroni advocate, while Professor Dudley does not feel that formal comparison by scientific methods is necessary.

I have some experience on the subject of radiological contrast media, and I am amazed at the false ideas that have abounded on the subject to the detriment and death of patients for want of simple comparative trials to refute the fixed ideas of received opinion. Many doctors are incapable of distinguishing fact from opinion or plausible hypothesis, and I feel strongly that hypotheses should be tested rather than applied randomly on the basis of individual whims.

PETER DAVIES

City Hospital,
Nottingham NG5 1PB

¹ McGlashan A. Treat the patient as a person. *The Times* 1983 Sept 24:8 (cols 6-7).

² Pietroni P. Body and mind. *The Times* 1983 Aug 25: 11 (cols 4-6).

³ Reichmann WJ. *Use and abuse of statistics*. Harmondsworth: Penguin Books, 1964:148-51.

⁴ Stevens SS. On the theory of scales of measurement. *Science* 1946;103:677-80.

SIR,—We were more than a little puzzled by Professor H A F Dudley's article (1 October, p 957) which concludes by advising us that clinical trials are not the only way of ensuring

reliable knowledge. In part, we feel that he has erected a straw man in order to destroy him.

The dogma that the controlled clinical trial is the only acceptable form of evidence in clinical research is so patently ludicrous that we doubt if anyone really believes this, and we even doubt if Professor Dudley thinks that anyone believes this. Not even the most enthusiastic protagonist of the controlled clinical trial would launch into a large randomised trial as the first step in evaluating a new treatment. This might come only after sufficiently encouraging results from observational studies or uncontrolled clinical experience, and then also after some small controlled trials. We all know this. We also know that the randomised controlled trial is the only way at present to provide a methodological guarantee that bias of allocation and assessment will not distort the comparison of treatment, given that the study is properly designed and executed. The fact that not all clinical trials are, in practice, properly designed and executed is a justified criticism of the application of this methodology but not of the methodology itself. Nor does it follow that because all clinical trials do not give unequivocal answers to important questions clinical trials are inherently flawed.

It is particularly surprising to find, in an article that refers to a book by Thomas Kuhn and to no less than three books by Sir Karl Popper, the concepts of certainty and objective scientific knowledge. It is clear to even the most casual reader of these philosophers of science that they view science as being essentially subjective in its foundations. Popper has, as a central tenet, the opinion that facts are theory laden. What we measure, and how we measure it, depends on the theory or hypothesis we have in mind when we design the experiment. Science, in the phrase Popper uses as the title of one of his books, is a series of "conjectures and refutations." His view of the role of theories in science is that they should be potentially falsifiable, and that scientists should aim to falsify their theories as vigorously as possible by appropriate experimentation. For a Popperian a controlled clinical trial is a powerful attempt to falsify a null hypothesis. Of course, even a single case can be a falsifying instance. The surgeon who believes that radical mastectomy is the cure for early breast cancer might have his theory falsified by just one patient so treated dying of the disease. This theory could be protected from falsification by claiming that the patient died, not because the theory was wrong, but because she came too late. The virtue of the randomised controlled trial is that the theory is exposed to a real threat of falsification. Even then