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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