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Editorial

Of cabbages and clinical reports

Achieving the scientific ideal of establishing definitive proof for the efficacy of one's clinical work must be the rarely achieved ambition of most behaviour therapists. I recently had the unusual opportunity of comparing our lot with that of an organic farmer who tests his cultivating methods with total scientific rigour. His cabbage plants were assigned (randomly, of course) in rows of 20 to one of two treatments. Follow-up was daily for four weeks and measures were of leaf-size. I believe he chose a multivariate statistical package for his results. Amazingly, he had had no trouble at all in recruiting a willing, homogenous sample. Cabbages did not suddenly become scarce when he chose to study them. None objected to the random assignment procedure—indeed their informed consent was never obtained at all and nor was the rationale of their treatment ever explained to them. No cabbage dropped out or failed to attend their daily appointments. My friend firmly denied the charge that he had established better rapport with some plants than others. As my complexion turned a luxuriant green, he claimed no plant had moved from the area during the experiment and that contact-rate at follow-up was 100%.

While I sincerely trust that we are all grateful that our clients are more demanding, varied and unpredictable than are my friend's cabbages, this fact does render the scientific study of our art a frustrating business. Even $n = 1$ experimental designs generally fail to achieve the elusive scientific ideal: unlike cabbage leaves, our client's symptoms typically fluctuate so wildly from day to day that prohibitively long baselines and no-treatment comparison periods are usually required. To comply with the demands of such designs, inevitably reduces the flexibility, responsiveness and, hence, value of treatment.

The spirit in which I make these obvious points is in no way intended to encourage the view that the scientific study of behavioural psychotherapy is either impossible or inappropriate. In a sense, every behavioural clinician adopts, or should adopt, an experimental approach to treatment. However, the *evidence* we employ to discover whether a particular approach is being successful is of a different kind, unless we are very fortunate, to narrowly defined scientific evidence. For example, we frequently ask for feedback from clients about procedures and will base clinical decisions upon this alone. Nonetheless, as the pages of this journal have testified over the past 2 years, both experimental and purely "clinical" reports may appear side by side and complement each other while having quite different styles. As Ian Robertson

cogently argued in our last issue, experimental work has frequently provided "models" which are readily applicable in the treatment setting. While we have an established format for reporting scientific work, what types of "evidence" are acceptable in a clinical report and what format might be best employed? We would like to encourage the submission of informative clinical reports and welcome readers views on this matter. Here are some idiosyncratic suggestions of my own.

A valuable clinical report should ideally incorporate the following features:

1. The case description(s) should not only document and attempt to quantify behavioural, cognitive and physiological aspects of a disorder, but also describe its responsiveness to current situations and past life events.

2. An analysis of the material should suggest both an aetiological model and a treatment rationale with reference to existing knowledge and previous studies. Clinical impressions or generalizations (e.g. "he was dependent on his mother") must clearly be avoided and detailed observations of reports of actual behaviour preferred (e.g. "his mother always accompanied him to school").

3. Therapeutic procedures should be presented fully not just the aims of these. It is unusual for practical problems faced in applying a therapeutic principle to get written up. Knowledge of these annoying snags and their resolution may be more valuable to the reader than any other aspect of the report.

4. Despite a primary reliance on accurate, vivid description, quantitative measures of change of *direct* relevance to target symptoms should be employed whenever possible.

5. Behavioural change is rarely gradual, dramatic gains or set-backs are more often the rule. The use of objective data in conjunction with clients' feedback may provide evidence concerning which components of treatment are useful and which ineffective.

6. In discussing the case, it is frequently possible to reflect as to how this particular clinical application of an aetiological and treatment model confirms or disconfirms the validity of this model. By comparing results and observations with those of previous studies, fruitful hypotheses may come to light both for scientific study and clinical application.

Maybe another useful format for communicating clinical material in this journal is that of a letter. The editors would willingly publish correspondence which briefly notes clinical experiences which speak of such topics as problems encountered in treatment, observations of relevance to treatment models and evidence for the manner of effectiveness of particular methods.

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