

## Where is John Paley when you need him?

I have to confess that I was disappointed by this review. John Paley has a certain reputation in nursing philosophy circles for perceptive views, incisive critique and rigorous thought, and I was therefore expecting an insightful, robust, critical (if, perhaps, scathing) engagement with our book. Unfortunately, Paley was nowhere to be found, and had inexplicably been replaced by a self-confessed 'curmudgeonly, anal-retentive pedant with no sense of humour'. Perhaps this absence was Paley's way of telling us that our book simply wasn't worth taking seriously. Well, he says as much, labelling it as a juvenile, trivial, facetious cop-out, full of 'giggly self-conceit' and 'sniggering behind the bike sheds'. However, whilst I am aware that I might be addressing an anal-retentive pedant with no sense of humour, I intend at least to do him the courtesy of a serious reply.

I will start with his general overview of our book. Paley (or whoever is writing under his name) clearly has some issues with its form and tone as well as its content. It is perhaps not an overstatement to say that its format and playfulness irritates him somewhat. Yes, there are a few jokes in the book, but Paley manages to miss them all, and instead sees jokes where none were intended. Thus, our quotation about books having no ending, which began on page 213 and ended on page 3, was intended to make a serious point about arbitrary beginnings and endings, and our do-it-yourself index (what Paley calls 'the silliest example') was another serious (if playful) attempt to emphasize the idea of the reader as (re)writer. And yes, it does illustrate our approach to the reader-writer relationship; and yes, we do object to Paley labelling this relationship as a facetious and patronizing cop-out.

And when we *do* make a joke, he either misses it completely (there was a rather good one about mas-

turbation on page 90) or takes it literally. Thus, when we joke about aggressive postpositivists suffering from penis envy, Paley writes:

Yes, seriously. The EBP enthusiasts don't have anything, you know, down there, and that's why they're so macho. Sorry, but this isn't Freud. It's sniggering behind the bike sheds.

Had we known that our book was going to be reviewed by an anal-retentive pedant (I think there's another Freudian joke in there somewhere) with no sense of humour, we would have printed all the JOKES in CAPITAL LETTERS for easy detection.

Paley does, however, make a number of substantive (if misguided) points about evidence and its lack, about paradigms, and about power, although unfortunately space only permits me to address them superficially. So what about the lack of evidence claim? Apparently there is plenty of evidence to justify evidence-based practice (EBP), it is just that we haven't looked; or worse, that we don't care. Well, I care enough to follow up the reference that Paley gave. He cites a recent review by two American psychologists in a law journal (Grove & Meehl, 1996) which, Paley claims, 'looks at 136 studies which compared actuarial (or mechanical) judgement with clinical judgement' and which found the judgements of clinicians to be sadly inadequate.

So what exactly did Grove and Meehl find? Well, they started by citing a study from 1928 in which 'a sociologist' showed that 'subjective, impressionistic, "clinical" judgements made by three prison psychiatrists about probable parole success' were less effective in predicting parole failure than judgements made simply by counting the number of 'objective factors' present in each case (Grove & Meehl, 1996). They followed this by citing a study from 1943 comparing 'the accuracy of a group of counsellors predict-

ing college freshmen academic grades with the accuracy of a two-variable cross validative linear equation', and a study from 1941 which attempted to predict outcome of electroshock therapy in schizophrenia.

These studies form part of a meta-analysis in which Grove & Meehl analysed 617 comparisons since the 1920s of 'mechanical, algorithmic predictions' made by computers and/or mathematical calculations such as multiple regression analysis with 'subjective impressionistic' decisions made by 'clinicians'. These decisions included judgements about mental and physical health, but also

personality description; success in training or employment; adjustment to institutional life (e.g. military prison); socially relevant behaviours such as parole violation and violence; socially relevant behaviours in the aggregate, such as bankruptcy of firms; and many other predictive criteria. (Grove & Meehl, 1996; p. 296)

In each case, the 'mechanical prediction' was compared with judgements of 'clinicians' including 'psychologists, psychiatrists, social workers, members of parole boards and admissions committees, and a variety of other individuals', whose 'experience levels ranged from none at all to many years of task-relevant experience'. Nowhere in their paper do Grove & Meehl even mention EBP, and yet according to Paley, these studies are comparing *evidence-based* judgements with *clinical* judgements.

Despite the fact that this is a poorly conducted, unfocused, unselective, unweighted and less-than-rigorous meta-analysis (see Eysenck, 1995), despite the fact that it tells us very little about EBP at all, the fact that Paley *thinks* that it does tells us a great deal about his own views of EBP as a mechanical, algorithmic, statistically driven top-down approach to clinical decision-making. This, as we point out in our book, is in direct opposition to most advocates of EBP, who have gone to great pains to reassure practitioners that it does *not* 'ignore clinical experience and clinical intuition' (Evidence-Based Medicine Working Group, 1992), that it is *not* a top-down 'cookbook' approach which ignores expertise (DiCenso *et al.*, 1998), and that ultimately 'it is this expertise that decides whether the external evidence

applies to the individual patient at all' (Sackett *et al.*, 1996). Never mind David Sackett's reassurance that 'clinicians who fear top down cookbooks will find advocates of evidence-based medicine joining them on the barricades'; all we need in order to make an on-the-spot clinical decision is to consult our actuarial tables or perform a quick multiple-regression analysis. Grove & Meehl have provided conclusive evidence that, in Paley's words:

Making professional decisions on the basis of evidence-based decision rules is a far better bet than using your clinical judgement, intuition, professional craft knowledge, embodied know-how, or whatever you're calling it this week.

Is this really the best evidence that Paley can muster? With advocates such as Paley, it would seem that EBP and the gold standard of the meta-analysis have no need for critics such as myself.

A brief word on paradigms, since Paley raises the subject. Whilst he agrees with us that the Evidence-Based Medicine Working Group misused the term, he completely ignores our discussion on the *implications* of this misuse. He then attempts to turn the argument around and implicate me in the misuse of the term. So what if antipositivist writers (and I was interested to see Paley equating *postpositivism* with *antipositivism*) are just as profligate in their use of the term 'paradigm'? Firstly, to be against positivism does not make me an 'antipositivist', and secondly, our book is in any case an argument against *all* self-proclaimed paradigmatic meta-narratives, including my own previous writing. *Et tu, quoque!* (FREUDIAN PUN).

And so to one last example. Paley takes exception to our use of the term 'dominant discourse' when referring to EBP, as conveying 'the impression that its hierarchies of evidence are dogmatic and arbitrary, an expression (merely) of power'. Our argument is nothing but 'an indignant squeal . . . like complaining that, if someone has noted the superior buying power of a £10 note, they must be prejudiced against 50p coins'. Not exactly an indignant squeal, more of an observation that the £10 note of EBP has no greater *intrinsic* worth than the 50p piece of clinical judgement; that the piece of paper is not in itself worth 20 times more than the lump of metal. Indeed, Paley very neatly

makes our point that the superior buying power of the £10 note rests precisely on the *power* of the chief cashier of the Bank of England to authorize it as such, on the decision by certain figures in positions of authority that things should be the way they are. We might wake up tomorrow to find that the £10 note is no longer legal currency, or that there are once again 240 pence to the pound, or perhaps to find that 50 pence pieces will henceforth be made of solid silver. Or, indeed, that the Department of Health has published a document elevating clinical judgements to 'gold standard' status. What then of the hierarchy? And who then will be squealing indignantly? (By the way, Paley should re-examine the second of his 'subliminal messages' that he puts in our mouths: we are not refusing to consider the methodological and epistemological reasons for regarding 'quantitative evidence as generally preferable to qualitative evidence'; rather, we are contesting the grounds on which Paley might consider such a statement to be 'perfectly reasonable').

And finally, we stand by our assertion that deconstruction is the enemy of clear and straightforward communication. The whole point of our book was to demonstrate that *no* communication is ever as clear and straightforward as it is sometimes presented, particularly in positivist research reports. It's just a shame that it was reviewed by a curmudgeonly anal-retentive pedant with no sense of humour rather than

by John Paley, who might have had the wit, intelligence and simple good manners to engage with it a little more actively (and perhaps humorously), and *then* written a scathing attack, but one based on rather more than prejudice and misinformed views about deconstruction and EBP. Oh well, perhaps next time . . .

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## References

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