

ECT controversy: Incomplete evidence and misleading reasoning

Abstract

This commentary discusses the science behind assessing the effectiveness of ECT and the issues limiting the usefulness of the hegemonic RCT methodology, compared to other techniques. This is intended to elucidate some of the reasons behind the polarization of opinion concerning the effectiveness of ECT (and MAOIs). It uses the example of a recent strongly anti-ECT paper by Read et al. to illustrate the problems with modern scientific publishing which allows meretricious works by tendentious authors to attain a prominence in the literature, and the mainstream and internet media, which is both undeserved and counter-productive. Such authors may have an anti-science agenda. I illustrate how to pick up the warning signs of biased and unscholarly publications both by the style of the authors and the unsuitable journals in which such work is published — probity and provenance are relevant. These various deficiencies in the scientific publishing world and academia are doing severe damage to the scientific enterprise.

Meta Description: A discussion about the incomplete and misleading nature of the information about ECT published by Read, Kirsch, and Bentall, as part of their objective of having ECT banned, and the poorly recognised limitations RCTs

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Au: P Ken Gillman

PsychoTropical Research, Bucasia, Qld

<https://psychotropic.com>

Orchid ID 0000-0001-8277-3397

Researchgate https://www.researchgate.net/profile/Ken_Gillman

Email: ken.psychotropic@gmail.com

Introduction

This is a supplement to my previous commentaries concerning scientific methodology and ECT; those explain why **RCTs (Randomised Controlled Trials)** are a less useful a technique than is supposed and how other methods more directly address the **key scientific issues of causality and mechanisms**.

These further comments illustrate the weaknesses of RCTs, particularly as they elucidate why there are such wide divisions of fervently expressed opinion, as exemplified by the recent emphatically anti-ECT paper of [Read and Kirsch](#) [1] and other of Read's work [2-6]. Supportive comments for Read have also come from [Bentall](#), a co-author with Read in a similar anti-ECT paper. This 'group' are attempting to mount a campaign to have ECT banned from use.

There is more to science than RCTs.

It is possible that Read's work and efforts will have so little visibility and impact that people reading this comment over the next year or two will never have heard about him, or the ripple he creates in the pond. Nonetheless, this commentary will remain a useful exercise in understanding

how to assess what is good objective science and what is poor partisan science, the adverse influences of the hegemony of RCTs, and other more nuanced themes.

My observations relate to the relevance, probity, and logic of the arguments that Read et al. advance, especially because RCTs, on which they exclusively rely, are only a limited part of a larger scientific picture.

The wider context in which this often-polarised debate about ECT, and other treatments (like MAOIs), has been conducted, over decades, is fuelled and confused by **unfamiliarity and misunderstandings concerning the nature and validity of different sorts of scientific evidence**. Further fuel is periodically added to the fire by various non-medical, and non-scientific, groups driven by one or other systems of dogma or belief, sometimes of messianic intensity, e.g. the Scientologists, and certain religious sects, and some people concerned about health-care and medical diagnosis who view the issues from a non-medical socio-political perspective (cf. Szasz 'The Myth of Mental Illness', Laing, and subsequent thinkers).

Some anti-ECT protagonists are sufficiently extreme to knowingly and purposefully engage in deceit and fraud.

Read, Kirsch, and Bentall are psychologists and therefore not medically qualified and without a general biological science background. Their papers and discussion are incomplete, misleading, and hard to assess for non-research-experienced doctors, as well as non-medical or non-scientific readers [1, 3-6]. They have hallmarks suggesting they have a socio-political origin and objective, rather than a scientific one.

General readers will maintain a perspective if they appreciate that Read's views are extreme — most of those with knowledge and expertise in the field regard their arguments as tendentious.

Read et al. have reviewed 35-year-old RCTs and have chosen to disregard much other recent evidence, especially that relating to the differential effectiveness of different forms of ECT. The idea that this field has been stagnant since then is the first clue, for perspicacious readers, about the partisan nature of their writings. Other recent evidence bears directly on the **key issue of causal explanatory mechanisms supporting the beneficial effects of ECT** [7-10]. Such data substantiate the 'cause-effect' nexus, especially because, as Sackeim, a distinguished ECT researcher has summarised:

The efficacy of RUL ECT continues to increase with increments in stimulus intensity, but seizure quality measures saturate at an electrical dosage resulting in only moderate efficacy [11].

His opinion is summed up here:

The evidence indicating that ECT is effective in the treatment of mood disorders is **diverse, long-standing, and incontrovertible**. In both the short term and long term, it appears to exert greater benefit than pharmacological alternatives [12].

In reviewing only old studies Read et al. have made it easier (and less obvious to inexperienced readers) to skate around the issue of the subsequent improvements in ECT technologies and techniques, using modified wave-forms etc., which preserve efficacy whilst reducing the ill-effects that they emphasise so prominently.

It is 35 years since the last study considered by Read et al. and modifications and improvements have ensued.

That more recent work indicates a dose-effect relationship for ECT, which is further compelling evidence that one is manipulating an effective therapeutic mechanism and demonstrating a relationship between the

intervention and the outcome — that is what science is all about. Additionally, there is the issue of the **time relationship between the intervention and the response**; the closer, and more consistent, that relationship is, the stronger the supposition of a cause-effect relationship becomes (cf. Hill). It is important to observe that just one application of ECT can produce a dramatic and rapid improvement in lethal catatonia (cf. analogy with cardioversion), and substantial rapid improvements (sometimes after one application) in severe depression.

That demonstration of a time relationship between the intervention, its magnitude, and the outcome is the causal-mechanism experiment that characterises and exemplifies good experimental science [13, 14]. In terms of causes and mechanisms it is an altogether more powerful technique than the RCT, which, by its very nature, cannot address questions of causality and mechanism.

This crucial issue of causality and mechanism is not accessible via RCT methodology, the ubiquity and hegemony of which has eclipsed other approaches. A general under-appreciation of the implications of that accounts for much of the division of opinion.

The development and application of causality-thinking has been slow since Hill's work 50 years ago [15-17]. RCTs merely compare one group with another, with little or no ability to elucidate causes or explanatory mechanisms*; thus, their heuristic utility is limited and one-dimensional. Science neutered.

*That problem is compounded by the difficulty of ensuring a homogenous starting sample and by the uncertainty of the suitability and reliability of outcome measures (rating scales) which are only interim proxy measures (thus inherently insensitive and unreliable) of true illness-treatment success (as opposed to symptom amelioration).

Read et al. rely on RCTs alone and insist that ECT has not yet been demonstrated to be effective and that 'a series of RCTs' is required to establish if ECT works. A narrow simplistic view of a complex issue.

Let me illustrate one of several weaknesses of that idea about RCTs by suggesting this hypothetical for readers: how many of you will volunteer for the RCT Read and his colleagues might, following similar reasoning, propose in order to test whether cardioversion* really helps people admitted as an emergency with a cardiac arrhythmia? No double-blind trial has ever been done on this. They just charge in with the paddles (pun intended) and give you a huge electric shock, no anaesthetic, which, just as in the TV programs, makes you jolt on the bed. Some patients get significant chest burns, a proportion do not survive. Barbaric, unscientific — you should refuse such unproven treatment — if you are still conscious, or alive.

*Cardioversion restores a normal heart rhythm in people with potentially fatal arrhythmias (of various sorts) by a powerful electric shock to the heart, through electrodes placed on the chest.

Although there is an element of flippancy in the above analogy it is scientifically valid in its major elements — try thinking it through yourself; if you cannot work out why and how it is scientifically valid, then a refresher course in scientific methodology and causation theory is in order.

It is difficult to imagine that anyone with experience of the responsibility for the life of a seriously ill patient could believe that such improvements with cardioversion or ECT represent a placebo response (which is a poorly defined concept). To hold that view is to demonstrate tenuous contact with not only logic but also reality — but if Read, Kirsch and Bentall wish to be given a placebo when they arrive in emergency department following their heart attack, or the psychiatric unit with delusional depression, then that is

their choice. Perhaps before making that choice they might profit by reading the story of the [Dutch professor who didn't believe in such things](#). [Various previous commentaries](#) explain more about all that.

One should also note in passing the double standard regarding the evidence they admit for different aspects of their arguments, only RCTs will do for effectiveness; but retrospective subjective case reports (a poor quality of evidence) will do in order to substantiate the case that it impairs people's memory and causes 'brain damage' — **memory loss is not synonymous with the emotive phrase brain damage**, yet another deliberately inaccurate, emotive, and prejudicial statement without evidence — the evidence tends to support the opposite view, which is that **it promotes neurogenesis, i.e. the growth of new neurons**.

They also say; '*Sadly, the severity and significance of the brain damage and memory loss is rarely studied.*' What utter piffle: **the most superficial search of the literature belies this mis-information and finds six papers this year (2020) alone** [18-23].

Doctors have always paid attention to the issue of memory impairment; I remember being involved in such research as a junior doctor in London in the mid 1970s. As the references above show, Read and his colleagues implicitly deride and belittle the researchers and doctors who continue to do careful research on this topic.

That may assist readers to form a dispassionate view of whether Read's writing presents a balanced and objective account.

Although there are diverse egregious aspects to both the data Read et al. present, and to their argument, that does not alter the point that increased monitoring, auditing, and improvement of standards, and thorough independent follow up, in relation to many medical procedures, including ECT, are desirable.

Let us consider their credentials, their argument, and their evidence.

The journal, the people

Sine ira et studio Tacitus

The [journal in which this paper is published](#) is relevant, it was in '*Ethical Human Psychology and Psychiatry*'. Normally you would expect researchers at respectable establishments (Kirsch is at Harvard; Read is at the less prestigious University of East London) to publish in **relevant high-ranking and reputable journals** — there are various suitable journals. Departing from convention, they chose a journal outside the relevant field, i.e. a psychology journal — not only that, but they also settled for an obscure and insignificant journal, of which Kirsch himself is on the editorial board — that is not necessarily improper or wrong, but it is relevant and noteworthy. Perhaps that choice tells us something.

The journal is the house journal of a private Institute set up by a well-known anti-drug and anti-ECT proponent called Peter Breggin (now aged 85). It has had a low and declining impact factor over the last 20 years and is most unlikely to have access to expert referees capable of reviewing a paper about ECT. I doubt that the paper has had any expert statistical review either — it is a paper fundamentally about statistics.

Referring to Read's paper as peer-reviewed is stretching a point.

The people

Read, Bentall, and Kirsch are psychology researchers who all have many publications and high H-indexes (see Google Scholar). None of them appear to have a general biological science background, nor medical experience or qualifications, nor one may presume, much involvement in the

medical treatment of hospitalized patients who are likely to need ECT (practical experience is a good teacher). Read's career path and experience do not appear in the information he gives prior to his arrival, when he was aged about 42 years, in Auckland in 1994, nor does the subject, or awarding University for his degree or his thesis: **that is most unusual***. I cannot find his scientific publications prior to 1994. He does not make available his earlier career details — he repeatedly avers to his '40-year career' but half of it is a mystery. It is possible, however unlikely, that he has other degrees in science**, or even medical experience of some sort, that we do not know about. Alternatively, this 20-year mystery may indicate an unusual situation: viz. that he wishes to actively conceal elements of his past, possibly because something in his past would not be to his credit.

*General readers may not appreciate that academics make available their full curriculum vitae, including the details of their degree, thesis etc, and all their publications; not to do that is highly unusual, indeed, in my experience, unique. It is difficult to think of a positive reason why anyone would do that.

**A Psychology degree is often an arts and sociology degree, with little basic biological science content (i.e. chemistry, pharmacology, physics, physiology etc.): that is why psychologists are often not regarded as mainstream scientists by the rest of the science community (even if they have a B.Sc.) Many psychologists have little university-level biological science education.

One would have to be naïve, or lacking in curiosity, not to wonder what is going on, what has shaped Read's passionate and contentious attitudes towards psychiatry? how will that affect one's estimate of his probity?

Another key issue that others will be equally puzzled about is the explanation for why researchers at their level have published such an ostensibly significant paper in such an obscure journal — after all, it forms a cornerstone of their claim that ECT should be banned. One explanation would be that it was rejected by more eminent journals — which I am sure it would have been — or that they fully expected it would be rejected by any quality journal, but wanted to get it into the computer databases quickly and easily via an low-ranked journal (with less rigorous peer review standards), thus making it citable, and giving it specious credibility.

It is also reasonable to suppose that they have modest knowledge of a range of relevant background scientific data, for instance relating to animal experiments done with ECT, pharmacological experiments with ECT, and all sorts of other clinical and research factors of relevance (e.g. see Sackeim).

Taking offence

These researchers rather readily resort to accusations that people are making *ad hominem* comments about them; e.g. [Bentall](#).

I was then subjected to a series of *ad hominem attacks*, for example by retired US psychiatrist Bernard Carroll who insisted that I was not allowed to have an opinion because I had, "as much standing to bloviate on ECT for incapacitating depression as I do to bloviate on neurosurgery for epilepsy".

Professor Carroll made a perfectly reasonable and correct point. I doubt if Read, Kirsch, or Bentall are qualified to discuss the design of nuclear reactors either.

Pointing out that one's experience, knowledge, and expertise affect the value of one's opinions, and the credibility that others will assign to them, does not constitute an *ad hominem attack* (note that their writings have more examples of this kind of not-so-subtle twist; viz. he does not refer to it as a 'comment', but as an 'attack').

One should take note when people defend their position by exhibiting sensitivity and readily resorting to claims of insult or offence — to garner sympathy, the ‘poor oppressed me’ tactic. Such is usually an indication of weak arguments and insecurity. To describe Carroll’s comment as being an ‘ad hominem attack’ is an exaggeration.

Incidentally (another not-so-subtle twist), the ‘retired US psychiatrist Bernard Carroll’ was not, as Bentall’s wording might lead you to assume, a run-of-the-mill psychiatrist in his dotage; he was a distinguished and respected Professor, and a researcher who continued to publish many papers after his retirement, up until his death; he died a couple of years ago and there is a dedication to him in my commentary about [medical science publishing](#).

I said something similar to Read, about ultracrepidarianism, in response to his article in ‘[The Conversation](#)’ about their own paper. His response was similar. I said, concerning the limited strength and scope of the evidence in their paper, in a measured and restrained comment:

‘What Read et al. have produced is insufficient to reach that conclusion [ECT should not be used] — they are all psychologists, who typically have limited practical experience in this field, and will therefore be regarded by many as expressing opinions beyond the range of their expertise, with some justification’.

To which he responded:

‘We welcome critique of the methodology of our review (which I note you have not done) but trying to undermine the findings on the basis of the profession/discipline of the researchers is not very convincing.’

This was followed by a puerile and irrelevant additional comment:

And as for the psychiatrists who still believe it works, some psychiatrists believed rotating chairs, surprise baths and, most recently, lobotomies worked.

Talk about the pot calling the kettle black — by his standards, that is surely an *ad hominem* attack on all those psychiatrists. I say ‘puerile and irrelevant’ because ‘psychiatrists who still believe’ is an indirect way of insinuating into the debate (not-so-subtly) the incorrect idea that they are in the minority and out of date — and then, ‘rotating chairs...’ I hardly need to comment on that, except to say — tongue-in-cheek, and attempting to be equally puerile and irrelevant — on that basis we should discount everything any psychologist ever says because for the best part of 100 years they all believed in phrenology, then parapsychology, then hypnosis, then recovered memories, and now the reproducibility crisis...

They preach objective discussion of science, but practice something different.

And going back to ‘*We welcome critique of the methodology of our review (which I note you have not done)*’: that statement is another indication of how debating with these folk is like wrestling with an eel. Here Read is again endeavouring to create a false impression, that I have failed to address the issue of critiquing their methodology (yet another not-so-subtle twist).

That is a distorted reflection of my point, which was that the methodology they had adopted was limited in its scope and validity, and only a small part of a bigger picture — **irrespective of how well done, or how poorly done, it was**. One is left to ponder whether he is exhibiting poor comprehension, or just naked disingenuousness — again.

Read replies to one correspondent on ‘The Conversation’

‘I agree it can work for some people. **If someone believes it worked for them then it worked for them.**’

This is the ‘Tinkerbell’ effect, we are in ‘*Peter Pan*’ territory here; for those whose memory of pantomimes has faded: ‘*every time a child says, "I don't*

believe in fairies" there is a fairy somewhere that falls down dead. Is he engaging in postmodern philosophy?

It is difficult not to comment on this obviously illogical statement [one of several, which one might have expected referees to pick up on] *'the cost-benefit analysis for ECT is so poor that its use cannot be scientifically justified'*. The scientific justification has got nothing to do with a cost-benefit analysis, which is to do with health economics, not medical efficacy.

I think that is quite enough to illustrate that they give the appearance of being less-than-straightforward as well as less-than-scholarly.

For them to fail to recognise and acknowledge that aspects of this discussion lie outside of their expertise is hubris.

Literature disregarded; debate polarised

The recent reviews on ECT, that I have cited in my [other commentaries](#), show that there is much literature that they have omitted from consideration — one simply does not know what the extent of their knowledge might be concerning science publications outside their discipline (e.g. pharmacology, neurophysiology, radiology etc.). They focused exclusively on some 40-year-old RCTs to make their argument: they did not discuss or review other research relevant to this multi-faceted issue.

Consideration of all the relevant scientific evidence is required if they are to stand even a remote chance of convincing the general scientific community, or others, that their non-mainstream claims and demands, justify attention or action [that ECT be immediately ceased].

Readers will find comprehensive references are, for instance, in the recent reviews by authorities in the field, such as Kellner, Sackeim, Prudic, and Anderson [21, 23-28]. The Sackeim review contains discussion and references outlining the dose-effect relationship between ECT and clinical improvement. That dose-effect relationship strengthens the rationale for asserting a cause-effect relationship between the treatment and the improvement — this is something that no RCT can ever achieve.

That clearly illustrates how, because RCTs are divorced from both mechanisms and from causality, which are the essentials of science, that imposes non-transcendable limitations on an RCT's scientific capabilities and usefulness.

Other researchers have informed me, concerning the previous anti-ECT paper Read and Bentall published in another lowly-ranked journal [4], that the journal did not respond to a request, from a group of ECT experts, to publish a comment. That is unusual and unscientific, because the whole basis of scientific publication and discourse is the facility and willingness to consider criticisms, comments, and contradictory evidence — to fail to engage in that process (by declining to publish a comment) is tantamount to disqualification from being considered 'Scientific'.

I was also informed that attempts were made to engage with them.

Bentall states his recollection; or is it an existential reconstruction [29]?

When I have put this proposal to leading advocates of ECT (I will preserve them from embarrassment by not naming them) they have invariably replied that such a trial would be unethical because it would involve withholding an effective treatment from some patients. Of course, the whole point of such a trial would be to discover whether ECT is effective. The very fact that they have failed to understand this point suggest that psychiatry as a profession needs a more thorough education in the idea of evidence-based medicine.

What view is a reader expected to form when Bentall declares he will *'preserve them from embarrassment by not naming them'*. That, one is entitled to infer, is calculated to produce a misleading impression; furthermore, it is

facile. It is yet another illustration — the more carefully one reads their material the more of these examples one notices — of why the word puerile keeps coming to mind as one reads their material. It is hardly the sort of thing one expects from a mature professor in the retirement phase of his career.

These people are less-than-straightforward disputants; I have communicated with these ‘unnamed persons’, who would be quite happy to be named, and who would relay a different version of the story. Bentall was asked to produce a design for the ‘definitive study’ that would be both feasible and ethical*** — there was no useful reply, except the suggestion to investigate the issue of whether or not ECT reduced suicide risk using audit data: that would be futile, since it would inevitably be confounded by patient selection and clinician choice, because serious suicidality is an indication for ECT — Bentall modestly self-describes as ‘an experienced clinical trialist’, yet he appears not to be capable of producing any more credible or sensible ideas. It is obvious that is a poor suggestion, even to someone like me, who is not ‘an experienced clinical trialist’.

***Non-medical readers need to be aware that scientists and doctors cannot just do any experiments they wish; they have to get funding, which entails presenting a rationale and protocol to a committee for funding approval, and then get an ethics committee to agree that it is a proper and necessary experiment to conduct. So, when he insinuates that doctors ‘refuse’ to do the trial that he suggests, because ‘they say such a trial would be unethical’, that is, yet again, calculated to be misleading — as he must know full well, if he is an ‘experienced clinical trialist’. Even if Bentall, Read etc. managed to team up with doctors who thought it was ethical, they would be unlikely to get funding and ethical approval to do it.

*Readers should pay careful attention to their choice of words which frequently betray their underlying attitude. Researchers are not ‘refusing’, they are not obstinate children spiting out their porridge, they are experienced practitioners advising it is unlikely to be possible or appropriate — not the same thing.

He engages in more misdirection when he says above; ‘*The very fact that they have failed to understand this point...*’. I doubt there are many researchers who could fail to understand that; so, if one can be pompous, puerile, and patronising all at the same time, then that about does it. What on earth can have been going on in his head when he wrote that? Did he imagine he was addressing a class of first year undergraduates? I do not think anybody will have difficulty in imagining the colloquial phraseology that most people would employ to describe how witless that is — the answer, for Australians, is ‘four blanks wit’.

It is hard to suppose that he genuinely thinks that these eminent researchers (a proportion of whom are more eminent than him) are that feeble-minded. No, I think that most people will see that as a blatant but clumsy piece of intellectual dishonesty. Also, again, haughty and hubristic.

The next part of his statement, in the same vein, just made me laugh; ‘*psychiatry as a profession needs a more thorough education in the idea of evidence-based medicine*’. First, he is not just challenging psychiatry, he is taking-on all of science. Second, it is recognised by many eminent scientists, as I have discussed elsewhere, that evidence-based medicine and RCTs are but one (over-rated) part of a bigger picture of scientific methodologies.

Thus, it is Bentall who needs educating; in his case about causality and non-RCT methods.

As Hamlet suggested:

There are more things in heaven and Earth, Horatio, than are dreamt of in your philosophy’ [RCTs]

Obfuscating and evading issues is a regular feature of their intellectual *modus operandi*: I recently [asked Bentall](#) — since he puts himself forward as ‘an experienced clinical trialist’ — whether he thought there was any methodology that could be used other than an RCT, after three months he has failed to respond by addressing that question. There are indeed other approaches (e.g. see Sackeim), some of which I have [mentioned previously](#).

Perspective

The Internet is volume-rich but quality-poor*. Scientific journals are following close behind as publishers chase profits and abandon quality and probity — indeed, we now have a plague of entirely bogus pay-to-publish scientific journals where people pay to have their third-rate papers published, unimpeded by any meaningful peer review.

That means many readers are brought more directly into contact with ‘scientific’ material without having the tools to enable them to assess the information with which they are presented. It also means that opinionated and non-expert researchers and commentators can attain a prominence in the discussion which was not previously possible — that has been exemplified disastrously, for all humankind, by the climate change debate. I now spend a greater proportion of my time explaining to people why what they are reading is not good science. Readers will find much discussion around this topic in other commentaries on my website.

One could start, in the instance of Read et al., by noting that they are commenting on something outside their field of expertise in a Journal which is not about that field; it is an obscure and lowly cited journal. That raises the first of a fistful of red flags — there are others.

*As one comedian said, about the notion that 1 million monkeys typing for long enough would produce the works of Shakespeare; ‘but now, thanks to the Internet, we know that is not true’.

Concluding thoughts

‘This juggler would think to charm my judgment, as mine eyes,
obtruding false rules pranked in reason’s garb.’ Milton

Their writings seem to be, primarily, part of a socio-political viewpoint and campaign to ban ECT; the scientific ‘garb’ being secondary. Thus, publication in an obscure low-ranked journal is not disadvantageous. Rather, their paper is linked to social media, and action groups, and leveraged with publicity in the media. Those sorts of followers — the troops for whom they have been ‘banging the drum’ — will be heartened by a scientific paper, no matter how spurious its credibility.

As Milton phrased it so delightfully *‘obtruding false rules pranked in reason’s garb’*.

As I said above, if they wish to be given a placebo when they arrive in emergency department following their heart attack, or the psychiatric unit with delusional depression, then that is their choice — but to have ECT ‘banned’ and thus deprive others of their choices is a bit autocratic.

Readers will by now appreciate why I see Read et al. as less-than-straightforward disputants, and as one-eyed armchair critics. For 20 years and more they have been writing about ECT (in their own words, ‘banging the drum’), they have not participated in any original research about ECT; but they have paid insufficient regard to research done by others which does not fit their obviously preconceived view.

Their concentration on RCTs illustrates they have insufficiently considered (perhaps insufficiently understood?) the wide range of scientific disciplines, considerations, and methodologies required to address these difficult and complex issues.

One might conjecture, even assume, that their approach has grown, not out of science, but rather out of a pre-existing anti-medicine stance, based on a socio-political ideology, or perhaps some personal experience of one sort or another — knowledge of Read's unknown early career path, which seems intentionally obscured, might illuminate that question; it is apparent that he has a long-standing vehement anti-psychiatry, anti-drug perspective, sometimes expressed in an unsavoury and unscientific manner. That betokens a deep underlying resentment.

The subject of his PhD thesis is, strangely, not available: several requests to him for that information have not elicited any reply either from him or his dean.

They could have tried, they could still try, to co-operate to do a better original study, however difficult that will be in practice. How would they fare trying to obtain funding? or in persuading an ethics committee to include a placebo or psychological treatment-arm vs ECT for patients with severe depression? would they get anyone to cooperate with them, especially considering their attitude and past record in debate and co-operation?

They would need to take responsibility for their trial and would need to take out special medical indemnity insurance. In view of their lack of appropriate experience and qualifications, who would offer it to them? They are 'in the evening' of their careers and, after 20 years of inaction, have left their run a little late: or has it always been a charade and a subterfuge?

ECT is regarded as effective by most doctors in most countries, and as 'incontrovertibly effective' by most research specialists in the field. That is the reality these psychologists must face, regardless of what they might wish reality to be. Therefore, for Read and his followers to convince the scientific community that all these doctors and researchers, all over the world, have all been incorrect and misguided for the last eighty years is a big task. They will require a great deal more evidence to make a convincing case — a rehash of 40-year-old RCTs does not come close to producing that level of evidence.

They could have ensured that the effort their paper must have been was appropriately rewarded by getting it published in a serious first-rate journal: we do not know, they may have tried that and failed? Instead, the paper is in a lowly-ranked journal which most researchers would never have heard of, and never will hear of — one could say that if it was previously rejected by more eminent journals then science publication is working, because few will now see the paper and it will have even less cachet and influence.

Finally, I remind readers that I am writing about the science of clinical trials and epistemology, using the topicality of their paper to illustrate some relevant issues. I am not writing directly about the effectiveness, or use, or misuse, of ECT in any clinical context.

Nevertheless, readers are reminded, and may be reassured that, most experts (and patients) regard ECT as effective. A key point is that inconclusive RCTs, even if well done, do not of themselves justify an assumption that a treatment does not work; that is because of the major epistemological and heuristic limitations of RCTs that I have outlined here and described in other commentaries.

It is appropriate to ensure that when ECT is used, best practices are followed, and that these are continually reviewed and improved, as is being promoted by various bodies like NICE. Indeed, I offered the opinion many years ago that mandatory reporting to a central database and auditing should be the *sine qua non* for many serious adverse-effects and procedures throughout medicine.

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