ECT: Scientific methodology gone wrong

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Electroconvulsive therapy (ECT), controversy, Kirsch, Read, effectiveness, dangers, scientific methodology, serious depression, psychotic depression, brain structure, brain damage, neurogenesis, relapse rate, response rate, guidelines, supplementary information,

Meta description:
An update on ECT and the Read & Kirsch renewal of the ECT controversy, and an explanation of why RCTs are not an adequate method of assessing many treatment questions.

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Introduction
There is little room for doubt that ECT is an effective treatment for some cases of serious depression (this commentary does not discuss its use in other conditions). It is under-used by some and mis-understood by others, even after all these years [1-11].

The context of this commentary, and the reasons I am commenting about ECT (I am not an ECT expert), is:

First; its effectiveness is again being questioned by eminent psychology researchers, which will cause some patients and their families, serious distress, and concern.

Second; the assessment of the efficacy of ECT with a standard RCT is an excellent example of inappropriate and inadequate methodology.

Third; some people are given ECT inappropriately and without an adequate trial of drug treatment; particularly, they are not given an MAOI prior to being offered ECT. Others are not given ECT when it might be life changing.

Fourth; trying an MAOI before ECT is strongly indicated and much preferred by most of the patients whom I have treated; however, it is rarely mentioned, never mind discussed. That represents sub-optimal knowledge and clinical practice.
I am not an expert concerning the latest research on the clinical mechanisms of action of ECT, nor the optimal mode of administration: nevertheless, the following comments, which focus on scientific methodology, will help people to see ECT in perspective, and also to understand my rationale for considering MAOIs before embarking on a course of ECT. These considerations also help to elucidate the mystery of why there is such a gap between the opinions of experienced clinicians and the pontifications of guidelines based on RCTs — that applies to MAOIs as much as it does to ECT.

Background

Sed quis custodiet, ipros custodes

A recent review by Read, Kirsch et al. [10], which suggests there is insufficient evidence that ECT is effective to justify its use, has triggered the updating of this commentary, which I first posted some years ago — one of the co-authors of this review is Irving Kirsch who has already stimulated much thought and reconsideration, and some angst, with his cogent criticisms of antidepressant drug trials [12-14].

Read’s review and Bentall’s comments center around, and highlight, the difficulties with Evidence based medicine (EBM) (as represented by the ubiquitous Randomised Controlled Trial (RCT), on which they rely in advancing their opinions — these gentlemen are psychologists and may have a lesser knowledge and experience of ECT and medical science generally, and particularly that relating to serious depressive illnesses — that comment is not intended to be dismissive of their views, just to indicate for general readers ‘where they are coming from’.

Some might accuse them of being ultracrepidarian: ‘You might think that … but I cannot possibly comment’. The above Latin tag about ‘who guards the guardians’ is frequently used because it expresses a commonly encountered and fundamental difficulty in debates.

Who gets to decide what constitutes the legitimate subject material admissible to the address the question at hand?

That is a difficult and consequential problem the EBM-community has not overcome, indeed, have hardly addressed. This is especially pertinent because EBM operates outside the general sphere of scientific knowledge — that is to say it does not demean itself by consideration of basic science, or animal experiments, or Bayesian prior probability (see below).

It might therefore be considered that the very phrase ‘EBM’ is self-contradictory, since it ignores most scientific evidence.
For instance, RCTs and EBM would be just as comfortable with a meta-analysis of whether people who see aliens from another planet experience them as predominantly green, or predominantly blue — the reality or meaningfulness of the subject matter is utterly irrelevant.

It is helpful to clarify why this is relevant to the ECT (and MAOI) issue: exactly how are the criteria set, concerning what is an acceptable type or quality of study? how are these studies assessed and dealt with? and most importantly of all, how have clinicians become bamboozled into believing that the only acceptable evidence is an RCT? These become crucial influencing factors on the result of any meta-analysis. That is especially so if the differences being looked at are small. Thus, differences between different meta-analysis in which particular studies were deemed admissible for consideration can and do change the conclusions reached.

An eminent researcher has stated that the publication of meta-analyses has become a plague in the medical literature [15]. There are now more meta-analyses about antidepressants than there are original studies of individual antidepressant drugs, as Ioannidis shows in ‘The Mass Production of Redundant, Misleading, and Conflicted Systematic Reviews and Meta-analyses’ [15] — that illustrates my point; you can pretty much select which ever one supports the view you favour. And, I hardly need to repeat, they are done by people purporting to be experts in EBM and meta-analysis.

One of my old acquaintances was an early publisher in the field of meta-analyses of drug trials. When I contacted him a little while ago he expressed regret that he had ever bothered with meta-analyses, because he now felt that they produced results that were unhelpful or unreliable (he expressly asked me not to mention his name in my writings) — as I have said repeatedly, ‘you can’t make a silk purse out of a sows ear’.

The review and epistemology

Read et al. might be considered by some to be emotive and hyperbolic, in their own words ‘…so appalling that ECT cannot be scientifically or ethically justified’ and ‘the dwindling number of psychiatrists still using ECT are doing so outside the parameters of science in general and evidence-based medicine in particular’ — that is a sweeping generalisation in which there is no precise meaning. It betokens emotion rather than logic. They seem to have assumed that RCTs constitute the entirety of the scientific enterprise. A racehorse with blinkers on does not represent the entire equine species.
They have not applied the same standards, or scepticism, to the claim that ECT damages memory, as they have to the contention that it confers no benefit on mood — again indicating that emotion may have influenced their logic; cf. ‘Given the high risk of permanent memory loss… use should be immediately suspended …whether there really are any significant benefits against which the proven significant risks can be weighed.’

It is an amusing contradiction that all the eminent and experienced scientists and doctors from around the Western world, who have produced guidelines, which recommend ECT, are doing so using the same EBM as Read et al. are invoking to say that they are culpably wrong — thus vividly exemplifying the deficiencies of this sometimes self-contradictory, inevitably blinkered, and frequently fallible, fashion of EBM.

This division of views about ECT, which is not helped by the polarized RCT-dominated approach of Read et al., is substantially related to misunderstandings concerning the epistemological validity of different forms of evidence, whereby the RCT is given undue precedence over other methodologies in medicine — from animal research on one hand to clinical observation on the other. This elevation of the status of RCTs to a supposed ‘Gold standard’ has a meagre scientific basis, as has been stated by a number of eminent scientists over the years; furthermore, EBM is RCT-centric. That is a restrictive and blinkered approach — it has little to do with what science is about.

**Recognition of the deficiencies of EBM**

I have written extensively about this previously: here, I will garner the support of some heavy guns and give a brief snapshot of the various eminent scientists who have, over many years, discussed views supporting what I said about the deficiencies, faults, and limitations of RCTs and EBM — one interesting paper, from more than 20 years ago [16], presaged the problems, ‘Problems in the “evidence” of “evidence-based medicine”’. Since then, EBM has not been analysed or judged by its own precepts.

**RCTs do not, and cannot, address causality. Science is nothing without causality (cf. Pearl).**

Repeat after me: Science is nothing without causality.

**Opinions of note**

Ashcroft [17] writes that EBM is, ‘autonomous of the basic sciences…blind to mechanisms of explanation and causation’

Pearl (the Turing prize-winner) pioneer of modern causation thinking, has said ‘Causality is the key: there is no way of doing science without causality, it is the *sine qua non* for all understanding and progress’ [18-20].

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Solomon states [21]; ‘Emphasis on EBM has eclipsed other necessary research methods in medicine, even those methods necessary for its own development and application. Clinical research requires an engagement with basic theory (e.g. physiological, genetic, biochemical) and a range of empirical techniques such as bedside observation, laboratory and animal studies.’

Berwick, the founder of the leading organization for quality improvement in healthcare (an EBM-centric organisation), says ‘we have overshot the mark with EBM and created an intellectual hegemony that excludes other important research methods from recognition.’ [22] — thank you Berwick, I have used similar words myself in a previous commentary.

Sir Michael Rawlins in his Harveian Oration [23] argued that: ‘the notion that evidence can be reliably placed in hierarchies [as all guidelines do] is illusory…striking effects can be discerned without the need for RCTs*…the findings of RCTs should be extrapolated with caution.’ Of RCTs he says, ‘Yet the technique has important limitations of which four are particularly troublesome: the null hypothesis, probability, generalisability, and resource implications.’

*He agrees with Sir Austin Bradford Hill (of fame in the smoking-cancer saga) who long ago expressed the opinion that statistics and randomisation were unnecessary unless the effects being sought were small.

If that does not persuade you to think twice about EBM’s claims and dominance, then your credibility as a scientific thinker may be called into question.

‘I requiem mea delect.

Read’s review & Bentall’s comment

The comments and criticisms of Read & Bentall are pertinent to key points about the nature of scientific methodology and the relative merits of other methodologies, other than simplistic RCTs — on which evidence-based medicine perches, like an elephant on stilts.

However, Read et al. evince little evidence of taking account of the above considerations. An untutored external observer of the debate could be forgiven for supposing that there is no other knowledge or methodology relevant or applicable, other than the RCT.

I have written about the poor quality of scientific investigation in medicine and the limitations of evidence based medicine elsewhere — I agree with Read’s views about poor RCTs, even though they are only one part of a bigger picture. EBM has problems (EBM

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and RCTs are pretty much synonymous), especially because the nature of the gathering of the RCT evidence, and the baggage of the sponsors who do most of it, almost always limits the way it can be generalized or extrapolated to the average patient: that introduces even more difficulties and uncertainties over and above any poor methodology and measurement techniques that may be utilized.

Doing good science is not easy, especially for doctors who generally have a relatively elementary grounding in scientific methodology, logic, and statistics; but clinging to the life-raft of EBM is not an adequate solution.

It is a fundamental and elementary misconception about scientific reasoning and methodology to conclude that because RCTs are imperfect, or inconclusive, that therefore there is inadequate evidence to support the use of ECT, or any other treatment.

In the blog of the Council for evidence-based psychiatry (CEP) Bentall (who describes himself as ‘an experienced clinical trialist’) comments on Read’s paper, essentially endorsing the view that there is inadequate (RCT) evidence for the effectiveness of ECT and castigating the psychiatric fraternity for their lack of understanding of EBM.

Incidentally, Bentall’s comment is titled, ‘ECT is a classic failure of evidence-based medicine’, but I am not sure if he thought about his wording quite carefully enough: I do not suppose that he intended to imply it is an illustration of how EBM itself is a failure.

There is a lot of second-rate RCT methodology in medicine generally, and the psychiatric fraternity are among the worst disciplines — as an aside, psychologists are the last people who should start throwing stones from inside their glass houses, their discipline is riddled with execrably poor science [24-26]. There are strange folk such as Daryl Bem [27], with his ridiculous work postulating ‘time-reversed causality’ — I shall not devote space to this champion of intellectual masturbation, you can read the story here (it is a great laugh):

https://replicationindex.com/2018/01/05/bem-retraction/

An important point (I have elaborated on this elsewhere) is that the fashion for RCTs has obscured other methodological approaches which are more valuable. The obsession with RCTs also fails to take account of our extensive knowledge of biology, evolution, physiology, genetics, pharmacology, and the other sciences that make up the foundations of medical science.

Although such subjects may be less likely to capture the attention of psychologists, they nevertheless form an important foundation from which rational therapeutics emerges. Knowledge from the basic sciences greatly increases the confidence underlying various
forms of medical treatment. Treatment results cannot be assessed in isolation without taking account of that pre-existing science and knowledge.

This wider and deeper consideration of science helps to resolve the difficulty of withholding a treatment [ECT] which is rightly regarded as likely to be **life-saving in some instances, and valuable in some serious depressions.**

Pearl and the ‘Do operator’

My opinion about the need to consider other methodological approaches is supported by the work of the 2011 Turing-award-winner, Judea Pearl. He has advanced scientific methodology with a set of techniques that involve [*inter tot alia*] manipulating an important variable in the equation, the ‘do operator’ [20].

I am not sufficiently well informed about the totality of Pearl’s work to understand it in depth, and certainly not well enough to explain it cogently to others. Those interested may consult original material and other sources of explanation of this inchoate field.

However, I think I have grasped enough to suggest that a better trial methodology can be accomplished by doing (for instance) a course of ECT where some of the initial treatments are inactive, or differently active, treatments — there are various ways of doing this — and using that ‘Do’ operator to establish that the timing of the changes and improvement subsequent upon treatment varies accordingly. This is a powerful methodology; it should be possible to harness Pearl’s techniques in such a scenario and thereby design better trials that avoid the ethical conundrums that are perceived.

This is close to what has already been done in ‘observational’ studies (and good clinical practice) and is what makes those clinicians who are good scientific observers confident that ECT works. An attempt has been made recently to consolidate the cause-and-effect relationship with a ‘dose-effect’ study using EEG parameters [28].

There are frequent cases where, in the course of normal clinical practice, ECT has been delayed for one reason or another, the frequency with which improvement occurs in a discrete and predictable timescale after the initiation of treatment is difficult to explain, except by invoking a causal role for the effect of the main intervention, i.e. the induction of fits by an electrical current (as opposed to the other attendant changes which might represent non-specific effects, I prefer to avoid the term ‘placebo effect’ which is an inadequately conceptualised and defined notion).

If the improvement after such interventions occurred evenly, or randomly distributed over a non-specific timeframe of a month or two, that would indicate the evidence was insufficient to

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substantiate a cause-effect relationship: however, since the time interval to improvement occurs, determined by when the course of ‘fit-induction’ has started, in a defined and relatively narrow time-frame, it is difficult to come to any other conclusion than that there is a cause-effect relationship between induction of a fit and clinical improvement — I am preparing a separate commentary discussing more about Pearl’s work which I hope to post soon, indeed it may be available by the time readers see this.

The same logic about how improvement follows a discrete predictable time course applies to drug treatment, where improvement is evident, to appropriately astute observers, within 5 to 10 days [29]. When people ask me how long it takes for MAOIs to start working my answer is, ‘it depends on the astuteness of the observer and on what they focus their observations.’ If on the other hand no improvement occurs for 2 to 4 weeks and then subsequently gradual improvement occurs, there is less sound reason to ascribe that to a treatment-effect. Such improvement is likely to be a non-specific effect (I prefer not to use the word placebo). In such cases it is logical, if the situation is stable, to reduce and possibly cease the treatment to see if a worsening of the condition occurs.

Next, we have the phenomenon of relapse after cessation of treatment — far from disproving effectiveness, that further substantiates the cause-effect nexus, because most of these patients when given maintenance ECT improve again, and then relapse if the interval between treatments is extended too much. You do not have to be Einstein to realise that this ‘treatment-no treatment’, linked to ‘improvement-relapse’ pairing much strengthens the supposition of a cause effect relationship.

Practicalities

Current best statistics indicate that about 50% of people relapse within six months of a successful course of ECT, which leaves 50% who stay well.

That constitutes a valuable intervention modality when one is considering a serious illness with a substantial mortality, which causes a great deal of suffering over a prolonged period, including sometimes-irreversible deterioration in work and family circumstances, and is sometimes unresponsive to many attempts at both psychological and drug treatment.

I have had occasional difficulty in suppressing an air of knowledgeable condescension when listening to psychologist colleagues, whom I have treated as patients, who have been restored to wellness by such treatments as MAOIs and ECT, having previously been desperately frustrated at their failure to benefit from CBT.
My advice was, in general, that if you were well enough to do CBT then you were not seriously depressed, and if you were seriously depressed then you would not be cognitively or motivationally able to do CBT.

I would go as far as to say that is a practical test of whether a person needs primarily medical treatment, or primarily psychological treatment.

I should record here that I actively disliked giving ECT; it entailed getting up at an ungodly hour in the morning to do it before the surgeons started on their general surgery list, it was a treatment patients were sometimes disquieted about, and it involved all the palaver of consent procedures and making sure all the family were comfortable with it. The financial recompense to me was poor and it often incurred burdensome added costs for patients. Furthermore, it did not always produce a sustained improvement. Therefore, I was disincentivised to use ECT: however, I did it — occasionally — I doubt that I gave half a dozen courses of ECT in my whole private practice career — because it made seriously ill patients better: I would not want any patient, or patient’s family, to discount the dramatic benefit it can confer as a result of the somewhat hyperbolic comments of these psychologists who (like most psychologists) probably have little (or no) firsthand experience in the field.

I would always offer and advise a course of an MAOI before considering ECT: because of that policy I rarely needed to give ECT; most patients got better with the MAOI.

However, that is another story — in most western countries few people referred for ECT have been offered and MAOI beforehand, which in my view is not just illogical, but also sub-optimal practice.

Astonishingly, and regrettably, most guidelines and discussions on ECT do not even mention MAOIs. Anyone who encounters a doctor who refuses to discuss ECT or dismisses it ‘out-of-hand’ — and the same goes for MAOIs — has met a person who neither understands serious depression, nor the life-saving and life-changing benefits that may be conferred by MAOIs and ECT. In such circumstances it would be advisable to seek a second opinion from someone experienced in the treatment of serious depression. Worryingly, the ‘psycho-pharmacologically competent’ sub-species of psychiatrist seems to be becoming increasingly rare.

The remainder of this commentary concerns research and evidence focusing especially on For Whom? How Good? For How Long? It is not a comprehensive overview of ECT, but rather is to explain and contextualize my view about a prior trial of an MAOI. Those wishing for more on particular aspects will find

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the sources and references cited herein contain much further information.

A few key questions are: what sort of depression responds to ECT? For how long do patients stay well afterwards? Why do some doctors refuse to use it? What are the disadvantages and side effects?

**Response and relapse**

The RANZCP document referenced to below is one recent authoritative summary of many points.

**For whom?**

Those suffering severe and/or treatment refractory ‘endogenous’ illness. The idea that particular 'endogenous' symptoms predict a differential response to ECT is not strongly sustained by evidence. It seems that with endogenous/biological depression the same high percentage of patients respond irrespective of the 'sub-type' of depression they have. i.e. the presence of retardation, melancholia, agitation, or psychotic features does not reliably predict a significantly different proportion of responders.

**How good and for how long?**

When making comparisons with drug treatment two points require consideration. First, patients reported in drug trials are significantly less severely ill than those in ECT trials [30]; second, improvement in ECT trials is often reported as remission (i.e. effective absence of symptoms equating to wellness and recovery), as opposed to drug trials that often use the measure of a 50% improvement in rating scale scores. One must be careful to compare like with like, this indicates the superiority of ECT over most anti-depressants is probably greater than it might at first seem. MAOIs are the exception to that statement.

The latest analysis of data indicates that with ECT at least 50% of patients have excellent improvement from what was previously a treatment resistant illness and 50% of those who improve remain well at 12 months after their ECT [31-35]. In context, that is a good result. It is one most anti-depressant drugs (except MAOIs) can only aspire to.

Depression in those with Bipolar Disorder may respond more rapidly than unipolar depression, but the final proportion of responders is probably the same for both [36-38].

ECT does appear to produce a somewhat more rapid improvement than medication (except MAOIs) and may be
particularly good in elderly patients who seem to have an even higher response rate [39]. In some circumstances this more rapid response may be an important consideration and benefit.

**Recent data about relapse post–ECT**

There is an analysis and comment about relapse post ECT in the British Medical Journal [40] the full text .pdf of which is available free from the link in the references below. It rightly emphasises that, of ECT responders, 50% remain in full remission one-year post-ECT.

**Side effects**

The inter-relationships between energy, motivation, cognition, and anxiety all influence the subjective and objective assessment of thinking and memory which makes estimating the possible adverse effects of ECT complex. One must not forget that severe depression may cripple people’s mental functioning in all these domains, to the extent of causing difficulty in distinguishing depression from dementia. Also long-lasting severe depression may contribute to the exacerbation of general ill-health, cardiovascular disease and therefore dementia and stroke [41-43]. To the extent that ECT may cause memory deficits it is appropriate to balance that against reduction of future complications resulting from improvements in the illness brought about by ECT.

General mental functioning may improve rapidly: Semkovska [44] suggested from their meta-analysis of a total of nearly 3,000 patients: ‘After 15 days, processing speed, working memory, anterograde memory, and some aspects of executive function improve beyond baseline levels’.

Further analysis concerning ECT effects on memory is contained in the sources below, and these references [3, 4, 45-54].

Some patients are distressed by sometimes severe disturbances in autobiographical memories. This aspect of memory difficulty is stressed more in non-medical sources, not all of which are purely anecdotal [46]. Research on this is incomplete, see relevant references for further information.

**Patients opinion and experience**

Patients opinions and subjective experience vary, as would be expected [54-56], and there is an interesting paper about doctors who have had ECT [57], as well as ‘UTube’ videos by doctors, both for and against. Most patients rate ECT as comparable to minor medical procedures or going to the dentist, but others have more powerfully negative views.

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The ‘Brain Damage’ Question

There is no evidence that ECT adversely affects brain structure or causes brain ‘damage’ — the opposite would appear to be the case. There is now good evidence that ECT reverses area specific changes, including atrophy, that seem to be associated with severe depression. This is an expanding area of research because new ultra-sensitive imaging technologies are continuing to produce better data, suggesting probable improvements in brain volume, structure and connectivity after ECT; this is attested to by a host of recent studies [58-71].

Relapse, drugs & lithium

The rate of relapse is around 50%, especially without appropriate follow-up drug treatment, which is considered by most experts to be essential. Approximately half of those who relapse do so within four weeks after ECT. There is not a substantial body of evidence concerning which particular drug treatment is most effective in preventing relapse, research points to nortriptyline and lithium as good choices: lithium and MAOI and would be my choices. It is interesting that the putative SNRI, venlafaxine, does not appear to be any more efficacious than nortriptyline, indeed it may be less efficacious ***. Both are significantly more effective than no treatment [32, 72, 73].

*** I interpret this finding as strongly supporting my contention that venlafaxine is not really an SNRI and is not superior to tricyclics like nortriptyline, amitriptyline or clomipramine. After all, this (large) group of patients are comprised of those who have been proven to have a biological illness responsive to ECT so they represent a purer sample of responders that would be expected to readily reveal any actual superiority of one drug over another by virtue of a lesser relapse rate. The fact that venlafaxine was less effective is therefore a telling indictment of its poor effectiveness.

There is also good evidence that lithium reduces relapse in the first 6 months after index ECT [73-75] and commentators seem bemused (rightly, in my view) by its underutilisation in this context. Auditing of ECT practice demonstrates that even after many years of use and education ECT treatment (like lithium treatment) is often performed sub-optimally. As Atiku et al. stated [75]:

“Not all psychotropic medication prescribing for patients receiving ECT for depression followed available and current guidance or consensus. More needs to be done to understand the reasons for the reluctance to use lithium if relapse rates after ECT are to improve” [75].

I cannot avoid commenting that this is yet another example that justifies my scathing opinion of the medical competence of many psychiatrists — it is disgraceful and reprehensible that practice of such an important medical procedure is, and has been for decades, so poorly taught and audited. The profession should be ashamed.
Present data are inadequate to guide the specific medication choice for post-ECT prophylaxis. In my opinion lithium combined with either clomipramine or nortriptyline, or lithium with tranylcypromine are strong contenders for a ‘first division’ choice. It is not coincidence that ECT and lithium are both underused and under-studied because there is no financial backing promoting their use, whereas vast money, effort and influence from pharmaceutical companies has gone into promoting newer drug treatments.

Type and Frequency of ECT
See relevant references

Efficacy of Continuation ECT
The balance of evidence at present suggests that in rare cases maintenance ECT may reduce relapse especially in combination with antidepressants [72, 76].

Surveys and Audits of Practice Worldwide
Context is provided by this review of the contemporary use and practice of electroconvulsive therapy worldwide [77]. National guidelines, supervision, auditing of standards, and continuing education seem now to be dragging ECT into the third millennium and seem generally adequate, even if not benchmark: see references. If that sounds like ‘damning with faint praise’ then you have read my comments correctly.

Opinion and Conclusion
There is little doubt that ECT is a safe and effective treatment for severe depression, when that has not responded to other treatments: chronic depression engenders a higher risk of morbidity and mortality than ECT.

I have written extensively about the mediocre quality of research concerning drugs and other treatments in psychiatry. It is exceedingly disappointing that, well into the third millennium, such glaring deficiencies exist in the quality of the research underpinning important treatments — therefore the Read et al. critique, irrespective of one’s agreement or disagreement with particular details, should be welcomed. Hopefully this will stimulate some overdue and methodologically better studies.

ECT should be administered by an accredited clinic and practitioner (see sources).
If in doubt see references about auditing etc. (e.g. ISEN), and/or seek advice from the nearest university teaching hospital (almost all of which use ECT regularly).

**I would suggest consideration of a course of an MAOI before embarking on ECT.**

Although ECT can be an effective treatment for severe depression therapeutic resolve should not be lessened if it fails; patients may subsequently respond to appropriate regimes designed for refractory cases [78]. Whether clinicians consciously adopt this negative 'last resort' view or not, they do tend to behave as if it were true; if a patient remains ill after ECT it is my experience that doctors give up on drug treatment.

Finally; the science is clear in showing that RCTs (and hence EBM) are 'autonomous of the basic sciences and blind to mechanisms of explanation and causation' and that 'causality, it is the *sine qua non* for all understanding and progress'. RCTs repeatedly demonstrate that A>B>C>A — after which you disappear up your own fundament: it is like Penrose stairs with drugs. It is time to break free of the hegemony of RCTs which for too long have been an albatross around the neck of sensible clinical science.

**Sources of Information on ECT**

**Wikipedia (a balanced appraisal)**


The International Society for ECT and Neurostimulation (ISEN), formerly the Association for Convulsive Therapy (ACT),

[https://www.isen-ect.org](https://www.isen-ect.org)

Sherwin Nuland’s TED talk: How electroshock therapy changed me

Royal College of Psychiatrists’ Centre of Quality Improvement: ECT Accreditation Service (ECTAS) Standards for the administration of ECT Twelfth Edition: January 2015


ECT Accreditation service (RC Psych)


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Electroconvulsive Therapy (ECT) – Summary of The RANZCP Guidelines: by Dr Sanil Rege


Mayo Clinic

http://www.mayoclinic.org/tests-procedures/electroconvulsive-therapy/basics/definition/prc-20014161

Administration Guidelines


Black Dog Inst.


SANE Australia

https://www.sane.org/information/factsheets-podcasts/445-electroconvulsive-therapy-ect

The Scottish ECT Accreditation Network

Scottish Electroconvulsive Therapy (ECT) Accreditation Network (SEAN)


National Prescribing Service (NPS)

http://www.nps.org.au/conditions/mental-health-conditions/mood-disorders/depression/for-individuals/treatment/what-is-ect

References


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