of some of the limitations of the trial, but are surprised that claims are still being made that the study demonstrates that CTOs do not achieve their principle purpose of reducing relapse and readmission.[2]

Imagine a hypothetical RCT comparing medication with placebo. The trial would be powered based on estimated effect size and its duration would be based on expected time for response. If, in this scenario, 25% of those in the placebo arm had inadvertently been given the active drug, and if the duration of the study had been only a third of that planned, it would be inconceivable that the investigators would claim a negative result proved the drug ineffective. Yet this is analogous to what has taken place with OCTET.

In OCTET, median length of compulsion in the community was 183 days in the CTO group *v.* 8 days in the Section 17 group. Although this seems to indicate that it was a trial of people who were largely either subject to long periods of community compulsion (CTO group) or only a few days of compulsion (Section 17 group), a more detailed examination brings this into question. Almost 25% of the Section 17 group were still subject to compulsion by the end of the study, and the mean length of compulsion in this group was 46 days. In the CTO group, only 50% were subject to compulsion by the end of the study, with a mean length under compulsion of 170 days. This has two main implications.

First, the difference in mean length of compulsion between the CTO group and the Section 17 group was only 125 days, or a little over 4 months. It is questionable whether this is sufficient time for any benefits of CTOs to become apparent, and presumably the initial intention had been to compare 12 months in each arm.

Second, in effect, a quarter of the control group were receiving the same type of intervention as the CTO group throughout the course of the study. Any possible benefit in the CTO group would have been offset by the same effects in a large number of control subjects, leading to a large reduction in the power of the study and to type 2 error. The sensitivity analysis does nothing to address this loss of power. We contend that given these problems, in conjunction with the broader issues of recruitment and selection,[3] it is not possible to claim that OCTET demonstrates CTOs to be ineffective.

1 Burns T, Racks J, Molodynski A, Dawson J, Yeeles K, Vazquez- Montes M, et al. Community treatment orders for patients with psychosis (OCTET): a randomised controlled trial. *Lancet* 2013; **381**: 1627–33.

2 Burns T, Molodynski A. Community treatment orders: background and implications of the OCTET trial. *Psychiatr Bull* 2014; **38**: 3–5.

3 Curtis D. OCTET does not demonstrate a lack of effectiveness for community treatment orders. *Psychiatr Bull* 2014; **38**: 36–9.

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The OCTET trial, community treatment orders and evidence-based practice

Based on the findings of the OCTET study,[1] Burns & Molodynski reject observations of consultants who reported directly observable benefits from community treatment orders (CTOs). They argue that it is not possible to 'see with one's own eyes' a probabilistic outcome that takes months to manifest itself.

This is a false analogy. In a subgroup of patients, CTOs result in a striking improvement in treatment adherence: if the CTO is lifted, patients discontinue treatment; re-implement the CTO (following relapse and re-hospitalisation) and treatment adherence is achieved again. In such cases, clinicians are able to 'see' the effect of CTOs on treatment adherence and reasonably expect improved clinical outcomes in the longer term. With such a dramatic response (treatment adherence) to the intervention (CTO), it would be scientifically unnecessary,[2] and ethically unacceptable, to refer patients to a randomised controlled trial (RCT).

A number of previous reports have highlighted the potentially detrimental flaws in the methodology of the OCTET,[3,4] which could explain the apparent paradox between the naturalistic observational studies that have shown significant benefit from CTOs,[5] and the negative findings of the OCTET.

Take the scenario of a young man with chronic schizophrenia, who attends the psychiatric out-patient department escorted by his carer. He has a long history of non-adherence to treatment, as well as multiple formal admissions. The patient is known to discontinue treatment immediately after discharge from hospital, invariably leading to rapid relapse and hospitalisation. Since discharge from hospital on CTO 3 months earlier, his mental stability has been maintained and he has been accepting his fortnightly antipsychotic depot injections. His positive psychotic symptoms are minimal. He has become more sociable and has applied for a part-time college course. The psychiatrist tells the patient and his carer that he is going to lift the CTO. To his dismay, the carer asks the psychiatrist 'Have you not seen with your own eyes that the CTO works?' The psychiatrist replies, 'Yes I have, but an RCT says this could not have been possible'. Would this be evidence-based practice?

1 Burns T, Molodynski A. Community treatment orders: background and implications of the OCTET trial. *Psychiatr Bull* 2014; **38**: 3–5.

2 Glasziou P, Chalmers I, Rawlins M, McCulloch P. When are randomised trials unnecessary? Picking signal from noise. *BMJ* 2007; **334**: 349–51.

3 Mustafa FA. On the OCTET and supervised community treatment orders. *Med Sci Law* 2014; **54**: 116–7.

4 Segal SP. Community treatment orders do not reduce hospital readmission in people with psychosis. *Evid Based Ment Health* 2013; **16**:116.

5 Rawala M, Gupta S. Use of community treatment orders in an inner-London assertive outreach service. *Psychiatr Bull* 2014; **38**: 13–8.

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Author reply: Evidence matters (hopefully). Dr Owen (like Dr Curtis[1] whom he cites) fails to distinguish between intervention and outcome in the OCTET trial. The intervention is the imposition of a community treatment order (CTO). The time under initial compulsion (183 *v.* 8 days on Section 17) demonstrates a clear and unequivocal difference. Where his figure of only 50% of CTO patients experiencing compulsion comes from baffles us. The difference in the total time under compulsion during the 12-month follow-up that he cites

includes the difference between the two outcomes (which includes in-patient compulsion from readmissions in both groups). There is no evidence that recruitment and selection were biased in any way and again we fail to understand on what Drs Owen and Curtis base this criticism. We adhered to the highest research standards throughout and the study has been extensively and rigorously peer reviewed.

Dr Mustafa in his letter advances no scientific critique of our work but does articulate the common response of many clinicians – 'I have seen it work'. We have sympathy with this – we both entered this study expecting to find improved outcomes from CTOs. However, they do not deliver them and we were as disappointed as Dr Mustafa. Psychiatry has a long history of clinicians clinging to ineffective treatments convinced that they work. This is not surprising given the variation in outcomes in psychiatry and the fluctuating natural history of psychoses. Naturalistic observational studies do not prove otherwise – they have produced contradictory results, some for, some against.[2] That is why we need rigorous randomised controlled trials. OCTET is such a rigorous trial and its findings, however unpalatable to some, are robust. It is also worth remembering that the only two other trials found the same.[3] A profession that aspires to evidence-based practice should take these results seriously.

1 Curtis D. OCTET does not demonstrate a lack of effectiveness for community treatment orders. *Psychiatr Bull* 2014; **38**: 36–9.

2 Maughan D, Molodynski A, Rugkåsa J, Burns T. Community Treatment Orders: a systematic review of clinical outcomes. *Soc Psychiatry Psychiatr Epidemiol* 2014; **49**: 651–63.

3 Rugkåsa J, Dawson J. Community treatment orders: current evidence and the implications. *Br J Psychiatry* 2013; **203**: 406–8.

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Insulin coma therapy

Anyone working in an insulin unit in the 1950s would not recognise Dr Pimm's account of the results of their treatment,[1] or details of what it involved. The patients received daily and increasing doses of insulin, rising to many hundreds of units, for a 6-week period. The depth of the resulting hypoglycaemic coma was determined by the patient demonstrating a Babinski response over a period of 15 min. They were then revived by ingesting glucose.

I worked in the insulin unit at Newcastle General Hospital from 1956 to 1959, when I was senior registrar to Sir Martin Roth. Insulin treatment was reserved for people experiencing their first attack of schizophrenia, and from memory I would say half made a complete remission and another 25% improved. Nobody thought that we were effecting a cure, but remissions lasted about 2 years. One woman relapsed 9 years after her treatment. Of course there were dangers, but in those days the alternative was incarceration in a locked ward in a Victorian asylum, with little hope of rehabilitation or discharge.

Martin Roth was an intellectual giant, but also a man who was perspicacious and compassionate, and who would not have contemplated using such a treatment if he did not think it effective. The depth of the coma seemed to me to be critical in terms of remission. A few patients did not regain consciousness when given glucose, but usually 'came out of it' after some hours, although there was the occasional death. Very occasionally, a patient who was clearly psychotic who had an 'irreversible coma' on recovery was greatly mentally improved. These days, people find this difficult to believe, but I witnessed it on one occasion. I find it inconceivable that a multitude of psychiatrists, working in Europe and North America over 25 years, would not have noticed that the treatment they were giving was having no effect, when it clearly was, if only for a limited period. The real question was not whether insulin worked but how did insulin work.

I have no wish to minimise the success of Dr Bourne's crusade, but what made insulin units redundant was the realisation that the new antipsychotic drugs actually worked, and at last, we had an effective, cheap and long-lasting method of managing a seemingly incurable disease. This was generally accepted by 1960.

1 Pimm J. Dr Bourne's identity – credit where credit's due. *Psychiatr Bull* 2014; **38**: 83–5.

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Hope and hopelessness in carers of a relative with schizophrenia

In her editorial, Rebecca McGuire-Snieckus warns clinicians against promoting optimism in their clients, since this can lead to unmet expectations and negative reactions when such expectations are not realised.[1] In his commentary on the editorial, Femi Oyebode criticises Martin Seligman for exaggerating the importance of happiness at all costs as a goal of existence, and quotes Aristotle as stating that it is the mark of a courageous man to face things that are terrible to a human being.[2] I wish to illustrate this in the context of family carers of relatives with schizophrenia. In particular, I focus on the overinvolved carer who is unable to relinquish her/his hopes and expectations for the affected relative. They are readily recognised by habitually referring to their relative in the past tense, for example, 'she was such a beautiful girl' or 'he was such a good student'. This form of speech reveals the fact that the carer is living in the past and has not come to terms with the reality of their relative's illness. This is particularly hard on the patient, who then feels driven to attempt to satisfy the carer's need for their success, and fails again and again. The remedy is to offer the carer grief work to mourn their losses and to accept the reality of their relative's disability and release both parties from this impasse, enabling them to develop a more realistic view. The patient will also benefit from grief work, administered separately from the carer.

1 McGuire-Snieckus R. Hope, optimism and delusion. *Psychiatr Bull* 2014; **38**: 49–51.

2 Oyebode F. Should psychology be 'positive'? Letting the philosophers speak. Commentary on . . . Hope, optimism and delusion. *Psychiatr Bull* 2014; **38**: 52–3.

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