

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.

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- 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.
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- 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 Curtis¹ 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