

Correspondence

Naturalistic studies evaluating 'real world' OPC patients are welcome

Debate ensues as to whether randomised controlled trials are realistically the best method of evaluating the impact of out-patient commitment (OPC) on hospitalisation.¹ Even a large and well-funded prospective randomised controlled study such as the OCTET² has been demonstrated to be potentially fundamentally flawed.³

In this context, naturalistic studies evaluating 'real world' patients, such as that reported by Castells-Aulet *et al*,⁴ are welcome and potentially useful. However, I would like to point out three issues that the authors may wish to respond to.

First, given that both controls and OPC patients had their index admissions within the same month, one could reasonably assume that the treating physicians must have had clinical grounds for choosing to place only patients forming the latter group on OPC. Those physicians may have drawn on their knowledge of individual patients (which is not necessarily reflected by the general characteristics described in the study) in reaching their decisions. For instance, the treatment adherence status before the index admission (which, remarkably, differs between the two groups) may have been used, understandably, as an indication of the suitability of patients for the OPC. Hence, one could justifiably doubt the similarity of the two groups, undermining any conclusions that could be drawn from the results.

Second, there potentially could have been another detrimental selection bias in the control group. Patients who were initially discharged informally, but were subsequently readmitted within the following 2 years and then discharged again on an OPC, would have been automatically excluded from the control group, which eventually comprised only patients who, even when re-hospitalised, were not considered by their physicians as requiring OPC, and thus introducing a type II error.

Thirdly, the authors fail to elaborate on the apparent general trend of reduced hospitalisation over the 4-year period, which may have been driven by factors that could potentially confound the results of this study.

- 1 Swanson JW, Swartz MS. Why the evidence for outpatient commitment is good enough. *Psychiatr Serv* 2014; **65**: 808–11.
- 2 Burns T, Rugkåsa 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.
- 3 Mustafa FA. On the OCTET and supervised community treatment orders. *Med Sci Law* 2014; **54**: 116–7.
- 4 Castells-Aulet L, Hernández-Viadel M, Jiménez-Martos J, Cañete-Nicolás C, Bellido-Rodríguez C, Calabuig-Crespo R, et al. Impact of involuntary out-patient commitment on reducing hospital service: 2-year follow-up. *Psychiatr Bull* 2014; 20 November [epub ahead of print]

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Authors' reply: There is a lack of scientific evidence evaluating the efficacy of OPC¹ and doubt around whether randomised

controlled trials are realistically the best method of evaluating the impact of OPC.² Our work is a retrospective case-control study, with all its limitations, and we welcome any contribution that could offer us some improvement for further studies.

Responding to the issues raised by Dr Mustafa, I would like to comment as follows.

First, we had considered in our study that there were differences in the motives for index admissions between the groups. In the OPC group the main reasons were clinical decompensation because of non-adherence to treatment (78%) and aggressive behaviour (22%). In the control group, admission occurred mainly because of clinical decompensation without a clear non-adherence to treatment (47%) and the reasons were inconsistent use of medication, changes in the pharmacological pattern or substance misuse. This could undermine the similarity of the two study groups and, therefore, the suitability for comparison.

Second, owing to the nature of the study, patients eligible for the control group were automatically excluded if within the subsequent 2 years of the study they were placed on OPC. Third, we agree there is a trend of reduced hospitalisation over the 4-year study period in both groups. This may have been driven by factors such as the improvement of community services or home services that could potentially confound the results of this study.

1 Kisely S, Campbell LA. Does compulsory or supervised community treatment reduce 'revolving door' care? Legislation is inconsistent with recent evidence. *Br J Psychiatry* 2007; **191**: 373–4.

2 Swanson JW, Swartz MS. Why the evidence for outpatient commitment is good enough. *Psychiatr Serv* 2014; **65**: 808–11.

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Cardiovascular screening in severe mental illness still awaits an evidence base

I welcome any contribution that might improve the shocking mortality disadvantage associated with severe mental illness. Yeomans *et al*¹ should, however, be aware that this issue was identified long before the 1990s. William Farr reported in 1841 that the mortality of lunatics in England and Wales varied between three and fourteen times from that of the general population.² He concluded that some of the excess deaths 'may be fairly ascribed to insanity. The excess above this must be attributed to the diseases generated by the limited space in which the unhappy lunatics are confined – to the collection of large numbers under the same roof – the impurity of atmosphere – the want of exercise and warmth – the poor unvaried diet – and the deficiency of medical attendance.' *Plus ça change*.

While it is positive that the excess natural mortality of mental illness is attracting greater attention, we should be wary of blindly jumping on the cardiovascular screening