Author's response to reviews

Title: The effectiveness of Assertive Community Treatment for elderly patients with severe mental illness: a randomized controlled trial

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Author's response to reviews: see over
Dear Mr Carlo Rey Chua,

Thank you for your email of 16 January 2014 and for the opportunity of changing our manuscript. We enclose our point-by-point response to the comments.

Sincerely,

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Point-by-point response

Comment 1

I suggest adding a sentence on testing the distributions of continuous variables in ‘Statistical analyses’.

Answer to comment 1:

In ‘Statistical analyses’ we added the sentence "First, all variables were checked for outliers and missing values. Next, we compared..." We did not use formal normality tests since these tests have no power in small samples, whereas the percentage signaling deviation from perfect normality increases with sample size. And if the test is not significant, we cannot conclude that the test confirmed the validity of the normality assumption. In accordance with CONsolidated Standards of Reporting Trials (CONSORT; Moher et al 2010) we dropped significance tests of baseline differences and present baseline information as descriptive statistics. (see page 13).

Comment 2.

It can be argued that the HONoS65+ is not a very good measure of psychosocial functioning. This could be mentioned as a further limitation of the study.

Answer to comment 2:

On the Strengths and limitations section we added:

“Furthermore it is possible that differences in psychosocial functioning between the intervention and control group were not detected because of measurement limitations. The sum score of the HoNOS65+ has been criticized for not properly measuring change in psychosocial functioning.”
**Comment 3**

In ‘Statistical Analyses’ (1st paragraph), analysis of the secondary outcomes of mental health care usage is not described. The reasons why these analyses were not undertaken were mentioned later in the ‘Results’ section, but I suggest the intention to analyse these secondary outcomes should be mentioned in this ‘Statistical Analyses’ section.

**Answer to comment 3:**

In the Statistical Analyses paragraph we added the sentence:

“Finally, the Wilcoxon test was used to investigate secondary outcomes, including within-group changes in unmet needs, the number of hospital days and crisis contacts during follow-up”.

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**Minor Essential Revisions**

**Comment 1**

The 1st paragraph of the ‘Background’ mentions one of the explanations advanced for the mixed effects of European ACT studies. I suggest expanding this to include some of the others – see for e.g. Rosen, A., Killaspy, H. & Harvey, C. Specialisation and marginalisation: how the Assertive Community Treatment debate affects individuals with complex mental health needs. The Psychiatrist, 37, 345-348 (early on-line).

**Answer to comment 1:**

In the Background paragraph we added:

“… due possibly to better quality of care in the TAU control-conditions and/or inadequate implementation of key ACT-components. Reduced effectiveness of ACT in Europe could also be explained by a loss of focus on preventing admissions when ACT-teams are confronted with very strict admission criteria, or conversely, when in-patients beds are readily available.
Comment 2

Related to the previous point, the ‘Introduction’ does not mention what is known about critical ingredients of ACT, although these are mentioned in the ‘Discussion’. I suggest the ‘Introduction’ would be improved by briefly mentioning these – again see above reference as a starting point.

Answer to comment 2:

In the Background paragraph we added and changed the text in:

“In the United States, ACT reduced hospital admissions more than treatment as usual (TAU). Critical components of ACT associated with reducing hospital admissions were shared caseload, community based services, 7x24 our services, a team leader who participated in patient care, full responsibility for treatment services, daily team meetings and time unlimited services. European studies however showed mixed effects of ACT when compared with TAU, due possibly to better quality of care in the TAU control-conditions. Although there is no agreement on which critical components of ACT are associated with psychosocial outcomes, better outcomes have both been shown to be associated with having a better team structure and with having a consumer provider in the team. Better engagement was associated with a smaller caseload in ACT than in TAU, and with a shared caseload”.

Comment 3

The ‘Methods’ (1st paragraph concerning ‘Intervention’) should mention in the general description of ACT that assertive engagement is an essential feature of ACT programs and that ACT typically targets those with frequent admissions and / or those who are difficult-to-engage.
Answer to comment 3:

In ‘Methods’ we added the sentence “ACT is developed for patients who are high users of inpatient hospital services and who are unwilling to use mental health services”.

Comment 4

In the ‘Methods’ (6th paragraph of ‘Trial Design and Recruitment’) – I suggest state why inclusion criterion 3 was dropped after 19 months.

Answer to comment 4:

In ‘Methods’ we added the sentence “We also dropped problems in various domains as inclusion criterion, since patients often received no medical or psychiatric treatment so that only limited information was available when patients were referred.”

Comment 5

5) The ‘Discussion’ concerning the lack of effects of ACTE on psychosocial functioning could mention that the differences in model fidelity between ACTE and CAU were small, as well as the low intensity of contact, as plausible explanations.

Answer to comment 5:

In the Discussions section we added: “Third, TAU used components of ACT, so in some critical components of ACT differences between the intervention and control group were small. Also contact frequency in ACTE was low. Lack of integrated care and degree of contacts was associated with lack of differences found in effect studies in Europe”.
Comment 6

The 1st paragraph of the results in the ‘Abstract’ and subsequent text mentions the high number of patients lost to follow up. This is unusual for ACT and deserves specific comment. I wonder whether it is linked with the low frequency of contact mentioned in the 5th paragraph of the ‘Discussion’?

Answer to comment 6:

In the Method section we added to the comments on Figure 1: “Patients were lost to follow-up because they were deceased or could not be contacted – sometimes because they were admitted to in-patient services (elderly homes) but mostly because patients didn’t open the door or refused contact.” We do not have evidence that the high number of patients lost to follow up is in response of the low frequency of contact.

Comment 7

The ‘Results’ section would benefit from an additional table showing all the item scores for the DACTS for both ACTE and CAU. This might assist in illustrating how similar (or not) CAU was to ACTE (as mentioned in the 4th paragraph of the ‘Discussion’) as well as in trying to understand any reasons for the lack of difference between the two approaches with regard to psychosocial outcomes. These additional details will also guide other researchers in planning future studies.

Answer to comment 7:

We have added a table with difference in DACTS scores between ACTE and TAU (Table 1).