Reviewer’s report

Title: Dual diagnosis clients' treatment satisfaction - a systematic review

Version: 1 Date: 22 December 2010

Reviewer: Morten Hesse

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Overall, this is an important review of an emerging subject. The search strategy is well-described, and a rich amount of relevant information is given about individual studies.

However, the reporting of the findings can be improved. Some of these improvements are simply a question of summarizing the findings in a more transparent fashion. One major step forward for this study would be to report quantitative syntheses of the data for two of the main questions.

Minor essential revisions.

There is considerable evidence that independently of types of services and patients, measures of patient satisfaction and patient assessed therapeutic alliance are skewed towards high scores. This must be mentioned in relation to conclusions regarding "high treatment satisfaction". After all, patients will often be able to withdraw from services that do not provide adequate care.

In the discussion of treatment related predictors of treatment satisfaction, it is important to clarify whether this is at the patient level, or at the program level. If a patient that receives more services is more satisfied than his peers within the same program, this may indicate that the satisfied patients are also those who are better at asking for help in relevant manners, clouding the association between cause and effect. If, on the other hand, the programs (or clinicians) that provide better or more services have on average higher satisfaction among their clients compared to programs or clinicians that provide poorer or less complete services, then we have strong evidence of a causal link between services and satisfaction. This should be clear from the narrative review.

Throughout the results, the authors must report whether or not random allocation was used for individual studies comparing integrated and non-integrated services. It is my clear impression that the studies in table 5 did, but it is not clearly stated anywhere, and if patients are either self-selected or referred, serious confounding is present.

Additional file 5 should be in the main body of the text, and should be revised to give a simpler and clearer understanding of the studies. Shorter text is needed, and basic information such as number of patients and main co-morbid diagnosis should be included.

A similar table showing the studies of differences between single and dual
diagnosis patients with number of patients and main diagnosis should be added to the text.

This is a nice reference for the reader. A similar table should be provided for studies comparing single and dual diagnosis patients.

Discretionary revisions.

In the introduction, the authors might consider some reflection over the amount of systematic error in clinical endpoints versus treatment satisfaction. In particular, I am thinking about the effect of unpredictable life events, such as losses, patients’ or their family members’ serious illness, or a range of other events that are not present at randomization but may impact patients’ lives much more than treatment alone, leading to deterioration in clinical outcomes. Unlike clinical outcomes, I would believe (but cannot document) that such events would have only a modest influence on treatment satisfaction, whereas they can have a tremendous influence on clinical outcomes.

Whilst not essential, I strongly recommend that the authors consider doing a quantitative synthesis of their data. A number of software packages are available that will allow the authors to do such a synthesis, many of them are even free (e.g., from the Cochrane Collaboration). This synthesis will strengthen the study considerably. At current, the conclusions of the study rest on what can be called a "vote count". With very small samples (some of them are), negative conclusions may mask moderate relevant differences. This is mentioned, but the conclusion that "several underpowered studies showed no relevant differences" is not very convincing as a synthesis of anything.

A forest plot of effect sizes for each of the two main comparisons (i.e., differences between DD and single diagnosis groups, and differences between integrated and non-integrated services) will give readers a good idea about the consistency of the findings, and even about the impact of sample size (because confidence intervals will give a strong visual illustration of this). Thus, it is not so much the summarized effect size that will be of interest, as it is the ability to compare effect size between studies.

Note that a random effects model must be used, due to variability in methods and samples.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.