Author's response to reviews

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

Authors:

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Author's response to reviews: see over
Dear Editorial Board Members of BMC Psychiatry,

We are pleased to enclose our revised manuscript on treatment satisfaction and would like to express our sincere thanks to the reviewers. Their comments were very useful and helped us improving the manuscript. We appreciate your continued interest in the manuscript and we should be most grateful if you consider it for publication in BMC Psychiatry. A point-by-point response to the reviewers’ concerns follows below.

Thank you very much.

Yours sincerely

Dr. Sabrina Schulte
Prof. Dr. Petra Meier
Dr. John Stirling

Note: Revisions in the manuscript have been marked by tracked changes and/or yellow highlighting.
REVIEWER 1
Reviewer's report:
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.
1. 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.

Authors' response:
We agree that this is an important aspect to address. Therefore, the following section has been added to the manuscript:
Ξ Page 20-21: “Finally, a more general point requires consideration: a recent review has shown that satisfaction studies disproportionately found positive accounts from clients throughout treatment modalities and client populations [70]. In order to avoid misinterpretation of client ratings due to social desirability or other potential bias, safeguards should be applied in future studies such as keeping assessments anonymous and comparing satisfaction ratings of treatment completers and dropouts.”

2. 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.

Authors' response:
We focused during the revision process on those studies that reported findings on treatment process and service-related variables linked to satisfaction. The relevant studies have been once more reviewed and where applicable additional information has been provided in the results section of the manuscript.
Ξ For details, please see highlighted sections on page 15 and 16.
3. 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.

Authors' response:
The relevant information about client selection and treatment allocation has been added to the text as follows:

⇒ p. 13/14: “Another strength of these three studies is that clients were randomly allocated to the different treatment options. In contrast, client selection biases due to non-randomization in the other four studies have to be considered.”

4. 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.

Authors' response:
Additional File 5 has been converted into ‘Table 2’ and is now part of the main manuscript (please see p. 33). Text and other information included in the table has been shortened to simplify understanding and an additional column (“Sample”, highlighted in yellow) has been inserted.

5. 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.

Authors' response:
A new table has been designed (“Table 3”) and included in the main text (please see p. 35). Table 3 follows a similar template as Table 2 and shows findings and characteristics of those three studies that investigated differences in satisfaction levels in DD and non-DD clients.
Discretionary revisions.
6. 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.

Authors' response:
While we acknowledge that this is an important aspect, we feel we cannot directly contribute to this question within the scope and purpose of the current review. We believe that this issue points toward another important area of research and study of its own that could help both practitioners and researchers to understand better the effect of external factors (e.g., unpredictable life events) on some but not all treatment outcomes (e.g. clinical outcomes vs. treatment satisfaction).

7. 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.

Authors' response:
We agree with the reviewer’s perspective that a quantitative synthesis of data could add great value to the manuscript and we intended to follow this recommendation by conducting two separate meta-analyses using RevMan 5. However, a number of factors made us reconsider the feasibility of these analyses for the current review:

- small number of studies available for each meta-analysis (n=7 and n=3, respectively)
- difficulties in the data preparation process: statistics required for calculating effect measures were missing in two articles\(^1\), which reduced the sample for the first meta-analysis from seven to five studies
• high level of heterogeneity in the meta-analysis addressing research question 2: $I^2 > 80$
• highly disproportional weighting of studies in the meta-analysis addressing research question 3: study weights: 91.5% vs. 4.0% vs. 4.5%

Therefore, we decided not to include a quantitative synthesis of the above-mentioned data in the main manuscript. Nevertheless, we would like to suggest - if considered useful by the reviewer – to prepare an additional file comprising the forest plots of both meta-analyses and a brief summary of results.

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We tried to approach the authors of these articles to ask for further details but unfortunately no current contact details could be found.

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**REVIEWER 2**

**Reviewer's report:**

The question by the authors is well defined, and the article is very relevant and thorough. Also the introduction is very well written and easy to read. Methods are appropriate and well described, and data are sound. The manuscript adheres to the relevant standards for reporting and data deposition, and has a fine disposition. Discussion and conclusions are well balanced and adequately supported by the data, and limitations of the studies well discussed. It would be an interesting key aspect to also investigate treatment effect – to show that patient satisfaction do in fact support more positive treatment effect. Finally the authors clearly acknowledge work upon which they are building, both published and unpublished with explicit appendixes/files. In conclusion the article is very suitable for publishing. There are however some need for revision of the article before it can be decided.

**Major Compulsory Revisions:**

1. The selection of potentially relevant studies p.6 are a bit confusing as to the steps that were taken, and could be explained more clear.

   a) 1097 met inclusion criteria, then 128 articles were obtained in full text – does that mean the rest where excluded or? And if yes, on what basis?

   b) That is a bit unclear to me. Also I am curious as to how the screening DD-related articles led to exclusion of 101 articles. Could that be made more clear?

**Authors' response:**

a) We addressed the first two aspects by adding the following information to the manuscript: “As a result, 969 studies were excluded based on the information given in the abstract (e.g. small sample size, participants younger than 18 years).” (p. 6)

b) We believe that the reasons for excluding 101 articles after screening the full texts are already outlined on p. 6 and in Additional File 2 - Flow Diagram 2: Study selection process. Nevertheless, the following information has been added for further clarification: “That is, relevant information (e.g. methods used for assessing satisfaction, sample size, research questions addressed) was extracted from each of the 128 articles so as to determine their eligibility for the current review.” (p. 6)
2. The writing is to a large extent acceptable, but some places language needs rewriting, especially the abstract for fluency, and generally checking for commas, vocabulary and use of certain words (i.e. the word “true” p.15 is a bit odd), use of (N= ) or (n=).

Authors' response:
Language, style and grammar of the manuscript and abstract has been carefully re-read and double checked for inconsistencies. Minor revisions (highlighted in text) have been made in order to improve fluency.

3. The article seems more fluent in the introduction and from p. 11 – have different authors written different parts of the article, and have you rewritten the article for English fluency?

Authors' response:
SJS has written most parts of the manuscript and the co-authors have made revisions throughout the text in different stages of the writing process. We have carefully reviewed the whole manuscript for English fluency and as mentioned above made minor revisions to improve the overall flow of the paper.

Minor Essential Revisions:
4. Would prefer more description of client profiles, i.e.:
   a. SD on age
   b. Although appendix is very detailed and well made, it could be nice with a little more background information in the article itself for its reader-friendliness – not in form of explanations, but in form of two lines of quick overview. For instance, if race is brought up, it would be nice to know about it in a little more details, so could they be described in percentage?
   c. Also details on target groups are interesting and could be described shortly.

Authors' response:
   a. Information about SD on age has been included in the manuscript on p. 8: “The mean ages ranged from 30 to 45 years (SD=6.3-14.0).”
   b./c. The paragraph on client profiles aims to provide the reader with an overview of sample characteristics by pointing towards major tendencies rather than describing specific details for each study sample separately. We are concerned that the inclusion of further details on client characteristics in the article itself (such as percentages of ethnic groups across studies) may lead to an overload of figures in the relevant paragraph. Therefore, we addressed the reviewer’s comment by including extra
information about study samples in Additional File 4 – Table 2. The new column labelled “Sample characteristics” provides the reader with data about gender distribution, mean age and ethnicity for each study.

5. Regarding “Client rapport on treatment satisfaction”:
   a. It is preferred that the high average satisfaction is calculated and explained in the text, although the file 4 shows it in details, again for reader-friendliness.
   b. Also variability in satisfaction scores would be nice to get an overview of in the text along with the other descriptions of pt. variability.
   c. Short overview of what treatment types that “form of integrated DD treatment” refers to would be preferred.
   d. Question: is the multi-study on p. 11 out- or inpatient?
   e. Page 12. Should the new study (31) not be started in a new paragraph like the other studies as for coherency in the analysis disposition?

Authors’ response:
   a./b. We agree with the reviewer that it would be useful to include this information in the manuscript. We considered a table displaying the required data in the main text the most appropriate format due to the fact that a range of satisfaction scales with varying scoring formulas and maximum scores were used by the different authors. Table 1 provides the reader with a detailed overview of clients’ satisfaction ratings and variability of scores for all studies (please see p. 31).

   c. Additional information has been included in the relevant paragraph on p. 10: “All seven studies that investigated associations between type of treatment approach and satisfaction ratings compared a form of integrated DD treatment (i.e. simultaneous care for both the mental health and substance misuse problems by the same provider) with standard care models without specific DD focus [30, 31, 37, 38, 41, 44, 52]. The range of specific interventions and settings of the integrated treatment programs differed to some extent across studies (e.g. depression- vs. trauma-focused care and residential vs. assertive settings; see also Table 2 and Additional File 4).”

   d. The authors of this study (Clark et al., 2008) did not specify whether the nine involved treatment programs were out- or inpatient programs.

   e. The authors agree with the reviewer and the description of study 31 starts now in a new paragraph (please see p. 13).