Author's response to reviews

Title: Factors associated with involuntary admissions among non-psychotic patients with substance use disorders and comorbidity: a cross-sectional study

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Author's response to reviews: see over
Dear Danrolf de Jesus,

Manuscript MS: 1959391252775934 - “Predictors of involuntary admissions among non-psychotic patients with substance use disorders and comorbidity: a cross-sectional study”

Thank you for reviewing our paper and for allowing us to resubmit a revised version for publication in *BMC Health Services Research*.

We appreciate the valuable and constructive comments and have, as far as possible, revised the paper according to your suggestions. We feel that the paper now reads better and underpins the need for improved service provision for the complex issue of comorbid disorders, relevant in an international perspective.

We hope the revised paper is acceptable to BMC Health Services Research’s standards. Please do contact me again if additional issues need to be addressed.

Detailed responses are provided below.

Yours Sincerely,

Anne Opsal
(on behalf of the authors)
Sørlandet Hospital, Addiction Unit
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Reviewer's report:

Reviewer: Jan Arlebrink

If you compare VA and IA maybe some remarks should be made on ethical issues regarding IA; to force someone to treatment has many ethical considerations. To be forced to treatment can also be a very traumatic experience that may end up in a crisis. It would also be interesting to know how many of IA that wanted or accepted this treatment.

Reply: Thank you for your suggestion. We have added the following paragraph to the manuscript:

Involuntary commitment of non-psychotic SUD patients, although a relatively marginal phenomenon, is controversial. To force someone to treatment has many ethical considerations and can also be a traumatic experience that may result in a crisis. Worldwide, there is growing concern regarding ethical issues related to the use of involuntary treatment. Such interference with personal autonomy should not be applied without an evidence-based foundation.

We do agree that it would be interesting to know how many of the IA patients wanted or accepted this treatment. However, most of the patients accepted the treatment, but we have no detailed registered data on this.

Reviewer: Marianne L Lindahl

According to the title the study is about predictors for involuntary admissions, in the abstract the study is about factors associated with admission and in the article the first aim is the description of socio-demographic characteristics, substance use patterns and psychiatric comorbidities. In addition to this aim associated factors was to be investigated. In the Discussion the main objective was to explore relationship between substance use patterns and involuntary admission. It feels as if there is a loss of direction and the authors may need to ensure an alignment between title, aims and the rest of the article. The above mentioned disparities are impediments for understanding the results of the study.

Reply: Thank you to the reviewer for the detailed feedback, which helped to improve the manuscript. The title, abstract, aims and discussion has been changed to ensure an overall alignment, and coherence throughout the article.

The title has been changed to: Factors associated with involuntary admissions among non-psychotic patients with substance use disorders and comorbidity: a cross-sectional study.

The aim has been changed to: The aims of the study were to investigate factors associated with involuntary admission to treatment institutions by describing the socio-demographic characteristics, substance use patterns, and psychiatric comorbidities among SUD-patients involuntary admitted to hospital pursuant to the Social Services Act by comparing them with voluntarily admitted patients.
In the study information has been collected from two groups of patients, involuntary (IA) and voluntary (VA) admitted. The patients in the IA group were recruited from three different centers but the VA group came from one center. There is no discussion in the article about how this may influence the results.

Reply: To include a sufficient number of IA patients, three centers were involved in recruiting IA patients. Before the study started we were discussing whether we should recruit VA patients from all the centers. Due to practical and economical reasons VA patients were included only from one center. We have no reason to believe that this influenced the results in any major direction, although we have no measures to back this assumption.

The period of inclusion lasts from 1st of Jan 2009 until 31st of May 2011 and since only 65 patients were included during the study, this would be approx. 2 per month. If the study was aiming at predictors any external factors influence the process needs to be evaluated.

Reply: The aim was not at predictors (ref. change of title and aim), thus we have changed the aims to factors associated with involuntary and voluntary admissions. External factors that might have had any influences on the process were not evaluated.

Also any changes at the centers that may or may not have provided the same type of initial treatment during the first weeks of stay? The flow needs to be clarified since 11 IA-patients were lost due to logistics but none of the VA, why? And the 11 IA-patients could be considered representing a fairly large group considering that only 65 patients were eligible and agreed to participate.

Reply: All of the eleven IA-patients lost due to logistics reasons belonged to the same research center. The reason for this loss was that that centers’ coordinator was on a long sick leave, and it took some time before we could hire a new person to start including patients again. The VA patients were included in another study center and were not influenced by this.

Since the inclusion of patients continued during almost 2 ½ years and the patients were consecutive, could there be patients with more than one stay?

Reply: The patients were only included once in the study. We had routines to avoid that patients were included more than once in the study.

Could IA-patients be legally transferred to VA?

Reply: The IA-patients could continue in voluntary treatment if they would like to, but they could not be included as VA patients in this study.

It could also be useful to be more stringent when describing the “study subjects” so one can easily understand inclusion and exclusion and the flow. The description could easily be reduced with half and still report all necessary information.
Reply: The description has been reduced in length.

It’s a little confusing with the differences between text in the article and text in tables, for example in text “Female gender, receiving public welfare benefits, and more frequent visits to physicians for somatic complains or injection of drugs during 6 months prior to treatment were all associated with involuntarily admission pursuant to the Social Services Act.

Reply: We have corrected that accordingly.

In table though it reads “Logistic regression analysis of the effect of involuntary admission....” The question is whether it is a description of status or effect? And at what point is the effect measured? It makes it definitely difficult to assess the data since it’s not obvious if there is any other collection of data than during the interview 3 weeks after admission?

Reply: We do agree that we do not measure the effect. The title of table 4 has been changed accordingly.

The second paragraph in the Methods section – Study subjects it is mentioned that patients are interviewed “at least 3 weeks of treatment”, what is the difference between intake and interview for the patients, overall?

Reply: To clarify we have changed the text. The patients were detoxified, verified by either negative urine tests, or a minimum of 14 days spent in detoxification to establish baseline values not influenced by withdrawal symptoms. The intake interview was conducted as part of the standard examination. To be included in the study, the hospital stay had to be more than three weeks. If the stay were shorter than this there was no time to include the patients

That more women are involuntarily treated is also interesting and especially since pregnant women were not included in this study. It would have been interesting if the authors had reasoned a little about the differences between the proportions of involuntarily treated women in Sweden and Norway.

Reply: Yes, it would have been of interest. However, we have now searched relevant literature and contacted experts working in the field and by looking closer into this we were not able to demonstrate any essential differences between the proportions of involuntarily admitted women in Sweden and Norway. The text has therefore been changed to: The sex distribution in Norway was not found to differ substantially from what has been found in Sweden.
The results in type of drugs used by involuntarily (injecting drugs) and voluntarily treated (alcohol) could be explored a little further in the Discussion section. In the US, for example, most states permit involuntary substance abuse treatment, and it would have been interesting to have the results of this study in an international context even though the differences of criteria may be impediment.

Reply: In the discussion part we have given priority to include the most important factors describing differences between the groups. Comparing our results with data from US was not the purpose of this study and we were not able to find comparable parameters from the two countries.

The only reservation is the use of test of statistical significance on very small samples, such as one patient, or two or three and so on.

Reply: We do agree. These P-values are removed from the tables.

The writing could be improved and the text needs a revision. For example Table 1: “usual living arrangements” could be “living arrangements” unless the opposite “unusual” is of importance to the results in the study. Is “no stable arrangements” equal to homeless and what is a “controlled environment” in regard to “living arrangements”?

Reply: We have changed the labels according to the suggestions.

In table 1 there are four headlines or underlined main groups of variables: Education, Sources of financial support, Usual living arrangements and Mental stress scores but there are at the same time another seven variables that don’t seem to be part of any of the main groups or are they? One could also question whether all the information in table 1 is necessary to understand the study since some of the information is or could be presented in text.

Reply: We believe that the information given in Table 1 would be of interest for those readers who are working with this kind of patients, and that this information is better presented in a table than in the text. We therefore have chosen to keep the table as it is.

In table 2 the underlined Substance Abuse, does that information refer to ASI? If so, it should be stated which variables that derives from ASI and ICD-10 respectively.

Reply: We have now stated which variables that derives from ASI and ICD-10 respectively.

Tables 2- 3 present 1. substance abuse, 2. mental disorder and, 3. Logistic regression of female, benefits, somatic complaints and substance use disorder and mental diagnosis (choice of words?) and the tables are relevant when presenting the results of this study.

Reply: We have changed the words as proposed by the reviewer and we are now using the same words as when presenting the results in the study.
A minor reflection: One may prefer that “women” is used instead of “female gender” and “patients” instead of “subjects” or “interviewed” instead of “subjected to”, “mate” could be “friend” or “partner”.

Reply: We have improved the language according to suggestions.

Quality of written English: Needs some language corrections before being published.

Reply: We have standardized the language and are now using the same terms consistently.