Author's response to reviews

Title: Treatment for illegal drug use disorders: the role of comorbid mood and anxiety disorders.

Authors:

Elena Prokofyeva (elena.prokofyeva@biomed-engineering.de)
Silvia S Martins (ssm2183@columbia.edu)
Nadia Younès (NYOUNES@ch-versailles.fr)
Pamela J Surkan (psurkan@jhsph.edu)
Maria Melchior (maria.melchior@inserm.fr)

Version: 2 Date: 5 November 2013

Author's response to reviews:

Simon Harold
Executive Editor
BMC Psychiatry

RE: ‘Health service use in case of an illegal drug use disorder: the role of comorbid common psychiatric disorders’.

Dear Professor Harold,

Thank you for forwarding to reviewers’ comments regarding our manuscript and giving us the opportunity to revise and resubmit our work. Below, please find our detailed responses to reviewers’ comments, which were included in the revised version of the manuscript.

In addition to the changes requested by the reviewers, we would like to modify the paper’s authorship to reflect a reassessment of the contribution of each author.

We hope that these changes address all of the questions raised regarding our paper and look forward to hearing from you.

Sincerely, Maria Melchior

Reviewer 1: Patrick McCabe

1. The reviewer asks us ‘to state explicitly that the outcome under study is substance use disorder treatment as opposed to health services use’.

   We changed ‘health service use’ to ‘treatment for illegal drug use disorders’ in the title, abstract, and text of the manuscript.

2. The reviewer questions our definition of unmet need for treatment ‘[which]
implies that those who did receive services were not asked this same question [about unmet need for treatment], which they were’.

The reviewer is right in having understood that only participants who did not receive illegal drug use treatment were asked whether they ever needed such treatment and why they did not receive it. Participants who did receive illegal drug use disorder treatment were not asked about perceived unmet need for treatment. This is stated in the Methods section (page 10), in the abstract, in Results (page 11), in the main findings section of the discussion (page 12), and in the conclusion.

3. The reviewer indicates that ‘The paper would be considerable stronger if the authors took advantage of the longitudinal nature of the dataset and explored factors that predicted initiation and/or maintenance of substance use treatment including mood and anxiety disorders. If a cross-sectional analysis is used I would recommend only using wave 1 as it has a larger sample size’.

As suggested by the reviewer, we did initially aim to conduct longitudinal analyses to test factors associated with treatment initiation. Unfortunately, due to a low number of participants with incident illegal drug use problems and treatment for substance use disorders between the two waves of the NESARC study (n=226; 97 received treatment and 86 reported unmet need for treatment), we did not have sufficient statistical power for this analysis. However, since new cases of illegal drug use disorder were reported at wave 2, by combining data from wave 1 and wave 2 we increased the sample size of respondents with drug use disorders for our study, similar to what was done in Fenton et al., 2010[1] and Keyes et al., 2012[2]. For clarity, we added this explanation to the sample and procedures section (page 7).

4. The reviewer ‘recommend[s] including prescription drug abuse/dependence as it was assessed as use outside the scope of prescribed medical use (i.e. illegal use)’.

Our measure of illegal drug use disorders included abuse and dependence related to nonmedical use of prescription drugs such as prescription opioids, sedatives, and tranquilizers. This is indicated in the Methods section (page 9).

5. The reviewer has several questions regarding the methods we used: ‘Clarification on variance estimation is needed. The authors note (Statistical analysis paragraph 3) that the analysis was “weighted to account for sampling design”. Were other design features (psu and stratum) used? Was the subpopulation nature of the sample taken into account in the variance estimation? What variance estimation technique was used? Why the choice of reporting unweighted means in Table 2?’

Data were weighted at wave 2 to account for differential loss to follow-up and to be representative of the target population. This analysis includes 34,653 respondents who completed interviews at wave 1 and wave 2. This information was added to the Methods section (page 8).

The final Wave 2 NESARC weight is the product of the NESARC Wave 1 final
weight and three other individual weighting factors (Type A person non interview adjustment, Ratio adjustment, and Type B and Type C person non interview adjustment). These weights were derived by Census Bureau survey methodologists. Other design features were also used, similar to other studies that analyze data from NESARC waves 1 and 2. Primary sampling units where the US counties, mirroring the county-based PSUs used in the Census Bureau’s Current Population Survey (CPS). Eligible households were selected within each PSU and then eligible respondents within each household[3,4].

6. The reviewer indicates that ‘some of the more important limitations would be mitigated in a longitudinal analysis. Also data on posttraumatic stress disorder, psychosis (psychotic episode or schizophrenia), and personality disorders are available in the dataset (Limitations and strengths paragraph 1).’

As indicated above (in response to point 3), due to insufficient sample size, we could not examine the incidence of illegal drug use disorders. We specifically chose to focus on the role of the most frequent forms of psychiatric comorbidity – mood and anxiety disorders, which is why we did not account for other disorders mentioned by the reviewer. In addition, in the NESARC schizophrenia is assessed by respondents’ self-reports of ever receiving this diagnosis from a doctor, rather than by meeting diagnostic criteria. Thus, the diagnosis of schizophrenia may have less validity than other diagnoses.

7. The reviewer asks us to add a reference indicating ‘that the studies data on illegal substance use was collected by federal employees likely suppressing substance use reporting’.

Although this is a potential limitation, in the NESARC study interviews were confidential and the reliability of the AUDADIS-IV is good to excellent for most diagnoses, so it is unlikely that the measures we used were affected by such bias[1]. This has been added to the limitations section of the Discussion (page 19).

Reviewer 2: Morten Hesse

1. The reviewer indicates that ‘The term “health service use” does not really reflect the item that is the outcome variable’.

As indicated in response to reviewer 1 (point 1), we changed ‘health service use’ to ‘illegal drug use disorder treatment’ in the title, abstract, and text of the manuscript.

2. The reviewer asks us to give details regarding our measure of substance use disorders ‘Please state if lifetime disorder must be confirmed at both time points, or (as I suspect is the case) must only be confirmed at one time point’.

Lifetime disorders were defined as a psychiatric diagnosis present in 2001-2002 or 2004-2005 (or both times). This was clarified in the methods section (page 8-9).
3. We are asked to give details about results presented in table 2: ‘In relation to the discussion of table 2, the authors state that 31.4% reported treatment. However, this term may again be slightly misleading, as this may include a small number who simply visited an emergency room’.

To address the reviewer’s comment, we now specify that 31.4% of study participants ‘ever received any form of treatment for an illegal drug use disorder’ (Results, page 12). As shown in Table 2, only 3.4% of study participants reported emergency room treatment, and a majority of participants who reported treatment consulted an addiction specialist.

4. We are asked to rephrase ‘“illegal drug use disorder without comorbidity”’. We modified this phrase and now refer to “illegal drug use without comorbid mood or anxiety disorders”.

5. The reviewer asks us to ‘state if you used the sampling weights from the survey for the regression analyses’.

As indicated in the Statistical analysis section (page 11), odds ratios and 95% confidence intervals were weighted to account for sampling design.

6. The reviewer brings elements which could contribute to differences in health care systems: ‘It would be interesting to know if the proportions receiving and needing treatment varied considerably between units that have different policies in terms of accessibility and payment for services. A candidate could be the state in which the respondent is living, but differences between rural/urban areas could also be important. Further, if some types of services are only available for those with some kind of private insurance, this could also be controlled for (if available in NESARC)’.

As indicated in the Introduction (page 6), we acknowledge that access to illegal drug use disorder treatment is lower in rural than in urban areas. Unfortunately, we do not have information which would allow us to test differences between treatment units or states, but these aspects should be addressed in future research. This was added to the discussion of limitations of our study (page 18).

References


