Reviewer’s report

Title: The introduction of the Practice Nurse Mental Health in general practices in The Netherlands: effects on number of diagnoses of chronic and acute alcohol abuse

Version: 0 Date: 13 Sep 2017

Reviewer: Thomas Zimmermann

Reviewer's report:

Dear authors,

thank you for having the chance to get this good impression on the health care system in the Netherlands relating to the introduction of the Practice Nurse Mental Health (PN-MH). You have given a sound account of the need for and the introduction of PN-MH in the background section of your manuscript.

Using NIVEL-data you've investigated the effect of the PN-MH introduction on the number of diagnosis of alcohol abuse. So far so good.

Unfortunately, methodologically you reduce your valuable longitudinal data (36 months) in two steps. Firstly, you sum up monthly statistics in half year chunks. Secondly, you relinquish to use data collected before the introduction of the PN-MH. In my point of view, your data do give you the chance to utilize a pre-post-design, comparing data collected in a particular practice before and after installing a PN-MH.

I recommend to shorten the aggregation level to a quarter of a year (3 months). Using every single month presumably produces too much noise. Aggregating data for a time period of six months produces an unnecessary loss of information. Three months seem to be a good trade-off between the two options.

Besides that, it seems to me a contradiction to employ a multilevel mixed model regression analysis using aggregated data. Every time point available should be used in a model like that. If you have done so, it is not mentioned in the manuscript. Aggregation may be reasonable in describing cases and prevalences, it is a rather suboptimal input for a regression analysis. Maybe, there could be a variable implementation PN-MH (yes/no) which separates controls from interventions. this variable could be used as covariate in the model as well as its interaction with time.

Table 1 describes characteristics of the practices. There is a spelling error in column 2 (PPC instead of PCP). Average patient populations of the practices in table 1 are not related to the time periods used in the analyses. In the table's caption there is %-symbol. What percentage is meant?

The model you utilize would allow to be used with more covariates (size of practices, numbers of patients and GPs, even opening hours or other data routinely collected). Is there a reason you have not
done so? Maybe there are more (available) predictors to explain the increase of alcohol diagnoses. It seems as if the design and the model were fitted post-hoc to come the most wanted outcome/conclusion: a confirmation of the hypothesis.

Some more annotations:

In the keyword section the authors should consider to add terms like collaborative care or interprofessional care, nurse-led care

I'd like to see an integrated table with number of GPs, number of patients, number of diagnoses, overall number of diagnoses to have a chance to relate these numbers. Maybe some additional numbers on the prevalences of depression, anxiety in these practices will broaden the picture, to put numbers of alcohol abuse in context. alcohol abuse is usually if not always a disorder with comorbidities.

Figure 1 is more confusing than explaining. Ain't the two parts a variation of the same? What is the meaning of the lines? In the lower part of figure 1 I'd miss practices which had 1, 3 or 5 periods of data.

It is not quite clear what sort of diagnoses you have on the record of your routinely collected data. Are these diagnoses prevalent or incident, i.e. do we see the same patients in the health record over time? Can you make a distinction between the two? If this is not possible, it should be discussed as a limitation.

The discussion starts with half a page repeating the results. Less is more.

Conclusion:

As I said before, there is a bit of confusion in the manuscript and some methodological questions that I request the authors to answer. I look forward to read the answers of the authors clarifying my confusion and invalidating my critique, respectively.

Best regards

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No
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I am able to assess the statistics

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