Author’s response to reviews

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

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Authors’ Response to the Review Comments

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Title of Paper: "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.”
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We appreciate the time and efforts by the editor and referees in reviewing this manuscript. We have addressed all issues indicated in the review report, and hope that the revised version meets the journal publication requirements.

Response to Comments from Reviewer 1: Thomas Zimmermann
Comment 1: Thank you for the revision of your manuscript. You very much improved the presentation of your data and you've cleared a couple of misunderstandings. The figures are now much easier to understand. Besides that I am not convinced regarding your argument about the pre-post-design of your analyses. Your method section itself delivers the strongest objection: You argue to use PN-MH post-implementation data over a certain time period (2011, 2012, 2013). As practices were free to implement on their own pace some practices implemented the PN-MH late in 2013, meaning they produce missing data. As you say, multilevel regression analyses can deal with it. But: What you do for post-implementation data (treating missings from practices that implemented a PN-MH late in 2013 as
MAR) should work for pre-implementation data as well. Treating missing pre-implementation data from practices who implemented PN-MH immediately in the beginning of the year 2011 as MAR. It would enable you to handle missings in a similar way as you handle it at the other end of the available time period. At least you could have given it a try. As your results are quite contrary to what you expected (and I need to apologize as I did not quite understand this in my first response), as a researcher I'd like to know and understand better - and your data would enable you to at least approximate a potential pre-post-effect. My suggestion would be to compare practices that had a pre-implementation period of at least six months (one time period in your design) with any time period post-implementation. In my point of view, after all, it should be easy to calculate this in the NIVEL-data (at least for a sub-group of practices) for pre-post-diagnoses P15 or P16.

Response to comment 1: Thank you for your comment. We have now included a sensitivity analysis with a subset of practices that have baseline data available. These practices have data available of 6 months pre-implementation (of the practice nurse mental health). These baseline data were included as a covariate in the analyses to control for baseline differences. This analysis assesses the differences in the post-test means after accounting for pre-test values. This way we could assess whether the post-test means, adjusted for pre-test scores, differ between the groups of practices. We have included the following part in the methods section on page 8: “To account for potential differences in number of diagnoses in the pre-implementation period between the three groups, the analyses were repeated with number of diagnoses in the pre-implementation period as an additional covariate. These analyses could only be conducted among the practices that introduced the PN-MH after the first six-month period (n = 99), and therefore, these analyses were considered as a sensitivity test.”

The reviewer suggest to “compare practices that had a pre-implementation period of at least six months (one time period in your design) with any time period post-implementation”. However, when we conduct the analysis as the reviewer suggest we would take the pre-implementation period as a reference point and need to compare it to the post-test periods, for the four groups of practices. In this type of analysis, one would not control for pre-implementation differences, but would assume change over time. However, our research question focusses on whether one group has a higher mean after the implementation of the PN-MH, and therefore a sensitivity analysis, which focusses on investigating post-implementation means, adjusted for pre-implementation scores, would be the appropriate analysis. Regarding the results, we have included the following part about the sensitivity analyses on page 11 in the manuscript:

“Sensitivity analyses with baseline data available of a subset of practices were conducted (41 control practices, 21 practices with a PCP, 24 practices with a PN-MH and 13 practices with both a PCP and a PN-MH) to control for pre-implementation variability in number of chronic or acute alcohol abuse diagnoses. A regression model was conducted with time as a continuous predictor and baseline as a covariate to investigate overall time effects. A multilevel linear regression analysis using a random intercept model was conducted with time, group and time x group terms included in the model and baseline number of chronic or acute alcohol abuse diagnoses, general practitioners’ Full-Time Equivalent (FTE) per practice and number of registered patients as covariates. Results show a significant decrease of chronic alcohol abuse diagnoses (β=-.41, p<0.05) while controlling for baseline number of chronic alcohol diagnoses. No significant decrease of acute alcohol abuse diagnoses was observed when controlling for baseline diagnoses (β=-.02, p>0.05). No significant differences with control practices at any time period was observed for both outcome variables.”

In the discussion, the following part were added: “Based on the results of this study it seems that the PN-MH does not contribute to increased chronic or acute alcohol abuse diagnoses in general practices, even when accounting for differences in number of diagnoses in the pre-implementation period.”. The following part was also added to the discussion: “In contrast to chronic alcohol abuse diagnoses, no significant decrease of acute alcohol abuse diagnoses was observed when controlling for baseline diagnoses. This indicates a small (non-significant) average increase of acute alcohol abuse diagnoses
from the pre-implementation period to the first post-implementation period followed by an average decrease in all subsequent periods.”

Comment 2: I am now more convinced than before that you may not be able to answer your research question at all using routinely collected data that do not distinguish between incident and prevalent diagnoses. I assume an artefact in the data that is not yet discovered or that is related to a certain way to collect NIVEL-data. I am not familiar with the NIVEL-data-collecting scheme, but maybe you get the chance to ask for one more year (2010) pre-PN-MH data - otherwise this research does not bring a serious argument for or against a potential effect of introducing a PN-MH in primary care. Instead, it substantiates the argument not to use routine data to measure the effect of an intervention.

Response to comment 2: It may have been unclear at first whether these were incidence or prevalence figures. We would like to clarify this and want to emphasize that it concerns prevalence data of diagnoses in 6-month time periods, and not incidence data (new patients). Our research question concerned whether the practice nurse mental health is related to an increase in diagnoses, as we expect the practice nurse to have more time to screen for alcohol abuse. Therefore it is possible that new episodes of abuse are present within the same patients. To capture the diagnoses of new episodes within the same patients we have therefore focused on the prevalence of diagnoses. We have requested additional information at the NIVEL, and it appears to be possible to distinguish between prevalence and incidence data (we apologize for the confusion). However, the ICPC codes include relatively mild behavioural disorders, such as binge drinking and problematic drinking that are often reoccurring and episodic. This means that a patient can have an episode of problematic drinking this year, and again in one year. We therefore hypothesize that the introduction of the PN-MH would be particularly suited to screen patients periodically (they have more time than GPs) and would therefore be able to diagnose alcohol misuse more often than practices that do not have a PN-MH. It would therefore be more appropriate to investigate prevalence data of diagnoses within a period, instead of investigating incidence data (i.e. only new patients and not new diagnoses within patients).

By correcting for diagnoses before implementation in the sensitivity analyses, we can exclude that differences after implementation are caused by differences that existed prior to the pre-implementation period. Differences that are observed would then be due to the introduction of the PN-MH. The use of routine data has drawbacks. Nevertheless, this naturalistic experimental study can give us insight into whether there are differences between the groups after implementation of the PN-MH.

Comment 3: In the discussion section you interpret - on an informed basis - why this (counter-intuitive) result could have happened. But you don't ask whether routine data is a thorough basis to answer the research question at all.

Response to comment 3: Thank you for your question. We do believe that routine data can be very informative to answer our research question. However, we do acknowledge its limitations. We have added the following to the discussion section: “Contrary to studies with a randomized controlled design, we were only able to analyze routinely collected data after the voluntary implementation of the PN-MH in general practices. This means that differences between groups or the lack of differences between groups may also have been influenced by other factors. Nevertheless, this naturalistic experimental study gives insight into what happens in practice after implementation of the PN-MH. Natural experimental studies are often recommended as a way of understanding the impact of policies when it is impossible to manipulate exposure to the intervention.”

In addition to the explanation of the methodological limitations, we have added another explanation of our findings to the discussion section on page 12: “It also appears that mostly patients with depression, neurasthenia, anxiety and stress are referred to the PN-MH, while patients with alcohol problems are less often referred to the PN-MH [13]. This suggest that in daily practice the service of the PN-MH is
Comment 4: Maybe the (decreasing) effect you report belongs to a phenomenon underlying this research, but having yet to be investigated: the interventional aspect of the practice nurse. Maybe therapeutic options were discussed. Maybe patients went into treatment much earlier than before without a PN-MH? Suppose, the decrease in diagnoses is a beneficial outcome of introducing a PN-MH?

Response to comment 4: Since the decreasing effect was observed for all four practice groups (i.e. also for practices without practice nurses), we think more general explanations might account for this observation. Therefore we had added the following two parts in the discussion on page 13: “One possible explanation for this result is a saturation effect. As increasing numbers of patients with psychological or social problems visit general practices [12] it may be the case that after a certain time period the number of new cases to be detected is lower.”

“The decrease in chronic alcohol abuse as well as acute alcohol abuse diagnoses may be related to the interventional aspect of the PN-MH: a practice nurse typically has more time and is involved in treatment as well as referring patients to specialized care [10]. This may cause earlier treatment and recovery, and a consequential decrease in diagnoses. However, this should be further explored in future studies.”

Comment 5: I suggest to change the order in the methods section: Please put the ethical approval part last. Design, measurement and analyses should stick together.

Response to comment 5: Thank you for your suggestion. We have now rearranged the order of the methods section.

Comment 6: On page 6 you write, that 155 of 187 practices were analysed. On page 9 at the end of the methods section you repeat this information and you explain why you included 32 practices less than you've originally got data from. I think you can delete the information on page 6. Maybe you even tinker with the idea to put this information on top of the results section, 😊.

Response to comment 6: Thank you for your suggestion. We have deleted the extra information on page 6: “Only data after implementation of the PN-MH was used in the analyses. Consequently, practices that have implemented PNs-MH late (e.g. in 2013) have less data available about the amount of diagnoses post-implementation.”

Comment 7: In the results section (p.10) I would want to read at least one or two lines pointing at table 1 as you did with table 2.

Response to comment 7: We have added the following part to the results section: “Data were obtained from a total of 155 general practices, of which 46 practices are control practices (without a PN-MH and PCP), 23 practices only have a PCP, 56 practices only have a PN-MH and 30 practices have both a PN-MH and PCP. Overall, general practitioners have an average workload of 2.35 Full-time equivalent (FTE).”

Response to Comments from Reviewer 2: Gert-Jan Hendriks

The manuscript has improved a lot compared to the previous one and the authors addressed my comments well. Much of the confusion has been solved in this manuscript. The conclusion is important because policymakers can have high expectations of adjustments, such as the introduction of mental health care nurses in primary care. This study puts these expectations into a more realistic perspective. I have some minor comments.

Comment 1: 1. Data of 187 practices were available of which 155 practices were included in the
analysis. It is not clear to me which criteria the authors used for in-/exclusion of the practices. Please, clarify.

Response to comment 1: The 155 practices included in the analyses were not selected. Of the 187 practices 32 practices had missing observations in the covariate ‘Full-Time Equivalent (FTE) per practice’. Complete data was available and analysed for 155 practices of the 187 practices (83% of the sample). We had added a part in the methods section on page 9 explaining this: “Due to missing observations in the covariate ‘Full-Time Equivalent (FTE) per practice’ in 32 practices, complete data was available and analyzed for 155 practices of the 187 practices (83% of the sample). The missing observations are assumed to be independent of the outcome variable.”

Comment 2: 2. Compared to the previous manuscript, the numbers of participating GPs have decreased. However, in Table 1 the means mentioned have remained unchanged in all cells of Table 1. This seems unlikely to me. Please, clarify.

Response to comment 2: The means as described in Table 1 are means of ‘Full-Time Equivalent (FTE)’ of all practices that have data about this variable available: 155 practices. The means as described in Table 1 are correct. In the previous manuscript the means were incorrectly described as means of the original sample of 187 practices.