Author's response to reviews

Title: Uptake of health services for common mental disorders by first-generation non-Western migrants in the Netherlands

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Author's response to reviews: see over
Reviewer: Rolf Kleber (minor essential / discretionary revisions)

Page 6: a better term is mental disorders (not psychiatric disorders).
We have changed this in the final version according to the comment above.

Page 6: the introduction shows a little redundancy.
 ?????????

Use more paragraphs in the discussion (especially the first paragraph is quite long now).
We have edited the text by dividing the first paragraph of the discussion into two separate paragraphs. We also divided the limitations section of the discussion into three separate parts.
Reviewer: Margarita Alegria

The response rates appear to remain incorrect, at least if the authors are using the standard definition (see Groves) of response rates when generalizing to the Amsterdam population in 2004. If I understand the information provided in the text, the actual response rates of this study is 30%, not 67.3% which is the follow-up rate. The authors state that they are trying to generalize to the composition of the Amsterdam population in 2004, which would require multiplying the response rate of time1 by the response rate of time 2.

If we understand the reviewer correctly, she finds the terminology of ‘response rate’ somewhat incorrect/misplaced and perhaps even misleading. We did not want to raise the wrong impression about the response rates in our study, which was indeed low. We agree with the reviewer that ‘follow-up rate’ would be a better term to refer to the 67% of people who (i) participated in the first wave, (ii) consented with a second approach, and (iii) actually participated in the second wave. As de Wit et al. (2008) reported in their study, the overall response rate for the entire study (i.e. both waves) was 26.5%, and we have added this information to the final version of our manuscript. However, the reason to explicitly mention the follow-up rate is that it was surprisingly high, considering the sensitivity of the subject within these particular ethnic subgroups. The paragraph in question (page 9) now reads as follows:

“After the second wave de Wit et al. [23] calculated an overall response rate of 26.5%, ranging between 20.8 (Moroccans) and 30.2 (ethnic Dutch). The follow-up rate was lower among Turkish and Moroccans (62.2% and 70.5% respectively) than among ethnic Dutch (76.9%; p < 0.001), and lower among men (68.1%) than among women (73.2%; p = 0.027). However, there was no selection with respect to age (p=0.856). Between participants and non-participants in the second wave, there were also no significant differences regarding perceived health status (p=0.101), psychological distress (p=0.635), general practice visits (p=0.101) and outpatient health care utilisation (p=0.480) in the past two months, any health care utilisation for mental health problems in the past year (p=0.903), and current use of psychotropics (p=0.903).”
There is an inconsistency in the results reported in Table 3 and the results and discussion section. In Table 3, Model 4 for the Moroccan migrants, the odds ratio presented is 0.41 with a 95% CI of 0.16-1.01. This results suggests that once SES (as measured by insurance status) is entered into the model, there is no significant difference in the likelihood of receiving primary care services for Moroccan migrants as compared to Dutch natives, adjusting for age, sex, and need measures. However, the authors state (in page 13) that differences in uptake between Moroccan and ethnic Dutch could not be explained by differences in SES, as step 4 in the analyses was highly insignificant. It is not clear how the authors harmonize these apparently contradictory statements. Please explain how the response to #4 in the authors’ letter answers such concern. It was not evident to the reviewer. Given that the differences in uptake of primary care services for mental health problems between Moroccan and ethnic Dutch seem to disappear once SES is entered, it might be important to confirm that it is actually differences in poverty rather than insurance coverage that explain these differences. This would make for a more compelling explanation.

According to the reviewer, the results in our study are inconsistent with respect to the influence (or rather the lack thereof) of SES. The reviewer states that our conclusion (i.e. that ethnic differences are not explained by SES) is not supported by the observation that inclusion of SES in the model leads to disappearance of a statistically significant association between Moroccan ethnic background and uptake of primary care services.

We regret that there appears to be an inconsistency in our results. Our statement that differences in uptake between Moroccan and ethnic Dutch could not be explained by differences in SES (page 13) was however based on two important observations. Firstly, the step in the analysis was statistically highly insignificant. Secondly, further investigation of the results learned that there was indeed no association between uptake of primary services for mental health problems and being higher educated (OR = 0.82; 95% CI = 0.43-1.56) or having private health insurance (OR = 0.90; 95% CI = 0.47-1.73). The same was the case for specialised mental health services, i.e. the step in the analysis was not significant, nor was there an association between
uptake of specialised mental health services and being higher educated (OR = 0.88; 95% CI = 0.41-1.91) or having private health insurance (OR = 1.24; 95% CI = 0.54-2.85).

We included the aforementioned information (i.e. about the significance of each step in the analysis) in the final version of table 3, and we regret that it was not in the table in the first place. Moreover we included the information mentioned above about the associations between uptake of services, and educational level/type of health insurance. By doing so we hope that we sufficiently explain why we did not go into further detail with respect to the influence of SES. We also explain in the discussion the limitation of the way SES was measured in our study. On page 19 the text reads as follows:

“Finally, SES was indicated only by two rough measures of education and income, while the concept of SES is much broader [1]. Given the fact that most non-Western migrants have a relatively low SES, the influence of socioeconomic position was very difficult to study in this particular sample. It is strongly recommended that future studies make efforts to further disentangle ethnic and socioeconomic influences in the context of mental health services research.”

At minimum the authors should explain why they did not use multiple imputation or hot deck imputation to deal with the problem of missing data.

We acknowledge that the small sample size, which limited the statistical power of the analyses, was partly related to individual item non-response and (subsequently) deletion of cases from the analyses. Nevertheless we refrained from data imputation techniques, mainly for two reasons. Firstly, it is known that imputation can distort coefficients of association and correlation relating variables. Secondly, the number of cases with missing data was relatively small. There is a rule of thumb that can be used in this situations which states that if a variable has more than 5% missing values, cases should not be deleted. In our study, ‘educational level’ (8.6%) was the
only variable that - barely - exceeded this level. In the revised text we added this argumentation. In the discussion section (under limitations) the text reads as follows:

“However, there are some limitations to this study that need to be addressed as well. Firstly, despite all measures to increase response, we acknowledge that the generalisability of our results is compromised by the high non-response and incompleteness of data. Considering the efforts that have been made to limit non-response, the response rate in the present study may yet be the highest feasible response for this type of research [17]. Although non-response in the second wave appeared to be non-selective regarding mental health outcomes, and weighting techniques were used to correct for selective non-response according to demographic factors, the small sample size limited the statistical power of the analyses. Though we acknowledge this is partially related to the deletion of cases with missing data, we refrained from data imputation techniques for two reasons. Firstly, it is known that imputation can distort coefficients of association and correlation relating variables. Secondly, the number of cases with missing data was relatively small. As a rule of thumb, if a variable has more than 5% missing values, cases are not deleted [59]. Educational level (8.6%) was the only variable that - barely - exceeded this level.”