Author’s response to reviews

Title: The relationship between acculturation strategies and depressive and anxiety disorders in Turkish migrants in the Netherlands

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Version: 3  Date: 13 August 2014

Author’s response to reviews: see over
Mr. Carlo Rye Chua  
Ms. Lindsey DeBoer  
Dr. Mark Powers  
BMC Psychiatry  
5/07/2013  

Dear dr. Powers,

Thank you for the opportunity to revise our manuscript entitled " The relationship between acculturation strategies and depressive and anxiety disorders in Turkish migrants in the Netherlands" (MS: 4549027891227590). The comments of the reviewers were very helpful in revising our manuscript. Please find attached a revised version of our MS, which we would like to resubmit for publication in BMC Psychiatry.

As instructed, we have carefully considered the comments and suggestions from the reviewers. Revisions in text are shown with the Track Changes feature. Below you can find each of the comments of the reviewers in point-by-point fashion and our reply.

We hope that the revisions and our accompanying responses will be sufficient to reconsider our manuscript suitable for publication.

We look forward to your decision.

Yours sincerely,

Burçin Ünlü Ince
Responses to the comments of Reviewer 1

Comment 1: Firstly, the paper could benefit from a more extensive and up to date literature search, references concerning health care utilization, the proposed low receiving and high drop out rates, are somewhat outdated (e.g., 2001). An extensive literature search considering more publications in which culturally diverse groups are studied, could also improve the argumentation in this section. Moreover, an attempt to increase the generalizability of the findings would be welcomed – how representative are the results for other migrants than the Turkish group?

Response 1: Up to date literature concerning studies, policy developments and (mental) health service use of immigrants in the Netherlands including the Turkish migrant population is, unfortunately, sparse. We have, however, taken on board the suggestion of the reviewer and included more recent studies in our manuscript. We incorporated the following paragraphs in the introduction:

Ethnic minorities are also known to use mental health services less often than indigenous populations (e.g. [4, 5]). Although Dutch national data is lacking, there are signals that the dropout rate is twice as high in ethnic minorities in mental health care compared to native Dutch people in the Netherlands [6]. Research also shows that the perceived need for mental health care is higher for Turkish migrants than apparently among Moroccan and Dutch people [7]. This may be related to their higher levels of mental distress and their less often met need for mental health care services as perceived by Turkish migrants themselves [7].

While the Turkish population in the Netherlands is at an increased higher risk for developing depression in comparison to other ethnic groups (Moroccan and Dutch), this is comparable with research concerning ethnic minorities in general. For example, in a European study in 23 countries, it was found that depressive symptoms were more prevalent among immigrants and ethnic minorities than among native populations [8]. A possible explanation for this higher risk may be lower socio-economic status and discrimination perceived by ethnic minorities in their host countries [8]. This increased risk for developing depression, is probably generalizable for to many ethnic minority groups, including Turkish migrants in the Netherlands.

Most research on ethnic minorities and mental health shows a negative association between acculturation and mental health. For example, in a study among Korean immigrants in the USA, self-reported language proficiency of English (which is part of the adaptation dimension was shown to be related with depression [9] Furthermore, integration has been shown to be associated with lower mental health problems in Black male adolescents in the UK [10]. In Chinese American students, it was found that maintenance of the ethnic (Chinese) culture was related to fewer depressive symptoms [11].

However, there are also examples of studies in which this association hasn’t been found. For example, Beirens and Fontaine (2010) [12] evaluated differences in well-being in Turkish immigrants in Belgium, Turkish majority members (in Turkey) and
Belgian majority members. Results showed no relationships between adaptation and maintenance (which were the only two acculturation dimensions) and sadness, anxiety nor with anger.

Comment 2: Secondly, in what way health professionals working with the target group can profit of the information provided in this paper is yet undecided – more attention could be given to implications of the findings regarding prevention, clinical work and health care policy. What can we do to help less integrated and participating migrants better? The reader may also want to know in what way a prevention program can be tailored to the needs of the less participating migrants. In addition, suggestions for a culturally sensitive approach in organizing the health care more adequately could be helpful to provide more insight in this phenomenon.

Response 2: We agree with the comment to pay more attention to the implications of the findings. We added the following paragraph to the implications and future research section in the discussion:

The finding that integration may play an important role in a lower risk of developing depression is also of importance for public health policy makers, clinicians as well as for researchers. Supporting immigrants in the process of adjustment to the host society, while encouraging ethno-cultural maintenance at the same time, is an important task for the Dutch society as well as for ethnic minorities themselves. This process can be aided through several pathways, including educational and public health policies, such as implementing acculturation in prevention programs. Although integration has been found to be related to a lower risk of depression, its causality and implications for prevention and clinical practice should be examined in more detail for example the potentials of including it as component in screening or treatment strategies. Awareness by practitioners and professionals of the acculturation strategies of ethnic minorities should be promoted in order to optimize health services for mental health problems.

Comment 3: Thirdly, there is no research question formulated. Consequently, specific theory driven hypotheses are absent. These issues could be discussed at the end of the introduction section and arguments underlying the hypotheses could be embedded in the literature.

Response 3: We added the following hypotheses in the introduction: using the bi-dimensional framework of acculturation, we hypothesized that 1) higher integration is associated with lower prevalences of depression and anxiety disorders and 2) higher integration is associated with higher GP-care uptake.

Comment 4: Lastly, a convenience sample was drawn which is appropriate but some more details concerning the data collection procedure would be welcomed, for example why is the non response for the Turkish groups so substantial, from 454 to 215, more than half(!) of the Turkish respondents were excluded – and are there any consequences for the representativeness of the sample. And most importantly, the absence of cultural validity of the instruments, in particular of the CIDI, should be more explicitly discussed.

Response 4: Firstly, we agree with the reviewer’s point about the absence of validity of the Turkish version of CIDI. We added this into the limitations section.
Secondly, we added detailed information about the data collection procedure in the methods section:

The original study population consisted of a random sample of the Amsterdam population, stratified by age group, and ethnicity. This population was invited for a general health interview (first wave). All of the respondents from the first wave were asked for a second approach, without mentioning the topic of the study, one year after the first phase. This second phase (follow-up, one year after the first phase) consisted of a structured interview conducted by bilingual interviewers. Summer vacation, Christmas and Ramadan were avoided for visits. The interviews could be held in Dutch, Turkish, Moroccan or Berber, depending on the preference of the respondent. The interviewers were intensively trained and coached before and during data-collection. The interviews were also recorded in order to check and coach the interviewers. After completing the interviews, these were checked for consistency and completeness.

Lastly, we also agree with the reviewer's point about the low response rate. The response rate over the first and second phases was 26%. It is not clear why the response was that low. However, despite efforts put into reaching and recruiting ethnic minorities, it seems that this low response rate is the highest possible response to be attained in ethnic minorities [13, 14]. There may have been a selective response, which is a limitation. However, the response rate of the Turkish group was similar to the other ethnic minorities in the data (Moroccan, Antillean and Surinamese), suggesting that in case of selective response this was similar in all the ethnic minority groups.

We incorporated this paragraph in the limitations section.

Responses to the comments of Reviewer 2

MAJOR COMPULSORY REVISIONS

Comment 1: 1. Over the past decade or more there have been a considerable number of criticisms of Berry's four-strategy approach to acculturation measurement, as well as conceptual concerns about issues such as the use of the word 'strategies'. See, for example, various critiques by Rudmin. A number of researchers have argued that bidimensional approaches to acculturation measurement are preferable to the four-strategy approach. On the other hand, the widespread use of Berry's approach, the considerable theoretical literature in its favour, and its use in recent years as part of large-scale acculturation studies, might serve to justify the use of the four-strategy approach. Either way, the authors should position their work within this literature and, in light of this literature, provide arguments to support the approach they have chosen to take.

Response 1: We agree with the reviewer that we should provide more arguments for choosing the two-dimensional approach. We added the following paragraph in the introduction:

In the past decades, alternative definitions have been given for acculturation, such as a second-culture acquisition ([15], p. 106) or enculturation ([16], p. 125). Both of these conceptualizations are viewing acculturation uni-dimensional, as one particular
culture [17]. When viewing acculturation in a one-dimensional manner, the migrant chooses either to adapt the host culture or to maintain the ethnic culture. However, this one-dimensional approach neglects the dynamic of acculturation. According to the very first definitions of acculturation by Redfield, acculturation includes the interplay or transmission of one or more cultures, which is a criterion for acculturation nowadays [17]. The bi-dimensional model posits the independency of the two cultural orientations, which is shown to be a more valid approach of acculturation [18].

However, we do not agree with the comment of the reviewer stating that the bidimensional approach is preferred by researchers to the four-strategy approach. According to the bidimensional approach, a migrant can choose how to move between the dimensions and even how to combine them independently. The four-strategy approach is actually based on the two dimensions, consequently leading to four orthogonal axes, which has become a core feature of acculturation nowadays (e.g. [17, 19, 20]).

Comment 2: Interestingly, given the above, it seems that the authors have taken an acculturation measure that was not meant to be scored in either a two-dimension or four-strategy manner, successfully re-scored it in a bidimensional way, and then used these two dimensions to derive four strategies. It appears that the four strategies have been derived by conducting median splits on the two dimensions, and then classifying participants into one of the four resulting quadrants. Confusingly, the authors claim that their approach results in four 'scales' (e.g., page 9). Berry's original method indeed results in four actual scales -- he most often has a separate dimensional measure of each of his four strategies. There is precedent for median splits of bidimensional measures, but this approach is not without its problems. For example, there is a conceptual problem in that some people will always be in each position, so that in a highly integrated sample there will nonetheless be some people classified as 'marginalized'. There is also an empirical problem -- dichotomizing scales inevitably leads to an unnecessary loss of information (indeed, the authors themselves describe low power as a limitation on page 17). My preference would be for researchers who have bidimensional measures to use them as such and study the relation between the two dimensions and whatever outcome measures they are interested in. If the authors prefer to use this four-strategy approach based on median splits, the choice should be fully justified.

Response 2: We agree with the reviewer that the median split provides results relative for this population. However, since we do not aim to present the level of acculturation, but only the association with depression, the variance within this population needs to be optimally used. Choosing a median split instead of a continuous measure was based on the fact that the distribution of the dimensions was not normal distributed, and therefore the dimensions could not be validly included as continuous measures and were therefore dichotomized. This also made it feasible to compare the four strategies instead of two separate dimensions.

Comment 3: Given the extensive literature on acculturation strategies and adjustment, did the authors have any specific hypotheses. At the moment, the study is presented as an investigation of the relationship between the four strategies and mental health. If the authors chose to conduct an exploratory study without hypotheses, how did they protect against alpha inflation?
Response 3: We added the following hypotheses in the introduction: using the bi-dimensional framework of acculturation, we hypothesized that 1) higher integration is associated with lower prevalences of depression and anxiety disorders and 2) higher integration is associated with higher GP-care uptake.

Comment 4: In order to better evaluate the bidimensional adaptation of the acculturation scale (for which it had not been originally designed), it would be helpful to see the 25 items and how they were assigned to the two dimensions. If this is not possible, the authors should report on the item numbers of the items on each dimension, and present a couple of examples from each dimension. Ideally, the division of the items into two dimensions would be established by some kind of factor analysis. What was the correlation between the two dimensions?

Response 4: We added the following results of the factor-analysis in detail in the Methods section:

“However, eight items concerning emancipation were excluded from further analyses because it was not possible to determine how this scale was associated with acculturation, due to the lack of information about emancipation in the ethnic Dutch and Turkish cultures. The two factors, participation and maintenance, were yielded with the rotation solution, as shown in Appendix A. The participation factor accounted for 16.9% of the item variance, and the maintenance factor accounted for 21.6% of the item variance. With a cut-off point of .40 for the loadings [21], only 3 items were excluded from both factors. In order to fit the LAS items to the scales, some items (items 10, 11, 13, 16 and 17) were recoded by adjusting the range of the response options, so that higher scores indicated lower levels of maintenance or participation.”

We also added an Appendix (A) of the factor loadings of the items on the LAS.

MINOR ESSENTIAL REVISIONS

Comment 5: In the abstract and throughout, p-values cannot equal .00 (i.e., zero). The authors should either show the full number of zeroes, or should report as < .01.

Response 5: P-values that were reported as .00 are corrected into <.01.

Comment 6: The authors should review the manuscript carefully for typographical errors. For example, on page 5, 'assimilation is the second strategy' is entirely rendered in italics, rather than just 'assimilation'. On page 8, 'withe' should be 'with'. There may be others that I missed.

Response 6: Both errors are corrected and the main text has been checked again for other typographical errors.

Comment 7: The authors note in the introduction that the relatively high rate of depression/anxiety among Turkish residents of the Netherlands might be due to their status as migrants. What explanation do they have for the much higher rates in Turkish versus Moroccan residents of the Netherlands? These findings
suggest that something else is additionally contributing to the high Turkish rates. I appreciate that the paper is not meant to be a comparison of different ethnocultural communities, but a brief discussion of this point would be helpful in terms of better summarizing the particular stresses faced by the Turkish-Dutch community.

Response 7: It is difficult to give a clear explanation about the differences in the prevalences of depression in ethnic minorities in the Netherlands due to a lack of epidemiological studies.

In an earlier study, conducted by van der Wurff and colleagues (2004) [22], it was found that Turkish elderly migrants had a higher prevalence of depressive symptoms (61.5%) than elderly Moroccan (33.6%) and native Dutch people (14.5%). Ethnic origin was found to be an important factor associated with the high prevalence of depressive symptoms. However, an epidemiological study examining the factors for this high risk is lacking, unfortunately. We can only guess why the Turkish population in the Netherlands has higher depressive symptomatology, although socio-economic status cannot explain the differences.

According to an international comparison study conducted by the WHO, it was shown that the prevalences of depressive disorders were lower in Turkey than for the Netherlands [23]. The high prevalence of depression for Turkish migrants in the Netherlands suggests that this may be related to their migration background or status.

Comment 8: More information should be provided about the translation procedure. Who were the translators? How were disputes resolved? Any particular issues that were difficult to resolve that might impact how results are interpreted? If any formal translation protocols were used, these should be identified and cited.

Response 8: The questionnaire was translated into Turkish by official translators. A back translation to Dutch was performed by another translator and checked by the researchers. Any inconsistencies with the original were discussed with both translators and adjusted. The interviewers also reported back when they had difficulties with the translation, and then together a standard was chosen. Of the acculturation scale (LAS) and the measurement of anxiety and depression (CIDI) official Turkish translations were available and used in this study.

Comment 9: Somewhere in the literature review and/or the discussion, the authors should at least address the issue of how depression and anxiety are presented in Turkish populations. There is a considerable literature on how culture shapes the presentation of psychopathology -- might such cultural variations have impacted the results? To what extent are the authors confident in the use of either their acculturation or diagnostic measures in Turkish samples?

Response 9: We agree with the discussion about the validity of one of the measures. The absence of the cultural validity of the CIDI has been added as a limitation in the limitations section.

As mentioned earlier, an epidemiological study examining the variations and factors across ethnic minorities, including Turkish migrants is lacking. In an earlier study by van der Wurff and colleagues (2004) [22] it was found that Turkish migrants had
higher prevalences of depressive disorders, which is comparable with the high prevalences found in our sample [14, 24] A study on these data by Schrier has also shown that symptom profiles of depression and anxiety do not differ between Turkish-Dutch and Dutch respondents, when all symptoms are systematically checked for [23]. It is still possible that the active presentation of symptoms in a doctor’s office is different, as was described by Veling [24], but since this study is performed with structured questionnaires, that would not affect our results.

In contrast to the CIDI, the acculturation measure was originally a well validated questionnaire in several languages (including Turkish) and shown to have a good reliability, as mentioned in the methods section. Furthermore, the reliability of the new scales showed also good results (.86 for the two dimensional scales).

DISCRETIONARY REVISIONS

Comment 10: Unless this journal has a different format than what I am used to in psychology, I recommend that Latin letters used as statistical symbols should be rendered in italics, whereas Greek letters should not.

Response 10: All Latin letters that are used as statistical symbols are rendered in italics.

Comment 11: The authors should consider simply removing the 16 second-generation participants. Doing so would improve sample homogeneity and remove a study limitation, without greatly decreasing the statistical power.

Response 11: That is an interesting consideration, however this would be problematic in another way. Since the sample size is small and some cells in table 2 and 3 are below the statistical range (<5), this would lead to cells with zero data. Therefore, we did not prefer to remove 16 participants from analyses.
Reference list


