PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Neighborhood characteristics and mental disorders in three Chinese cities: Multilevel models from the World Mental Health Surveys
AUTHORS	Chiavegatto Filho, Alexandre; Sampson, Laura; Martins, Silvia; Yu, Shui; Huang, Yueqin; He, Yanling; Lee, S; Hu, Chiyi; Zaslavsk, Alan; Kessler, Ronald; Galea, Sandro

VERSION 1 – REVIEW

REVIEWER	Jaap Nieuwenhuis Delft University of Technology, the Netherlands
REVIEW RETURNED	30-May-2017

GENERAL COMMENTS	Many thanks for the opportunity to review this interesting paper. I have a few concerns that I would like to see addressed in a revised version: 1) The use of multilevel modeling is appropriate, however, to be better able to assess the models, some additional statistics are needed, e.g., the variances on the neighbourhood and individual level, as well as the intraclass correlations. 2) The outcome variable 'Mental disorders' is quite broad, it includes anxiety, mood, behavioural, and substance use disorders. You collapse all of these disorders (both internalising and externalising) into one broad measure of mental disorders. Does this then also mean you expect neighbourhood effects to be the same for all these disorders? If so, please elaborate on this in the paper. An extra sensitivity analysis could be to separate the disorders into different models. 3) Besides lifetime diagnoses, you use past-year diagnoses as dependent variable. Does this mean that the respondents also lived in the same neighbourhood last year? Do you have this information? If you don't have this information, using this measure may just capture social drift into more deprived neighbourhoods after a mental illness diagnosis. Please elaborate on this in the paper. 4) You also use a measure of relative individual income, but in the paper I'm missing the rationale for this choice. I recently did a study on relative individual income deprivation, neighbourhood effects and mental problems that may be of interest for this study as well. Perhaps it could provide some insights (J Youth Adolescence, doi: 10.1007/s10964-017-0668-6). This may also explain that you do not find effects of neighbourhood income, because the effect of neighbourhood income on mental illness may be dependent on individuals' own income (i.e., how individuals' income compares to that of the neighbourhood). 5) The two neighbourhood-level variables in which you are most interested are both split into 3 categories. I wonder why such a crude measure was chosen.

	What's wrong with continuous measures? Did you also try other splits? Please elaborate on this decision. 6) Finally, you acknowledge the potential bias of neighbourhood selection and reverse causality, however, I would like to see more discussion on how this problem may affect the interpretation of your results. As you acknowledge, with cross-sectional data it is hard to deal with this problem, but that makes it therefore even more important to give some more attention to it in light of this specific study.

REVIEWER	Richard Shaw Institute of Health and Wellbeing, University of Glasgow, United Kingdom.
REVIEW RETURNED	20-Jul-2017

GENERAL COMMENTS	This is an Interesting and well-written paper investigating the relationship between income and percent married measured at the neighbourhood level with lifetime and current mental disorders. The authors find that relationships between lifetime and current mental disorders and neighborhood marital status, but they do not find a relationship between mental disorders and neighborhood income. The sample (approx. 4000) comprises residents aged 17 or older living in three cities in China Shenzhen, Beijing and Shanghai. The statistical methodology used is appropriate. Unless otherwise stated all aspects of the Strobe checklist have been addressed. However, I have some concerns about the way some of the measures are coded and parts of the paper are described. On page 9. Mental disorders are operationalised using a wide variety of categories including anxiety, mood, behavioural, and substance use disorders. I have some concern with the inclusion of disorders defined as beginning in childhood (attention-deficit/hyperactivity disorder, conduct disorder, and oppositional defiant disorder) as reverse causation appears a much more likely explanation of any relationship than for the other measures. On page 6 the authors need to make a stronger theoretical justification for using percentage marriage at area level as a risk factor. One of the proposed mechanisms "family disruption" would suggest that the possible risk factor was divorce, separation or spouse's dearth rather than marriage itself. If possible the authors should consider alternative ways of classifying marital status at individual or area level. Page 22, the authors imply that neighbourhood income is associated with persistent mental disorders while the results and abstract do not support this. Other more minor issues Title: The authors only use two neighbourhood characteristics, it would be better just to state them explicitly. Page 3, Abstract: The authors need to state the specific groups being compared to produce the odds ratios. Page 3: Abstract. I would refrain from speculating on discussing mechanisms in the conclusion. Page 6: The authors should state the upper age range of the study participants. Depending on the age range in the study, the paper might be improved if an additional older age category was added. The 50+ age group may include a diverse range of people including retirees and widows.

	I also wonder if the data would support the use of a 25-34 year old age category. Page 10: What were the reasons that authors chose not to use a continuous measures of income both at neighbourhood and individual level? Page 12, Table 2: I would suggest including all categories for the individual level variables within the table. Page 15: Top of table 3 and subsequent tables. Please provide column headings indicating which model is being referred to. Page 23: the authors might consider extending the discussion of possible explanations for the relationship between neighbourhood marital status and mental disorders.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Jaap Nieuwenhuis
Institution and Country: Delft University of Technology, the Netherlands
Competing Interests: None declared

Many thanks for the opportunity to review this interesting paper. I have a few concerns that I would like to see addressed in a revised version:

1) The use of multilevel modeling is appropriate, however, to be better able to assess the models, some additional statistics are needed, e.g., the variances on the neighbourhood and individual level, as well as the intraclass correlations.

Our response: Thank you for pointing out this omission. We have added the intraclass correlation coefficients for each outcome to the results section (page 15) as well as variance parameter estimates and zero G chi squared tests for the random intercept in each model in the last row of tables 3-6. We have also added a description of these methods on page 11, and explained the respective findings in the results text corresponding to each table.

2) The outcome variable ‘Mental disorders' is quite broad, it includes anxiety, mood, behavioural, and substance use disorders. You collapse all of these disorders (both internalising and externalising) into one broad measure of mental disorders. Does this then also mean you expect neighbourhood effects to be the same for all these disorders? If so, please elaborate on this in the paper. An extra sensitivity analysis could be to separate the disorders into different models.

Our response: We would like to thank the reviewer for this very important point. While running the suggested sensitivity analysis that split our outcome into internalizing and externalizing disorders separately in order to address this point, we found that the proportion of married residents was actually only statistically significant in explaining externalizing disorders, made up of substance use disorders and intermittent explosive disorder, which was likely driving our previous results when using the more general aggregated outcome. Therefore, we have decided to present these separate models as our main analyses, as we think this is an important and interesting distinction. Furthermore, we have removed disorders that tend to onset during childhood (oppositional defiant disorder, conduct disorder, and attention-deficit/hyperactivity disorder) from the outcomes in order to further reduce the heterogeneity of our outcomes and address the issue of temporality of onset (please see our response to reviewer #2 for more information).

We have updated the methods, tables, results and discussion sections throughout the paper to reflect these changes.

3) Besides lifetime diagnoses, you use past-year diagnoses as dependent variable. Does this mean that the respondents also lived in the same neighbourhood last year? Do you have this information? If you don't have this information, using this measure may just capture social drift into more deprived neighbourhoods after a mental illness diagnosis. Please elaborate on this in the paper.

Our response: Yes, the information on neighborhood was derived from the location at which each respondent lived at the time of the survey (i.e., the neighborhood in which they lived in the past year). We unfortunately do not have information on which year the respondents moved to their respective neighborhoods in relation to when their symptoms began. We hope that by modeling past-year disorders in addition to lifetime disorders, we were able to capture current symptoms that may be explained by the respondents' current neighborhoods. Unfortunately, we do not have an adequate number of new-onset cases in the past year in this study to explicitly assess temporality of onset.

In order to add clarity to our neighborhood construct, we have added, "These neighborhoods represent the areas of China in which respondents currently lived at the time of the survey" to the methods section on page 8.

4) You also use a measure of relative individual income, but in the paper I'm missing the rationale for this choice. I recently did a study on relative individual income deprivation, neighbourhood effects and mental problems that may be of interest for this study as well. Perhaps it could provide some insights (J Youth Adolescence, doi: 10.1007/s10964-017-0668-6). This may also explain that you do not find effects of neighbourhood income, because the effect of neighbourhood income on mental illness may be dependent on individuals' own income (i.e., how individuals' income compares to that of the neighbourhood).

Our response: Thank you for pointing us to this very interesting paper, which we have now cited on page 5 (ref #12). As we understand it, your paper used individual income relative to the neighborhood, whereas we used individual income relative to the city. This construct was chosen in order to control for differences in costs of living between the three cities, which could plausibly have an effect on mental disorders beyond the effect of neighborhood relative income. We have attempted to make this rationale and method more clear both in the methods section on page 10 and in the discussion section on pages 26-28; for example, on page 27-28 we added, "In order to control for city-level differences in costs of living, we measured individual income as the ratio of individual income to median city-level income."

5) The two neighbourhood-level variables in which you are most interested are both split into 3 categories. I wonder why such a crude measure was chosen. What's wrong with continuous measures? Did you also try other splits? Please elaborate on this decision.

Our response: We would like to thank both reviewers for encouraging us to be more deliberate in operationalizing these variables. Our original thinking was that exposures would be more interpretable in terms of results and conclusions when constructed as categorical variables, because a one-unit increase in continuous variables is often difficult to conceptualize. However, we agree that tertiling the exposures may be unnecessarily crude and cause a loss of information. Thus, we tested the linearity of both neighborhood-level variables as well as individual income in relation to the outcomes and found that they could be operationalized as linear variables. We therefore used them as continuous exposures for our main analyses in the revised version. This change was made throughout the paper including in the methods, results, and tables.

We also opted to keep a few sentences in the abstract and results sections interpreting the tertiled versions of neighborhood-level percent married (not shown in tables) for extra interpretation, in case readers find that interpretation more understandable than that of the continuous variables. We feel that having two different ways to operationalize of our main exposure variables has added to the overall interpretation and robustness of our results, so we thank both reviewers for this helpful suggestion.

6) Finally, you acknowledge the potential bias of neighbourhood selection and reverse causality, however, I would like to see more discussion on how this problem may affect the interpretation of your results. As you acknowledge, with cross-sectional data it is hard to deal with this problem, but that makes it therefore even more important to give some more attention to it in light of this specific study.

Our response: We would like to thank the reviewer for encouraging us to expand our discussion of this issue. We now discuss the possibility of reverse causation in two different parts of the discussion section, on pages 25 (in terms of social causation vs. social selection) and 27. We have added on page 27, "…we cannot exclude the possibility of reverse causation that concentrates individuals with externalizing mental disorders in neighborhoods with fewer married individuals, as posited by the social selection theory.(33) Although this potential for reverse causation cannot be discarded, recent systematic reviews have suggested that there is a consistent association of neighborhood characteristics with mental health.(42) Further, Dohrenwent and colleagues found that social causation was a more likely theory than social selection for substance use disorders in men.(33) Men with substance use disorders are a primary group of those characterized as having externalizing disorder in our study, the outcome for which we found the significant relationship with neighborhood marital status, suggesting that there may in fact be a causal link."

Further, we have removed disorders that typically onset during childhood (oppositional defiant disorder, conduct disorder, and attention-deficit/hyperactivity disorder) from our outcomes in order to further reduce the likelihood of reverse causation for these disorders that might occur early in life and are likely to affect the types of neighborhoods in which residents end up living in adulthood.

Reviewer: 2
Reviewer Name: Richard Shaw
Institution and Country: Institute of Health and Wellbeing, University of Glasgow, United Kingdom.
Competing Interests: None declared

This is an Interesting and well-written paper investigating the relationship between income and percent married measured at the neighbourhood level with lifetime and current mental disorders. The authors find that relationships between lifetime and current mental disorders and neighborhood marital status, but they do not find a relationship between mental disorders and neighborhood income. The sample (approx. 4000) comprises residents aged 17 or older living in three cities in China Shenzhen, Beijing and Shanghai. The statistical methodology used is appropriate. Unless otherwise stated all aspects of the Strobe checklist have been addressed. However, I have some concerns about the way some of the measures are coded and parts of the paper are described.

Our response: Thank you!

Comment: On page 9. Mental disorders are operationalised using a wide variety of categories including anxiety, mood, behavioural, and substance use disorders. I have some concern with the inclusion of disorders defined as beginning in childhood (attention-deficit/hyperactivity disorder, conduct disorder, and oppositional defiant disorder) as reverse causation appears a much more likely explanation of any relationship than for the other measures.

Our response: We would like to thank the reviewer for this excellent point. We removed disorders that tend to onset during childhood (oppositional defiant disorder, conduct disorder, and attention-deficit/hyperactivity disorder) from the outcomes in order to address this issue of temporality and likely reverse causation.

To further address the heterogeneity and broadness of our previous outcome, we have also split the outcome into internalizing disorders (made up of anxiety and mood disorders) and externalizing disorders (made up of substance use disorders and intermittent explosive disorder), in response to one of reviewer #1's suggestions.

We have updated the methods, tables, results and discussion sections throughout the paper to reflect these changes.

Comment: On page 6 the authors need to make a stronger theoretical justification for using percentage marriage at area level as a risk factor. One of the proposed mechanisms "family disruption" would suggest that the possible risk factor was divorce, separation or spouse's dearth rather than marriage itself. If possible the authors should consider alternative ways of classifying marital status at individual or area level.

Our response: Thank you for this suggestion. We agree that our previous language of "family disruption" in our introduction was inappropriate, considering that the "non married" category also includes individuals who have never been married, possibly due to relatively younger age. We were actually more interested in the positive effect of being married on mental disorders, as we have now cleared up on page 7. This is an interesting topic for China as the median age at marriage is very low (24.8 years), and previous studies have more frequently analyzed family structure instead of family disruption. Further, in our sample, the proportion of divorced or widowed respondents was very low (3.4%), making it difficult for us to focus on those specific events as exposures.

To further illustrate this choice, we pointed out on page 7 that Shen et al (Psychological Medicine 2006) found using the Beijing and Shanghai portions of this same data that never having been married was associated with severity of disorder, potentially due to the lack of social support. In our understanding, this finding was more about the lack of being married than it was about family disruption. It is also worth drawing attention to the fact that being divorced or widowed in their analysis was not associated with disorder, potentially due to a lack of power as a result of the low number of divorced or widowed respondents that we have in our larger sample as well.

Finally, we would like both of our neighborhood-level exposures to be considered potential protective measures in the same direction as each other for consistency.

Comment: Page 22, the authors imply that neighbourhood income is associated with persistent mental disorders while the results and abstract do not support this.

Our response: Thank you for pointing this out. We agree that in the previous version of our manuscript, we were not as clear as we could have been regarding the results of the persistence models, which were originally done as a secondary analyses and therefore not a main part of the paper. We have since removed these analyses altogether, to make room for our new models which are now split between externalizing and internalizing disorders, and to minimize this original confusion.

Other more minor issues:
Comment: Title: The authors only use two neighbourhood characteristics, it would be better just to state them explicitly.

Our response: Thank you for this suggestion. In response, we considered using the title "Neighborhood-level income and proportion of married residents in relation to mental disorders in three large Chinese cities: Multilevel models from the cross-sectional World Mental Health Surveys" but are worried that it is too long and unwieldy, and have decided to keep the shorter title. However, we will defer if the editor thinks that we should specify the two characteristics.

Comment: Page 3, Abstract: The authors need to state the specific groups being compared to produce the odds ratios.

Our response: We have now explicitly added the reference group to the sentence in the abstract referring to the tertiled analysis: "When split into tertiles, individuals living in neighborhoods in the top tertile of percent of married residents had 54% lower odds of a past-year externalizing disorder (OR=0.46, 95% CI: 0.24-0.87) compared to those in the bottom tertile" (page 3).

Now that we have also transformed our main exposure variables to continuous constructs throughout the paper as a result of your other helpful suggestion below, reference to comparison categories for those sentences is no longer needed.

Comment: Page 3: Abstract. I would refrain from speculating on discussing mechanisms in the conclusion

Our response: Thank you for this suggestion. We have now removed discussion of mechanisms from the conclusion section of the abstract, and instead added this more general statement: "Possible mechanisms for this finding are discussed and related to social causation, social selection, and social control theories" (page 3).

Comment: Page 6: The authors should state the upper age range of the study participants. Depending on the age range in the study, the paper might be improved if an additional older age category was added. The 50+ age group may include a diverse range of people including retirees and widows. I also wonder if the data would support the use of a 25-34 year old age category.

Our response: We have now added an age category for 65+ to all our models (shown in tables 2-6 and also updated in the methods section on page 10). As the reviewer correctly pointed out, our previous categorization of 50+ includes a diverse group of people, as we do have a high upper age range (88), which is now also explicitly stated in the abstract (page 3) and methods (page 7).

This categorization is now also in alignment with other WMH papers using the same data (Shen et al).

Comment: Page 10: What were the reasons that authors chose not to use a continuous measures of income both at neighbourhood and individual level?

Our response: We would like to thank both reviewers for encouraging us to be more deliberate in operationalizing these variables. Our original thinking was that exposures would be more interpretable in terms of results and conclusions when constructed as categorical variables, because a one-unit increase in continuous variables is often difficult to conceptualize. However, we agree that tertiling the exposures may be unnecessarily crude and cause a loss of information. Thus, we tested the linearity of both neighborhood-level variables as well as individual income in relation to the outcomes and found that they could be operationalized as linear variables.

We therefore used continuous exposures for our main analyses in the revised version. This change was made throughout the paper including in the methods, results, and tables.

We also opted to keep a few sentences in the abstract and results sections interpreting the tertiled versions of neighborhood-level percent married (not shown in tables) for extra interpretation, in case readers find that interpretation more understandable than the continuous variables. We feel that having two different ways to operationalize of our main exposure variables has added to the overall interpretation and robustness of our results, so we thank both reviewers for this helpful suggestion.

Comment: Page 12, Table 2: I would suggest including all categories for the individual level variables within the table.

Our response: We have now made this addition on pages 13-14.

Comment: Page 15: Top of table 3 and subsequent tables. Please provide column headings indicating which model is being referred to.

Our response: Thank you for this suggestion. We have now made this addition at the top of tables 3-6.

Comment: Page 23: the authors might consider extending the discussion of possible explanations for the relationship between neighbourhood marital status and mental disorders.

Our response: We thank the reviewer for this suggestion. We have expanded the discussion on pages 25-26 to include, "The inverse association of neighborhood-level percentage of married individuals and externalizing disorders found in our study suggests a protective community effect of marriage, possibly through its effect on social cohesion. The social control (or social bond) theory primarily used in criminology, which states that traditional social relationships may buffer against externalizing behavior in the form of crime,(37) may potentially be extended to our results in terms of communities of married families acting as a buffer against its residents developing externalizing disorders such as substance abuse. Additionally, living in a neighborhood with more married individuals has been associated with higher neighborhood satisfaction,(38) and marital status is frequently associated with more political participation and social support.(24) We were not able to directly analyze neighborhood social cohesion with our data, but we welcome new studies that test this hypothesis. Another possible explanation is that the neighborhood marriage distribution is an indicator of other neighborhood characteristics not measured. For example, previous studies in other contexts have found that areas with higher marriage rates also have more upward mobility,(39, 40) which could plausibly affect local mental health."

Responses
Finally, we would like to point out some additional changes we made to the manuscript not mentioned above:

-After removing childhood-onset disorders from our outcome, increasing the number of age categories to four, and splitting our outcome into two separate sets of models, we had sparser data and were no longer able to keep individual income as a valid random effect. As a result, our models now only have a random intercept varying at the neighborhood level (which was the case with the persistence model in the previous version of our manuscript, for the same reason of smaller overall numbers in those models).

-We removed the persistence models from the manuscript, which we felt didn't contribute a great deal to the paper and which we had trouble fitting into the manuscript after adding tables to split our outcome into two

-We moved the limitations section toward the end of the discussion section

-We added some additional discussion on internalizing vs. externalizing disorders specifically in both the introduction (page 6) and conclusion sections (pages 25-27).

Thank you very much for your consideration and for this opportunity to strengthen our manuscript.

VERSION 2 – REVIEW

REVIEWER	Jaap Nieuwenhuis Delft University of Technology, the Netherlands
REVIEW RETURNED	28-Aug-2017

GENERAL COMMENTS	The authors have addressed all comments clearly and thoroughly, resulting in a much improved manuscript. I have no new comments to add to this version.

REVIEWER	Richard Shaw Institute of Health & Wellbeing, University of Glasgow, United Kingdom
REVIEW RETURNED	26-Aug-2017

GENERAL COMMENTS	Overall I happy with the changes the authors have made. A slightly pedantic but hopefully helpful comment is that by adding intraclass correlation coefficients, variance parameters and zero G chi squared test for random intercepts, the authors now formally test neighbourhood influences on mental disorders in general in addition to the two specified characteristics. As a consequence the authors would be justified in using more general neighbourhood terms in their title.