

PEER REVIEW HISTORY

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ARTICLE DETAILS

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| TITLE (PROVISIONAL) | Do social interactions explain ethnic differences in psychological distress and the protective effect of local ethnic density? Evidence from a cross-sectional study of 226,487 adults in middle-to-older age |
| AUTHORS | Feng, Xiaoqi; Astell-Burt, Thomas; Kolt, Gregory |

VERSION 1 - REVIEW

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| REVIEWER | Laia Becares, Research Associate University of Manchester, UK |
| REVIEW RETURNED | 25-Feb-2013 |

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| THE STUDY | <p>Abstract The third sentence of methods section reads odd, please rephrase</p> <p>Introduction The introduction is underdeveloped. The authors argue about the importance of examining the ethnic density effect in Australia, but the reasoning behind the exploration of social support needs to be better explained. Why has been social support chosen for the study? It's not sufficient to say that studies in the US and UK have found contrasting results. Why have the authors focused on social support, and not the other hypothesised mechanisms behind ethnic density?</p> <p>Methods -Please provide more information on the 45 and UP study. Was the baseline data collected throughout the three years? 18% response rate is very low, how does this affect the generalisation to the population of NSW? -What were the response categories to the ancestry question? How many responses were participants allowed to select? And how have the authors dealt with multiple choices (if this was allowed)? -Were participants asked about Aboriginal or Torres Strait Islander origin? How have the authors treated this information in their analyses? - More information is needed on the imputation of missing values. What was the missingness mechanism? How was missing data imputed? What predictors were included in the imputed model? - Why were four questions selected from a validated measure of social support? As mentioned above, these items are not neighbourhood-specific, so it is unclear why these variables were assessed to measure the role of social support in explaining the association between ethnic density and mental health. - More information needs to be provided on the Socio-Economic Index for Areas (SEIFA). What variables were included in the Index? How was it computed? - It is unclear from the description of the statistical analyses conducted how the ethnic density effect was assessed. Were</p> |
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| | models stratified by ethnic group and a measure of own ethnic density was included as predictor, or were all ethnic groups included in the categorical variable described by the authors, and then an interaction with ethnic density and this variable was included in the model? This is briefly described in the results, but should be moved to the methods section. |
| RESULTS & CONCLUSIONS | <p>Discussion</p> <ul style="list-style-type: none"> - Like the introduction, the discussion is underdeveloped. The authors compared their result to two other studies, but do not place their findings within the wider literature. A more critically engaged discussion of the findings should be provided. Information is also needed in terms of the implications of the findings for understanding the ethnic density effect. How does this study contribute to understanding this phenomenon? - The limitations should also include the lack of representativeness that results from the study's response rate. |

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| REVIEWER | Richard Shaw, Investigator Scientist MRC/CSO Social and Public Health Sciences unit Glasgow, Scotland. |
| REVIEW RETURNED | 25-Feb-2013 |

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| THE STUDY | <p>Overall the methods and data are largely appropriate for the questions to be addressed. However, there is one major issue which the authors may have difficulty to address and a number of minor improvements that could be addressed.</p> <p>The biggest concern with this paper is the data set's response rate which is 18%. The potential for biases here are massive and the response rate is not even mentioned as a potential limitation. At the very least the authors need to compare the sample to the originally intended population (or cite references that do so). Preferably the comparison should provide some indication of how response rates vary by ethnicity, socioeconomic characteristics and health (preferably mental health).</p> <p>Whilst good overall, some improvements could be made of the abstract and summary. In particular the methods section of the abstract could be clearer, and I do not agree entirely with the authors conclusions.</p> <p>Given the brief nature of the introduction it is reasonably described. However, the study would be greatly improved if more consideration was given to the levels of ethnic density required to have a protective ethnic density effect and how this is operationalised. This is mostly an issue relating to how results are interpreted (see below). However, improving conceptual clarity at the start may help with how the results are presented and interpreted. The introduction might also benefit from more detail on why social interactions are theorised to explain the ethnic density hypothesis. In addition, given the paper uses Australian data the authors should probably refer to a paper by Krupinski.</p> <p>Krupinski J. Psychological maladaptations in ethnic concentrations in Victoria. In Cultures in Collision (ed. I Pilowsky): 49–58. Australian National Association for Mental Health, 1975.</p> <p>The methods section is reasonably well written overall but there is</p> |
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| | <p>scope for some improvements.</p> <p>On page 5 line 8 the authors state that they used imputation. However, the method used and under which assumptions is not reported this is a concern as some methods of imputation can increase biases.</p> <p>It would be interesting to know the extent to which the Kessler scale and the measures of social interaction have been validated for the ethnic groups in the study rather than just general validation references.</p> |
| RESULTS & CONCLUSIONS | <p>On the whole the results are well presented although I have a couple of concerns relating to their presentation and interpretation.</p> <p>The first relates to the paragraphs at the bottom of page 9 and top of page 10. The key issue here is that the results focus on the means for ethnic density rather than looking at the overall distribution and this has consequences for interpretation and understanding of the results. This illustrated best by Australian born Chinese. Using figure 1 the mean percentage at which these people live is <5% yet the maximum density for this population is over 60%. Suggesting a small group of individuals living in ethnically dense areas and a large number isolated from their own ethnicity. The variability in densities at which people live is almost certainly more important determinant of finding an ethnic density effect than the mean. 5 out of the 6 ethnic groups for which there is an ethnic density effect would appear to have a maximum ethnic density of >50%. In addition, it may be helpful to know the number of CCDs in which ethnic minority people are resident (Level 2 sample size for each ethnic group). This is an area that needs a great deal more thought and discussion, and results selected accordingly.</p> <p>I assume that the odds ratios presented in table 3 are for a 1 % change in ethnic density, given that in some cases the odds ratios and the confidence intervals all have the same value, the results may be easier to interpret if they represented a 10% change or use of some other metric. For some ethnic groups with a low maximum a 1 % change may be appropriate, however, for others, e.g. UK born English people whose densities range from aprox 2 to 75%, the actual effects of ethnic density are probably very large and not adequately presented.</p> <p>Whilst I agree that these results would suggest that social interactions do not explain ethnic density effects. I would tone down the extent of influence that individual and neighbourhood characteristics have on explaining ethnic density effects. A case could be made individual and neighbourhood effects explaining the results for Australian born Irish people and Australian born Australians, However, for the other ethnic groups individual characteristics have little impact on the point estimates and the change in significance would appear to be due to widening confidence intervals.</p> <p>In addition, Could you please clarify what the p-values in table 1 are for and how they are calculated. It might also be worth noting that the large number of ethnic groups studied necessitates a large number of statistical tests and a corresponding increase in chance findings.</p> <p>Overall, the discussion needs to place a greater emphasis on the scale at which ethnic density is measured and the densities required to find an effect. The results also need to be discussed in the context that most of the ethnic minority groups investigated are from a white</p> |

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| | European background and this contrasts greatly from most other studies which investigate people who may be from a much more visible minority group. |
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| REVIEWER | Mai Stafford Programme Leader Track MRC Unit for Lifelong Health and Ageing London, UK. No competing interests. |
| REVIEW RETURNED | 07-Mar-2013 |

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| THE STUDY | <p>The introduction does not develop the aim of looking at ethnic differences in social interactions, or social interactions as explanation for ethnic differences in psychological distress though both these components feature heavily in the results and conclusion.</p> <p>There is additionally a literature on ethnic density and social cohesion/social capital. Although these did not examine the contribution of social interactions to the link between ethnic density and psychological distress, they seem relevant here. See e.g. Laurence, J. and Heath, A. (2008) Predictors of community cohesion: multilevel modelling of the 2005 Citizenship Survey. Department for Communities and Local Government, London. Letki, N. (2008) Does diversity erode social cohesion? Social capital and race in British neighbourhoods, Political Studies, 56, pp. 99–126. Bécares, L., Stafford, M., Laurence, J., & Nazroo, J. (2011) Composition, Concentration and Deprivation: Exploring Their Association With Social Cohesion Among Different Ethnic Groups In The UK. Urban Studies.</p> <p>It would be interesting to see the association between ethnic density and social interactions presented in this paper.</p> <p>Please explain how ethnic identity was assessed in the census, and discuss implications if this differed from the way it was assessed in the 45 Up study.</p> <p>Please describe what method of imputation was used.</p> <p>"Model 1 adjusted ethnic density for age and gender" (page 10) is not quite correct. The association between ethnic density and distress is adjusted.</p> <p>What units of ethnic density are used in the model? ie are the OR for a 1% increase in density?</p> |
| RESULTS & CONCLUSIONS | <p>Interpretation: The statement that social interactions only explained higher risk of psychological distress experienced by Chinese born in China seems to rely overly on statistical significance. The odds ratios in model 2 of Table 2 indicate social relations contribute to explanation in some other ethnic groups.</p> <p>I think some comment on why the association between ethnic density and psychological distress depends on ethnic group and country of birth is warranted.</p> |
| GENERAL COMMENTS | <p>Major:</p> <p>I think the 18% response rate should be included in the limitations.</p> |

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| | <p>Minor:</p> <p>Abstract first line of methods needs to clarify the regression model (association of Kessler with what?)</p> <p>Were age x ethnicity and gender x ethnicity interactions considered?</p> <p>Please spell out NSW on first appearance.</p> <p>Were those of mixed ethnicity excluded?</p> |
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VERSION 1 – AUTHOR RESPONSE

Reviewer: Laia Becares, Research Associate
University of Manchester, UK

Authors: Dear Laia. Thank you for your positive feedback on our paper.

R1: Abstract - The third sentence of methods section reads odd, please rephrase

Authors: We have rephrased the sentence.

R1: Introduction - The introduction is underdeveloped. The authors argue about the importance of examining the ethnic density effect in Australia, but the reasoning behind the exploration of social support needs to be better explained. Why has been social support chosen for the study? It's not sufficient to say that studies in the US and UK have found contrasting results. Why have the authors focused on social support, and not the other hypothesised mechanisms behind ethnic density?

Authors: We have added a sentence to clarify the hypothetical mechanism in the Introduction. We feel that the brevity of the introduction is an advantage and that the rationale for a focus on social support – due to a severe shortage of empirical studies testing a widely cited hypothesis – is justified. We cite the lack of variable to measure racism as a limitation in the discussion.

R1: Methods - Please provide more information on the 45 and UP study. Was the baseline data collected throughout the three years? 18% response rate is very low, how does this affect the generalisation to the population of NSW?

Authors: Baseline data was collected between 2006 and 2009. We have added “inclusive” to indicate that data was collected throughout the three-year period. We have provided an extra sentence which describes the data source from which the 45 and Up Study was sampled. We have also extended the sentence that now reflects on the 18% response rate and a recent comparative study involving ‘representative’ data, which reported similar findings with the 45 and Up Study.

R1: What were the response categories to the ancestry question? How many responses were participants allowed to select? And how have the authors dealt with multiple choices (if this was allowed)?

Authors: We have clarified in the text that the first response out of a maximum of two for the ancestry question was the subject of our investigation. Secondary responses to the ancestry question were not permitted for use in this study.

R1: Were participants asked about Aboriginal or Torres Strait Islander origin? How have the authors treated this information in their analyses?

Authors: Participants were asked separately from the ancestry question about Aboriginal or Torres Strait Islander origin, but this data was not available for use at the time of this investigation. A follow-up study is underway to examine this group, for which special permission is necessary to access responses on this particular question. We have reflected this in the last paragraph of the Discussion section.

R1: More information is needed on the imputation of missing values. What was the missingness mechanism? How was missing data imputed? What predictors were included in the imputed model?

Authors: We have clarified that a single imputation strategy.

R1: Why were four questions selected from a validated measure of social support? As mentioned above, these items are not neighbourhood-specific, so it is unclear why these variables were

assessed to measure the role of social support in explaining the association between ethnic density and mental health.

Authors: We used all four questions from the shortened version of the Duke Social Support Index that were available in the 45 and Up Study. No other measures of social support were available. We have reflected in the Discussion on the imperfect measures of these social interactions with respect to social support drawn from within versus outside the neighbourhood of residence.

R1: More information needs to be provided on the Socio-Economic Index for Areas (SEIFA). What variables were included in the Index? How was it computed?

Authors: The SEIFA variable is widely used indicator of area deprivation or affluence within Australia, akin to the indices of multiple deprivation that are used widely in the UK. We have specified that this is a variable made available by the Australian Bureau of Statistics (ABS) and have provided two examples of Census variables which are used in its construction.

R1: It is unclear from the description of the statistical analyses conducted how the ethnic density effect was assessed. Were models stratified by ethnic group and a measure of own ethnic density was included as predictor, or were all ethnic groups included in the categorical variable described by the authors, and then an interaction with ethnic density and this variable was included in the model? This is briefly described in the results, but should be moved to the methods section.

Authors: We have specified that the regression models in which the measure of own-group ethnic density as fitted were ethnic and country of birth-specific (i.e. stratified) models. We have highlighted where this is indicated in the Methods section.

R1: Like the introduction, the discussion is underdeveloped. The authors compared their result to two other studies, but do not place their findings within the wider literature. A more critically engaged discussion of the findings should be provided. Information is also needed in terms of the implications of the findings for understanding the ethnic density effect. How does this study contribute to understanding this phenomenon?

Authors: We have developed the discussion section in this regard, taking into account other helpful comments from Reviewers 2 and 3.

R1: The limitations should also include the lack of representativeness that results from the study's response rate.

Authors: We have reemphasised the point made in the Method section on the 18% response rate and that, while low, results from the 45 and Up Study compare favourably to those derived from a 'representative' dataset in a previous study.

Reviewer: Richard Shaw,
 Investigator Scientist
 MRC/CSO Social and Public Health Sciences unit
 Glasgow, Scotland.

Authors: Dear Richard. Thank you for your positive feedback on our paper.

Overall the methods and data are largely appropriate for the questions to be addressed. However, there is one major issue which the authors may have difficulty to address and a number of minor improvements that could be addressed.

R2: The biggest concern with this paper is the data set's response rate which is 18%. The potential for biases here are massive and the response rate is not even mentioned as a potential limitation. At the very least the authors need to compare the sample to the originally intended population (or cite references that do so). Preferably the comparison should provide some indication of how response rates vary by ethnicity, socioeconomic characteristics and health (preferably mental health).

Authors: We have reflected on the 18% response rate as a potential limitation within the Discussion section as recommended.

R2: Whilst good overall, some improvements could be made of the abstract and summary. In

particular the methods section of the abstract could be clearer, and I do not agree entirely with the authors conclusions.

Authors: Thank you for your positive feedback. We have made adjustments to the Methods section of the Abstract to improve the clarity of the writing. We have also inserted "fully" into the Conclusion section to lessen the emphasis.

R2: Given the brief nature of the introduction it is reasonably described. However, the study would be greatly improved if more consideration was given to the levels of ethnic density required to have a protective ethnic density effect and how this is operationalised. This is mostly an issue relating to how results are interpreted (see below). However, improving conceptual clarity at the start may help with how the results are presented and interpreted. The introduction might also benefit from more detail on why social interactions are theorised to explain the ethnic density hypothesis. In addition, given the paper uses Australian data the authors should probably refer to a paper by Krupinski. Krupinski J. Psychological maladaptations in ethnic concentrations in Victoria. In Cultures in Collision (ed. I Pilowsky): 49–58. Australian National Association for Mental Health, 1975.

Authors: Thank you for your comment on the brevity of the introduction. We have added sentences to clarify the hypothetical mechanism in the introduction.

R2: The methods section is reasonably well written overall but there is scope for some improvements. On page 5 line 8 the authors state that they used imputation. However, the method used and under which assumptions is not reported this is a concern as some methods of imputation can increase biases.

Authors: We have clarified that a single imputation strategy was used.

R2: It would be interesting to know the extent to which the Kessler scale and the measures of social interaction have been validated for the ethnic groups in the study rather than just general validation references.

Authors: We have inserted a comment with references on the validation of the Kessler scale across different countries and ethnic groups.

R2: On the whole the results are well presented although I have a couple of concerns relating to their presentation and interpretation. The first relates to the paragraphs at the bottom of page 9 and top of page 10. The key issue here is that the results focus on the means for ethnic density rather than looking at the overall distribution and this has consequences for interpretation and understanding of the results. This illustrated best by Australian born Chinese. Using figure 1 the mean percentage at which these people live is <5% yet the maximum density for this population is over 60%. Suggesting a small group of individuals living in ethnically dense areas and a large number isolated from their own ethnicity. The variability in densities at which people live is almost certainly more important determinant of finding an ethnic density effect than the mean. 5 out of the 6 ethnic groups for which there is an ethnic density effect would appear to have a maximum ethnic density of >50%. In addition, it may be helpful to know the number of CCDs in which ethnic minority people are resident (Level 2 sample size for each ethnic group). This is an area that needs a great deal more thought and discussion, and results selected accordingly.

Authors: We assure the Reviewer that we have given the presentation of Results a great deal of thought. The results displayed for ethnic density analyses are focussed upon those ethnic and country of birth groups with mean ethnic densities of 2% or more. These groups also broadly correspond to those for which the maximum ethnic density reported is also reasonably high. The groups which are not reported in this analysis had both very low mean and maximum ethnic densities.

R2: I assume that the odds ratios presented in table 3 are for a 1 % change in ethnic density, given that in some cases the odds ratios and the confidence intervals all have the same value, the results may be easier to interpret if they represented a 10% change or use of some other metric. For some ethnic groups with a low maximum a 1 % change may be appropriate, however, for others, e.g. UK born English people whose densities range from approx 2 to 75%, the actual effects of ethnic density are probably very large and not adequately presented.

Authors: Due to a helpful suggestion by Reviewer 3, we have inserted a new Table and the old 'Table 3' is now 'Table 4'. This Reviewer is correct; the results are for a 1% change in ethnic density. We feel that it is crucial that the measure of ethnic density is implemented in a consistent format across all ethnic groups. As such, we disagree that a 10% change should be examined for one group but a 1% is sufficient for another. We have inserted text as a clarifier.

R2: Whilst I agree that these results would suggest that social interactions do not explain ethnic density effects. I would tone down the extent of influence that individual and neighbourhood characteristics have on explaining ethnic density effects. A case could be made individual and neighbourhood effects explaining the results for Australian born Irish people and Australian born Australians, However, for the other ethnic groups individual characteristics have little impact on the point estimates and the change in significance would appear to be due to widening confidence intervals.

Authors: We have amended the sentence to note the influence on odds ratios and 95% confidence intervals.

R2: In addition, Could you please clarify what the p-values in table 1 are for and how they are calculated. It might also be worth noting that the large number of ethnic groups studied necessitates a large number of statistical tests and a corresponding increase in chance findings.

Authors: We have inserted a comment stating that p-values in table 1 were calculated using logistic regression. We have also inserted the reference group (Australian, Australia) within parentheses at the bottom of Table 1.

R2: Overall, the discussion needs to place a greater emphasis on the scale at which ethnic density is measured and the densities required to find an effect. The results also need to be discussed in the context that most of the ethnic minority groups investigated are from a white European background and this contrasts greatly from most other studies which investigate people who may be from a much more visible minority group.

Authors: We have commented on the imperfect correlation between administrative boundaries and residents' perceptions of neighbourhood boundaries.

Reviewer: Mai Stafford
 Programme Leader Track
 MRC Unit for Lifelong Health and Ageing
 London, UK.

No competing interests.

Authors: Dear Mai. Thank you for your positive feedback on our paper.

R3: The introduction does not develop the aim of looking at ethnic differences in social interactions, or social interactions as explanation for ethnic differences in psychological distress though both these components feature heavily in the results and conclusion.

Authors: We have amended parts of the introduction to show that we are also interested in ethnic differences in social interactions and mental health, in addition to the potential influence of ethnic density.

R3: There is additionally a literature on ethnic density and social cohesion/social capital. Although these did not examine the contribution of social interactions to the link between ethnic density and psychological distress, they seem relevant here. See e.g. Laurence, J. and Heath, A. (2008) Predictors of community cohesion: multilevel modelling of the 2005 Citizenship Survey. Department for Communities and Local Government, London.

Letki, N. (2008) Does diversity erode social cohesion? Social capital and race in British neighbourhoods, *Political Studies*, 56, pp. 99–126.

Bécares, L., Stafford, M., Laurence, J., & Nazroo, J. (2011) Composition, Concentration and Deprivation: Exploring Their Association With Social Cohesion Among Different Ethnic Groups In The UK. *Urban Studies*.

Authors: We thank the Reviewer for these suggestions and have integrated them into the first paragraph of the Introduction.

R3: It would be interesting to see the association between ethnic density and social interactions presented in this paper.

Authors: We have included this in a new 'Table 3' and inserted text in the Results section.

R3: Please explain how ethnic identity was assessed in the census, and discuss implications if this differed from the way it was assessed in the 45 Up study.
 Authors: The question on ancestry in the Census and the 45 and Up Study was broadly comparable and we have inserted a comment on this in the Method section.

R3: Please describe what method of imputation was used.
 Authors: We have clarified that a single imputation strategy was used.

R3: "Model 1 adjusted ethnic density for age and gender" (page 10) is not quite correct. The association between ethnic density and distress is adjusted.
 Authors: Amended as recommended.

R3: What units of ethnic density are used in the model? ie are the OR for a 1% increase in density?
 Authors: The units are for a 1% increase in own-group ethnic density. We have inserted a comment on this in the results section.

R3: Interpretation: The statement that social interactions only explained higher risk of psychological distress experienced by Chinese born in China seems to rely overly on statistical significance. The odds ratios in model 2 of Table 2 indicate social relations contribute to explanation in some other ethnic groups.
 Authors: We have expanded our reporting to attenuation of the size of odds ratios between model 1 and 2 without overly relying upon statistical significance as recommended. We have commented that this tends to occur among groups born outside Australia, using the Lebanese born in Lebanon and Croatians born in Croatia as examples.

R3: I think some comment on why the association between ethnic density and psychological distress depends on ethnic group and country of birth is warranted.
 Authors: This is an excellent point and we have expanded upon it within the Discussion section.

R3: I think the 18% response rate should be included in the limitations.
 Authors: Included as recommended.

R3: Abstract first line of methods needs to clarify the regression model (association of Kessler with what?)
 Authors: We have amended this sentence.

R3: Were age x ethnicity and gender x ethnicity interactions considered?
 Authors: These interactions were not investigated.

R3: Please spell out NSW on first appearance.
 Authors: Amended as recommended.

R3: Were those of mixed ethnicity excluded?
 Authors: Only the first response (of two maximum) was available for analysis within our dataset, though follow-up investigations of mixed ethnicity are planned.

VERSION 2 – REVIEW

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| REVIEWER | Richard J Shaw, MRC/CSO Social and Public Health Sciences Unit |
| REVIEW RETURNED | 30-Mar-2013 |
| THE STUDY | Overall, the authors have improved the paper considerably. However, there are still some minor points that definitely need changing. In addition, there are other points on which I disagree with the authors although they may not necessarily be a barrier to publication. |

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| | <p>Introduction - An important if minor point is that the paper I referred the authors to last time by Krupinski is an ecological ethnic density study using Australian data. Whilst the authors' study is a considerable advancement on Krupinski's it is not the first ethnic density study. Please change the introduction. Krupinski J. Psychological Maladaptations in Ethnic Concentrations in Victoria. In Cultures in Collision (ed. I Pilowsky): 49-58. Australian National Association for Mental Health, 1975.</p> <p>Methods - The authors should be very wary about using mean imputation to tackle missing data. It reduces the variability in samples and can bias regression coefficients. I would suggest that the authors conduct sensitivity analyses to see if the odds ratios are the same using complete case analysis.</p> |
| RESULTS & CONCLUSIONS | <p>My key issue in the results and discussion relates to the discussion of the response rate. The authors should mention that Mealing's paper found heterogeneity between studies for psychological distress and English Language use at home. Whilst regression methods are robust to missing data assumptions there is still the possibility of bias. Given the rarity of large studies with any ethnic minority data the authors study is still publishable, but they do need to mention that the data could be biased.</p> <p>Results- My key issue with the results refers to table 4 again. Whilst I agree that it is easier for readers if ethnic density is measured using the same scale for all ethnic groups. I do not believe that a 1% change is appropriate. Figure 2 indicates that for some ethnic groups the ethnic density range is from 0 to 100%. For these ethnic groups a 1% change represents a tiny proportion of the variability and as a consequence interpretation of the results in the tables is extremely difficult. This is illustrated by the point estimates for Odds ratios and the confidence intervals having precisely the same value. I would suggest the authors either look at alternative ways of coding ethnic density or report the results to more decimal places. In addition the authors should note that they have indicated that *** refers to a p value of $p < 0.05$.</p> <p>Page 13: When discussing the scale at which ethnic density is measured I would refer the authors back to their own previous paragraph and suggest that geographic scale is also an issue that may require a more nuanced approach.</p> |
| GENERAL COMMENTS | <p>The revisions are a considerable improvement over the previous draft. However, the authors need to make some minor changes to the introduction and discussion before the paper is publishable. In addition, whilst not necessarily required for publication the authors would considerably improve the paper by including additional analyses testing for the sensitivity of their results to the mean imputation and presenting the results in table 4 differently.</p> |

VERSION 2 – AUTHOR RESPONSE

Reviewer: Overall, the authors have improved the paper considerably. However, there are still some minor points that definitely need changing. In addition, there are other points on which I disagree with the authors although they may not necessarily be a barrier to publication. Introduction - An important if minor point is that the paper I referred the authors to last time by Krupinski is an ecological ethnic density study using Australian data. Whilst the authors' study is a considerable advancement on Krupinski's it is not the first ethnic density study. Please change the introduction. Krupinski J.

Psychological Maladaptations in Ethnic Concentrations in Victoria. In Cultures in Collision (ed. I Pilowsky): 49-58. Australian National Association for Mental Health, 1975.

Authors: Thank you for this suggestion. We have amended the second paragraph of the Introduction and cited Krupinski's work. See below:

'This is especially the case in Australia (with the exception of an earlier ecological study 19)'

Reviewer: Methods - The authors should be very wary about using mean imputation to tackle missing data. It reduces the variability in samples and can bias regression coefficients. I would suggest that the authors conduct sensitivity analyses to see if the odds ratios are the same using complete case analysis.

Authors: We have conducted sensitivity analyses with complete case data, finding similar results. We have acknowledged this in the 'result' section. See below:

'We have conducted sensitivity analyses with complete case data, finding similar results.'

Reviewer: My key issue in the results and discussion relates to the discussion of the response rate. The authors should mention that Mealing's paper found heterogeneity between studies for psychological distress and English Language use at home. Whilst regression methods are robust to missing data assumptions there is still the possibility of bias. Given the rarity of large studies with any ethnic minority data the authors study is still publishable, but they do need to mention that the data could be biased.

Authors: We have addressed this issue in the final paragraph of the Discussion section, indicating that Mealing's paper provides reassurance but only to an extent. See below:

'However, the comparisons in the aforementioned study did find heterogeneity between psychological distress and English spoken at home, and did not have an explicit focus on ethnic differences. Although regression methods are robust to missing data assumptions, there is still the possibility of bias.'

Reviewer: Results- My key issue with the results refers to table 4 again. Whilst I agree that it is easier for readers if ethnic density is measured using the same scale for all ethnic groups. I do not believe that a 1% change is appropriate. Figure 2 indicates that for some ethnic groups the ethnic density range is from 0 to 100%. For these ethnic groups a 1% change represents a tiny proportion of the variability and as a consequence interpretation of the results in the tables is extremely difficult. This is illustrated by the point estimates for Odd ratios and the confidence intervals having precisely the same value. I would suggest the authors either look at alternative ways of coding ethnic density or report the results to more decimal places. In addition the authors should note that they have indicated that *** refers to a p value of $p < 0.05$.

Authors: We maintain that for this paper we need to keep the measurement of ethnic density

consistent across ethnic groups. Therefore, we opt for the alternative recommendation of reporting the results in Table 4 to more decimal places.

Reviewer: Page 13: When discussing the scale at which ethnic density is measured I would refer the authors back to their own previous paragraph and suggest that geographic scale is also an issue that may require a more nuanced approach.

Authors: We have added two sentences in discussion of this point in this paragraph on page 13. See below:

‘Such perceptions may vary depending upon location, circumstances and individual characteristics; including ethnicity. Therefore, it would appear that future research may need to explore the ethnic density hypothesis with customised measures of neighbourhood scale.’