PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Adjusted indices of multiple deprivation to enable comparisons within and between constituent countries of the UK including an illustration using mortality rates.
AUTHORS	Abel, Gary; Barclay, Matthew; Payne, Rupert

VERSION 1 - REVIEW

REVIEWER	Dr Mark Ashworth King's College London UK
REVIEW RETURNED	05-Jun-2016

GENERAL COMMENTS	Thank you for asking me to review this paper. This is an important study and the first to standardise mortality rates across the 4 countries of the UK. As such it is likely to be a highly cited paper. Specific comments: 1) The title is misleading and needs revising. It simply suggests that this is a rather dry (but important) technical exercise to standardise deprivation reporting across the 4 countries of the UK. In fact, it uses these tools to compare mortality rates stratified according to deprivation. This paper delivers important findings about mortality rates. 2) Table 3 contains some stark discrepancies between deprivation quintiles in the 4 nations. I think the authors could place more emphasis on the fact that 0.0% of the population in NI live in the least deprived UK quintile. This is remarkable. Is it worth adding another decimal point (i.e. was it 0.02%, etc?). The distributions in the mid-range quintiles are similar but again, its remarkable that 36.6% of the NI population are in the most deprived UK quintile. Reading the footnote at the foot of Table 4 appears to provide the answer, stating that 'no areas in NI were in the least deprived fifth of the UK'. Even so, I think this point needs discussion. 3) There is evidence linking health inequalities to mortality, not just with increased deprivation. Can the authors comment on health inequalities in each of the 4 nations? As I read it, this study is all about absolute deprivation, not relative deprivation. 4) Table 4 provides powerful justification of the methods used by the authors to standardise deprivation quintiles across the UK. Once adjusted, the 'protective effect' of living in England disappears to an extent. Instead, the strongest protective effect is appears to be conferred upon residents in NI (apart from in the least deprived quintile for whom there were no NI data). The differences though are relatively small apart from in one domain: the most deprived quintile

	in Scotland for whom there was a particularly large mortality excess. This point needs to be emphasised. I am unclear whether any of these values could be displayed with 95% CIs, but if so, it would enable the reader to determine whether an excess mortality of, say 32.8 (Scotland, least deprived quintile) was of notable public health significance. 5) The authors greatly strengthen their work through considering external comparators such as productivity, employment rates, gross disposable income etc (pg9, line 28). I think it would be helpful to have more detail: what is the GDP of each country and what % of GDP is spent on health. Relating unexplained mortality differences to health expenditure might be a step too far for this study (perhaps a next study?) but at least a consideration of these factors would be helpful. 6) Excess mortality is discussed (pg9, Line 36). However, it is only discussed in the context of being consistent with other literature. It would be helpful to have some discussion about possible reasons for this excess. 7) Limitations. The authors discuss the possible mis-alignment of timescales between the indices in each of the 4 countries. However, it is not just the indices that might be misaligned; the components might be misaligned too. Thus the IMD-2010 consists of 7 domains and several are updated at different times, some with latest survey data, some with mid-Census estimates based on small sample surveys and some by imputation. 8) The authors only refer to the dominant domains of the IMD score (Income and Employment). I would have thought that the 3rd domain, 'Health Deprivation and Disability' would be a strong determinant of deprivation-related mortality. I think this would be worth some discussoin. I trust these comments are of help. 5)

REVIEWER	David Hussey NatCen Social Research, UK
REVIEW RETURNED	11-Jul-2016

GENERAL COMMENTS	The paper is clearly written and easy to follow. It would be helpful but not essential to include: i) Some detail of the alternative regression models considered (with interactions/quadratic terms) perhaps in an appendix; ii) A bit more investigation/ discussion of the issue of contemporaneity of the four IMD scores; iii) Some comparison of the results of this paper with the previous version (reference 3); iv) Another example or two to illustrate the difference between using the country-specific indices vs the UK-wide index using different data (i.e. not just mortality rates).

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name
Dr Mark Ashworth

Comment 1) The title is misleading and needs revising. It simply suggests that this is a rather dry (but important) technical exercise to standardise deprivation reporting across the 4 countries of the UK. In fact, it uses these tools to compare mortality rates stratified according to deprivation. This paper delivers important findings about mortality rates.

Response - In conducting this piece of work we have always felt that this was indeed a technical exercise. As the reviewer points out it is a technical exercise of importance and we hope that the adjusted index will be very useful to a wide range of researchers from a number of disciplines. The analysis we included on mortality was only ever meant to illustrate the potential shortcomings of ignoring the differences we have uncovered in absolute levels of deprivation. However, we do not feel that it is right to place undue emphasis on these results as before making firm conclusions on the role of deprivation on mortality rates in the UK we would want to conduct a more comprehensive study exploring such issues as raised by the reviewer in points 3 and 5. As such we do not wish to make some of the suggested changes to the paper. As the reviewer points out there is the potential for these things to be explored in future work. Our decision not to shift the focus of the paper has also been influenced by the suggestion of reviewer 2 that a further example be included implicitly concurring with our view that the mortality analysis is included as an example only, rather than as a finding in its own right.

However, we do recognise that the findings regarding mortality may be of some interest in their own right and so we have now changed the title to recognise this. The new title is

Adjusted indices of multiple deprivation to enable comparisons within and between constituent countries of the UK including an illustration using mortality rates.

Comment 2) Table 3 contains some stark discrepancies between deprivation quintiles in the 4 nations. I think the authors could place more emphasis on the fact that 0.0% of the population in NI live in the least deprived UK quintile. This is remarkable. Is it worth adding another decimal point (i.e. was it 0.02%, etc?). The distributions in the mid-range quintiles are similar but again, its remarkable that 36.6% of the NI population are in the most deprived UK quintile. Reading the footnote at the foot of Table 4 appears to provide the answer, stating that 'no areas in NI were in the least deprived fifth of the UK'. Even so, I think this point needs discussion.

Response - We agree with the reviewer that this is a stark finding and worthy of note. In the text of the results we already say “None of the areas in Northern Ireland were in the least deprived fifth of the UK, while 36.6% of the population of Northern Ireland were in the most deprived fifth of the UK.”

However, in response to the reviewers comment we will re-emphasise the point in the discussion changing the sentence that reads

“Our country-level results suggest that England and Scotland have similar levels of deprivation, Wales is more deprived and Northern Ireland is the most deprived of the UK countries.”

To

“Our country-level results suggest that England and Scotland have similar levels of deprivation, Wales is more deprived and Northern Ireland is the most deprived of the UK countries with no areas in the

least deprived 20% of the UK."

Comment 3) There is evidence linking health inequalities to mortality, not just with increased deprivation. Can the authors comment on health inequalities in each of the 4 nations? As I read it, this study is all about absolute deprivation, not relative deprivation.

Response - See response to comment 1 – As we do not feel this is the focus of the paper we are not making any changes in response to this comment at this time.

Comment 4) Table 4 provides powerful justification of the methods used by the authors to standardise deprivation quintiles across the UK. Once adjusted, the 'protective effect' of living in England disappears to an extent. Instead, the strongest protective effect is appears to be conferred upon residents in NI (apart from in the least deprived quintile for whom there were no NI data). The differences though are relatively small apart from in one domain: the most deprived quintile in Scotland for whom there was a particularly large mortality excess. This point needs to be emphasised. I am unclear whether any of these values could be displayed with 95% CIs, but if so, it would enable the reader to determine whether an excess mortality of, say 32.8 (Scotland, least deprived quintile) was of notable public health significance.

Response - As pointed out above we feel it would be wrong to emphasise the mortality findings without conducting a more in depth study. Further, we do not feel that these results are the focus of the study. As such we are not wanting to change the text of the paper to emphasise these results further as the focus is on the differences between the upper and lower half of table 4 which reflects the need for our adjusted deprivation score. There is little change in the figures for Scotland due to the fact that the adjusted deprivation levels in Scotland are very similar to those in England.
With regards to confidence intervals, as our focus is on bias (i.e. an absolute difference due to the IMD score used) rather than precision confidence intervals would potentially distract from the argument we are trying to make.

Comment 5) The authors greatly strengthen their work through considering external comparators such as productivity, employment rates, gross disposable income etc (pg9, line 28). I think it would be helpful to have more detail: what is the GDP of each country and what % of GDP is spent on health. Relating unexplained mortality differences to health expenditure might be a step too far for this study (perhaps a next study?) but at least a consideration of these factors would be helpful.

Response - See response to comment 1 – no changes made
Comment 6) Excess mortality is discussed (pg9, Line 36). However, it is only discussed in the context of being consistent with other literature. It would be helpful to have some discussion about possible reasons for this excess.

Response - See response to comment 1 – no changes made

Comment 7) Limitations. The authors discuss the possible mis-alignment of timescales between the indices in each of the 4 countries. However, it is not just the indices that might be misaligned; the components might be misaligned too. Thus the IMD-2010 consists of 7 domains and several are updated at different times, some with latest survey data, some with mid-Census estimates based on small sample surveys and some by imputation.

Response - In response to this comment we have added further details to the corresponding sentence which now reads

"The main weakness of our approach is that the underlying datasets do not necessarily relate to the

same time periods, with the country-specific measures used here being published between 2010 and 2015. Further, the underlying data for individual domains of the deprivation indices come from varying periods prior to publication (Supplementary material Table 1)"

Please also see response to reviewer 1 comment 7

Comment 8) The authors only refer to the dominant domains of the IMD score (Income and Employment). I would have thought that the 3rd domain, 'Health Deprivation and Disability' would be a strong determinant of deprivation-related mortality. I think this would be worth some discussoin.

Response - See response to comment 1 – As we do not feel this is the focus of the paper we are not making any changes in response to this comment at this time.

Reviewer: 2
Reviewer Name
David Hussey

Comment i) Some detail of the alternative regression models considered (with nteractions/quadratic terms) perhaps in an appendix;

Response - We have added an additional appendix which outlines our analysis.

Comment ii) A bit more investigation/ discussion of the issue of contemporaneity of the four IMD scores;

Response - Please see response to reviewer 1 comment 7

Also we have added the following text to the discussion to expand what was already presented in the paper.

That said given the economic changes occurring in the past decade there may be particular challenges posed by the fact that the income and employment data underlying the country-specific IMDs relate to different periods (2008/09 for Northern Ireland, 2010/11 for Scotland and 2012/13 for England and Wales). Changes in the economy over that period means that these data may not be directly comparable. For example, the unemployment benefits claimant rate in Northern Ireland was 4.1% in December 2008, compared with 3.6% for the UK, while in December 2012 it was 7.4%, compared with 4.7% for the UK.[17] When adjusted deprivation indices are used this limitation should always be borne in mind.

Comment iii) Some comparison of the results of this paper with the previous version (reference 3);

Response - We have added the following text to paragraph 3 of the discussion to address this point.

If we compare the regression coefficients from this study (Table 2) to those in the previous work (Table 1 of previous paper [3]). We find that some regression coefficients change substaintially. However, there is very little change in the proportion of variance explained between the original and this study. This almost certainly reflects the fact that there is a degree of collinearity between the two domains and the exact weighting with which they are combinned matters little. Our finding that Wales and NI are more deprived than England and Scotland was also shown in our previous study.[3]

Comment iv) Another example or two to illustrate the difference between using the country-specific

indices vs the UK-wide index using different data (i.e. not just mortality rates).

Response - This suggestion by reviewer 2 prompted a great deal of discussion by the author team. We all agreed that in an ideal world a further example would be worthwhile. However, we were not aware of any suitable data that we had open access to at the LSOA level (which also identified LSOAs rather than give deprivation scores or groupings) for multiple countries that we could apply this to in a timely manner. As such we have left the paper unchanged in this regard and leave it open for those with access to suitable datasets to independently validate the adjusted indices.

VERSION 2 – REVIEW

REVIEWER	Mark Ashworth King's College London
REVIEW RETURNED	02-Sep-2016

GENERAL COMMENTS	The authors have addressed the issues raised and justified sufficiently the reasons for not amending certain sections of the text in response to the referee comments. I particularly welcome the minor change of Title and the inclusion of the phrase 'mortality rates' since this more closely relates to the Methods followed in this study. The added descriptions of the uniqueness of Northern Ireland (in terms of its deprivation distribution) has also contributed to the overall message. I would be very happy to accept these revisions

REVIEWER	David Hussey NatCen Social Research, London, UK
REVIEW RETURNED	19-Sep-2016

GENERAL COMMENTS	The authors have considered the comments very carefully and have responded appropriately. The additions are useful, especially those that relate the the contemporaneity of the respective country IMDs and their components. With regard to the idea of including more examples, the justification not to do so is reasonable as there are indeed few datasets that are publicly available that allow such a comparison to be made. I wasn't able to view the residual plots for some reason but I don't think this matters.