Author’s response to reviews

Title: Mid-term Body Mass Index increase among obese and non-obese individuals in middle life and deprivation status: A cohort study

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Author’s response to reviews: see over
Authors response to comments received by Reviewers

Manuscript (title as originally submitted):

**Body Mass Index increase in middle life is not linked to deprivation status: a cohort study**

The authors are very grateful to both expert Reviewers for their very useful and constructive comments and the time spent in reviewing our article in detail.

**Reviewer 1 (Rebecca Hardy)**

We present the original reviewer comments reproduced *verbatim* in Arial font, and our reply in Times New Roman, for added legibility. We have numbered the Reviewers comments for ease of reference.

**Major compulsory revisions**

“1a. Although considerable analyses have assessed the characteristics of those with missing data, potentially important implications for the findings have not been discussed. It is possible that there is a greater drop out among those in lower deprivation categories. It is possible that those who dropped out from these lower deprivation areas may have been those of lower individual social class compared with those from the same areas who remained in the study. This could have made all deprivation groups more similar in terms of their individual social class”.

We tested this hypothesis empirically, by analysing “loss to follow-up” (i.e. lack of second screening) both by Townsend index quintiles (as in the paper) and deciles. If the proposed hypothesis is true, it would be expected that the coverage in decile 8 (i.e. the most deprived “half” of quintile 4) would be lower than the coverage in decile 7 (i.e. the least deprived “half” of quintile 4), and similarly for deciles 9-10 and quintile 5. The “Reply to Reviewers Figures 1-2” below show that, as the Reviewer suspected, there is differential loss to follow-up within quintiles 4 and 5 in favour of the individuals in the least deprived “halves” of the deprivation quintiles. It is worth however noting that the order of magnitude of these “within quintile” deprivation differentials of “loss to follow-up” is of small scale in absolute terms. Given this, we believe that this factor is unlikely to have biased the results in any considerable way. Notwithstanding our judgment on the matter, the information we provide here ultimately enables a better informed interpretation of the findings by the readers.

1b. “The fact that a relatively greater proportion of those lost to follow-up were also obese, hypertensive and to have high cholesterol, as the authors state are likely to have been excluded from further screens because they are being offered “usual” care for management of their high cardiovascular risk, might also mean that those not re-screened might well have put on more weight compared to those screened”.

It is indeed possible that individuals that were “lost-to-follow up” (i.e. not re-screened) may have had a different weight gain experience to those followed up, as their baseline risk profile was significantly different to those with complete follow-up. We do however again believe that given the overall small magnitude of deprivation group differentials in follow-up in absolute terms (see Figures above), this factor unlikely to have biased the results in any considerable way, in relation to the effect of deprivation group status.

1c. Finally, in terms of overall missing data it is unclear how results presented in Tables 1 and 2 might combine to influence the findings.”
To address this valid point we included an additional file (Additional File 2) showing the combined effect of differentials in loss to follow-up and differentials in “dual BMI ascertainment” (i.e. BMI measured on both screens) by deprivation group. We also now make the degree of combined loss of information due to loss to follow-up and incomplete BMI ascertainment explicit both in the text and the Results section of the Abstract. Again, it worth noting that although the deprivation group differences in both follow-up and “dual BMI ascertainment” were significant statistically (partly given the rather large sample), they were in absolute terms of small magnitude.

To better acknowledge and discuss all the above points, the original paragraphs 3-5 of Discussion were edited.
Reply to Reviewers Figure 1. Completeness of follow-up ("2 screens") by Townsend score quintile (diamond shape) and decile (bars), women (n=21,976)

Completeness of follow-up (%)

Decile / quintile

Reply to Reviewers Figure 2. Completeness of follow-up ("2 screens") by Townsend score quintile (diamond shape) and decile (bars), men (n=19,158)

Completeness of follow-up (%)

Decile / quintile
**Minor Essential Revisions**

2. “Second paragraph of discussion. Clarify discussion of cohort and period effects.”

This paragraph has now been edited out, for brevity, and as it was relating to an issue that was non-central to the objective of the paper. The reader will in any case have access to this information from the “publication history” of the final paper. Please see also relevant points below.

3. “Page 13, second paragraph. In what way might individual measure of socioeconomic stats be “less accurate”.

We thank the Reviewer for providing us with the opportunity to explain this point further. The dataset does include Social Class information for about 77% of women and 78.3% of men (compared with nearly complete ascertainment of deprivation status). There are however two main reasons why Social Class measurement in the dataset appears problematic / unreliable, and for which we have opted not to use it in the main paper.

Firstly, there are questions about the validity of Social Class measurements. Anecdotal information from key informers involved in the delivery of the Programme suggests that measurements contained inaccuracies. Additionally, comparison of the Social Class distribution of screening participants (both for the Census year 1991 and the period 1989-1993) with the Social Class distribution of the enumerated Stockport residents in the 1991 Census identified large discrepancies raising questions about external validity. Although such differences may reflect true differences in the Social Class distribution of participants (as opposed to all residents) this seems unlikely, as there appears to be no Social Class pattern / gradient of over or under-representation.

Secondly, one of the particularities of the Social Class ascertainment methodology historically used by the Programme was that a sizeable proportion of women categorised as “housewives” (occupation-based Social Class categorisation instruments customarily ascribe to housewives the Social Class of the Head of Household and do not include a “housewife” category). This effectively reduces the degree of Social Class ascertainment for women by another 14.7% (i.e. to 62.3% overall).

Notwithstanding all the above points, since the information was available, and as suggested by the Reviewer, we present the mainstay of analysis for Social Class-defined socioeconomic group below. The overall finding of a null effect of socioeconomic status on BMI change among the participants does not change.
<table>
<thead>
<tr>
<th></th>
<th>Women</th>
<th></th>
<th></th>
<th></th>
<th>Men</th>
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<th></th>
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<td>Mean</td>
<td>LCI</td>
<td>UCI</td>
<td>p</td>
<td>Mean</td>
<td>LCI</td>
<td>UCI</td>
<td>p</td>
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<td>All individuals (Women: n=11,158) Men: n=9,831</td>
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<td>0.19</td>
<td>0.16</td>
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<td>&lt;0.000</td>
</tr>
<tr>
<td>Model 2</td>
<td></td>
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<td>I-II</td>
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<td>0.08</td>
<td>0.24</td>
<td>&lt;0.000</td>
<td>0.20</td>
<td>0.15</td>
<td>0.25</td>
<td>&lt;0.000</td>
</tr>
<tr>
<td>III NM</td>
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<td>0.17</td>
<td>0.32</td>
<td>&lt;0.000</td>
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</tr>
</tbody>
</table>

Model 1. Adjusted for age and follow-up time. Reported co-efficient of Model 1 denotes mean annual BMI change (kg/m²/year).
Model 2. As for model 1, but stratified by Social Class group. Reported co-efficients denote the mean annual BMI change for each Social Class group (including "housewives" and those with "missing" social class).

4. “Clarify difference in the meaning of the coefficient between the effect of year (models 1 and 2) and the effect of deprivation (model 3) in Table 3.”

The coefficient values for models 1 and 2 denote the mean annual change in BMI, either for all individuals (model 1) or by each deprivation group separately (model 2). The co-efficient values for model 3 denote the amount of increase in mean annual BMI change from moving from a given deprivation group to the one above it in the ordinal scale 1-5. This is explained in the Methods, but in addition, to the Reviewers suggestion, we have added a relevant explanation at the foot of all tables.

**Discretionary revisions**

**Reviewer comment**

5. “The authors state that the results of no difference between deprivation category in terms of increase in BMI are in contrast to previous findings from the Whitehall II Study (Martikainen and Marmot reference). However, the do not discuss the differences between this and their own study. The previous study was on civil servants rather than a general population sample and the follow-up time was much longer (25 years) than in the current study (5 years). The relatively short length of follow-up is a disadvantage of the current study.”

The above points are now properly acknowledged:

a) in the study Title, which has been edited to reflect this point, raised by both Reviewers 1 and 2 (see below)

b) in Introduction, where differences in study populations, between this and other relevant studies (as also suggested by Reviewer 2) are briefly alluded.

c) in Discussion, along with reference to other studies, again as similarly suggested by Reviewer 2, and with the use of an additional Table (5). We would however wish to emphasise that a systematic literature review on the subject was beyond the remit of the paper.
6. “The Whitehall study also adjusted for baseline BMI in the models of change. Whether to adjust for BMI at baseline or not is a matter of debate when considering change over time. The authors should at least acknowledge this.

We thank the reviewer for raising this point, which led us to additional analysis, and findings that considerably add value and enhance the utility of those originally reported.

Originally we had thought that the question of BMI baseline adjustment is best addressed by restricting the analysis to non-obese individuals, as presented within Table 3. This is because adjusting for baseline BMI assumes that baseline values would influence BMI change in a uniform, linear, way, across the entire spectrum of BMI values, for example that a baseline BMI value of 19 influences future BMI change in the same way as a baseline BMI value of 29 –intuitively and empirically there are problems with this assumption.

To address the Reviewers suggestion, we repeated all analyses adjusting for baseline BMI (i.e. BMI at first screening), and we present the findings as a new Table (4). Overall the findings for all individuals (Model 1), and by each deprivation group (Model 2) showed trivial change from original analysis, however for women only, “test for trend” (Model 3) shows a significant effect of deprivation group on mean annual BMI change (in the order of 0.04 kg/m$^2$ / year). When restricting analysis to individuals who were non-obese (BMI < 30) at baseline, all models show a null result, indicating that the above significant result (in “test for trend” for “all” women) were due to a differential deprivation effect among women who were obese at baseline.

Based on the above observation, in further analysis, both in the (original) Table 3 and the (new in the revised version) Table 4, we provide information stratifying the analysis on individuals that were obese at baseline. It can be seen that both in models with and without baseline BMI adjustment (i.e. both in Tables 3 and 4), there is generally no effect of deprivation group on mean annual BMI increase, except when stratifying the analysis to obese individuals.

This is an important new finding, which has therefore been taken into account in reworking a) in the Title of the paper (making a distinction about obese and non-obese individuals) b) in the Abstract (results and conclusion) c) in Discussion d) in Conclusion

7. “Some of the association and difference discussed were not tested formally in analyses. Specifically the differences by deprivation category in BMI at baseline.”

This has now been properly tested, using logistic (obesity status dichotomous variable) and linear (mean BMI value) regression. A relevant entry was made in the Methods section. The results showed that the already observed gradients were highly significant.

8. “A test for interaction could be used to assess whether there were significant differences in change in BMI by age group.”

We have tested this statistically, and indeed, and as expected by looking at the original analysis presented in the (original submission) Table 3, there was a statistically significant interaction, showing that the younger age group (35-44) was gaining BMI faster than the older age group (45-55). However this is an issue that is aside the main objective and research question of the paper, and given this factor, and also for brevity, we have opted not to describe this in the revised submitted paper. This means that relevant parts of the original
Table 3 were also edited out, also in addition to a relevant paragraph in Discussion (see point 2 in reply to comments received by Reviewer 1 above).

9. “In contrast presentation of the p-values for each deprivation strata presented in table 2 are not particularly informative as it the effect of deprivation on BMI (test for trend) that is the effect of primary interest.”

Table 2 has no been re-worked, along the approach used in Table 1 –this makes it more informative and more easy-to-interpret. We hope this indirectly addresses this point. We have probably not fully captured the issue raised, and would be grateful for additional clarifications, if the Reviewer feels that the improved Table 2 does not address the point.

10. It would appear that individual social status was available, at least on a sub-sample of those in the study. It would be interesting to see whether the results were the same using such measures.

This analysis is now presented above – however please note that the caveats mentioned above about the use of Social Class measurements in this dataset do apply.

Second Reviewer (Karri Silventoinen)

General.

We note the Reviewers agreement that the findings have important public health implications. We particularly welcome the Reviewer’s attention to the relatively large sample size of this study, as well the potential for publication bias (for “positive” findings) in the literature so far.

Major compulsory revisions.

1) The Reviewer recommended a more detailed discussion of the similarities and differences between the present and previous studies on the same matter, both in the abstract and in the main.

To address this:

a) The title of the paper has been changed, to more accurately reflect the length of the follow-up period (see also relevant point “5” made by Reviewer 1).

b) A brief reference is made in the Abstract (“Background”) on the fact that previous studies measures socioeconomic status directly, and that in this study the measurement is ecological.

c) A more detailed discussion of previous studies (see also the second Reviewer’s Major Compulsory Revision point No 2) is made in “Background” of the main article.

d) A more detailed discussion of the findings, in light of previously presented evidence is made in “Discussion” of the main article, with the aid of a new Table (Table 5). Again, we would however wish to emphasise that a systematic literature review on the subject was beyond the remit of the paper.
2) We are thankful to the Reviewer for pointing out these additional previous studies.

To address this:

References to the studies are made in both the “Background” and the “Discussion” section of the main article as alluded (see points “c” and “d” immediately above).

3) The Reviewer recommended that additional information is provided about

- the population coverage of the screening Programme
- the method by which diseases used as exclusion criteria for participation to the Programme were diagnosed / ascertained at baseline
- the definition of hypertension, as part of exclusion criteria (e.g. measurement, medication …)

We fully concur with the Reviewer about the importance of this information. In the original submission we felt somewhat constrained by the need to present an article that will not be of excessive size. A particular problem that we are faced with is that there have been no previous description of this novel dataset in any previous peer-reviewed publications. We therefore attempted to capture relevant information in a brief form in the “Background” section and also in “Appendix 1” of the original submission.

Given the Reviewers comments, we have now tried to address all this issues in more detail in an additional file (“Additional File 1”), which now also incorporates the information originally presented in Appendix 1.

4) Suggestion about description of the quality of the data.

To address this we have added additional explanation about the evaluation of the dataset in the main text, and provided additional information about the dataset in Additional File 1.

5) The reviewer recommended that a description of the Townsend Deprivation Score would be helpful, and that attention should be drawn on the ecological nature of the measurement of socioeconomic status, and on how this may have influenced the results.

To address this:

a) A brief description of Townsend Deprivation Score is now being given, as well as a web-based open-access reference, in addition to the originally given reference. Further details can be found at: http://census.ac.uk/censusdatasystem/chapter9%20scores.htm

b) A discussion about the potential importance / relevance of the ecological nature of measurement of socioeconomic status is included in Discussions, along the lines of “1c” and “1d” above in this reply.

Minor Essential Revisions.

6) p value presentation of “0.000”. We apologise for omitting to consistently present such values correctly (<0.000) in the original submission. Although in principle we agree we the point about likely superfluous presentation of both p values and confidence intervals, we felt that on this occasion it may be more informative to the general reader to present both. We would be happy to review this if the Reviewer suggests otherwise.

7) “The author name in the reference 11 is mis-spelled” –, apologies, this has now been corrected.
Other important changes (not suggested by the reviewers)

- We have added a paragraph in Discussion, to examine the question about whether the observed BMI change experience of the cohort was “natural” or whether it was in any way influenced by the screening process *per se*.

- Minor clerical (copying) errors in numerical values were detected, both in text and Tables, and those were corrected.

**Summary of response**

In conclusion, we have been able to address all the points raised by the reviewers, in a way that also adds considerable value to the original manuscript. We once more are thankful to the Reviewers for their very helpful feedback.

**Georgios Lyratzopoulos, 08.03.2005**

(On behalf of the group of authors)