Author's response to reviews

Title: UK adults’ implicit and explicit attitudes towards obesity: a cross-sectional study

Authors:

Stuart W Flint (S.Flint@shu.ac.uk)
Joanne Hudson (Joanne.Hudson@leedsbeckett.ac.uk)
David Lavallee (david.lavallee@stir.ac.uk)

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Author's response to reviews: see over
Reviewer's report
Title: UK adults' implicit and explicit attitudes towards obesity: a cross-sectional study
Version: 3
Date: 16 April 2015
Reviewer: Debbie Smith

Reviewer's report: As I stated in my previous review, I enjoyed reading this paper. I want to clarify that in my previous review I did not state that the manuscript was unsuitable for publication, however, I felt the manuscript was unsuitable for publication in the journal it was submitted to (BMC Public Health). The manuscript has been revised and in parts are much clearer. However, I do still have a few comments that I feel need to be addressed before it can be published in BMC Obesity.

We are glad that the reviewer found the article enjoyable to read and that with the changes below; the article would be suitable for publication in BMC Obesity.

Major compulsory revisions
- The introduction still lacks information about why this study is needed. The authors outline that there a lack of UK studies but the outline of why anti-fat attitudes need to be researched is limited. Reviewer 2's second primary concern is related to this point and has been addressed to some extent but more is needed.

We thank the reviewer for this comment and feel the rationale for the study is much stronger as a result. In line with the reviewer's comment, we have strengthened the need for the current study in the introduction, as well as the potential of informing health policy as suggested in the reviewer's comment below and from Reviewer 2's second comment.

- The study is the first large sample in the UK but the discussion still lacks detail that would make this article relevant to public health practitioners and obesity researchers. The suggestion that an antifat attitude intervention is needed is not supported with further discussion about what these interventions should look like.

We make suggestions in the conclusion that based on the success using educational interventions previously and our results that suggest UK adults strongly believe that obesity is controllable, that interventions to address controllability beliefs appear warranted. We feel that to discuss in detail what these interventions would look like is beyond the scope of this paper. Furthermore this would extend the paper, which the reviewer has suggested is fairly long already.

The findings need to be discussed in the context of public health policy. Reviewer 2's comments made me consider if more analysis is needed to examine some of the associations. This may give the authors more to discuss in relation to policy.

We thank the reviewer for this comment. As mentioned in response to the reviewer's comment above, we have included information in both the introduction and discussion in relation to health policy.

In addition, we have conducted further analysis to examine associations between explicit measures in line with Reviewer 1 and 2. We have included results from the correlations in the results section, a new table with the correlation results, and include information in the discussion included correlations between the explicit measures.

Minor essential revisions
The abstract lacks detail that is important to the reader (e.g., why is ant-fat attitude of importance? what are the implications of these findings?)
In line with the reviewer’s comment, we have added detail to the abstract.

-Sex or gender? I would suggest gender is more appropriate as you are not measuring things relating to biological make-up
In line with the reviewer’s comment, we have replaced sex with gender.

-There is a lot of discussion in research and clinical practice about using the term 'obese'. So more contribution to this is great. However, your sample was mostly normal weight people so can you make the claims that you make on page 13 about the use of the term 'obese' when treating people?
We agree with the reviewer, terminology relating to research and clinical practice continues to be warranted and is a contemporary topic of investigation. We have also amended the claim that the Reviewer has referred to on page 13.

Also, the sentence on line 9-11 should be reworded as you say that using the term 'obese' should be avoided but then you state 'obese people'
We understand the comment made by the reviewer; however, at present the accepted standpoint is to use the term obese people/persons. We are happy to change this if the editor feels this is necessary, but at this stage we are inclined to follow guiding principles. This is also in line with the guidance stated on page 13 in relation to ‘persons with diabetes’ rather than diabetic.

Discretionary revisions
-There are still a few typos (e.g., Running head)
We thank the reviewer for identifying this error. We have corrected this typo.

-The BAOP scores are interesting and could be discussed more (especially the underweight participants)
We feel that we have addressed this result in the discussion as much as we can in terms of providing a possible explanation for underweight participants reporting a belief that obesity is less controllable than normal weight participants. Our reason for this finding is speculative as suggested in the paper, and as suggested future research to examine why this occurred would certainly be of interest. Based on the current study data we do not feel that we can offer more detail about this finding, but that we have offered an educated rationale for why this may have occurred.

-Page 9, the MANOVA main effects should be listed in the same order as they are then discussed below (e.g., age is first in your list but sex is the first to be discussed)
In line with the reviewer’s comment, we have changed the order of the results, so that it matches the order they are discussed in the MANOVA.

-I agree with two of the other reviewers that this manuscript would suit a short report.
We thank the reviewer for their suggestion. Given the amount of data discussed in this paper, we feel that a shorter report would not be sufficient to discuss and justify the findings in detail. Whilst we have managed to reduce the length of the introduction, given that more detail in the introduction and discussion, and additional analysis and explanation was requested by Reviewer 1 and 2, a shorter report does not appear to be appropriate.
Level of interest: An article whose findings are important to those with closely related research interests
We agree with the reviewer, the findings of the study discussed in this paper are of high importance to individuals working in this area.

Quality of written English: Acceptable
We agree with the reviewer.
Statistical review: No, the manuscript does not need to be seen by a statistician.
We agree with the reviewer.

Declaration of competing interests: None
Reviewer's report
Title: UK adults’ implicit and explicit attitudes towards obesity: a cross-sectional study
Version: 3
Date: 21 April 2015
Reviewer: Fiona Kate K Barlow

Reviewer's report:
Major Compulsory Revisions

I think that this paper is much improved. Having said this, there are still a number of concerns I have about the manuscript in its current form. In particular, the theorizing on Page 4 regarding SJT seems to be out of the blue. Why would overweight and obese people be motivated to justify the system? I agree that they might be, but this should be brought out and argued if it is going to be used to justify the hypotheses. Further, many studies have investigated (either as a focal or secondary point) the impact of BMI on anti-fat attitudes (e.g., at the end of last year Alperin et al., 2014, Social Science and Medicine). From my reading of this, it is about a split, with most researchers finding that as weight increases, anti-fat biases decrease (at least explicit ones). In fact, the authors do make reference to past findings that show a low BMI is associated with higher anti-fat attitudes. As such, it seems strange to disregard this, but make a hypothesis for no differences based on a social psych theory that has not really been extended to weight (although the authors may want to give this a crack with a longer intro).

We would like to thank the reviewer for this comment, and acknowledge that this element was not as clear as it should have been. We expected lower BMI groups to have higher anti-fat attitudes that higher BMI groups in line with previous research. System justification Theory was included as we would still have expected overweight and obese participants to hold anti-fat attitudes due to internalisation of societal messages and the commonality of derogatory media portrayals. It was not expected that this would occur to the same extent as underweight or normal weight participants. Consequently, we have removed System Justification Theory from the paper in both the introduction and discussion as we agree with the reviewer that this is confusing and our standpoint was not clear. We have also removed hypothesis 3 relating to BMI.

I would also like to see better justification for the study. On page 3 the authors make the case that they are going to look at a broader sample than past work, and also that changes in population weight may have resulted in changes in anti-fat attitudes. They cannot test this, but what they can do is look to have a comprehensive study on weight bias and its relationship with demographic factors in the UK. I think that this point could be brought out more cleanly and unapologetically. They could also highlight that in doing so, the work might guide policy makers and practitioners about where it is particularly important to intervene.

We thank the reviewer for this comment. We feel that we have strengthened the justification for the study by including information in the introduction in relation to health policy. Also in line with the reviewer's suggestion, we have included information in the introduction that highlights to the reader that a comprehensive study examining specific demographic factors in relation to anti-fat attitudes is warranted and that such work would be beneficial to policy makers and practitioners.

Hypothesis 1 appears to actually be three hypotheses. The fact that the authors assess both explicit and implicit attitudes, as well as assumptions about the basis of weight is very cool.
Again, I think that this should be brought out in the introduction as a strength, and hypotheses formed about these variables. 

We thank the reviewer for bringing this to our attention. In line with the reviewer’s comment, we have amended the statements about the hypotheses in the introduction and in accepting/rejecting the hypotheses in the discussion. As mentioned above we have removed the hypothesis relating to System Justification Theory, thus there are now two hypotheses.

We agree with the reviewer about the importance of assessing both implicit and explicit attitudes, and as a result the importance of the findings for individuals working in this area.

Throughout the manuscript in general I felt that there was a bit of a lack of clarity. It was hard to keep track of what scale measured what (e.g., Q1 and Q2 etc...), as well as which results were significant and which were not (one of the most interesting findings I think is around the null effect of pretty much everything on implicit attitudes). I would very much like to see the authors go back and tidy up their method and results section with an eye to improving clarity, flow and comprehensiveness.

In line with the reviewer’s comment here and below, we have made changes to improve the language throughout the paper and believe that the manuscript is much clearer now. We agree with the reviewer that the implicit results are of interest, and have included greater discussion of these findings as suggested.

As an aside, the stats switch between 1 and 2 decimal places. It would be good to be consistent (ideally two decimal places throughout). To me, the problems with clarity extend into the writing.

We have included two decimal places for all of the results in line with the reviewer’s comment. In mentioning the mean values from previous research that act as a comparison, the articles only report one decimal place in some cases. We have made a lot of changes throughout the paper to the writing, so we feel this is much better now.

There are a lot of run on sentences, and sentences that do not really make sense (e.g., page 3, second sentence, should it be: "...IS a less obvious"?). I think that the whole paper should be very carefully edited, with the aim of improving both the language, and the clarity of the message/descriptions of past research/hypotheses etc...

We thank the reviewer for their comments. We have amended the sentence highlighted above and as mentioned above, made a lot of changes to improve the writing throughout the paper.

Minor Essential Revisions
It is possible that some of the comments that I listed above could be considered minor. I will leave this to the editor's discretion. In terms of benchmarking the scores against past means (page 8) I was just not sure. Perhaps to demonstrate anti-fat bias scores could be compared against the mid-point?

The section that compared past mean scores to the current study scores was removed prior to the last submission in line with the feedback from the first review. We have kept the sentence relating to the scores reported as anti-fat bias in previous research as we feel this is a better representation of anti-fat bias on using these scales, and is a more stringent criteria that is beyond the midpoint of the scales.
In addition, we do not get any clear evidence that anti-fat bias is increasing from this dataset. It could just be that the sample was different. I think it is sufficient to state throughout that anti-fat bias is evident, and in part explained by gender, BMI and age.

In line with the reviewers comment, we have amended comments suggesting that anti-fat bias might be increasing as we agree, this is beyond the current study. We followed the reviewer’s guidance in explaining that anti-fat bias is evident and explained in part by the demographic factors.

Discretionary Revisions
In the discussion the authors were speculating about the association between beliefs about the controlability of weight and anti-fat attitudes in the different groups. They could easily include a table of correlations, or one split by group, to look at this (as well as some simple moderated regression analyses) if they so chose. It would certainly add to the paper.

We have conducted further analysis to examine associations between explicit measures in line with Reviewer 1 and 2. We have included results from the correlations in the results section, a new table with the correlation results, and include information in the discussion included correlations between the explicit measures.

Overall, I think that the authors have done a really good job in revising the manuscript. I hope that my suggestions are helpful to them.

We thank the reviewer for acknowledging the extensive revisions to the paper, and for their helpful comments.

Level of interest: An article whose findings are important to those with closely related research interests
We agree with the reviewer, the findings of the study discussed in this paper are of high importance to individuals working in this area.

Quality of written English: Needs some language corrections before being published
In line with the other two reviewers’, we feel that the quality of the written English is acceptable for publication. Despite this, we have made a number of changes throughout the paper to improve areas that might have required clarity.

Statistical review: Yes, and I have assessed the statistics in my report.
It is assumed that the reviewer’s assessment is in line with the other two reviewers’ comments that the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests.
Reviewer's report
Title: UK adults' implicit and explicit attitudes towards obesity: a cross-sectional study
Version: 3
Date: 29 April 2015
Reviewer: Jamie Sims
Reviewer's report:
Discretionary Revisions Only The authors have amended several issues raised by the reviewers and I feel this has happened sufficiently well to warrant publication. I retain my reservations regarding the length of the report, but the authors are free to disagree with this.
We thank the reviewer for their suggestion. Whilst we have managed to reduce the length of the introduction, given the amount of data discussed in this paper, we feel that a shorter report would not be sufficient to discuss and justify the findings in detail. Additionally, given that more detail in the introduction and discussion, and additional analysis and explanation was requested by reviewer 1 and 2, a shorter report does not appear to be appropriate.

Level of interest: An article whose findings are important to those with closely related research interests
We agree with the reviewer, the findings of the study discussed in this paper are of high importance to individuals working in this area.

Quality of written English: Acceptable
We agree with the reviewer.

Statistical review: No, the manuscript does not need to be seen by a statistician.
We agree with the reviewer.

Declaration of competing interests: I declare that I have no competing interests