PEER REVIEW HISTORY

This paper was submitted to the BMJ but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open. However, both of the reviewers for the BMJ declined to give permission for their comments to be published so only the reviews conducted for BMJ Open are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	A pragmatic randomised controlled trial in primary care of the Camden Weight Loss (CAMWEL) programme
AUTHORS	K Nanchahal, T Power, E Holdsworth, M Hession, A Sorhaindo, U Griffiths, J Townsend, N Thorogood and D Haslam

VERSION 1 - REVIEW

REVIEWER	Shirley W. Flatt Senior Statistician Moores Cancer Center University of California, San Diego USA No competing interests.
REVIEW RETURNED	03/01/2012

GENERAL COMMENTS	CAMWEL Study The manuscript clearly documents the CAMWEL study design, and would be a helpful addition to the literature. Given the large amount of missing follow-up data, multiple imputation is preferable to using baseline substitution (assuming complete relapse) which would deflate the variance, and the authors have used MI appropriately. Table 2 data would contradict any assumption of "missing completely at random", and is helpful to readers. Some suggestions for the authors: 1. In addition to the odds ratios presented for losing 5% of initial weight, the proportion of each group losing 5% (by multiple imputation) would strengthen the abstract, results text, and Table 3. 2. Sample size calculation allowed for 40% missing data, surprisingly close to the observed rate of 43%. I do wonder, however, whether more than 3 attempts to contact someone for a follow-up visit might have reduced the amount of missing data and thus enhanced the robustness of the findings. 3. Sex is not listed in Table 2. If it is related to missingness, it should be added. 4. Table 3: The Multiple Imputation column shows adjusted differences in most cases, except for the lost 5% row, where odds ratios are presented instead. The Table would be more consistent if differences were presented throughout, with odds ratios mentioned in the text. Switching from one data presentation tactic to another mid-table is distracting. 5. p. 11 para 5 mentions a significant group difference in 4 weight-related outcome variables in an exploratory analysis excluding 27

	participants who underwent weight loss surgery or took weight loss drugs. It is not clear whether these results are from MI or not. This should be clarified, either here or in the Stat Methods section. 6. I suspect many readers would be content if Table 6 were omitted entirely. The topic list in the Appendix would likely suffice. The attempt to conclude (p. 11 para 3) that some session topics were more helpful than others is not well supported by the data. Most of the topics were helpful to fewer than 50% of respondents. In particular, Sessions 2, 3, 4, and 12 do not appear to differ from each other in their popularity, but Session 12 (less popular than Session 4) is listed among the top 3 topics whereas Session 4 is not. There is little discrimination in "helpfulness" of the sessions, and trying to rank these small differences seems foolish. The paragraph on the Intervention programme could be readily deleted from the manuscript. 7. Did randomized participants provide informed consent? That could explain why some members of the control group achieved 5% weight loss. The authors seem surprised (p. 12 para 5) that weight loss was observed in the control group, although obesity literature often reports considerable "drop in" in controls when subjects know that they are enrolled in a weight loss trial. 8. One year is not a long time for subjects with mean BMI 33.5 to move out of the obese stratum. A large majority of intervention subjects found the program helpful, and a majority were satisfied with a mean loss of 2.4 kg. This might suggest that the program will continue to accrue benefits to participants in the future, as they slowly implement guidelines learned in the intervention.

VERSION 1 – AUTHOR RESPONSE

Thank you for the very helpful comments. A response to each point raised by the reviewr is provided below.

1. We have added the proportions to the abstract, results and as a footnote to Table 3 to help interpret the results obtained from the logistic regression.

2. We feel that the response rate would not have improved with further attempts at trying to contact participants and it is unlikely that an ethics committee would have given us permission to do so.

3. This is already included in Table 2 as % (n) Female.

4. The result from the logistic regression is the 'definitive' one for this categorical outcome so we have left this in the table but have included percentages (as suggested in point 1) elsewhere in the paper.

5. We have clarified that MI was not used for the exploratory analyses in the Statistical Methods section.

6. We have removed Table 6, only including the key findings in the paragraph on the Intervention programme in the text on p11.

7. All participants provided informed consent as stated in the Methods section (p8). We have now included a sentence on triggers of behaviour change as part of the research process in the Discussion section.

8. We have included a sentence in the paragraph on Implications to emphasise this point.

VERSION 2 – REVIEW

REVIEWER	Shirley W. Flatt, senior statistician University of California, San Diego USA
REVIEW RETURNED	28/03/2012

The reviewer completed the checklist but made no further comments.