Author's response to reviews

Title: The effect of a brief social intervention on the examination results of UK medical students: a cluster randomised controlled trial

Authors:

Katherine Woolf (k.woolf@medsch.ucl.ac.uk)
I Chris McManus (i.mcmanus@ucl.ac.uk)
Deborah Gill (d.gill@medsch.ucl.ac.uk)
Jane E Dacre (j.dacre@medsch.ucl.ac.uk)

Version: 2 Date: 9 June 2009

Author's response to reviews: see over
9\(^{th}\) June 2009

Dear Robin,

Thank you for your email of 5\(^{th}\) June 2009. We would be happy to submit our article for publication in BMC Medical Education.

As requested, we have addressed the referees’ comments in the manuscript. Details of changes are given below (referees’ comments are in Arial, our responses are in Times New Roman). We would also like to take this opportunity to thank the referees for their careful re-reading our study.

We look forward to hearing from you.

Yours sincerely,

Katherine Woolf, I Chris McManus, Deborah Gill, Jane Dacre

Response to referees’ comments:

Referee 1

The authors have adequately addressed all of my concerns. A minor issue remaining to be addressed... as requested they have standardized the language used to refer to the intervention and control groups. This has been done throughout the manuscript, however the changes have not been made in Table 4 (column 4).

We thank the referee for noticing this. It has now been changed.

Referee 2

Woolf et al. state in the discussion section “However, detailed post hoc comparisons of the means of the groups showed that the decrease in the ethnic gap was not due to increased performance of the ethnic minority students as hypothesised, but instead was due to a decreased performance of the white students in the intervention condition.”.

In my point of view, this means that a better outcome of the EM-group is due to a worse performance of the W-group. Therefore the intervention seems to worsen the W-group and solely does lead to a relative positive effect for the EM-group, that in fact did not perform better post-intervention. As this probably should not be the aim of an educational intervention, this point should be discussed on more detailed.

The referee’s interpretation of our results is correct. We expected the intervention to narrow the gap in ethnic performance but we certainly did not expect, or desire, that the intervention should reduce the white students’ performance. We have altered the third paragraph of the discussion section (p.19) to underline this point. This section now reads:
“The finding that the intervention reduced white students’ performance was completely unexpected. The intervention was designed to build self-confidence and therefore should not have reduced performance in any group. These results also defy interpretation in terms of stereotype threat, particularly as white students generally tend to overperform in assessments (as shown in the Additional Information).”

Referee 3

I think the authors might mention that this evidence is only suggestive for a number of methodological reasons, but can be one source of evidence as more is gathered

We have altered a section in the discussion to further emphasise this point (ps 21 & 22):

“The present study raises serious questions for medical educators (as well as social psychologists). The study was in many ways a success: the intervention was small and the effects were significant. And yet the outcomes were unexpected and difficult to explain. If the effects we had found were the results of a pharmacological or surgical intervention in patients, then a host of questions would have to be answered. We believe they also have to be answered here, not least by further replications with more and better controls, which would enable a meta-analytic review of the effects of this type of intervention on medical students’ examination performance.”