Author's response to reviews

Title: Physical fitness and mental health impact of a sport-for-development intervention in a post-conflict setting: randomised controlled trial nested within an observational study of adolescents in Gulu, Uganda

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Author's response to reviews: see over
The Editor  
BMC Public Health  

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Dear editorial board of *BMC Public Health*,  

Thank you for forwarding me the reviewer comments for the paper originally titled: “Physical and mental health impact of a sport-for-development intervention in a post-conflict setting: randomised controlled trial nested within an observational study of adolescents in Gulu, Uganda”. We have considered the comments carefully and made amendments to the manuscript. We feel that these changes have significantly strengthened the paper and are grateful to the reviewers.  

As requested, please find attached a point-by-point response to each of the reviewer comments. We hope that this brings the manuscript up to a standard that is suitable for publication in BMC Public Health. If you have any further concerns or queries please contact me via email on justin.a.richards@gmail.com.  

Thank you again for your consideration.  

Yours sincerely,  

Justin Richards, on behalf of the authors
RESPONSE TO REVIEWER COMMENTS

Reviewer 1: Pedro Hallal

Major Compulsory Revision
1. The single major criticism I have is that the paper assumes fitness and body composition to be the measured of physical health, and concludes that the intervention had no effect on physical health. For me, physical health is much more than fitness and body composition. What about blood pressure? Glucose levels? Bone health? I do not think a new intervention should be done, but this issue needs to be discussed in more detail.

We have replaced all references to "physical health" with "physical fitness" (including in the manuscript title). We believe that this more accurately describes what was measured and directly addresses the reviewers concern.

Discretionary Revisions
1. Although the authors mention the problems with the sample size, I would like to see some power calculations in the discussion. Post-hoc power analysis assumes the observed effect to be the true one and its main purpose is to assist with planning future studies. All the necessary data to do this is reported in the manuscript and is therefore available for future calculations if needed. When considering the power of the current study it is more useful to consider the width of the confidence intervals for the differences between groups and whether these are clinically relevant. This data is reported in the study results and contributed to drawing the conclusion that there was an inadequate sample size.

Reviewer 2: Ferdinand Salonna

Minor Essential Revisions.
Article contains six large tables some of them unnecessarily detailed or redundant information.
1. Table 1 should be reduced or deleted. I suggest to omit info about attended school or division of residence and to keep only the narrative description of history of abduction.

We believe that this table presents important information about potential clustering of participants according location of residence, school and history of abduction. These are all factors that are adjusted for in the subsequent analyses. However, we recognise that there is a lot of information tabulated in the paper and we have therefore shifted this table to "supplementary material" of the manuscript. We have edited the text in the first paragraph of the results section accordingly.

Discretionary Revisions
1. Tables 2-6 include plenty of redundant information in narrative parts and tables. I suggest to avoid that. As BMC Public Health has no limits for number of tables or their size this is really discretionary.

We have retained tables 2 – 4 from the original manuscript (now renamed tables 1 – 3). The original tables 2 and 3 present critical data that challenges common anecdotal misconceptions about the fitness and nutritional profiles of adolescents in Africa. The original table 4 has been separated into two tables to improve readability. The original table 5 has been shifted to "supplementary material". The original table 6 has been shortened by removing redundant raw measurement data and has also been shifted to the "supplementary material" of the manuscript. We have made these changes despite the discretionary nature of these suggested revisions and thank the reviewer for assisting us streamline the delivery of the salient issues addressed in the manuscript.
Major Revisions

1. The major change recommended is to indicate explicitly the lack of effect (either positive or negative) on girls in the abstract, opening paragraph of the discussion, and conclusion. These three sections have clear statements regarding boys but not girls.

   The lack of effect in the girls sample has been captured in the final sentence of the Results section of the Abstract (see underlined):
   "There was no significant effect on the girls for any of the outcomes and no other serious adverse effects reported."

   The effect on the girls is also now explicitly described in the second, third and fourth sentences of the first paragraph of the discussion (see underlined):
   "This occurred despite mental health improvements in the broader community for both genders that were particularly pronounced for the boys in the wait-listed group. There also appeared to be a community-wide increase in cardiorespiratory fitness during this period for both boys and girls. However, the GMKL intervention had no additional effect on the physical fitness of the participants when compared to the wait-listed and non-registered adolescents for both genders."

   The absence of effect in the girls group has also been made more explicit in the conclusions section (see underlined):
   "Despite community-wide improvements in cardiorespiratory fitness, only the boys who participated in the competitive sport-for-development programme experienced negative mental health outcomes. It is possible that the concurrent improvement in fitness and mental health in all of the other study groups for both genders may have resulted from increased local capacity and resources for engaging in physical activity."

2. Is the magnitude of the negative effect on the intervention boys’ mental health substantial? For readers unfamiliar with the assessment tools, some indication of what these values represent in regards to mood state and whether these are small or large changes would be valuable.

   This is a study limitation that has been identified in the second last sentence of the "strengths and limitations" section of the discussion:
   "...This weakened the conclusions that could be drawn about programme effects, which were further limited by the absence of clinically relevant criterion for the metrics utilised"

3. Given the proposed explanation for the negative impact on mental health in boys is that the intervention exposed the boys to new stressors (value placed on winning/performing well) was there an observation that the girls’ competition lacked this focus on winning?

   Sentences 3-5 of paragraph 3 in the "interpretation and implications" section of the discussion addresses this issue:
   "Although the league structure and coaching workshop focussed on community-building initiatives, ethnographic field observations confirm that the majority of the coaches and participants emphasised football performance. These expectations were particularly pronounced in the boys league and may explain the negligible differences in the mental health outcomes observed between the girls groups. A previous evaluation of a sport-for-development intervention in South Africa also indicated that boys focused on winning and associated self-worth with football success."

4. Is it possible in boys that there was substantial expectation associated with participation in the program that was not met in the intervention boys but was still present in the control boys while they were waiting for the program to start? In comparison, did girls have lower expectations of what the program would deliver or were they more oriented to the community-building aspects of the program? Was the marketing of the programs the same to boys and girls? Is there any way to tease any of these aspects out of the study?

   The following text has been included in the third paragraph of the "interpretation and implications" section of the discussion:
Conversely, the improvement in the mental health of the wait-listed boys beyond that observed in the non-registered group may be attributable to anticipation of participating in the next season of the GMKL. These expectations were particularly pronounced in the boys league and may explain the negligible differences in the mental health outcomes observed between the girls groups.

As subsequently stated in this paragraph of the manuscript, we did not collect adequate "process indicators" of program delivery to be able to confidently tease out any further explanation relating to the issues identified by the reviewer (e.g. differential program marketing according to gender).

5. For consideration – would you expect one training session and one game per week to be sufficient training to improve aerobic fitness (MFT)? Should this be considered a limitation of the intervention (as opposed to the study design)?

The following text has been included in the fourth paragraph of the "interpretation and implications" section of the discussion:

"... existing guidelines indicate that deconditioned individuals may benefit from introducing training 1-3 days per week.[33-35]"

Minor revisions
1. Some redundancy in background and study setting/participants (emerging from war aspect)

We believe that this content provides important contextual information that sets this paper apart from previous research. The post-conflict nature of the study setting has important implications that are drawn upon in the discussion. Therefore, we have made some minor edits to this text, but retained the bulk of the content.

2. Tables 4 and 6 – indicators of statistical effects in top section of tables – the symbols chosen are difficult to discern; I would recommend changing these to more easily recognisable symbols OR letters

These tables have been altered according to the comments of Reviewer 2 and these symbols have been changed to improve readability. Thank you for this feedback, which we believe improves the delivery of the salient issues addressed in the manuscript.

Discretionary revisions
1. Baseline physical performances – higher for SBJ but lower for MFT compared to global norms. Are there any local PA preferences or body structure/composition characteristics that might be reflected in these differences?

The following text has been included at the start of the fourth paragraph of the "interpretation and implications" section of the discussion:

"For the physical fitness outcomes, we hypothesise that a high volume of low-intensity physical activity in the form of active transport (i.e. slow walking to/from school) may have contributed to the large proportion of subjects in the healthy range for BFA at baseline. This may have also contributed to the relatively high performance in the SBJ of the Gulu sample when compared to global norms (i.e. lower body mass to move when jumping). Conversely, the relatively poor performance in the MFT at baseline and the low incidence of malnutrition were consistent with an urbanised setting.[1,20] This suggests low levels of aerobically challenging physical activity and is consistent with the 2003 WHO Global School-Based Student Health Survey that indicated Ugandan adolescents in urban areas engage in low levels of moderate- and high-intensity physical activity.[32]"