Author’s response to reviews

Title: The association between social capital and mental health and behavioural problems in children and adolescents: An integrative review

Authors:

Kerri E McPherson (kerri.mcpherson@gcu.ac.uk)
Susan Kerr (s.m.kerr@gcu.ac.uk)
Elizabeth McGee (elizabeth.mcgee@gcu.ac.uk)
Antony Morgan (antony.morgan@gcu.ac.uk)
Francine M Cheater (f.cheater@uea.ac.uk)
Jennifer McLean (Jennifer.McLean2@glasgow.gov.uk)
James Egan (James.Egan2@glasgow.gov.uk)

Version: 3
Date: 25 February 2014

Author’s response to reviews: see over
Dear Dr Murray

Re MS: 1598706184102937 - The association between social capital and mental health and behavioural problems in children and adolescents: An integrative review

On behalf of my co-authors, we are grateful to the reviewers and editorial team for taking the time to suggest ways in which our manuscript could be enhanced and we outline our response to each of the suggested revisions here.

Reviewer 1, Femi Oyebode, suggested one discretionary revision:

“The authors may wish to consider in the limitations section saying what the drawbacks of a narrative review are and how their careful methodological stance has minimised the identified problems.”

This review was a systematic review with a narrative synthesis, which is different to a traditional narrative review. However, by noting this the reviewer has highlighted that we have not been as clear as we might so we have strengthened this in the: 1) last sentence of the background section of the abstract; 2) the first sentence of the final paragraph of the background section on page 8; the first sentence of the first paragraph in the method section (page 9); and, the first sentence of the first paragraph in the discussion section (page 29). We have also updated the first paragraph of the limitations section on page 34 and the title.

Reviewer 2, Margaret Kern, raised a number of issues.

Major compulsory revisions:

1. The reviewer requested that we clarify the purpose of the larger piece of work from which this current review was part. We have sought to do this and made explicit our rationale for analysing and reporting on mental health and behavioural problem outcomes independently from other outcomes included in the larger review (see page 9). That is, we grouped mental health and behavioural problem outcomes together and analysed and synthesised them separately from other health and wellbeing outcomes (e.g. health risk behaviours) because we believe that this offers the greatest conceptual and practical value. Indeed, we explicitly

---

Dr Kerri E McPherson
Reader/ Deputy Director

Dr Alice Murray
Executive Editor
BMC Psychology

Parenting & Family Support
Research Programme

kerri.mcpherson@gcu.ac.uk
+44 (0)141 331 8174
criticise previous work for taking this broader approach (e.g. Ferguson 2006). Conflating outcomes across conceptually different domains (e.g. mental health and health risk behaviours), in our view, results in a dilution of the key issues and limits recommendations that can be made about the practical implications and future research directions. The larger piece of work was referenced in the original manuscript as McPherson et al (2013a) and we have left it as such.

2. Second, the reviewer makes the point that having a large number of predictors and outcomes makes it difficult to extract the ‘clear message’ from the review. Aware of this from the outset, we sought to minimise the potential for the review to be ‘too big’ by thematically grouping the outcomes into those that assess self-esteem and self-worth, those that assess elements of internalising behaviours and cognitions, and those that assess elements of externalising behaviours and cognitions. By providing a separate analysis and synthesis for each of these thematic areas, as well as a synthesis across all of the outcomes, we offer the reader the opportunity to delve into the evidence at the micro, thematic level and the macro level, which looks across the mental health/behavioural problem outcomes. We have provided, throughout, short summaries of the key findings at both the micro and macro level. We also feel in addressing the reviewer’s comments that follow, in terms of the definitions and conceptual underpinnings of the review, we have strengthened the main messages that emerge from the review.

Minor Essential Revisions:

1. The reviewer has asked for a more explicit definition of social capital. We have altered and added to the appropriate paragraph on page 6 to strengthen our pluralistic approach to defining social capital. While strengthening this, we have sought to avoid delving too far into the nuanced debate that exists about how social capital should be defined and measured. Instead we provided interested readers with key pointers to this literature. We have also added a reference point on page 9 to direct readers to the section where family and community social capital are fully articulated. We feel referring the reader on in the paper is better than inserting the key elements earlier; this would, in our view, result in unnecessary repetition. We feel this also addresses the reviewer’s additional comment “P 33 notes that future studies need to operationalize social capital…”.

2. The reviewer has asked for an explicit definition of mental health. We have further emphasised our philosophical approach to mental health by including the WHO definition in the opening paragraph. This, we feel, further strengthens the discussion on pages 4 and 5 where we note the difficulties in defining mental health and particularly in the context of children and young people. We have also added an additional paragraph on page 36 (Moreover…) that highlights both our cognisance of the more positive approach that has emerged in the policy literature and also the constraints in terms of the research evidence available to us.

3. The reviewer asks if the search presented in Appendix 1 was applied across all of the databases. As noted in the manuscript (page 13 at the end of the first paragraph in the Search terms and delimiters section), this search forms the basis of the search in each database; however, we modified the terms as appropriate to reflect indexing differences across the various databases and we note this in the manuscript. We have added a footnote to additional file 1 to reiterate this. As we have also included a note on page 10 that a protocol is available (see below), this should prompt those wishing to rerun our search to contact us for the detailed search for each database.

4. The reviewer asks for inter-reviewer reliability data. In all stages of the review (i.e. screening and quality assessment) we sought consensus rather than, for example, the aggregating of reviewer score. This consensus was, as is noted in the manuscript, achieved through discussion between reviewers following independent screening.

5. The reviewer suggests that the implications of the findings be integrated into the discussion. We have sought to make more explicit the implications of this work by providing illustrations of the ways in which the findings might be translated into impact. For example, we state on page 30 that in demonstrating that the parent-child relationship element of FSC is associated
with more positive outcomes in young people, the findings of this review support the need for access to interventions designed to foster effective family relationships.

6. The reviewer’s final comment under ‘Minor Essential Revision’ suggests that the paper isn’t about health and wellbeing. We dispute this and feel that our additional strengthening of our philosophical stance in relation to mental health and wellbeing supports our position. We have been guided by the WHO definition in which mental health is explicitly acknowledged as being a “state of wellbeing”. We develop this with further articulation of the issues associated with defining mental health in the context of children and young people and we refer the reader to key documents in this respect; including the work conducted by Shucksmith and colleagues, which illuminates for us the ways in which children and young people conceptualise mental health. We are acutely aware of the debates associated with defining mental health and wellbeing but we do not feel we can, nor would it be appropriate to, fully address these here. Suffice to say, we have tried to provide the reader with our own perspective and thus the context to the types of outcomes we included in this review.

Discretionary Revisions (author’s comments identified by bullet points):

Abstract

- The abstract notes that the findings illuminate gaps in knowledge, and yet this is not explicitly discussed in the discussion section. Throughout the discussion, it would be helpful to add the implications of findings for researchers, the youth themselves, and policy.

As above, we have made the implications of the findings more explicit by providing additional illustrative examples of potential impact.

- Conclusion is rather vague, noting social capital can influence mental health – in what way?

We have updated the result section to emphasise that our findings demonstrate an association between FSC/CSC and mental health/behavioural problems. The word limit in the abstract means that it is not possible, nor necessarily appropriate, to provide more detail than our general summative statement in the abstract, which emphasises that we have found social capital to be an important, and neglected, factor in the context of mental health and behavioural problems.

- Keywords include wellbeing, but this paper is not about wellbeing (it’s about negative things, except self-esteem, which is not the same as well-being).

As discussed in the sixth point of the ‘Minor Essential Revisions’ section. We consider this to be a philosophical difference between the reviewer’s and our conceptualisation of mental health. We have, where possible, sought to highlight the positive impact that social capital can have on children and young people’s mental health/behavioural problem outcomes. However, as a review we were constrained by the evidence available to us and we do recognise that much of the focus is on negative outcomes. This is a reflection of the existing literature which has tended to take a ‘deficit’ rather than asset based approach.

Background

- The background starts off talking about well-being, but the paper is about mental ill-health, and thus the beginning is rather deceptive about the focus and purpose of the paper as a whole.

As above, we have addressed this by making explicit the definition of mental health that underpins this work.

- 1st paragraph – almost all citations come from WHO – there is plenty of other literature on this that could be cited.

We are aware of other literature but in seeking to ensure that this review had international relevance we drew on the work of the WHO. Other literature that contains relevant prevalence and incidence statistics tend to be localised. We believe that the WHO has a credibility and significance that enhances the inclusivity of our review from the outset.
P 8 criticized Ferguson’s review for having a very broad definition of wellbeing, making it hard to enable conclusions to be drawn, and yet this paper does the same thing, with broad definitions of both social capital and mental health/behavior.

While we offer pluralistic definitions of social capital, mental health and problem behaviours we have provided an explicit justification for these in a way that previous literature has not. Moreover, we sought to disentangle the outcomes included in this review and grouped, analysed and synthesised the results thematically rather combining as one single body of evidence. This, we believe, is a significant strength of our analytical process and is in contrast to Ferguson’s review.

Methods

- P. 9 – what mean by an integrative approach? Define, don’t simply give a citation, as integrative can mean several different things.

We have clarified this on page 10.

- Why not include unpublished studies? There can be a major file drawer problem. It’s ok to do this, but need justification for this decision.

It is not uncommon in systematic reviews to limit the scope of the search to published work only. The decision was primarily a pragmatic one related to the time, cost and difficulties associated with identification of unpublished work.

- P. 10 notes that a pluralistic approach was used for social capital, but draws on Ferguson’s findings as a framework. What is meant by a pluralistic approach, and what Ferguson’s findings were needs to be summarized.

In addressing the first point in the ‘Minor Essential Revisions’ section we have now addressed this on page 6.

- In terms of community social capital, peer support seems quite different than aspects such as civic engagement and attendance at religious services. Community thus needs to be defined (going back to the problem of not defining the main concepts being used).

As above, this issue has been strengthened (page 6).

- The inclusion for outcomes is rather broad. What was excluded?

This relates to our definition of mental health and behavioural problems, which have been strengthened in the Introduction. Our inclusion criteria were developed, as alluded to in the manuscript, from previous research in the field and, in particular, the work of Shucksmith. We excluded all group- and population-level outcomes.

- The entire search process does appear thorough, though it is somewhat hard to follow through the method section

We have applied the same approach as in our BMC Public Health article, which reported on a separate element of the results (i.e. health risk behaviours), and we believe that we have included a description of the relevant stages in sufficient detail as to make this work replicable.

- It appears numbers are held off until the results, but I’d like to see this upfront in the methods section – how many articles were identified through the search, how many excluded where. For example, the data analysis notes that numbers were too low for meta-analysis, and yet from the search strategy, there could seemingly be thousands of studies. (This may just be a stylist preference, and leaving in the result section is fine).

As the convention for systematic review reporting is to include the outcome of the search process in the results section and not the methods section, we have not altered this.

Results

- Does include the recommended figure with the reporting of search and screening, which is good.
No response required.

- It's not clear until the figure that this sub study focuses on risk behavior articles.
- This was an error on our part and the figure has now been updated.
- Table 1 is really interesting and well-done. Instead of ordering alphabetically within each outcome, consider grouping by quality (then alphabetical).
- We have retained the order to make it easier for the reader to locate the various studies.
- Table 1 needs to have a note defining FSC, CSC, and what the beta symbol means.
- The beta symbols appear to be a conversion issue. In the Word version of the manuscript these appear as crosses (✗) and ticks (✓). This will need to be address during the production process.
  - The second Table 1 should probably be Table 2?
- This was an error and has now been corrected.
- Consider flipping Table 2 (so the categories are horizontal)
- We would like to discuss the layout options with the publication team to ensure that the best format is applied in the production process.
  - Also, consider including a few more associations: age group, also number of studies with self versus other reporting the data, and quality of study.
- These additional associations did not form part of the analytical process.
  - In some cases, there is not enough studies to do a full meta-analysis, but it seems like some sort of effect sizes could be commuted. That being said, if effect sizes can’t be clearly determined, then the literary review including more studies is good.
- As noted, the majority of studies were cross-sectional studies so effect sizes are not available.
  - For range of sample size, it would make sense to go from smallest to largest (as range is typically minimum to maximum)
- We have included, where possible, the actual same size rather than a range.
  - I recognize that Table 1 already takes up a lot of space, but as for the numbers reported on page 17, it might be helpful to summarize the demographics across the studies in a table. It’s hard to keep track of what was categorized, and how this really breaks down across the 55 studies.
- We feel that adding an additional table would overload the review. The information is contained in Table 1 and summarised in the narrative. This is the same approach as we have taken previously and it has been received well elsewhere.
  - Last paragraph on P. 18, in describing the results, the writing falls apart a bit, with sentences not making as much sense.
- We have edited and amended as appropriate.
  - P. 19 – protestant should be capitalized.
- This has been amended on page 20 and in Table 1.
  - It still seems that when there are so many studies within the broader domains, some degree of quantitative combination can be done.
- As noted, meta-analysis was not possible because of heterogeneity in the outcomes and the majority of studies being cross-sectional surveys so we have applied another systematic analytical process.
• I particularly like the summaries at the end of each section in the results – this is helpful for following the main findings.

No response required.

Discussion

• Mental health is essential measured as self-esteem. Really, the review is focused on self-esteem and internalizing and externalizing behaviors. Call it what it is.

As discussed above, this is a conceptual issue and we have sought to provide some clarity for the approach we have taken.

• P. 30 notes that half the studies failed to find a significant relationship, and yet so many positive results are reported throughout the study. This makes me concern then what the prior results are saying. With all qualitative review, it’s hard to determine what are actual findings and what are simply the authors’ readings of things.

This statement relates to one element of FSC, family structure, the other positive associations in the review relate to other elements of social capital. As noted on page 16, many of the studies reported on multiple associations between the elements of social capital and the various outcomes which results in more relationships being reported than studies. The evidence reported in this review is the individual study findings and not the authors’ interpretation of the findings.

• In the discussion, it would be helpful to start again with what FSC and CSC is, and then fit the findings into these models. Also, several frameworks were mentioned in the intro and method, but then it’s not clear how they are used or map onto everything. Bring these pieces together.

The clarification of the definition of social capital in the introduction should enhance this and we have made more explicit reference to family and community social capital in the summary of results to aid the reader. We have not repeated the definition of FSC and CSC to avoid unnecessary repetition.

• The discussion reports some results as positive, others as non-significant. Are the non-significant relationships the function of lack of association, or not enough studies? What are the implications of this (perhaps these are areas researchers need to focus on more, or should they ignore it)?

The non-significant relationships discussed refer to a lack of identified association in the analysis conducted in the primary research i.e. p values > 0.05 (see the description of the analytical process on page 15).

• P 32 notes that religiosity was not associated with outcomes, but a lot of other research (perhaps all in adults?) has found very different results. It would be helpful to comment on the differences – perhaps these are different outcomes or a different age group (or again, not enough studies have looked at it).

We do not feel we have the information to do anything other than speculate about these findings. We have speculated on page 32 that the pattern of results may be related to measurement but we have no further evidence with which to work. This would clearly be an important issue for future research and we have now included an explicit comment about this (page 33).

• P 34 notes that the large body of evidence demonstrates conclusive evidence, and yet as a literary review alone, we as readers are forced to believe the authors’ claim, without strong evidence. Soften your language to note that you have supported this, and future work should quantitatively support this.

The large body of evidence means that we have been able to demonstrate that FSC and CSC are both associated with mental health/behavioural problems. However, as we acknowledge in the manuscript, we cannot shed more light on the direction of causation in these associations but we
believe there is enough evidence to support the development of future research that might illuminate for us how social capital and mental health/behavioural outcomes interact.

- One other limitation to discuss is what are the implications that most research is cross-sectional? How does sample size affect things (one study had 28 people, all the way to a very large study)?

We highlight the difficulties associated with cross-sectional research on page 36. Many of the studies had large sample sizes and combining the evidence across the studies means that the conclusions that we drew from the evidence are strengthened. It is important to note that the study with 29 participants was qualitative.

The editor has also asked that we:

"Please ensure that you take the time to check through the PRISMA checklist on reporting systematic reviews, as previously requested. We thank you for making the amendments so far, however we still require you to change a few points, as directed by the checklist found here: [http://www.prisma-statement.org/2.1.2%20PRISMA%202009%20Checklist.pdf](http://www.prisma-statement.org/2.1.2%20PRISMA%202009%20Checklist.pdf) (e.g. Point 1. Identify the report as a systematic review in the title)."

The manuscript has been reviewed with reference to the PRISMA 2009 Checklist and we have: updated the title to include the term “systematic” (Point 1); updated the abstract; added information about the protocol on page 9 (Point 4); and, added an acknowledgements section to allow us to further articulate the source of funding for this work (Point 27).

The manuscript was prepared with reference to the author instructions at this link [http://www.biomedcentral.com/bmcpsychol/authors/instructions/researcharticle#formatting-references](http://www.biomedcentral.com/bmcpsychol/authors/instructions/researcharticle#formatting-references). This appears to be different to the link in the email and, in particular, the referencing styles are different. We have continued to adhere to the journal-specific guidance but can make changes if there has been a change in styles not reflected in the journal guidance.

We hope that you are happy with our response to the suggested corrections and look forward to hearing from you in due course.

Yours sincerely,

Dr Kerri E. McPherson