Author’s response to reviews

Title: Social gradients in child and adolescent antisocial behaviour: A systematic review protocol.

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Author’s response to reviews: see over
Dear Dr Moher,

The authors would like to thank you and Professor Shlonsky for your thorough and thoughtful review of our manuscript: ‘Social gradients in child and adolescent antisocial behaviour: A systematic review protocol’ (MS ID: 213698446706571). We have carefully taken these comments into consideration when revising our original draft. During the revision process, we have also edited the original manuscript to improve readability and completed the contribution section. We are pleased to submit a revised and more comprehensive manuscript and hope that we have addressed all the points raised. Substantive as well as minor changes and additions to the text have been highlighted in red in our revised text. Here we summarise our response to the reviewer’s comments.

1.  **Lit review (discretionary):**
   *Is there room in the lit review for a very short discussion of the social construction of antisocial behaviour (i.e., it is only anti when social is defined as the norm). I don’t think this needs to be very long, but it is worth noting that the DSM criteria for antisocial have been roundly blasted by social scientists for quite some time, and this is particularly the case for linking oppositional defiant disorder with conduct disorder. For a particularly compelling argument, see Kutchins and Kirk ‘Making Us Crazy’.*

   We have added some discussion of the advantages and disadvantages of the DSM-IV approach to measuring antisocial behaviour and also considered some alternative approaches to measurement that will be included in the review (p. 6-7).

2.  **Methods intro (no revisions):**
   *I like that the authors are going after subtypes and even getting at the heritability factor. I suspect that they are correct in assuming there will be variability – that some behaviours are more influenced by the gradient than others.*

   We appreciate reviewer’s comment; no revisions have been made.

3.  **Search (major compulsory revisions):**
   *Searches should include disciplinary databases from these additional disciplines (at least – or a clear rationale as to why this is not important to do): Economics (e.g., PAIS; EconLit), Sociology (Sociological abstracts; Social Sciences Abstracts), Crime and Justice, Political Science.*

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Many thanks for these very helpful suggestions. We have extended our search strategy to include the following databases to cover all disciplines mentioned by the reviewer (sociology, criminology, politics, and economics). We have included Applied Social Sciences Index and Abstracts (ASSIA), Sociological Abstracts, Worldwide Political Science Abstracts, National Criminal Justice Reference Service and EconLit (p. 10). In the current version of our manuscript we have not included further databases, such as the Public Affairs Information Service (PAIS) or additional databases in the area of criminology, economics or political science. We do not believe that including these databases would substantially change our result as Web of Knowledge already includes the Social Sciences Index, and Scopus covers the broad areas of social sciences, psychology, economics and criminology. However, we are prepared to revise our manuscript further if you felt this was important.

4. Search terms (minor essential revisions):
   - No need for ‘young pe*’ if you include ‘young*’, but you might find ‘young’ gives too many hits. If so, I would include ‘young ad*’ in addition to ‘young pe*’
   - include Youth* (to cover youths, youthful, youth-
   - include Oppositional defiant (rather than just oppositional-defiant)
   - include Poor

All the suggestions seem very useful and will surely improve our search strategy; therefore the first two suggestions have been incorporated into our protocol (see revised Figure 1). In the current draft we have not added ‘poor’ because our preliminary pilot searches showed that including ‘poor’ results in an unmanageable number of hits that were largely unrelated to the review topic, such as ‘poor relationship’, ‘poor correlation’. In considering the reviewer’s suggestions we have revised our search terms to include *pover* as a search term which results in more relevant hits. Moreover, we believe that other search terms incorporated (i.e., ‘poverty’, ‘disadvantage*’ and ‘depriv*’) will successfully substitute for not using ‘poor’. We have also decided to add ‘oppositional*’ to our search terms instead of the suggested ‘oppositional defiant’ as our pilot searches showed that this search term will give us all the hits that would have been found using ‘oppositional defiant’, but also in addition to this, will find studies investigating oppositional behaviour without mentioning ‘defiant’. We will be happy to consider it further if you think that ‘poor’ as a search term is essential in our review.

5. Inclusion criteria (minor essential revisions):
   - When referring to social position, do you mean parent’s social position? How will you define this? What if they are being cared for by someone else? When you say social position, do you mean social position at the time of the survey / study or do you mean exposure to poverty (for example) at any age?
   - Why limit to English only when the databases will search terms in other languages? Can’t you sort it out at the end of the day? The likelihood that studies will be found, for example, in the Nordic countries is fairly high (though these may also be in English).

When referring to social position, we meant carer’s social position or socioeconomic status which has now been clarified in the manuscript (p. 11). We expect that the majority of studies that we find will measure current social position, though studies considering history of poverty will also be accepted in the review. On the remaining point, our pilot searches found few studies not published in English. As mentioned by the reviewer, studies conducted in Nordic countries tend to published in English. However, we will be happy to revise this inclusion criterion if you feel it is important.

6. Screening (Major compulsory revisions):
   *It is being done this way due to resource constraints? Why not follow Cochrane procedures for screening at the various levels. In particular, I’m concerned that there is no mention of training to reliability before the effort gets under way. The 10% second author check is merely a check and will only tell you if things have gone wrong. Better to spend time at the front end, training to reliability, and doing periodic spot checks as you go to make sure reliable decisions are maintained.*

These are very thoughtful suggestions. As in our original protocol, we propose that the 1st stage screening (on the basis of titles and abstracts) will be performed by the first author only. In our opinion, this should be
We have now emphasised that all the included studies will be assessed using a pre-piloted and standardised quality appraisal form which we have submitted as Additional file 2. Quality criteria include aspects of methodological and conceptual quality and will allow reviewers to independently assess each study in regard to its methodology and relevance to the review. We have decided to perform the quality appraisal using a scale because, to our knowledge, risk of bias tables are only available for clinical trials. Quality appraisal checklists will be completed by the two reviewers independently and the inter-rater reliability will be calculated to ensure the consistency of quality assessment. We will also observe the requirement to keep a list of all excluded studies during the 2nd stage screening, providing the primary reason for exclusion according to Cochrane guidelines.

7b. How will each of the constructs be measured in this review? For instance, how to we know that someone is anti-social? How do we know when to consider people poor? The term ‘gradient’ implies more than a dichotomous approach (i.e., poor / not poor). How will this be operationalized? Will the psychometric properties of each of the constructs be considered? What if there are none (i.e., they have not been tested as will likely be the case for many)? How are you defining ‘general population’? Does the survey have to be representative? Of what? A province? State? Country? City? Representative of what? Child population? Can it be stratified by ethnic group, for instance, to ensure representation? How will you deal with population weights in the review?

A wide range of antisocial behaviour measures will be included as a result of the review of cross-disciplinary research. Included studies may be grouped according to the scope of a study, quality score or the type of behaviour studied, for example diagnoses of physically aggressive and non-aggressive, delinquency, and oppositional behaviour. This has now been clarified in the manuscript (p. 10-11). Similarly, a wide range of socioeconomic indicators, both absolute and relative, will be accepted. These may include income, educational/occupational or social position measures. Although studies measuring the social
gradient (i.e., defining more than two categories) are more likely to provide the most useful information, studies using dichotomous measures of socioeconomic status will also be included in the review.

In regard to the suggestion of considering psychometric properties, we agree that this is valuable and also that studies may not always present this information. Although we expect substantial variability in measures across the studies, the majority of them are likely to use reliable and well established measures. If psychometric properties are not presented, reviewers will seek this information in other papers using the same measures. We have revised the quality appraisal form to include assessment of the quality of the measures used.

We define general population as a non-clinical population which has not been recruited due to specific characteristics or clinical conditions. This is often a representative sample, though its representativeness, including the application of sampling weighting and stratification, will be assessed in the quality appraisal. However, if meta-analysis is performed, then N-weighted average coefficients will be used to take into account population size.

7c. I’m also a bit concerned here that the authors seem to be leaning toward a narrative synthesis before the data are in. The studies should be meta-analyzed if at all possible. Heterogeneity can be dealt with if sufficient studies are present (which I think there are! Just the survey data alone will be enormous!). Given this very real possibility, how will the data be analyzed? Specifically, what techniques will be used (e.g., fixed or random effects?)? What meta-analysis program? How will the final slate of studies be assessed for their completeness (e.g., funnel plots, etc)?

We have provided more detailed information on our meta-analysis plans (p. 13), though any final methodological decisions will be made once the data has been acquired. If possible, meta-analysis will be conducted on all studies or on a group of them that are sufficiently homogenous in regard to the construct measured. For example, studies may be grouped on the basis of type of antisocial behaviour studied. Such decisions as to whether studies may be grouped together will be made upon conducting homogeneity tests. Meta-analysis would be conducted using Schwarzer’s META program and appropriate tables and funnel plots presented. The decision regarding applying fixed- or random-effects models will be made at the later stage of the review process.

I would strongly suggest, wherever possible, following the Cochrane guidelines for screening studies. Although Cochrane is largely focused on effectiveness studies, the public health group has made substantial in-roads into other types of questions.

While we agree with this suggestion in principle, not all the Cochrane guidelines seem applicable to our proposed review. For example, although quality scales are not supported by the Cochrane Collaboration, this seems to be the best tool to reliably and consistently assess quality and relevance of studies in our review. However, we believe we have now made significant improvements to raise the standard of the review. Where possible, our revisions have been designed to align us more closely with the Cochrane guidelines, for example with the focus on piloting and training to reliability work before the processes get under way (p. 11-12).

We have also clarified and completed the information on the processes of quality appraisal and synthesis. This should now enable readers to follow the process of the review at every stage.

We are grateful to the editors and the reviewer for their careful reading and thoughtful comments on our protocol and we hope that the revised manuscript is now suitable for publication.

Yours sincerely,

Patrycja J. Piotrowska