Author's response to reviews

Title: The parent-child relationship and adolescent alcohol use: a systematic review of longitudinal studies

Authors:

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Author's response to reviews: see over
Dear editor,

Thank you for the opportunity to revise and resubmit our manuscript “The parent-child relationship and adolescent alcohol use: a systematic review of longitudinal studies” to BMC Public Health. We are very thankful for your valuable suggestions and those provided by the reviewers. We have pasted the suggestions and comments into the text below. To this, we have added our responses, preceded by [RESPONSE], and the changes (changes are underlined) that we made in the manuscript, followed by an indication of the pages and paragraphs where these changes occur in the revised manuscript. In the revised manuscript these changes have been underlined too. We hope that this revised version can make a contribution to the contents of BMC Public Health.

Sincerely,

Also on behalf of the other authors
Leenke Visser, MSc
Response to the Editor's and Reviewers' remarks

Editor:

1. Please include some context information in the Background Section of your Abstract.

[RESPONSE]
We added the following sentence to the Background Section of the Abstract:
“Alcohol use among adolescents has become a major public health problem in the past decade and has large short- and long-term consequences on their health.”
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2. For reporting systematic reviews, please adhere to PRISMA guidelines. PRISMA ? Systematic Reviews
http://www.prisma-statement.org/

We checked the 27 items listed in the PRISMA guidelines. We added information about the data sources to the Method section of the abstract by adding the words:

“Medline, PsycINFO, and EMBASE”

Regarding all other items, we already adhered to the PRISMA guidelines.

We added the following sentence to the Introduction (page 4, final sentence):

“We adhered to the PRISMA guidelines for reporting systematic reviews.”
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Referee 1

Reviewer's report
Title: The parent-child relationship and adolescent alcohol use: a systematic review of longitudinal studies

Version: 1 Date: 2 August 2012

Reviewer: Ad A Vermulst

Reviewer's report:

The authors did a great job with this systematic review of longitudinal studies about the relation between parent-child relationship (PCR) and adolescent alcohol drinking. The selection of the studies and the quality assessment of these studies is a very conscientious and time consuming job. I cannot improve this part of the review study and have no comments. The result of this article is clear, there is only weak evidence of PCR on adolescent alcohol use.
It is correct to preclude a meta-analysis because PCR and alcohol drinking were measured with different scales and heterogenic operationalization of alcohol use. The review study is restricted to longitudinal studies "while longitudinal studies can provide more evidence of a causal association because the cause precedes the effect in time". The 28 studies included this review article are summarized in Table 2. The last two columns of this table (analysis and results) need more attention because at a first glance the results in combination with the types of analyses were puzzling. To explain I have numbered the studies from 1 (Adrados, 1995) to 28 (Wu et al., 2006).

[RESPONSE]
Thank you for these encouraging comment, and for the meticulous advice regarding our presentation of our findings on which we report point by point in the further response.

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Study 1 for example has two waves and in combination with linear regression analysis I understand that one regression coefficient is reported. Study 2 has six waves and in combination with GEE also one regression coefficient is reported. Study 3 has four waves and in combination with HLM one regression coefficient is reported. Study 4 has 7 waves and in combination with latent growth curve modeling the result was ns. Study 7 has four and three waves and accordingly three and two regression coefficients were reported using logistic regression analysis. Why one regression coefficient in studies 2 and 3? And what is the meaning of these coefficients? These differences in reported results has to do with the way the data were analyzed. 12 studies used a form of regression analysis (linear regression, OLS-regression, logistic, loglog), 8 studies used SEM, 3 used GEE, 3 used HLM (as latent growth curve analysis), 1 used latent class growth analysis (LCGA) and tested differences between three class with ANOVA and 1 used ANOVA.

15 Studies (1,5,7-11,13,14,19,23-27) showed results in accordance with the number of waves. Of these studies, 9 used regression analysis and 6 SEM.

[RESPONSE]
Thank you for the comment. Our reporting of the findings of some indeed requires some additional explanation. As indicated by the reviewer, differences in reporting results has to do with the way the data were analyzed. We will explain this in our responses to the comments below. We added the following sentence to the Result section to introduce that additional explanations will be given (Results section, page 9, 3rd paragraph):

"Some studies showed results that need an additional explanation."

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1 Study (16) used GEE but I don't understand the results. "Predicted means at high/medium/low level of maternal warmth for boys: 1.29/ 1.85, p<.05/ 2.41, p<.05." I guess the high level (1.29) is the reference group? And: Predicted means at high/medium/low level of maternal warmth for girls: 1.58/ 1.48, ns/ 1.38, ns." Is the high level (1.58) the reference group?

[RESPONSE]
The high level group is indeed the reference group (1.29 and 1.58 for boys and girls respectively). We have clarified this by adding the word ‘REF’ to the category concerned in Table 2.

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12 Studies (2,3,4,6,12,15,17,18,20,21,22,28) showed results that need more explanation.
GEE. 2 Studies (2,12) used GEE. GEE is a repeated measures analysis for all types of dependent variables and this explains why one beta (for father and mother exclusive the interaction effects) or OR (for three levels of attachment) was reported. The waves were used as repeated measures and were combined into one main effect of PCR on the outcome variable.

[RESPONSE]
These results were not clear and need more explanation. We added the following text to the Results section (page 10, 1st paragraph) to clarify this:

“Andrews et al. [Andrews et al., 1997] and Droomers et al. [Droomers et al., 2003] did a GEE-analysis. In these GEE-analyses all waves were combined, leading to one estimate of the effect of the dependent variable on the independent variable and thus to only one beta [Andrews et al. 1997] or odds ratio [Droomers et al. 2003].”

HLM/LGCA. 3 Studies (3,4,17) used HLM or latent growth curve analysis (LGCA). For these studies HLM is equivalent with LGCA. HLM (LGCA) was used to estimate latent growth curves resulting in intercept (starting level of outcome variable) and slope (increase or decrease of the outcome variable over time) as latent variables. PCR was related to these two variables and not to the original wave scores. In study 4, a non significant result was reported (direct effect of support on alcohol misuse), but in the article of Barnes et al., an indirect effect was also reported via parental monitoring (p.182): “The coefficient from parental support (nurturance) to the alcohol misuse intercept (alcohol initiation) was statistically significant, indicating that the relationship in Figure 2 is truly mediated by monitoring.”

[RESPONSE]
For the studies 3, 4 and 17, the LGCA-analyses as used in study 4 were indeed equivalent to the HLM-analysis as used in the studies 3 and 17. We added the following text to the Results section (page 10, 1st paragraph) regarding this:

“Aseltine and Gore [Aseltine and Gore., 2000], and Gutman et al. [Gutman et al., 2011] used HLM-analyses which were equivalent to the LGCA-analyses as done by Barnes et al. [Barnes et al., 2000]. In all three studies latent growth curves were estimated resulting in one intercept and one slope for the PCR was related.”

Moreover, although the focus of this systematic review was on the direct effect of the PCR on alcohol use, study 4 reported an indirect effect. We have added a sentence on this to the Results section. This sentence is (page 10, 2nd paragraph):

“Although the focus of this systematic review was on the direct effect of the PCR on alcohol use, Barnes et al. [Barnes et al., 2000] found an indirect of the PCR on alcohol misuse that operated via parental monitoring.”

SEM. 3 Studies used SEM (6,21,22). For study 6 three waves were claimed but it is unclear how these three waves were used in the structural equation model. Study 21 has three waves but only one regression coefficient was reported. This can be explained by the model they used with PCR measured at T1 and alcohol use at T2. The reported beta of -.22 is significant with p < .01 (and not with p <.05). Study 22 used SEM in the form of a multiple mediation model. It is unclear how the three waves are used in this model. Only one beta is reported (for warmth and for tension), but there are three waves.

[RESPONSE]
In study 6 [Chuang et al., 2005] the PCR was measured at T2 and alcohol use was measured at T1 and T3. Alcohol use at T3 was predicted by the PCR at T2, controlling for alcohol use at T1 (and other variables as mentioned in Table 2). The authors did not report betas, but only reported that this association was not significant.

In study 22 [Latendresse et al., 2008] the PCR (i.e., warmth and tension) was measured at T1 and alcohol use was measured at T2 and T3. In the analysis alcohol use at T3 was predicted by the PCR at T1, controlling for alcohol use at T2 (and other variables as mentioned in Table 2).

We added the following text to the Results section (page 9, 4th paragraph): “Chuang et al. [Chuang et al., 2005] and Latendresse et al. [Latendresse et al., 2008] measured the PCR at one of the first two waves (T2 and T1, respectively) and alcohol use at the remaining two waves. In their analyses alcohol use at the last wave was predicted by the PCR, controlling for alcohol use measured at the other wave at which it was assessed in that study (T1 and T2, respectively).”

In study 21 [Kuntsche et al., 2009] indeed only two of the three waves were used for the analysis (PCR measured at T1 and alcohol use measured at T2), because alcohol use was not measured at T3.

We adapted the significant level to p<.01 in Table 2 and added the following text to the Results section (page 9, 4th paragraph): “A number of the studies reported only one beta or odds ratio while the study had more than two waves [Horton and Gil, 2008; Kosterman et al., 2000; Andrews et al., 1997; Droomers et al., 2003; Aseltine et al., 2000; Chuang et al., 2005; Kuntsche et al., 2009; Latendresse et al., 2008]. One of these studies used only two of the three available waves without a clear reason for this [Horton and Gil, 2008] whereas the other study did this because neither the PCR nor alcohol use was measured at the third wave [Kuntsche et al., 2009]. For the other studies that reported only one beta or odds ratio but the study had more than two waves, the data analytic approach provided an explanation for this.”

Regression. 2 Studies used a form of regression analysis (18,20). Study 18 has three waves but only one beta was reported. I guess that only two waves were used for the analysis. Study 20 has 8 waves and report only one regression coefficient. An explanation is that survival analysis was used.

[RESPONSE]
In study 18 [Horton and Gil, 2008] indeed only two of the three waves were used for the analysis (PCR measured at T1 and alcohol use measured at T2). Without clear reason the measurement of the PCR at T2 was not used. See our responses to the previous comment, including the presentation of the added text regarding study 18.

In study 20 a complementary log-log model was used for a survival analysis. It is more accurate to denote survival analysis, a form of regression analysis, as the method of analysis that was used in this study. We adapted this in Table 2. An explanation for the reporting of only one regression coefficient whereas the study has eight waves is indeed that survival analysis was used.

We have added a comment about this in the Results section (page 10, 1st paragraph). The added text is: “Kosterman et al. [Kosterman et al., 2000] used survival analysis which yielded only one beta based on the eight waves.”

LCGA with ANOVA. 1 study (15) used both techniques. It is unclear what the value F=2.67, ns means. Looking at the article first a LCGA analysis was done to detect subgroups (early onset, late onset and non users). In a second step differences between the subgroups with respect to PCR was tested with ANOVA.
Our description of this result was indeed not clear and needed more explanation. Therefore we adapted Table 2 regarding this and added the following text to the Results section (page 10, 2nd paragraph):

“Flory et al. [Flory et al., 2004] used LCGA with ANOVA. First a LCGA analysis was done which resulted in the identification of three subgroups: early onset, late onset, and non-users. Next ANOVA was done to test differences between the subgroups with respect to PCR.”

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ANOVA. 1 study used ANOVA (28) but results were reported in % and chi-square. How is this possible? In Table 1 of this article the reported mean for the use-group was 2.4 and for the non-use-group 2.3 (this is a mean score, not a percentage) The same applies for discipline. This was tested with ANOVA, F-values were not reported, but p-values were given (.278 and .155).

[RESPONSE]
We erroneously mentioned that a chi-square test was used. For the continuous variables ANOVA was used. We adapted Table 2 regarding this and added the following text to the Results section (page 10, 2nd paragraph):

“Also three other studies need an additional explanation. Wu et al. [Wu et al., 2006] used ANOVA for which F-values were not reported but p-values were given....”

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My suggestion is to include this information in the text as a kind of additional explanation for Table 2.

[RESPONSE]
We agree. The added information was mentioned in our responses to the previous comments.

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Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.
Referee 2

Reviewer's report

Title: The parent-child relationship and adolescent alcohol use: a systematic review of longitudinal studies

Version: 1 Date: 5 September 2012

Reviewer: Conor Gilligan

Reviewer's report:

The authors explore an important issue but I question what this paper adds to existing knowledge. The limitations of previous reviews are given brief mention but it need to be made more clear how this review fills a knowledge gap in the area.

It seems that the key premise for the review is that previous ones did not assess methodological quality. It should be explained more specifically that while Ryan et al did review longitudinal studies they did not assess methodological quality and used a somewhat questionable statistical approach of combing p values. In order to make the case that the present paper adds to the field, the authors should more extensively describe and justify the methodological rigour of their approaches.

For example, the domains of bias assessed are given only brief mention but could be described more fully with some justification for why these domains cover the key factors associate with methodological rigor and the interpretability of results. More could also be made of the best evidence synthesis process – this is a strength of the review ad could be described more fully.

[RESPONSE]
In order to make the added value of this paper more clear, we paid more attention in the revised manuscript to the limitations of previous reviews and to the use of the best evidence synthesis.

The fourth paragraph (page 3/4) in the Introduction is changed from:

“Although the influence of PCR has been studied, previous reviews did not evaluate the methodological quality of the studies included [Foxcroft and Lowe, 1991; Ryan et al., 2010; Vakalahi, 2001]. The evaluation of the methodological quality of primary studies in systematic reviews is important because it may have an important impact on the results of systematic reviews [Jadad and McQuay, 1996; Khan, 1996, Shamliyan, 2012] and might affect the implications and recommendations of a review. Furthermore previous reviews included both cross sectional and longitudinal studies [Foxcroft and Lowe, 1991; Vakalahi, 2001], while longitudinal studies can provide more evidence of a causal association because the cause precedes the effect in time [Rothman, 1998].”

to the following two paragraphs:

“The available reviews all have methodological shortcomings. A first one is that none of the previous reviews [Foxcroft and Lowe, 1991; Ryan et al., 2010; Vakalahi, 2001] did evaluate the methodological quality of the studies included. If summarizing the results of primary studies it is important to take into
account their methodological quality because this may have an important impact on the results of systematic reviews [Jadad and McQuay, 1996; Khan et al., 1996; Shamlayan et al., 2012] and on their implications and recommendations.

A second shortcoming of the available reviews was that they combined p-values to summarize findings [Foxcroft and Lowe, 1991; Ryan et al., 2010] or only gave a descriptive summary [Vakalahi, 2001]. A real synthesis of the best evidence helps to summarize the results taking into account the quality of the studies. This method draws conclusions based on the best available evidence or may conclude that conclusions cannot be drawn considering the currently available evidence [Slavin, 1995].

A third shortcoming is that the reviews of Foxcroft and Lowe [Foxcroft and Lowe, 1991] and of Vakahali [Vakahali, 2001] included both cross-sectional and longitudinal studies, while longitudinal studies can provide more evidence of a causal association because the cause precedes the effect in time [Rothman and Greenland, 1998]. Although Ryan et al. [Ryan et al., 2010] included only longitudinal studies they did not take into account whether or not previous alcohol use was accounted for in the analyses of the included studies. Controlling for the effects of previous alcohol use allows stronger statements to be made about the directionality of the association between the PCR and alcohol use."

We have added a reference:

Some general improvements could also be made to the writing.
Some specific comments:

1. I notice there is no abstract included

[RESPONSE]
We added an abstract and keywords to the document (page 1/2)

Introduction:

2. Paragraph 2 – sentence 1 needs a reference

[RESPONSE]
We have added a reference:


3. Paragraph 2 sentence 3 is unclear – I assume you are referring to negative PCR but please clarify. In the same sentence ‘frequently’ should be changed to ‘frequent’

[RESPONSE]
We agree that this sentence is not clear. We were indeed referring to a negative PCR. We have adapted the text regarding this. We also changed ‘frequently’ to ‘frequent’.
We adapted the sentence from:
“This construct is sometimes measured as the opposite of a warm and supporting relationship expressed in rejection, criticizing ideas frequently, having frequently arguments or withholding of affection [Maccoby 1992 and Wood et al., 2003].”

to

“This construct is sometimes measured as a negative PCR expressed in rejection, criticizing ideas frequently, having frequent arguments or withholding of affection [Maccoby, 1992 and Wood et al., 2003].” (page 3, 2nd paragraph)

4. Paragraph 3 – this is clumsily written. The two reviews exploring negative PCR could be easily covered in one sentence. The authors interchange between referring to a negative relationship with PCR and negative PCR. I think these terms have the same meaning and one should be used consistently throughout.

[RESPONSE]
We agree. We changed this paragraph from:

“There is also evidence that the PCR is related to adolescent alcohol drinking. Foxcroft and Lowe [Foxcroft and Lowe, 1991] have shown in a review that the PCR has a negative linear relationship to adolescent drinking. Another review of the effects of the PCR on adolescent substance use has also found that this was negatively related to substance use [Vakalahi, 2001].”

to

“There is also evidence that the PCR is related to adolescent alcohol drinking. Foxcroft and Lowe [Foxcroft and Lowe, 1991], Vakalahi [Vakalahi, 2001], and Ryan et al. [Ryan et al., 2010] have shown in their reviews that the PCR has a negative linear relationship with adolescent drinking.” (page 3, 3rd paragraph)

5. Page 3 paragraph 1, sentence 2 – this should refer to the results of primary studies as well as systematic eviews.

[RESPONSE]
We have fully adapted this paragraph to clarify its intended meaning, see above.

Methods

6. The search strategy could be described simply in a couple of sentences rather than Figure 1.

[RESPONSE]
We removed Figure 1 and added the following text to the Methods section (page 5, 1st paragraph):

“(1) alcohol us* OR alcohol drink* OR alcohol dependen* OR alcohol abus* OR alcohol consum* OR binge drink* OR heavy drink* (textwords), OR alcohol drinking (Mesh-term); (2) parent* (textword), OR parenting OR parents OR parent-child relations OR family OR child rearing (Mesh-terms); (3) longitudinal OR cohort OR follow-up OR prospective OR baseline OR mixture/mixed/growth model* OR growth curve* OR generalised/generalized estimating/estimation equation* (textwords), OR GEE (title or abstract), OR longitudinal studies (MeSH-terms).”
7. Quality assessment – the details relating to criterion K, C, E and F could be included as subscripts to the table OR the criteria should be included in the text rather than the reference numbers alone.

[RESPONSE]
Regarding criterion K we changed the text from:
“For the judgment of criterion K, requirements concerning assessment of alcohol consumption were taken into account [Greenfield and Kerr, 2008].”
to
“For the judgment of the validity and reliability of the measurement of alcohol use (criterion K), requirements concerning assessment of alcohol consumption were taken into account [Greenfield and Kerr, 2008].” (page 6, 3rd paragraph)

Regarding criterion C, E and F we removed the following sentence from the Methods section:
‘For the judgment of criteria C, E, and F an adequate participation or response rate was defined as > 80%, or as 60-80% and non-participation or non-response is not selective.”
And we added the following sentence as subscript to Table 1:
“An adequate participation or response rate was defined as > 80%, or as 60-80% and non-participation or non-response not selective.”

Results and Discussion

8. It would be helpful to include some description of the amount of ‘overlap’ with previous reviews. That is – how many of the included papers were also reviewed by Ryan et al, Foxcroft et al etc?

[RESPONSE]
We added the following sentence to the Results section (page 8, 2nd paragraph):
“Fifteen of these 28 studies were not included in the review of Ryan et al. [Ryan et al., 2010], 27 studies not in the review of Foxcroft and Lowe [Foxcroft and Lowe, 1991], and none of the studies were included in the review of Vakahali [Vakalahi, 2001].”

9. Study quality – page 8. How were the disagreements between assessors dealt with?

[RESPONSE]
In case of disagreement between the two assessors consensus was reached by discussion. If agreement could not be attained the third author could be consulted for a final judgment (Method, page 7, 1st paragraph). However, it turned out that this was not necessary. We have added a remark on this to the Results section (page 11, 2nd paragraph final sentence).
This sentence is:
“In all cases of disagreements, the two assessors could reach consensus during discussion.”

10. Some of the writing in the results is unclear. For example the sentence starting ‘However, Gutman et al.’ (page 9) does not indicate whether the Gutman paper is one of the five mentioned in the previous sentence or an additional one. Such poor clarity is found throughout.

[RESPONSE]
We added the following sentence to page 11 (3rd paragraph):
“However, the findings in some of these studies [Crawford and Novak, 2002 and Gutman et al, 2011] were equivocal.”

Furthermore we checked the Results section on vagueness in writing and corrected these (second and third sentence of 2nd paragraph on page 12 and final sentence of 3rd paragraph on page 12).

11. The authors refer on page 9 and page 10 to Table 4. I do not have a table 4 and assume they mean Table 2??

[RESPONSE]
On page 12 and page 13 (revised manuscript) we referred to Table 4. Because of the guidelines of the journal, this table was included at the end of the manuscript, and was not submitted as additional file – apologies for this, but we could not do this in a different way.
Note: Table 2 and 3 were separately uploaded because of its size.

12. The discussion needs some work based on my general comments above. The authors seem to suggest that improve PCR could potentially be detrimental. There is no evidence for this and it does not seem to be a logical argument following from the results.

[RESPONSE]
The first sentence of the final paragraph (page 18) of the Discussion might be confusing. Considering the results of our study it does not seem to be useful to focus on the improvement of the PCR in order to reduce alcohol consumption. We adapted this sentence from:
“Policymakers should be cautious in regard to developing prevention and intervention programs aimed at improving the PCR in order to reduce alcohol consumption, given the vagueness regarding causality.”
to
“Regarding reduction of alcohol consumption, the development of prevention and intervention programs by policymakers aimed at improving the PCR does not seem to be useful, given the unclear causality.”

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests

11