PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Maternal alcohol use disorder and child school attendance outcomes for non-Indigenous and Indigenous children in Western Australia: a population cohort record linkage study
AUTHORS	Hafekost, Katherine; Lawrence, David; O'Leary, Colleen; Bower, Carol; Semmens, James; Zubrick, Stephen

VERSION 1 - REVIEW

REVIEWER	Taisto Sarkola Childrens Hospital Helsinki University and Helsinki University Central Hospital Finland
REVIEW RETURNED	28-Dec-2016

GENERAL COMMENTS	This is a retrospective register-based cohort study titled ”Maternal alcohol use disorder and child school attendance outcome: A population cohort record linkage study”. The objective was to examine the relationship between maternal alcohol-use disorder and child school attendance outcomes in Western Australia. The study concludes that “Maternal alcohol-use disorder was associated with a significantly increased odds of poor school attendance.” Alcohol exposure was defined based on alcohol related diagnoses without further grading in severity of abuse or exposure level. The data set also includes a number of variables including a heterogeneous study population with different races and background variables making the assessment and reporting of confounding challenging. Major limitations include lack of paternal and CPS data. The line of reasoning was somewhat unclear to this reviewer throughout in the text making the interpretation of the data challenging although reading the text several times. Please find suggestions to improve the manuscript below. Title and Abstract The study reports major differences for different study populations (for indigenous and non-indigenous), this is, however, not reflected in the title. The line of reasoning in the abstract could be improved indeed: Please provide information on case and control cohort study population sizes, timelines/child ages at assessment, and estimates of primary outcome measures (i.e. school attendance) in the abstract. Currently odds for poor school attendance is reported without definition of the variable; if school attendance is recorded as a continuous variable please consider alternative statistical approach. The rationale for reporting results for timing of maternal diagnosis in relation to pregnancy and school attendance is unclear, please clarify. The results regarding race “Indigenous vs non-indigenous” could perhaps be more briefly reported as not connected with title or objective. The “time periods” of the maternal

	diagnosis is somewhat confusing in the abstract conclusion; this reviewer initially interpreted that the alcohol diagnoses were made antenatally for all cases as study population recruitment is based on Midwifes Notification system (i.e. antenatal, perinatal or postnatal diagnoses?) or is this related to different time periods somehow (?), please clarify, nevertheless: if reported in the conclusion there is a need to provide information and clarification related with this in the results for justification. Overall the conclusion seems to need more focus. Introduction The rationale for studying and reporting numbers for indigenous and non-indigenous populations separately and how this relates to the objective of the study is unclear and could perhaps be introduced in this section to help the reader. Subjects and Methods Please provide numbers for study populations (similar to numbers for school attendance records). Please also provide measures relating to size of study populations, size of background delivery population in the study area, and study time periods. Any estimates, information or measures for the association between the alcohol related diagnoses (what diagnoses?) mentioned and alcohol abuse or dependence would be of benefit to describe exposure. Please provide information on possibility of polydrug abuse among the exposed and how this was assessed. Please provide information on the use of Child Protective Services and paternal abuse data in case and control cohorts, if available. Alcohol exposure. The paragraph on the classification on the timing of the diagnosis is confusing. What is the rationale for this (i.e. why was this of interest)? Moreover the hierarchy described is difficult to comprehend indeed: e.g. alternative a) states “may include women who have an additional diagnosis before and/or after pregnancy”, alternative b) states “women with an additional exposure recorded more than one year before pregnancy or any exposure post-pregnancy”, alternative c) states “women who had a recorded exposure for more than one year before or after pregnancy”, alternative d) states “…more than one year before pregnancy, and this could include exposure greater than one year post pregnancy”, and e) states “more than 1 year after pregnancy.” What is the hierarchy (time in relation to pregnancy/delivery or duration of abuse problem or something else)? Overlap between categories? Could this be clarified or perhaps presented more clearly in a figure somehow? The proportion of children with no information on school attendance (i.e. main outcome) should be provided in order to put the data into perspective. Could the authors comment on the statement “In addition, students who had less than 30 per cent attendance were removed from the analysis to remove the impact of those leaving the Western Australian government school system or those who were not attending school.” Could this exclude the most severe end of the spectrum? Any comparison between exposed and non-exposed for excluded students available? How is the statement “For the purpose of this analysis, poor attendance was defined differently for non-Indigenous and Indigenous cohorts” in agreement with comparisons in main outcomes between these groups in the abstract? Risk for bias? How does this impact on data analyses? Overall the impression is that the main outcome (school attendance) is continuous. Please provide rationale for the different categories.

	This is largely reflected in the data analyses as well. School information. Please provide rationale and relevance for this information. How does this relate to the objective? Child variables. Do the authors have any information on neonatal abstinence syndrome for the exposed and non-exposed cohorts? Data analyses. This section is challenging to follow. Please provide rationale for “Indigenous and non-indigenous data were modelled separately” (why separately?) and for “the impact of maternal alcohol use on the number of days absent… within the exposed cohort” (why was non-exposed not included in the analyses?). Overall the combination of the different cohorts in the analyses related with significant confounding and how this is addressed in the analyses is unclear. Sensitivity analyses. Please provide rationale for using predefined cutoffs and categorized outcome data. Results Overall this section is difficult to follow as much of the text and tables focuses on reporting differences between indigenous and non-indigenous groups rather than focusing on the objective and main outcomes. Tables. The separation of the data into indigenous and non-indigenous groups makes the exposed vs non-exposed comparison (relating to the objective) in the table challenging. The results text first page refers to comparing groups but as no measures of the statistical testing is provided this process remain unclear. Table 4. Alcohol diagnosis. There seems to be no difference between different groups which is discordant with other variables suggesting a dose-trend (please provide numbers on statistical testing). Could this be related with a problem in the hierarchical classification of the variable or is school attendance not related with timing of diagnosis? What is the author conclusion of this? The sensitivity analyses could perhaps be moved to the Data Analyses section. SES and race could perhaps be taken into account when comparing school attendance between exposed and non-exposed cohorts. This aspect remain, however, unclear for this reviewer although reading the manuscript 2-3 times. The aspect of confounding could be presented more clearly. Discussion Numbers for main outcomes are reported in the first paragraph. Preferably this data could be reported in the results but this information is currently difficult to find in the results section. Overall the line of reasoning could be improved substantially. Lack of school attendance data (missing data) could be mentioned in the limitations. Lack of CPS and paternal data could be mentioned if applicable. Conclusion The paragraph is somewhat speculative and could be shortened and more focused. References With the submitted journal in mind, this reviewer suggests to prioritize original international study references (and journals) on the study subject over references (and journals) from the study continent in order to provide more credibility to the text and be of interest to the international readers. Please also double check references.

REVIEWER	Gabriel Gulis University of Southern Denmark, Unit for Health promotion research, Denmark
REVIEW RETURNED	03-Feb-2017

GENERAL COMMENTS	Congratulation to your work! In addition to the subject, I value very high use of existing large routine datasets!

REVIEWER	Shane Allwright, Professor emerita Trinity College Dublin, the University of Dublin, Ireland
REVIEW RETURNED	21-Mar-2017

GENERAL COMMENTS	This paper provides evidence linking diagnosed alcohol misuse of mothers with poor school attendance of their offspring. This is important as evidence is cited linking poor school attendance with 'negative outcomes such as greater risk of poor academic performance, risk taking behaviours, delinquency and early school dropout'. The paper finishes with a recommendation that there should be 'detailed and mandatory collection of data on alcohol use during pregnancy'. This would 'improve the quality of research and understanding regarding alcohol use and child outcomes' and would also 'improve the ability to intervene when risky drinking behaviours are identified during pregnancy or post-pregnancy'. The authors conclude that the similarity of the strength of association at each of the observed time periods suggests that, unlike some other studies, school attendance is driven by complex family and social environment interactions rather than by neurobehavioral effects of alcohol during pregnancy. If maternal alcohol misuse is indeed causally related to higher school dropout rate, providing ongoing support for families who are exposed to heavy maternal alcohol use may/'should' help to improve child outcomes. But, as pointed out by the authors, 'non-diagnosed' maternal alcohol mis-use, 'comorbidities, the family environment, and additional unmeasured confounders were not captured by administrative datasets'. There are a lot of 'ifs' here, and the authors need to be a little more cautious in their strongly worded Conclusions, including the Abstract Conclusions (as per Review Checklist points 2 and 11). This paper is well written and clearly presents the findings from large and complex linked data sets. However, further clarity could be brought to the Methods with regard to: • P7 para 1 – I found the description of hierarchy of alcohol diagnosis timings, (a) to (e), difficult to follow. Most readers won't want to refer to or (in my case) may not be able to access ref 18 for clarification. Perhaps a diagram would help clarify. • It would be helpful to non-Australian readers to state what age group Year 10 refers to. • P7 line 48-55. Data were collected for 2008-2012 for children in Year 10 and below. Average number of semesters of data per child = 3, i.e. one academic year. Not clear which 3 semesters were selected for each child, and why/how. A flow chart to show births and school record would help clarify this. Edits

	P9 line 26 - should read <.05 rather than =.05? P12 line 8 – HAD instead of HAS P12 2nd half of para – high parity is linked with increased risk for non-indigenous (as well as indigenous) mothers. The text as worded implies that this pertains only for indigenous mothers. P14 lines 13-15 - last part of sentence not clear. 'While any diagnosis of an alcohol-use disorder was associated with poor school attendance, in the final models there was little difference between the timing of diagnosis relative to pregnancy, or the strength of association with attendance outcomes.' Should OR be replaced by AND? P16 lines 28-30 state that 'an alcohol diagnosis recorded during pregnancy was not strongly associated with poor school attendance'. However, the 4 ORs all lie between 1.48-1.60 and all are statistically significant. Should this text read: 'an alcohol diagnosis recorded during pregnancy was not MORE strongly associated with poor school attendance THAN AT THE OTHER TIME PERIODS'? Tables T1 title – The table does not contain data referring to time of birth as per title. T1 - The heading for the last row should read 'child intellectual disability' (as per T5). T2 – I read the (Foot)Note to imply that there could be up to 5 records for some children (2008-12), but only one record per year? But the text states that the average number of semesters per child is (only) 3. Please clarify. T4 & T5 – see column heading 'OR*'. There is no reference to the asterisk in the footnote.

VERSION 1 – AUTHOR RESPONSE

Reviewer comment	Response	Page (tracked version)
Reviewer 1		
Title and Abstract The study reports major differences for different study populations (for Indigenous and non-Indigenous), this is, however, not reflected in the title.	Titled updated to: *Maternal alcohol use disorder and child school attendance outcomes for non-Indigenous and Indigenous children in Western Australia: a population cohort record linkage study*	1
The line of reasoning in the abstract could be improved indeed: Please provide information on case and control cohort study population sizes, timelines/child ages at assessment, and estimates of primary outcome measures (i.e. school attendance) in the abstract.	Additional information about the cohort and primary outcome measure has been included.	2
Currently odds for poor school attendance is reported without definition of the variable; if school attendance is recorded as a continuous variable please consider alternative statistical approach.	As above, the definition of poor school attendance for Indigenous and non-Indigenous cohorts has been included. Justification for treatment of the variable as categorical has been included in the methods.	2, 9
The rationale for reporting results for timing of maternal diagnosis in relation to pregnancy and school attendance is unclear, please clarify.	We have reworded the interpretation of outcomes relating to timing.	2
The results regarding race "Indigenous vs non-indigenous" could perhaps be more briefly reported as not connected with title or objective.	Indigenous and non-Indigenous results have been more briefly reported. In addition, we have updated the title and the aims to highlight that Indigenous and non-Indigenous results are reported separately.	2
The "time periods" of the maternal diagnosis is somewhat confusing in the abstract conclusion; this reviewer initially interpreted that the alcohol diagnoses were made antenatally for all cases as study population recruitment is based on Midwifes Notification system (i.e. antenatal, perinatal or postnatal diagnoses?) or is this related to different time periods somehow (?), please clarify, nevertheless: if reported in the conclusion there is a need to provide information and clarification related with this in the	The conclusion has been rewritten. The exposed group were identified via a diagnosis on a number of different administrative datasets (morbidity, drug and alcohol office, and mental health datasets). Therefore, diagnoses may have occurred at any time point to 2007. Unfortunately, due to the small word limit for the abstract, we were unable to add much additional information regarding the timing of alcohol diagnosis. However, this has been clarified in the methods. Further, we have included a diagram	2, 6, and Table2 supplementary material.

results for justification. Overall the conclusion seems to need more focus.	to assist the readers understanding of the hierarchical categorisation of timing of diagnosis in the supplementary material.	
Introduction The rationale for studying and reporting numbers for indigenous and non-indigenous populations separately and how this relates to the objective of the study is unclear and could perhaps be introduced in this section to help the reader.	Introduction has been updated to include more information about the differences in the school experience, educational outcomes and, in particular, attendance for Indigenous and non-Indigenous young people in Australia.	4,5
Subjects and Methods Please provide numbers for study populations (similar to numbers for school attendance records). Please also provide measures relating to size of study populations, size of background delivery population in the study area, and study time periods	Size of the background population, population cohort numbers, and number of linked records has been included.	6
Any estimates, information or measures for the association between the alcohol related diagnoses (what diagnoses?) mentioned and alcohol abuse or dependence would be of benefit to describe exposure.	The in-scope diagnoses, which have been previously published, have been included in supplementary material. We can be confident that those women who had a record of an alcohol related admission had consumed significant amounts of alcohol at that time point, and would require an overt alcohol related problem to receive a diagnosis on administrative datasets. However, a weakness of the study is that we cannot examine the relationship between records of recorded alcohol abuse and long term dependence. This has been highlighted in the limitations.	Table 1, Supplementary material,
Please provide information on possibility of polydrug abuse among the exposed and how this was assessed.	Information regarding poly-drug use is limited to administrative records relating to illicit drug use, which has been included in the analysis. However, this is likely to be a substantial underestimate of actual use in the population. The lack of information regarding comorbid drug use has been highlighted in the limitations section.	18

Please provide information on the use of Child Protective Services and paternal abuse data in case and control cohorts, if available.	Paternal factors, including drug and alcohol use, cannot be identified via by this data source. This has been highlighted as a weakness of the study in the limitations. Child protection data has been included as a potential covariate in modelling.	18
Alcohol exposure. The paragraph on the classification on the timing of the diagnosis is confusing. What is the rationale for this (i.e. why was this of interest)? Moreover the hierarchy described is difficult to comprehend indeed: e.g. alternative a) states "may include women who have an additional diagnosis before and/or after pregnancy", alternative b) states "women with an additional exposure recorded more than one year before pregnancy or any exposure post-pregnancy", alternative c) states "women who had a recorded exposure for more than one year before or after pregnancy", alternative d) states "…more than one year before pregnancy, and this could include exposure greater than one year post pregnancy", and e) states "more than 1 year after pregnancy." What is the hierarchy (time in relation to pregnancy/delivery or duration of abuse problem or something else)? Overlap between categories? Could this be clarified or perhaps presented more clearly in a figure somehow?	This information has been included as a figure in supplementary material. This classification has been used in previously published work, and its inclusion allows for comparison of results to those previously published. These comparisons are included in the discussion.	Table 2, supplementary material. 15
The proportion of children with no information on school attendance (i.e. main outcome) should be provided in order to put the data into perspective.	Information about unlinked records has been included in the methods. Around 31% of records, which were in-scope, could not be linked to attendance data. This is consistent with rates of independent and private school attendance in Australia.	7
Could the authors comment on the statement "In addition, students who had less than 30 per cent attendance were removed from the analysis to remove the impact of those leaving the Western Australian government school system or those who were not attending school." Could this exclude	A limitation of the use of administrative linked data is that we are unsure as to whether children with very poor attendance records remain within the government school system or even the state, or they have moved schools and/or locations and continue to be marked as absent. Of note, this	8

the most severe end of the spectrum? Any comparison between exposed and non-exposed for excluded students available?	disproportionately effects Indigenous students as they tend to be more mobile due to cultural reasons. Therefore, those children who attended less than 30 percent of days during the semester were considered unlikely to be in attendance. As such, they were removed from the analysis. This was viewed as a conservative approach. Records, rather than children were excluded based on the 30 percent cut point. This has been clarified in the text. As a result of this cut-point, 917 children were completely eliminated from the analysis (2% of linked sample). 81 percent were Indigenous, which reflects the bias mentioned above, and 44 percent were in the exposed cohort. This information has been included in the methods to assist the reader. In addition, references to support the high level of mobility of Indigenous students in rural and remote areas have been included.	
How is the statement "For the purpose of this analysis, poor attendance was defined differently for non-Indigenous and Indigenous cohorts" in agreement with comparisons in main outcomes between these groups in the abstract? Risk for bias? How does this impact on data analyses?	Substantial differences between the attendance distributions of Indigenous and non-indigenous young people resulted in two cut points being used. The outcome measures included in the abstract have been clarified and additional information regarding the decision to treat these populations differently has been included in the methods.	2, 8
Overall the impression is that the main outcome (school attendance) is continuous. Please provide rationale for the different categories. This is largely reflected in the data analyses as well.	Additional information has been included in the methods to explain the decision to treat school attendance as a binary outcome. The factors which lead to the use of a binary outcome include the highly skewed nature of the attendance distributions. The use of Poisson regression did not result in satisfactory model fit, and we were unable to find a suitable transformation for the data to normalise the distribution . In addition, categorisation, based on Department of Education cut-points was useful, and a shift out of 'poor attendance'	10

	categories is easier to interpret and perhaps more meaningful in this at risk population when compared to a shift of a number of days within a single category. For example, the impact of additional days absent on a student who would otherwise have zero days absent may be quite different from one who would otherwise have missed twenty days. Further, the use of a categorised outcome variable reduces the impact of outliers in the population without significant loss of data. This was of particular relevance in the Indigenous cohort.	
School information. Please provide rationale and relevance for this information. How does this relate to the objective?	This information was provided both for context and because school levels factors significantly impact on school attendance. This is supported by the reported results. For example, higher school level was associated with worse attendance. In addition, particularly in Indigenous young people, those attending remote schools were significantly more likely to have poor attendance. Not including these school factors may have unnecessarily confounded the results.	NA
Child variables. Do the authors have any information on neonatal abstinence syndrome for the exposed and non-exposed cohorts?	Unfortunately we do not have information on neonatal abstinence syndrome for either cohort.	NA
Data analyses. This section is challenging to follow. Please provide rationale for “Indigenous and non-indigenous data were modelled separately” (why separately?) and for “the impact of maternal alcohol use on the number of days absent… within the exposed cohort” (why was non-exposed not included in the analyses?). Overall the combination of the different cohorts in the analyses related with significant	The original population cohort selection (i.e. frequency matching on Indigenous status) was designed specifically to allow for Indigenous and non-Indigenous estimates to be produced. This was intended as the experience of these two groups is very different in Western Australia. This is evident in the vastly different school attendance profiles. However, we	1,2,12

confounding and how this is addressed in the analyses is unclear.	have evidently not made this reasoning clear in the manuscript and this has been clarified throughout. Results should largely be interpreted independently for Indigenous and non-Indigenous cohorts. As the reviewer has highlighted, the results section was unclear and appeared to contrasted Indigenous and non-Indigenous cohorts. This has been reworked to compare exposed and comparison groups within Indigenous status. The included comparisons aim to highlight the difference between the school experience and attendance profiles of these groups. We have included both overall population attributable fraction and attributable fraction amongst the exposed children to highlight that although only a modest proportion of children are exposed to maternal alcohol use disorder, the impact on those children can be substantial.	
Sensitivity analyses. Please provide rationale for using predefined cut-offs and categorized outcome data.	We have added additional justification of decisions regarding categorisation of outcome data (highly skewed attendance distribution, impact of non-attendance, interpretation of results). This is detailed above. Additional material has been added to the methods. To help inform whether the outcomes of the analyses were critically dependent on the cut-points chose, we undertook sensitivity analyses using alternative cut-points, and have reported that the final models were stable to different choices of thresholds.	10

Results Overall this section is difficult to follow as much of the text and tables focuses on reporting differences between indigenous and non-indigenous groups rather than focusing on the objective and main outcomes.	As mentioned above, Indigenous and non-Indigenous results were intended to be interpreted separately. This result have been updated to reflect this.	12
Tables. The separation of the data into indigenous and non-indigenous groups makes the exposed vs non-exposed comparison (relating to the objective) in the table challenging.	Again, we have attempted to refocus the results to allow for clear comparison of exposed and non-exposed groups. At the outset of the project it was anticipated that, where possible, that results for Indigenous and non-Indigenous groups would be modelled separately and so we have adhered to this. We hope rewording the results has made interpretation less challenging.	12
The results text first page refers to comparing groups but as no measures of the statistical testing is provided this process remain unclear.	As suggested, statistical tests have been included for comparisons within Indigenous status.	12, and tables 1 and 2
Table 4. Alcohol diagnosis. There seems to be no difference between different groups which is discordant with other variables suggesting a dose-trend (please provide numbers on statistical testing). Could this be related with a problem in the hierarchical classification of the variable or is school attendance not related with timing of diagnosis? What is the author conclusion of this?	Confidence intervals for all ORs are reported in the table. There was no evidence of any trend in association between timing of alcohol exposure and school attendance. We have no other variables relating to dose of exposure to alcohol. There are some limitations to the timing of exposure data. While the variable relates to the time at which the mother was treated for an alcohol use disorder, it is highly likely that many of these mothers have persistent alcohol use. As such, we cannot be sure that women who did not receive a diagnosis during pregnancy were not drinking heavily during this time period. With this limitation in mind, we tentatively conclude that these data suggest the poor school attendance of exposed children may not be driven entirely by timing of alcohol exposure and may be mediated by family and social factors such as persistent alcohol use and related impacts on family functioning.	15,17

The sensitivity analyses could perhaps be moved to the Data Analyses section.	As this includes results of an analysis, we have left this in the results section, to conform with the Journal's style.	NA
SES and race could perhaps be taken into account when comparing school attendance between exposed and non-exposed cohorts. This aspect remain, however, unclear for this reviewer although reading the manuscript 2-3 times. The aspect of confounding could be presented more clearly.	SES has been included in modelling. Race information refers to Indigenous and non-Indigenous status only. These have been modelled separately with the reasoning for this decision outlined above. However, to improve clarity we have removed any references to 'race' and replaced it with 'Indigenous status'.	1,6
Discussion Numbers for main outcomes are reported in the first paragraph. Preferably this data could be reported in the results but this information is currently difficult to find in the results section. Overall the line of reasoning could be improved substantially.	Numbers reported in the discussion have been removed. The discussion has been reworded and we hope this has improved reader understanding. The discussion has been reworked taking into account the reviewers' concerns. We hope the changes have improved the clarity of the text.	15-17
Lack of school attendance data (missing data) could be mentioned in the limitations. Lack of CPS and paternal data could be mentioned if applicable.	Missing school attendance data, and lack of paternal data have been added to the limitations. As noted above, CPS data have been included in the analysis.	17
Conclusion The paragraph is somewhat speculative and could be shortened and more focused.	The conclusion has been shortened.	17
References With the submitted journal in mind, this reviewer suggests to prioritize original international study references (and journals) on the study subject over references (and journals) from the study continent in order to provide more credibility to the text and be of interest to the international readers. Please also double check references.	References have been checked. Where possible we have referred to relevant International studies.	
Reviewer 3		
If maternal alcohol misuse is indeed causally related to higher school dropout rate, providing ongoing support for families who are exposed to heavy maternal alcohol use	The abstract conclusion has been reworded to reflect the degree of uncertainty.	2

may/'should' help to improve child outcomes. But, as pointed out by the authors, 'non-diagnosed' maternal alcohol mis-use, 'comorbidities, the family environment, and additional unmeasured confounders were not captured by administrative datasets'. There are a lot of 'ifs' here, and the authors need to be a little more cautious in their strongly worded Conclusions, including the Abstract Conclusions (as per Review Checklist points 2 and 11).		
I found the description of hierarchy of alcohol diagnosis timings, (a) to (e), difficult to follow. Most readers won't want to refer to or (in my case) may not be able to access ref 18 for clarification. Perhaps a diagram would help clarify.	As suggested, we have included a diagram in the supplementary material to clarify the hierarchical coding.	Table 2, Supplementary material
It would be helpful to non-Australian readers to state what age group Year 10 refers to.	Average age for those in year 10 is 15 years. This has been included in text.	8
P7 line 48-55. Data were collected for 2008-2012 for children in Year 10 and below. Average number of semesters of data per child = 3, i.e. one academic year. Not clear which 3 semesters were selected for each child, and why/how. A flow chart to show births and school record would help clarify this.	This has been updated to 'one per academic year'. First semester data was provided by the Department of Education (i.e. not selected), as data is more consistent (i.e. less affected by differences in Christmas holidays or end of year exams). This has been clarified in text.	7
P9 line 26 - should read <.05 rather than =.05?	Thank you, this has been updated in text.	10
P12 line 8 – HAD instead of HAS	Updated.	13
P12 2nd half of para – high parity is linked with increased risk for non-indigenous (as well as indigenous) mothers. The text as worded implies that this pertains only for indigenous mothers.	Updated to include results for both Indigenous and non-Indigenous mothers	12
P14 lines 13-15 - last part of sentence not clear. 'While any diagnosis of an alcohol-use disorder was associated with poor school attendance, in the final models there was little difference between the timing of diagnosis relative to	Updated	15

pregnancy, or the strength of association with attendance outcomes.' Should OR be replaced by AND?		
P16 lines 28-30 state that 'an alcohol diagnosis recorded during pregnancy was not strongly associated with poor school attendance'. However, the 4 ORs all lie between 1.48-1.60 and all are statistically significant. Should this text read: 'an alcohol diagnosis recorded during pregnancy was not MORE strongly associated with poor school attendance THAN AT THE OTHER TIME PERIODS'?	Updated	17
T1 title – The table does not contain data referring to time of birth as per title.	Updated	Table 1
T1 - The heading for the last row should read 'child intellectual disability' (as per T5).	Updated	Table 1
T2 – I read the (Foot) Note to imply that there could be up to 5 records for some children (2008-12), but only one record per year? But the text states that the average number of semesters per child is (only) 3. Please clarify.	Children who had an attendance record in years 2008 through 2012 inclusive, had 5 records. However, the average number of records was 3. This has been clarified.	Table 2
T4 & T5 – see column heading 'OR*'. There is no reference to the asterisk in the footnote.	Updated.	Tables 4 and 5

VERSION 2 – REVIEW

REVIEWER	Taisto Sarkola Children's Hospital, Helsinki University Central Hospital and Helsinki University, Helsinki, Finland.
REVIEW RETURNED	02-May-2017

GENERAL COMMENTS	The authors have addressed all comments provided by this reviewer in their revised manuscript and in their point-by-point response. The wordings in the limitations section and the conclusion has been adequately revised.

REVIEWER	Shane Allwright Trinity College Dublin, the University of Dublin, Ireland
REVIEW RETURNED	08-May-2017

GENERAL COMMENTS	The re-submitted version of the paper is much clearer. My comments have been addressed and I think the comments of the other reviewer have also been comprehensively dealt with. The table set out by the authors in their 'response to reviewers' document, outlining each review comment and the authors' responses, together with the tracked changes version of the paper, made it easy to check and I greatly appreciated this. One minor comment In the Methods, it states that records of children ‘with less than 30 percent attendance were removed from the analysis. Removing records of less than 30 percent attendance was viewed as a conservative approach to estimating the impact of maternal alcohol use diagnosis on attendance outcomes’. I think this should be cited again in the Discussion under the Strengths and Limitations heading as an additional possible contribution to biasing the findings towards the null. Typo End of 4th para of RESULTS section (Attendance profiles sub-heading) : 'when compared to those in the comparison cohort (non-Indigenous: 1.9 vs 4.8, p <0.001, Indigenous: 19.7% vs 30.5%, p < 0.001).' - add '%'s to 1.9 vs 4.8