

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Birth cohort trends in the global epidemiology of alcohol use and alcohol-related harms in men and women: systematic review and meta-regression
AUTHORS	Slade, Tim; Chapman, Cath; Swift, Wendy; Keyes, Katherine; Tonks, Zoe; Teesson, Maree

VERSION 1 - REVIEW

REVIEWER	Joel M Francis National Institute for Medical Research, Tanzania Harvard T.H.Chan School of Public Health, USA
REVIEW RETURNED	21-Apr-2016

GENERAL COMMENTS	<p>Thanks for an opportunity to review this well written paper addressing one of the main public health problem. The paper is well written and publishable.</p> <p>I have few observation for the authors to clarify:</p> <ol style="list-style-type: none"> 1. Inclusion of 3 databases only- this is not exhaustive and potentially have missed papers published in Africa. It would have been very helpful to include a geographical databases for example, Africa Wide Information, AJOL and perhaps Global Health. 2. I noted that cannabis was included as one of the search term but not described anywhere in the paper, it would be useful to clarify this. 3. I wonder whether authors reviewed other cross sectional studies reporting prevalences of use by gender?
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REVIEWER	Meenakshi Sabina Subbaraman Public Health Institute, Alcohol Research Group, USA
REVIEW RETURNED	07-May-2016

GENERAL COMMENTS	<p>This manuscript reviews the literature regarding gender differences in alcohol use and problems across birth cohorts. The review is comprehensive and a useful addition to the literature. My major concern regards the meta-analysis-- I am just not convinced that a meta-analysis is appropriate given the extremely wide variation in measurement across the studies included. The attempt to combine studies across so many countries that have different drinking patterns, drinking cultures, and ways of measuring alcohol use/problems exacerbates this concern. I strongly believe that the meta-analysis should be dropped, and that the manuscript is more compelling as a literature review. The results from the review would still seem to support the overall conclusions while avoiding statistical</p>
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	<p>issues.</p> <p>Other comments:</p> <ol style="list-style-type: none"> 1. The Intro is a little thin and could use some more fleshing out in terms of how health policies could be improved with a better understanding of these gender differences. Also, have any past studies speculated why the gap is closing? 2. The refs in the Intro appear misnumbered and do not match the ref list (paragraph 2 in particular) 3. Methods-- why are different search terms used for different databases? I might be misunderstanding, but this isn't clear to me. 4. The inclusion of high school and college students appears problematic given that we know young adults have different drinking patterns than older adults. 5. The inclusion of cannabis use is a bit much. I would take that out. 6. Figure 1 appears to be missing 15 studies (314 full text assessed -> 231 excluded -> 68 included?) 7. Methods, p. 11-- how is prevalence categorical and frequency continuous? 8. Results, p.15-- first paragraph talks about decreases in sex ratios but it is not clear which sex ratios? 9. P. 17-- please spell out what "OECD" countries are 10. Tables-- for the "Evidence of Convergence" column, it is a little confusing to read that "No, trends are similar for males and females" just because at first look it sounds like convergence (b/c it kind of sounds like they are the same). I wonder if there is a clearer way to state this, but probably fine if not.
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name

Joel M Francis

Institution and Country

National Institute for Medical Research, Tanzania
Harvard T.H.Chan School of Public Health, USA

Please state any competing interests or state 'None declared':
No competing interests

Please leave your comments for the authors below
Thanks for an opportunity to review this well written paper addressing one of the main public health problem. The paper is well written and publishable.

I have few observation for the authors to clarify:

1. Inclusion of 3 databases only- this is not exhaustive and potentially have missed papers published in Africa. It would have been very helpful to include a geographical databases for example, Africa Wide Information, AJOL and perhaps Global Health.

Author response: As Reviewer 1 notes, the databases searched were not exhaustive. A number of country and region specific databases have not been included. A sentence has been added in to the limitations section alerting readers to this.

2. I noted that cannabis was included as one of the search term but not described anywhere in the paper, it would be useful to clarify this.

Author response: The present study had a secondary aim of examining evidence for the closing sex gap in indicators of cannabis use and the screening protocol was designed to screen records for both alcohol and cannabis. Findings with respect to cannabis are presented in another paper (Chapman et al., under review). This is stated on page 10 of the manuscript and explanatory footnotes have been added to Tables 1-3 to avoid confusion.

3. I wonder whether authors reviewed other cross sectional studies reporting prevalences of use by gender?

Author response: As described on page 9 of the manuscript the search strategies were designed to retrieve both repeated cross-sectional studies that specifically examined sex convergence by birth cohort as well as single cross-sectional studies that reported lifetime estimates of prevalence of alcohol use or related harms by sex and age (as a proxy for birth cohorts).

Reviewer: 2

Reviewer Name

Meenakshi Sabina Subbaraman

Institution and Country

Public Health Institute, Alcohol Research Group, USA

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below

This manuscript reviews the literature regarding gender differences in alcohol use and problems across birth cohorts. The review is comprehensive and a useful addition to the literature. My major concern regards the meta-analysis-- I am just not convinced that a meta-analysis is appropriate given the extremely wide variation in measurement across the studies included. The attempt to combine studies across so many countries that have different drinking patterns, drinking cultures, and ways of measuring alcohol use/problems exacerbates this concern. I strongly believe that the meta-analysis should be dropped, and that the manuscript is more compelling as a literature review. The results from the review would still seem to support the overall conclusions while avoiding statistical issues.

Author response: The challenges inherent in pooling estimates across methodologically heterogeneous studies are significant. Despite this, we believe the meta-analysis contributes significantly to knowledge in this area by allowing us to empirically quantify birth cohort changes over time. This is the first time this has been done and extends previous narrative reviews in this area. The meta-regression allowed us to further explore the impact of between-study methodological differences and the results of this analysis are presented in the manuscript. The choice to pool estimates within three broad categories rather than across all studies was based on the fact that there was more homogeneity within these categories than across these categories. We also provided detailed

information about methodological differences between studies in Table 4. Within this context we believe this was a justifiable approach to our research question.

Other comments:

1. The Intro is a little thin and could use some more fleshing out in terms of how health policies could be improved with a better understanding of these gender differences. Also, have any past studies speculated why the gap is closing?

Author response: We have added a statement to the introduction highlighting the potential implications of gender differences in alcohol use, citing a recent synthesis of evidence around gender differences in the impact of alcohol policy interventions. We have also added in a statement expanding on the reasons for a narrowing gender gap over time. Finally, we have described two examples of the conflicting evidence emerging from the APC studies.

2. The refs in the Intro appear misnumbered and do not match the ref list (paragraph 2 in particular)

Author response: There were five references that were not properly converted in the Endnote referencing process. This has been corrected, a new reference list generated and all references have been checked against the new reference list.

3. Methods-- why are different search terms used for different databases? I might be misunderstanding, but this isn't clear to me.

Author response: Different databases use different Medical Subject Heading (MeSH) terms and different hierarchical structures to organize citations under these headings. Database specific terms were derived in consultation with the National Drug and Alcohol Research Centre librarian to ensure that appropriate MeSH terms were used in each search strategy. Despite these differences in search terms comparable information was collected across all databases.

4. The inclusion of high school and college students appears problematic given that we know young adults have different drinking patterns than older adults.

Author response: The review included studies of all adolescent and adult populations. Where appropriate each estimate extracted was mapped against both birth cohort and age at time of data collection. Age at time of data collection was included in the meta-regression to account for the potential confounding effect of different drinking patterns across specific age groups within the population.

5. The inclusion of cannabis use is a bit much. I would take that out.

Author response: The present study had a secondary aim of examining evidence for the closing sex gap in indicators of cannabis use and findings with respect to cannabis are presented in another paper (Chapman et al., under review). This is stated on page 10 of the manuscript, however, explanatory footnotes have been added to Tables 1-3 and to Figure 1 to avoid confusion.

6. Figure 1 appears to be missing 15 studies (314 full text assessed -> 231 excluded -> 68 included?)

Author response: Thank you for bringing this to our attention. In fact, the number excluded should have been listed in the flowchart in Figure 1 as 246 as 15 studies were excluded due to measuring cannabis only. We have corrected this in the flowchart in Figure 1.

7. Methods, p. 11-- how is prevalence categorical and frequency continuous?

Author response: We thank the reviewer for identifying this issue. Prevalence was generally measured as the number of cases, for example the number of people reporting any alcohol use in the last 12 months, and was always categorical, generally dichotomous. Frequency was either measured as an ordinal variable (e.g. never, less than monthly, monthly, daily alcohol use) or as a continuous variable (e.g. number of heavy episodic drinking occasions in the past year). We have now more accurately listed the frequency variables as continuous or ordinal.

8. Results, p.15-- first paragraph talks about decreases in sex ratios but it is not clear which sex ratios?

Author response: The findings refer to the meta-regression of indicators of any alcohol use. We have changed the results section to reflect this.

9. P. 17-- please spell out what "OECD" countries are

Author response: "OECD countries" has been replaced with "Organization for Economic Co-operation and Development (OECD) countries".

10. Tables-- for the "Evidence of Convergence" column, it is a little confusing to read that "No, trends are similar for males and females" just because at first look it sounds like convergence (b/c it kind of sounds like they are the same). I wonder if there is a clearer way to state this, but probably fine if not.

Author response: This has been replaced with "No convergence – trends are similar for males and females".

EDITORIAL REQUIRMENTS:

Please update the search to 2016.

The search cutoff for a systematic review is always arbitrary and thus, once published, the review will always miss some of the more recently published papers. Ideally the search cutoff date should be as close as possible to the publication date, allowing for analysis time and publication lag. In the present review, the breadth of analysis achieved by extracting estimates on all indicators measured in each study and individually mapping them to birth cohorts was important for the research question but it meant that the data extraction and analysis timeframe, carried out after the search, was necessarily long.

The studies included in the analysis currently span birth cohorts from 1891 up until 2000 with a combined sample size of over 4 million. It is almost certain that studies published within the last 2 years will not contain estimates from cohorts born later than 2000. Whilst it is important to keep monitoring population changes as more data become available, we feel this might be better achieved as current birth cohorts age and new younger cohorts reach drinking age. With this in mind, we have not updated the search to 2016. Whilst we appreciate why this recommendation was made, we hope that our reasons for not meeting it are reasonable.

Please explain how study quality was assessed and include the results of this.

Study quality was rated based on the critical appraisal tool for use in systematic reviews addressing questions of prevalence developed by Munn et al. (2014), as well as the study design and analysis

used to examine gender convergence in indicators of alcohol use and related harms. Level 1 studies were repeated cross-sectional studies that conducted Age-Period-Cohort analysis; Level 2 studies were repeated cross-sectional studies that separated age and cohort effects (either by presenting data across cohorts in a single age group or by presenting data across cohorts in separate age groups); Level 3 studies were repeated cross-sectional studies that did not attempt to separate age and cohort effects; Level 4 studies were single cross-sectional studies that reported lifetime estimates of at least one target indicator by sex and age groups (proxy for birth cohorts). Study quality was assessed for all included studies by two independent raters, with final ratings achieved through consensus. A description of this process has been added to the methods on page 12, and study quality ratings for individual studies have been included in Table 4 along with an explanatory footnote. The Munn et al (2014) reference has been added to the reference list.

We feel that these changes have greatly increased the clarity of the manuscript. Thank you once again for the reviewers' thoughtful comments. We hope you find it of interest to your journal.

VERSION 2 – REVIEW

REVIEWER	Joel Msafiri Francis National Institute for Medical Research, Tanzania Harvard T.H. Chan School of Public Health, USA
REVIEW RETURNED	02-Jul-2016

GENERAL COMMENTS	I have no additional comments. I am satisfied by author's responses to my earlier observations.
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